The Effects of the Opioid Crisis on Employment: Evidence from Labor Market Flows*

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Abstract

We show that the opioid crisis slows transitions to employment from unemployment and non-participation. We identify the effect of the opioid crisis from cross-state variation in triplicate prescribing regulations, which produced long-lasting reductions in opioid use by reducing the initial distribution of the blockbuster opioid OxyContin. Difference-in-differences estimates show that triplicate regulations induce unemployed and non-participating workers in triplicate states to return to employment about 10 percent faster than workers in non-triplicate states. These estimates imply a 1.1 percentage point higher level of employment in steady state.

Keywords: labor market flows; recession recovery; opioid regulation
JEL codes: J21, E24, K32, E61, E71

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1 Introduction

In the years since the mid-2000s, the opioid crisis in the United States has steadily worsened, culminating with 108,000 overdose deaths in 2021, more than in any prior year. At the same time, long term unemployment remained persistently high, especially in the wake of The Great Recession (Kroft et al., 2016). In this paper, we examine the role of the opioid crisis in explaining the slow exit from unemployment. Specifically, we estimate the effect of the opioid crisis on labor market transitions (flows) using quasi-exogenous variation in opioid availability across the country. We show that when availability is lower, people return to employment faster, with faster flows out of both unemployment and non-participation. Simple simulations based on these results imply that widespread opioid use and abuse stemming from availability results in slower employment recovery from recessions and lower employment in the long run.

Our work is motivated by the connection between economic conditions and mental health, which raises the possibility that economic downturns can cause people to slip into long-lasting addictive behaviors. For example, during periods of high unemployment, the hazard of initiating opioid use and alcohol abuse rises (MacDonald and Pudney, 2000; Dávalos, Fang and French, 2012; Carpenter, McClellan and Rees, 2017; Maclean, Horn and Cantor, 2020). The effects are broader than opioids: Bradford and Lastrapes (2014) finds that for each 1% decline in employment, prescriptions for drugs treating depression or anxiety climb by 10%. Over the longer run, economic declines are associated with worsening health and growing deaths of despair from suicide, alcohol abuse, and opioid abuse (Case and Deaton, 2017).[1] Similarly, declines in the stock market and housing wealth can cause acute mental health distress (Engelberg and Parsons, 2016; Jou, Mas and Vergara-Alert, 2020), and policy uncertainty increases risky behaviors such as binge drinking, smoking, and drug use (Kalcheva, McLemore and Sias, 2021).

[1] We note that unemployment insurance can reduce vulnerability to opioid addiction - at least among women (Ahammer and Packham, 2021).

Our central hypothesis is that the opioid crisis has increased the likelihood that unemployed people slip into drug use, slowing their re-employment. These effects occur throughout the business cycle and may be more pronounced in recession recovery. We provide direct evidence on this hypothesis by showing how opioid availability affects flows in and out of employment. This evidence complements existing research on the economic and financial effects of the opioid crisis. Cornaggia et al. (2022) uses a suite of causal inference methods to show that the opioid crisis lowered credit ratings, increased new offer yields, and reduced bond issuance in municipal finance markets. Using similar identification methods, Custódio, Cvijanović and Wiedemann (2020) shows that the opioid crisis increased mortgage delinquency and thereby lowered long-term home values. These papers find an indirect link between the opioid crisis and the macroeconomy, through financial markets. We show an additional link, through the labor market.

We make three contributions in this paper. First, we explicitly consider the role of the opioid crisis in the recovery of the labor market following recent economic downturns. Second, we focus the analysis on the effects on labor market *flows*, not levels, and we also convert the net effect of flows into steady state levels for comparison. While previous literature has focused on level effects, our focus on dynamics is natural given the evidence that unemployment may increase the risk of substance abuse, which would imply that access to opioids reduces the probability of returning to employment. The dynamic focus also implies that the effect of opioids is temporally heterogeneous, with more adverse labor market effects following negative shocks, an insight not evident from previous approaches; we provide a more detailed motivation for our focus on flows in Section 2.2. Our findings complement a set of related papers examining the effect of the opioid crisis on employment levels (Powell, 2021; Harris et al., 2020; Currie, Jin and Schnell, 2019; Aliprantis, Fee and Schweitzer, 2019; Savych, Neumark and Lea, 2019; Beheshti, 2022; Park and Powell, 2021; Laird and Nielsen, 2016). Our work is the first to examine the effect of the opioid crisis on labor market flows in this set of papers. We provide more detail on how our results relate to this literature and particularly the closely related Powell (2021) in Section 5.5.

Our third contribution is to use a new source of identifying variation to estimate the labor market effects of the opioid crisis. As this crisis has affected the entire country, obtaining valid control groups has proven difficult. We use state-level variation in triplicate prescribing regulation as a quasi-exogenous shock to opioid use. The regulation requires physicians to issue prescriptions for opioids and certain other controlled drugs using three copies for record-keeping and monitoring. The regulation predates the opioid crisis by several decades. As Alpert et al. (2022) shows in their seminal work, states with triplicate regulation experienced dramatically lower rates of opioid mortality, driven by lower use and ultimately lower marketing of blockbuster drug OxyContin.[2] We estimate differences-in-differences models of the impact of triplicate regulation on labor market flows in the post-1996 period. With the context from Alpert et al. (2022), we view our results as reduced form estimates of the impact of reductions in the opioid crisis on employment flows.

We note that while the triplicate regulation directly targeted prescription availability, they led to changes in the overall drug environment that limited the availability of other opioids such as heroin or fentanyl in particular. For example, in 2010, when the original formulation of OxyContin was removed from the market and replaced with an abuse-deterrent version, large differences in overdose deaths involving heroin and synthetic opioids emerged across triplicate and non-triplicate states. This is consistent with prior evidence that areas with early exposure to OxyContin experienced differential transitions to illicit opioids post-reformulation as people substituted from OxyContin to heroin (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2019).

Our analysis uses monthly data from the Current Population Survey (CPS) on labor market status over 29 years (1991-2019) to examine the effects of opioid availability on a comprehensive set of labor market transitions between employment, unemployment, and non-

[2]The central role of OxyContin in the opioid crisis is consistent with prior evidence that the reformulation of OxyContin ignited a heroin epidemic (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2019).

participation. This period includes three recessions and their recoveries, with one occurring prior to widespread opioid use and abuse. As Davis, Faberman and Haltiwanger (2006) shows, these data underscore the lumpy nature of micro-level employment adjustments. We also compute the steady state effects implied by the treatment effects on the monthly labor market transitions.

Our main result is that mitigating the opioid crisis via triplicate regulation has positive effects on labor market flows. Triplicate states experience an 8.1% faster flow from unemployment to employment, and 12.3% faster flow from those not in the labor force to employment.

Additional analyses reveal that our main estimates on labor market transitions are present across many race/ethnicity, age, and education groups, and higher for those employed in physically demanding occupations. The results we document are robust to including covariates, a stacked DID design with each treated state and its bordering states comprising a group with a common time trend, and dropping one treated state at a time. We also demonstrate that our employment results are too large to be driven by the greater number of opioid overdose deaths in non-triplicate states.

To better interpret the estimates, we calculate the implied effect of triplicate regulation on steady state employment. Our estimates suggest that triplicate regulation increases the employment-to-population rate by 1.1 percentage points, about 1.7% of the mean. This employment increase comes in roughly equal parts from reduced unemployment and reduced labor force non-participation. A simulation analysis shows that states with triplicate regulation in force experienced a 10.6% faster employment recovery from an unemployment shock equal to the magnitude of the Great Recession.

This paper proceeds as follows. Section 2 describes the triplicate regulations and motivates our study of labor market flows; Section 3 details the empirical methods for estimating the dynamic and steady state results; Section 4 provides an overview of the CPS data and construction of labor market transition variables, along with a discussion of summary statistics; Section 5 presents and interprets these results; Section 6 contains additional analyses and robustness checks; and Section 7 concludes.

2 Background

Here, we describe the triplicate regulation that provides quasi-exogenous variation in the extent of opioid use across states. We also provide the basis for our focus on labor market flows.

2.1 Triplicate regulations and the opioid crisis

Triplicate regulations are a set of state-level regulations requiring that prescriptions for certain pharmaceuticals must be written in triplicate. Copies of the prescription go to the patient, the pharmacy, and the state. Triplicate regulations typically apply to all "Schedule II" drugs, which include stimulants, narcotics, and barbiturates. The goal of the regulations is to facilitate tracking of these substances and preventing misuse, abuse, and diversion to illicit markets. Recent work demonstrates that prescription monitoring regulations in general may reduce some marginal prescribing by introducing hassle costs to the process (Alpert, Dykstra and Jacobson, 2020).

State-level triplicate regulation dates back to 1939, and they were not enacted with the opioid crisis in mind. Yet seminal work by Alpert et al. (2022) shows that triplicate regulations had a profound effect on opioid prescribing and the subsequent opioid crisis. Specifically, when Purdue Pharma brought the blockbuster opioid OxyContin to market in 1996, five states had triplicates regulations in force (California, Idaho, Illinois, New York, and Texas). Purdue's focus groups revealed that physicians in these states would be unlikely to prescribe OxyContin in high volume, because triplicate regulations had created a general reluctance to prescribe narcotics. Purdue therefore decided not to market OxyContin in these states.[3]

As Alpert et al. (2022) shows, this decision had long-lasting consequences. Dispensing of oxycodone—the active ingredient in OxyContin—grew dramatically faster in non-triplicate states, and by 2010 was threefold more in these states compared to triplicate states. Hydrocodone, an opioid that was not subject to triplicate prescribing (because of a lower risk of potential abuse), shows no such differential trend (See Appendix Figure B1a – we note that although *pure* hydrocodone, as studied in this exhibit, was not subject to triplicate regulation, Beheshti (2022) discusses how hydrocodone *combination* products such as Vicodin, which comprise the majority of hydrocodone prescribed, did not require triplicate prescribing). As OxyContin use skyrocketed in non-triplicate states, opioid overdose deaths also erupted. Prior to OxyContin's emergence in 1996, drug overdoses were higher in triplicate states than in non-triplicate states, but after 1996 the subset of opioid overdoses accelerated in the latter group, resulting in far more opioid overdose deaths (See Appendix Figure B1b).

The importance of triplicate regulations grew over time as the opioid crisis worsened. The opioid crisis has been characterized by different "waves", as shown in Appendix Figure B2. Prior to the early 2000s, drug death rates (a proxy for the incidence of opioid-related mortality) were roughly three per 100,000 population and triplicate states actually had a higher rate than non-triplicate states (Buckles, Evans and Lieber 2020). The CDC reports opioid overdose deaths starting in 1999, thus for many, the 1996-2000 period represents the first distinct wave of the opioid crisis as deaths began to appear in greater numbers in these years. During the second wave (2001-2010), there were rapidly rising deaths from prescription opioid use (including methadone, a prescribed narcotic). Lastly, the third wave (2011-present) is marked by a spike in deaths due to heroin overdose (2011-2015) and synthetic

[3] All these states discontinued their triplicate regulation by 2004. Their initial triplicate regulation in 1996 had long-lasting effects, however, for two reasons. First, prescription drug advertising ("detailing") typically targets high volume prescribers (see, e.g., Manchanda, Rossi and Chintagunta 2004), so low initial prescribing would result in less advertising and prescribing going forward. Second, opioids are addictive (Barnett, Olenski and Jena, 2017; Barnett et al., 2019; Eichmeyer and Zhang, 2021), so low initial demand is likely self-perpetuating. The triplicate regulation was broadly replaced by prescription drug monitoring programs, forms of which have been shown to decrease opioid abuse (Buchmueller and Carey, 2018).

opioids overdose such as fentanyl (2016-present).[4] These waves are linked because much of the later abuse of illegal (non-prescription) opioids appears to have grown out of addiction to prescription opioids, in particular OxyContin (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2019). Alpert et al. (2022) shows that, as the opioid crisis worsened, the overdose-reducing effects of triplicate regulation grew stronger. The effect on overdoses is weakest in the 1996-2000 period, stronger in 2001-2010, and strongest on 2011-2019.

To summarize, five states had triplicate prescribing regulation in place in 1996, when Purdue Pharma began marketing OxyContin. These states experienced dramatically lower rates of opioid dispensing and overdoses over the next 25 years, relative to non-triplicate states. The overdose reductions were greater in the post-2000 period, when the opioid crisis worsened.

2.2 Motivation for studying labor market flows

The previous section establishes that triplicate regulation, interacted with OxyContin availability starting in 1996, produces a shock to general opioid availability because of the downstream consequences of addiction caused by OxyContin entry. We expect that this shock affects employment by increasing the likelihood of legitimate drug use and misuse. We consider this impact on drug use and misuse as the "first stage," and we rely on Alpert et al. (2022) for that evidence. The present paper instead contributes the "reduced form" impact of triplicate regulation on labor market flows.

Our focus on flows is based on two hypotheses. Our first hypothesis is that the first stage effect of opioid availability on drug misuse varies with employment status. The main reason for this hypothesis is that unemployment is a time of exceptionally low subjective well-being (e.g., Di Tella, MacCulloch and Oswald 2001), and people may turn to drugs to alleviate

[4] If individuals are induced to illegal substances by opioid availability and develop criminal records, this could have a causal effect on their future employment (Agan and Starr, 2018; Doleac and Hansen, 2020). Recessions also increase eviction, which are linked with more drug mortality (Bradford and Bradford, 2020).

their mental pain. For example, Darden and Papageorge (2020) provides some evidence in support of this idea of "rational self-medication". Empirically, drug misuse is strongly associated with non-employment. We show in Appendix A that employed individuals in the US are much more likely to report drug misuse, and also that they are more likely to initiate new drug misuse, than the currently employed.[5] The greater level of drug misuse in unemployment suggests that opioid availability has a greater effect on drug use on individuals who are unemployed.

Our second hypothesis is that drug misuse affects employment rates by reducing the job finding rate of the unemployed, although drug use – not necessarily misuse – may reduce the job separation rate of the employed. On one hand, drug misuse can reduce the job finding rate by reducing overall functioning or by disqualifying candidates (because of drug testing). Consistent with this hypothesis, several studies find negative effects of drug misuse on employment (French, Roebuck and Alexandre 2001; DeSimone 2002; MacDonald and Pudney 2000; Greenwood, Guner and Kopecky 2022; see also the review by Cawley and Ruhm 2011). On the other hand, painkiller use may make it easier to remain employed. Garthwaite (2012) finds that the withdrawal of the anti-arthritis medication Vioxx reduced employment in physically demanding occupations. Krueger (2017) documents the high levels of pain among the unemployed, raising the possibility that untreated pain reduces work capacity.

These hypotheses motivate our focus on flows in three ways. First, they imply that the effect of triplicate regulation may be heterogeneous by employment status, with potentially positive effects on flows out of unemployment (less drug misuse) but negative effects on flows out of employment (less legitimate use of painkillers). Focusing on flows allows us to better understand the mechanism behind any impact of triplicate regulation on employment.

[5] We note that earlier work in Ruhm (2000) suggests the opposite direction – that recessions could improve health, as marked by reduced smoking, greater physical activity, and improved diet. Suicides are an important exception in this work, as they consistently increase during worse economic conditions. Echoing this stylized fact, more recent research as in Ruhm (2003) and Ruhm (2015) suggest that poor mental health and drug use are *counter*-cyclical, i.e., increase during worse economic conditions.

Second, understanding the impact of triplicate regulation on labor market flows is important because of the distinct policy implications. For example, if we know that opioid exposure brings down the unemployment-employment flow more heavily than it does the nonemployment-employment flow, it implies that unemployed individuals are more marginalized to the risk of opioids and should be protected from opioid abuse. Limiting the analysis to levels, however, would not capture such mechanisms.

Third, these hypotheses imply a history-dependent effect of opioid availability and triplicate regulation on employment levels. If unemployment exogenously increases in one period (for example, because of a recession), then heightened opioid availability may increase drug misuse of more people, leading to lower future employment. This history dependence makes it difficult to estimate long-run effects on employment levels. Our focus on employment flows accounts for history dependence (by conditioning on prior employment level) and allows us to simulate long-run, steady-state effects on employment levels.

3 Empirical Strategy

The triplicate regulation motivates our difference-in-differences (DID) specification to estimate the effects on labor market flows. To better interpret the magnitude of these flow effects, we explain how we translate flow effects into steady state level effects.

3.1 DID estimation on labor market flows

Motivated by the powerful role of triplicate regulations in mitigating the opioid crisis, we begin by estimating pooled DID regressions of the following form:

$$y_{s,t} = \alpha_s + \gamma_t + \delta \times \mathbb{I}(Triplicate_s)\mathbb{I}(1996 \leq year_t \leq 2019) + \epsilon_{s,t}. \quad (1)$$

Here s denotes state and t denotes the month-of-sample. Our outcomes are the six labor market flows, i.e., all the pairwise flows between employment, unemployment, and non-participation. Our interest is in δ, the effect of triplicate regulation over the post-1996 period. We control for state fixed effects α_s and time fixed effects γ_t. A positive value of δ means a greater flow for triplicate relative to non-triplicate states, in the post-OxyContin period. For example, if the outcome is the unemployment-to-employment flow, $\delta > 0$ means that people transitioned out of unemployment and into employment faster in triplicate states than non-triplicate states, relative to the pre-1996 difference.

We additionally estimate the following DID regressions to examine heterogeneity by wave of the opioid epidemic:

$$
\begin{aligned}
y_{s,t} = \alpha_s + \gamma_t \quad &+ \quad \delta_1 \times \mathbb{I}(Triplicate_s)\mathbb{I}(1996 \leq year_t \leq 2000) \\
&+ \quad \delta_2 \times \mathbb{I}(Triplicate_s)\mathbb{I}(2001 \leq year_t \leq 2010) \\
&+ \quad \delta_3 \times \mathbb{I}(Triplicate_s)\mathbb{I}(2011 \leq year_t \leq 2019) + \epsilon_{s,t}, \qquad (2)
\end{aligned}
$$

where the δ coefficients capture the effect of triplicate regulation in three separate post periods.

These specifications imply a constant effect of triplicate regulations on labor market flows (in a given time period). The constant *flow* effect, however, implies heterogeneous *level* effects, with potentially greater effects following negative shocks. For example, consider a shock which increases the stock of unemployed workers equally in triplicate and non-triplicate states (for example, the onset of the Great Recession). If triplicate regulations increase the flow out of unemployment into employment, i.e., $\delta > 0$, then all else equal the recovery from this shock will be faster in states with triplicate regulations, meaning that the effect of triplicate regulations on employment levels grows over time.

Inference in our setting can be challenging because of the small number of treated states. We provide two complementary approaches to inference. First, following standard practice in DID settings, we report confidence intervals based on cluster-robust variance estimates, with state-level clustering. Second, we calculate confidence intervals based on a clustered wild bootstrap, which is potentially more appropriate in settings in which large-sample assumptions may not hold due to the small number of treated units (Roodman et al., 2019). We use a six-point weight distribution (Webb, 2013) as in Alpert et al. (2022). Recent results in Hahn and Liao (2021) indicate that bootstrapped standard errors are larger than true standard errors, so our bootstrap approach may be conservative.

3.2 Paths from triplicate regulations to employment flows

Because triplicate regulations are state-wide interventions, there are many causal paths from the regulations to employment flows. The direct path is that some patients may have doctors who, counterfactually, would have prescribed OxyContin in the absence of triplicate regulation, and those patient would have, counterfactually, become a heavy user and unable to hold down or return to work. This heavy use could be legitimate but reduce work capacity, or it could be misuse. However, other indirect paths are possible.

One set of paths operates through equilibrium responses in the illicit drug market. Heightened OxyContin prescribing increases the number of people with some physical dependence on opioids, and hence increases the demand for opioids or painkillers more generally. Heightened opioid demand in turn may increase the attractiveness of heroin and fentanyl dispensing. Quinones (2015) presents ethnographic evidence consistent with this channel, arguing that people addicted to OxyContin turned to heroin as its relative price per effective dose fell. Also consistent with this view, Alpert, Powell and Pacula (2018) and Evans, Lieber and Power (2019) show that the 2010 reformulation of OxyContin led to a surge in heroin overdoses. The induced entry of heroin dealers could in turn increase the use of heroin, even among people whose doctors do not counterfactually (absent triplicate regulation) prescribe them OxyContin. Another set of paths operates through the labor market. Heightened opioid use in general may lead to increased drug testing of job candidates. This testing can discourage or disqualify applicants who do not use OxyContin in particular.

Thus there are many channels through which triplicate regulations can affect employment flows, and our DID estimators will capture their combined effect, as long as they are specific to triplicate states. In light of these many channels, we interpret our evidence as reflecting the effect of increased opioid availability, including both greater use as well as misuse.

3.3 Steady state implications and adjustment dynamics

The effect of triplicate regulations on labor market flows, δ, can be difficult to interpret because it does not easily translate into a level effect; in general, the implications for employment levels depend nonlinearly on each flow effect. For example, whether triplicate regulation increases or reduces employment levels depends on how it affects flows out of employment and into employment, from both unemployment and from non-participation. To ease the interpretation, we therefore translate our six flow estimates into effects on steady state employment, unemployment, and non-participation.

The estimation of steady state effects requires three steps, described more completely in Appendix F. First, we calculate the steady state levels of labor market activity for triplicate states as implied by the flow summary statistics. Then, we calculate the counterfactual labor market flows in triplicate states, absent the effect of triplicate status on flows (i.e. we subtract off our differences-in-differences estimates), and on counterfactual steady state levels. In the third step, we subtract the counterfactual steady state from the triplicate steady state to obtain estimates of the triplicate effect on steady state levels of labor market activity.

To summarize, we determine steady state effects by calculating the change in steady state employment, unemployment, and non-participation for the triplicate states, had they lost the protective effect of triplicate status. This effect depends on both the magnitude of the triplicate flow effect as well as counterfactual flows (which we measure as actual flows less the triplicate effect). The steady state effect can be interpreted as a "treatment on the treated" parameter, i.e., the effect of triplicate status on triplicate states during our sample period.

4 Data on labor market flows

We measure monthly labor market transitions using the Current Population Survey (CPS). The CPS is well-suited for estimating the effects of the opioid crisis on employment recovery due to its large, regionally and nationally representative sample containing information on employment status and occupation along with demographic characteristics of potential interest such as age and race. We use the longitudinal nature of the survey to measure labor market transitions. The survey features a 4-8-4 rotating panel design; household members are interviewed for four consecutive months, excluded for eight months, and then again included for four more consecutive months. New rotation groups are brought into the CPS sample each month following a uniform distribution across the sampling months. Labor market transitions can be measured when respondents are surveyed in consecutive months.

Following Alpert et al. (2022), we begin the analysis in 1991, five years prior to the release of OxyContin. Our analysis ends in 2019; we exclude 2020 because of the abnormally large labor market flows in that year due to the COVID-19 pandemic. We also exclude June to September 1995 because changes in the household numbering system in the CPS made it infeasible to link respondents longitudinally (Flood et al., 2020).

4.1 Defining the transition rates

Following Davis, Faberman and Haltiwanger (2006), we consider three labor market states: employed, unemployed, and not participating in the labor force. There are thus six between-state transitions, which we construct using respondents' labor market statuses. We begin by constructing indicators for employed $E_{i,t}$, unemployed $U_{i,t}$, and non-participation $N_{i,t}$, for each person i in month t. We define individual-level flows as transitions between states: for example, the unemployment-to-employment transition is $E_{i,t}|U_{i,t-1}$.

We aggregate the individual labor market flows to the state-month-year level (treating Washington, D.C. as its own state). This process generates three labor market status counts and six transition counts for each of our 17,544 month-year observations.[6] We define rates using the relevant denominator: for example, the unemployment-to-employment transition rate is $\frac{E_{s,t}|U_{s,t-1}}{U_{s,t-1}}$, where $E_{s,t}|U_{s,t-1}$ is the mass of unemployment to employment transitions in s between $t-1$ and t, and $U_{s,t-1}$ is the unemployment mass in $t-1$.

In our main analysis we work with flows aggregated to the state-month-year level. In heterogeneity analyses we aggregate to a finer level, for example by within-state age group, race, or occupation characteristic.

4.2 Summary statistics

Table 1 presents summary statistics for our sample of CPS respondents aged from 15 to 64. We provide this information for the baseline period of 1991-1995, prior to the entry of OxyContin in 1996. Columns 1 through 5 contain summary statistics for each triplicate state, while columns 6 and 7 aggregate the information to triplicate and non-triplicate states. Triplicate states have a larger Hispanic population than the nation as a whole. In terms of education, industry, and occupation[7], however, the groups are relatively similar. Triplicate states are also especially large in population.

Appendix Figure C1 shows how the labor market flows vary by triplicate regulation

[6] (29 years $\times$ 12 months - 4 months (June - Sept. 1995)) $\times$ 51 states = 17,544 observations. Note that the CPS contains 89,014 individual observations per month-year, on average, and 2,797,727 unique individuals over the time period studied. We apply the CPS sampling weights to aggregate to the state-level.

[7] We categorize physically demanding occupations by using skill measures in Acemoglu and Autor (2011), which is based on task measures in the Occupational Information Network (O*NET); the classification of occupations by whether they are physically demanding jobs is provided in Appendix C.3.

over time.[8] Unsurprisingly, recessions are marked by periods when flows to employment fall sharply and flows to unemployment rise sharply. The falls are roughly similar for triplicate and non-triplicate states. Prior to OxyContin's introduction in 1996, flows to employment are lower in triplicate than non-triplicate states, but they equalize by the worsening of the opioid epidemic in the mid-2000s.

5 Results

5.1 Pooled estimates

We begin with the pooled regression results in Table 3 to understand the aggregate effect of triplicate regulation on the labor market. These estimates correspond to equation (1). The columns separate the flows into three sets by destination: the first two are flows into employment, the next two are flows into unemployment, and the final two are flows into non-participation. The table also reports the counterfactual baseline to help assess the magnitude of coefficients; this baseline is obtained by subtracting the DID coefficients from the labor market flows for triplicate states over the post period. We report two types of 95% confidence intervals: ones based on cluster-robust standard errors are in parentheses, and ones based on the wild bootstrap are in brackets.

We observe that triplicate states experience a monthly increase in flows into employment in 1996-2019 (relative to 1991-1995): there is an 8.1% increase over baseline in the flow from unemployment to employment (column 1), and a parallel 12.3% increase in the flow from non-participation to employment (column 2). Employment is the largest state, so these two flows are important for considering the effect of triplicate regulation on the labor market.

In principle the greater flows to employment could come from reduced flows either to unemployment or to non-participation. In practice we find they come from reduced flows to

[8] Table 2 provides this information in table format, summarized by the time periods studied.

unemployment, as we show in columns 3 and 4. The flow from employment to unemployment declines by 6.8%, and re-entering the labor force through unemployment declines by 8.8%. We see slight increases in flows to non-participation in columns 5 and 6. We observe 7.5% and 0.9% greater flows from employment and unemployment to this state, respectively. These findings are a bit of a puzzle; one hypothesis is that some people would be able to work only with access to OxyContin, such as people in physically demanding occupations. We explore this hypothesis in Section 6.1, but preview here that we have limited power to investigate it fully.

The major finding from our analysis is that triplicate regulation substantially increases flows from both unemployment and non-participation into employment. This result is consistent with our hypothesis that triplicate regulation helps individuals find and maintain employment as they are less vulnerable to opioid addiction or related problems.

While our focus is on labor market flows, in Appendix Table C1, we report effects on labor market levels. Consistent with our flow effects, we find positive effects on employment and negative effects on unemployment and non-participation. These estimates are not as precisely estimated, however.

One might ask whether employment results could be driven by differential mortality, as Alpert et al. (2022) shows opioid-related mortality grew substantially slower in triplicate than non-triplicate states after the release of OxyContin. The magnitude of the mortality effect appears to be too low, however, to mechanically explain our observed employment response; Alpert et al. (2022) finds triplicate states experienced, at most, seven fewer deaths per 100,000 per year. To convert this to a rate of 100,000 unemployed (to compare with our unemployment to employment flow), we use the 4.29% unemployment-to-population rate over the study period (1991-2019), and a bounding assumption that all deaths belong to those unemployed. Then, the mortality rate has an upper bound at $\frac{7}{100{,}000 \times 0.0429} \approx \frac{163.2}{100{,}000}$ unemployed. This numerator of 163.2 is less than 10% of our unemployment to employment flow numerator, 1,820 column 1 in Table 3), suggesting that our results are not driven by a mortality selection effect.

5.2 Heterogeneity by extent of opioid crisis

Next, we examine how the coefficients evolve over the post-1996 period. The six labor market transition plots in Figure 1 present the event study analysis with the DID coefficients estimated at the yearly level (1995 is the reference year). We observe that most of the effects of triplicate regulation begin to appear following the onset of the opioid crisis. In parallel to changes in the economy, the opioid crisis also experienced important changes in the post period as discussed in Section 2.1. We observe this in Table 4, which separates the post period into three time bands: 1996-2000, 2001-2010, and 2011-2019. We find that indeed, there are no statistically significant effects (using either inference method) in the first period ending in 2000. The magnitude of effects across columns is larger than the pooled estimates in Table 3, but comparable because the two later time bands comprise the majority of the post period and show the largest effects.

5.3 Steady state implications

Table 5 presents our calculated effects of triplicate regulation on steady state labor market levels. Columns 1 to 3 show the steady state rates of employment (e), unemployment (u), and non-participation (n) for the pooled estimation (Panel A) and for the disaggregated time bands (Panel B). We observe that columns 1 to 3 are consistent with distinctive features of the US labor market. First, the unemployment-to-population rate is highest in the 2001-2010 period in column 2, which is an effect of the Great Recession. Second, columns 1 to 3 illustrate a known trend in the labor market during the 1996-2019 time period, which is that the proportion of individuals not in the labor force is increasing, and that the employment-to-population rate is decreasing (Elsby et al., 2011). For context, the employment-to-population rate dropped by 7% (from 70% in 1996-2000 to 65% in 2011-2019) over the post period.

Columns 4 to 6 report the triplicate regulation effect on these steady state rates.[9] The dynamic flow results imply a steady state employment increase of 1.1 percentage point (column 4), which translates to a 1.7% increase over the mean in column 1. About half of this increase in employment is from a reduction in unemployment, and half from a reduction in non-participation. The steady state rise in employment coming from reductions in unemployment is consistent with the cited prior literature showing vulnerability to opioid use during unemployment; it is also consistent with the variation observed in unemployment (but not non-participation) over this time period due to the two economic recessions (2001 recession and The Great Recession). We do not observe a statistically significant reduction in the steady state rate of non-participation, but mechanically the reduction in this state contributes 48% of the effect in the employment-to-population rate increase. As before,the statistically significant effects are concentrated in the post-2001 period in Panel B.

5.4 Adjustment dynamics

A natural question that arises from our study is how long it might take for the steady state effects to materialize. Figure 2 plots the adjustment dynamics for all three labor market states. The starting point in each of these plots is the steady state implied by the counterfactual baseline labor market flows (without triplicate effect). We then simulate the transition to triplicate status by adding the triplicate effects to each of the labor market flows, and repeatedly apply the transition matrix until we observe that the steady state is reached. We observe that it takes about 2.5 years for the dynamic effects to fully reflect in the steady state employment-to-population rate. The adjustment period for the unemployment-to-population rate is about 18 months, and for the non-participation-to-population rate it is about 30 months. Our simulated steady state effect does not arrive immediately. Analyses studying levels directly (rather than flows) may not therefore detect the full effect of the

[9]We use the delta method to calculate the state-clustered standard errors for the steady state. See Appendix G for details.

opioid crisis, especially short-run analyses.

Next, to demonstrate how triplicate regulation speeds employment recovery from the Great Recession, we simulate a one-time unemployment shock and use our estimates to compare the adjustment dynamics of triplicate versus counterfactual non-triplicate states. Specifically, we generate an exogenous increase in the unemployment-to-population rate of 3.47% to triplicate states, which is the magnitude of the Great Recession shock (December 2007 − June 2009). The simulation keeps the labor force participation rate constant, so the entire increase in unemployment comes from a reduction in employment. We then simulate adjustment back to steady state, given the observed flow rates in November 2007. We apply equation (6) in Appendix F to calculate the steady state employment-to-population rates with and without the triplicate regulation; the steady state employment-to-population rate with the regulation incorporates the triplicate effect from the 2001-2010 estimates in Table 4. We then calculate how many months it takes for the employment level to "recover", which we define as reaching 90% of the steady state rate.

The results of this simulation are in Figure 3, which shows the employment-to-population rate recovery paths for triplicate states with and counterfactually without triplicate regulation. We observe that the recovery takes about 15.24 months with triplicate regulation, compared to 16.86 months without the regulation.[10] These dynamics suggest that triplicate regulation results in a 10.6% faster employment recovery.

5.5 Comparison to previous findings

We estimate substantial effects of triplicate regulation on employment. An immediate question is whether these are consistent, or at least plausible, in the context of prior work on the opioids-employment connection. Our estimates are difficult to compare to earlier work

[10]This recovery is much faster than what we observe in reality. A likely reason for this speed is that these simulations assume constant transition rates. Transitions back to employment likely fall with duration (Kroft, Lange and Notowidigdo, 2013; Katz, 2015), slowing recovery. Such duration dependence does not bias our DID estimates, however, because they reflect the duration of unemployment in the data.

because of differences in identifying variation (generating different local average treatment effects), differences in outcomes (most work has focused on levels, not flows), and differences in size of the opioid shock. To compare to prior work, we therefore focus on level effects, and note differences across studies and note that the opioid shock in our case is a roughly 50% decrease in OxyContin dispensing (Alpert et al., 2022). We find this translates into a 1.13 percentage point increase in the steady state employment-to-population rate and a 0.55 percentage point decline in the steady state non-participation rate.

Our dynamic estimates are new to the literature, but our estimate of a 1.13 percentage point steady state employment effect is in the middle of the range of prior estimates of the effect of opioid prescribing and opioid related policies on employment. Our estimate is most comparable to those that rely on aggregate variation in prescribing intensity and look at employment responses.[11] On the high end, using 2010-2015 data from 10 US states, Harris et al. (2020) finds that a 10% increase in per-capita opioid prescriptions leads to a 0.56 percentage point reduction in the employment-to-population rate and a 0.53 percentage point reduction in labor force participation. Similarly Aliprantis, Fee and Schweitzer (2019) finds that 10% higher prescribing results in 0.15 to 0.47 percentage point decreases in employment. Extrapolating with a linear relationship assumption, these results would imply that triplicate protection increases employment by about 2.5 percentage points.

On the lower end, Beheshti (2022) studies the impact of hydrocodone's rescheduling, which reduced dispensing of that opioid. Looking across areas with different pre-reformulation levels of hydrocodone, Beheshti (2022) finds that each 10% *decrease* in hydrocodone prescriptions induced by the rescheduling increases the labor force participation rate by about 0.7 percentage point. Similarly Park and Powell (2021) examines the reformulation of OxyContin, which initiated the transition to more dangerous heroin and fentanyl (Alpert, Powell

[11] Two other papers that are relevant but less comparable are Laird and Nielsen (2016) and Savych, Neumark and Lea (2019), which also find substantial labor supply effects. Laird and Nielsen (2016) uses physician migration to show that being assigned to a higher-opioid-prescribing physician induces a 4.5 percentage point increase in opioid prescribing and a 1.5 percentage point decrease in employment. Savych, Neumark and Lea (2019) shows that greater opioid prescribing is associated with increases in the duration of disability.

and Pacula, 2018; Evans, Lieber and Power, 2019), finding an overall effect of a 0.7 to 1.1 percentage point decline in the employment-to-population rate. Finally, Currie, Jin and Schnell (2019) finds no effect of opioids on men's employment, and small but negative effects for women.

A contemporaneous paper, Powell (2021), also studies the effect of the opioid crisis on employment using geographic variation from triplicate regulation. One of the primary contributions of that paper is methodological in exploring how traditional accommodation for covariates in DID designs can conflate heterogeneous treatment effects with covariate trends that differ across treated and control groups. The paper proposes reducing such scope for bias by a two-step procedure in which first one residualizes the outcomes using pre-period observations from all units but post-period observations only from untreated units, and the second step regresses the treatment effects on these residualized outcomes. Using this residualization approach, Powell (2021) estimates that triplicate regulation led to a 1.9 percentage point increase in labor force participation, more than three times the size of our estimated steady state effect on participation employment of 0.55 percentage points. We show in Appendix E that the difference between Powell's results and ours is the combination of covariate adjustment and longer pre-period. However we focus on the short pre-period, unadjusted estimates for two reasons. First, the violations of parallel trends are mostly evident in the longer pre-period, and hence there is not necessary need for covariate adjustment with the shorter pre-period. Second, Powell's approach requires adjusting for time-varying covariates, and there is not a consensus in the econometrics literature on whether or how to do so. (Powell and Pacula (2021) offers one approach, and Caetano et al. (2022) another, while Callaway and Sant'Anna (2021) argue against such adjustment at all.)

6 Additional analyses

6.1 Heterogeneity by occupational and worker characteristics

The opioid crisis has been marked by substantial differences across regions, in part due to differences in occupational and demographic composition. On one hand, because prescription medications for opioid pain relievers are frequently the starting point for later addiction (Barnett, Olenski and Jena, 2017; Barnett et al., 2019; Eichmeyer and Zhang, 2021), workers in occupations that are physically demanding are at higher risk for injuries or physical stress that can make them vulnerable to opioid abuse. On the other hand, opioids could help those who experience physical pain to continue work: Garthwaite (2012) shows that the removal of certain chronic pain relievers from the market in 2004 led to a 22 percentage point reduction in labor supply among those with joint conditions (and a 0.35 percentage point reduction in overall labor force participation). The effectiveness and availability of pain relief is likely to be particularly important for those employed in physically demanding occupations. Thus it is an empirical question whether workers in physically demanding occupations experience changes in employment if they are living in a state with triplicate regulation. We use Acemoglu and Autor (2011)'s categorization of occupational skills to classify occupations as physically demanding.

Columns 1 and 2 of Table 6 present the results of this analysis, estimated using equation (1). Column 1 shows the effect of triplicate status on flows for workers in physically demanding occupations (at time $t-1$), and column 2 shows the analogous effect for workers in non-physically demanding occupations.[12] Triplicate policies appear to offer more employment protection to workers at greater risk of using opioids, consistent with Maestas and Sherry (2020), which finds that workers experiencing pain are more likely to leave the

[12] We observe very few non-participating people who report physically demanding occupations, so we lack the sample size to estimate models of transitions out of non-participation for them.

labor force. The treatment effect in our analysis is not significant for unemployment-to-employment transitions, but it is for employment to unemployment transitions.

Despite this evidence, we acknowledge that it is challenging to think through the effects of triplicate regulation on workers in physically demanding occupations. There are two important forms of possible selection in the analysis. First is on the composition of workers in each labor market state as a result of triplicate regulation — e.g., some workers in physically demanding occupations have a harder time staying employed without access to OxyContin. Our analysis accommodates this type of selection by examining each flow. The second type of possible selection is of workers into certain occupations because of triplicate regulation — e.g., some workers may avoid physically demanding occupations without access to opioid pain relievers. Our analysis does not accommodate this second type of selection on occupational choice.

Though all demographic groups have suffered from the opioid crisis, its impact on mortality has initially been most severe among middle-aged, white, non-Hispanic Americans (Case and Deaton, 2015). Citing data from the Kaiser Family Foundation on the breakdown of opioid overdose deaths by race/ethnicity, however, Shihipar (2019) highlights that the recent breakdown in 2019 shows that the crisis spans across racial/ethnic groups: among those who died, 72% were White, non-Hispanic; 15% were Black, Non-Hispanic; and 11% were Hispanic. For context, the US population in 2020 is about 58% White, non-Hispanic; 12% Black, non-Hispanic; and 19% Hispanic. Part of the multifold rise in deaths among these other groups since 2010 is due to the expansion of heroin and fentanyl supply, with fentanyl-laced cocaine in particular reaching more drug afflicted individuals across racial groups with opioids (Shiels et al., 2018; Alexander, Kiang and Barbieri, 2018). A recent paper, Aliprantis, Fee and Schweitzer (2019), also finds substantial labor market effects of the opioid crisis for nonwhite men, particularly those with lower education.[13] Echoing these findings, our own

[13] That paper finds: "The magnitude of the coefficient [on the effect of opioid prescriptions on employment] for nonwhite men without a college degree is even larger than the coefficient for white men without a BA. . . .Another point worth emphasizing is that while Case and Deaton 2015's results focused attention on

analysis in Table A2 of 2015-2019 data from the National Survey on Drug Use and Health shows that the rates of opioid use and misuse are comparable across racial/ethnic groups, with all being most prone to use or misuse when unemployed.[14]

To assess whether the triplicate regulation yields heterogeneous effects, we investigate the labor market effects across demographic groups. Columns 3 to 4 of Table 6 present the results for males and females, respectively — here, we observe that the U-E flow is similar for both groups but the employment to unemployment flow is higher for males. The race/ethnicity breakdown in columns 5 to 7 shows effects across groups, with pronounced effects of triplicate regulation on the adverse labor market experience of Black, non-Hispanic individuals.

Columns 8 to 10 of Table 6 present the results for different age bands (15-24, 25-44, and 45-64). We observe that our main results on the impact of triplicate regulation is present across groups and generally increases with age. In columns 11 to 13, we present the results broken down by education levels of less than high school, high school, or more than high school. We observe the coefficient for example on the unemployment to employment flow appears substantially smaller for the most educated group, consistent with the socioeconomic divide of the crisis.

Taken together, these heterogeneity results suggest that the protective effects of triplicate status extend to most of the working-age population. This could be for many reasons, including the direct reduction in opioid prescriptions and reduced opioid availability in areas with the regulation in force.

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white households, our results are just as troubling for nonwhite prime-age men. The coefficient for nonwhite men with less than a BA is a startling -0.101, larger than the -0.070 experienced for less-educated white men, although the difference is not quite statistically significant at the 95 percent confidence level. Reinforcing the pattern, nonwhite men with a BA also experience a larger likelihood of not being employed in higher opioid prescription areas than their white counterparts (-0.045 versus -0.021). By our measures it is hard to argue that white prime-age men have been more affected than their minority counterparts."

[14]This analysis is provided in Appendix Table A2.

6.2 Robustness checks

The identifying variation we obtain from state triplicate regulation has been used in prior work. Still, there may be concerns about the small number of treated states, or other underlying labor market trends that are not captured in our regression specification. Here, we provide an overview of three sets of robustness checks that we conducted to mitigate these concerns, followed by a discussion of the results from these robustness checks.

First, we address the role of time-varying covariates. Changes in labor market flows over the studied time period could be (at least partly) attributable to demographic shifts rather than triplicate regulation. To investigate the role of such trends, we re-estimate our main DID regressions with state-specific, time-varying covariates related to demographics, education, and occupational mix. Namely, we include the median age, along with the proportion non-Hispanic white, non-Hispanic black, with college degree, in the manufacturing industry, in the services industry, and in physically demanding occupations.

The second robustness check provides a stacked DID analysis to address the possibility of a trend unique to the group of a treated (triplicate) state and its bordering states.[15] We need to stack the separate data sets, rather than just add fixed effects for state-group-month, because California and Idaho share the border states Oregon and Nevada. Our stacked approach allows these shared border states to be in the control group for both California and Idaho. Our cluster-robust standard errors account for the covariance stemming from the same observations being duplicated in the stacked data set.

The third robustness check addresses the concern that one of the treated states could drive the estimated triplicate effect. This is potentially relevant given that two of the treated states are among the most populous in the US: California and Texas. We re-estimate our pooled DID five times, each time dropping one of the five triplicate states.

Figure 4 plots a summary of the results from these robustness checks. Each of the six

[15]See Appendix C.4 for details.

panels shows the results for a labor market flow, and the pooled DID coefficient is plotted for the base (main analysis in Table 3), then with adding covariates, then by restricting the comparison group for each treated state to its neighbors, and then by dropping one the five triplicate states at a time. The 95% confidence intervals shown are the ones from cluster-robust standard errors, though we show both types of standard errors for all analyses in the comprehensive tables in Appendix D. Overall, the point estimates are stable across the checks – in particular, labor market flows into employment demonstrate a strong triplicate effect across the different checks.

Last but not least, in Appendix E we further investigate the possibility of extending the pre-period to 1981. As we do this, we must also consider the role of covariates as there are differential trends between triplicate and non-triplicate states in the demographic controls, namely shares of non-Hispanic White, Hispanic, College^{+}, and age 45-64 (shown in Appendix Figure E1). As there is not yet a general consensus on how to accommodate such trends in our DID framework of equation (1), in Appendix Table E1 we present the results from the baseline difference-in-difference and two different methods proposed by Callaway and Sant'Anna (2021) and Powell (2021); we detail the applied methods in Appendix E. Appendix Figure E2 shows that these covariate adjustment methods flatten the pre-trends in our context. We observe that the estimated coefficients on the effect of triplicate regulation on labor market flows are substantially higher, consistent with the general direction of effects found in Powell (2021).

7 Conclusion

We have shown that, after OxyContin's arrival in 1996, workers returned to employment from unemployment and non-participation faster in states with triplicate regulation than in states without it, relative to the difference in earlier years. Given the strong protective effect of triplicate regulation in preventing opioid use documented by Alpert et al. (2022), this finding suggests that opioid use slows the return to employment. We did not explore in this paper the effect that opioid availability might have on changing worker productivity, though Ouimet, Simintzi and Ye (2020) finds that firms in areas afflicted with the crisis experienced dampened growth and were more likely to invest in labor-substituting technology.

Our finding has three broader implications. First, they help shed light on the slow recovery from the Great Recession, which remains a puzzle.[16] Because The Great Recession hit as the opioid epidemic transitioned to its most serious phase, our estimates indicate that rising opioid abuse contributed to the slow recovery of the unemployment rate. The triplicate regulation effects we uncover occur throughout the business cycle and may be more pronounced in recession recovery.

Second, our results point to an underappreciated cost of opioid abuse: rising unemployment insurance payments. A back-of-the-envelope calculation based on our estimate implies that the expected duration of unemployment is about 10 days shorter in triplicate states because of triplicate policies.[17] With an average weekly benefit of $387 (prior to the insurance expansion caused by the COVID-19 pandemic, taken from Kovalski and Sheiner 2020), the reduced duration represents a savings of $550 per unemployed individual.

Finally, our results provide indirect evidence on the causes and consequences of opioid abuse, and addiction more generally. Specifically, they are consistent with the joint hypotheses that unemployment increases substance abuse, substance abuse reduces re-employment rates, and triplicate states reduce the likelihood of substance abuse.

[16] Note that the recent paper Hall and Kudlyak (2020) suggests more conformity in US recession recoveries, but the Great Recession remains an exception as shown in Sahin (2021).

[17] The expected duration of unemployment is 1/(hazard of exit). In the data, the exit hazard in triplicate states is .24 and, absent triplicate policy's effect of .018, it would be .222, for an effect of (1/.24 - 1/.222).

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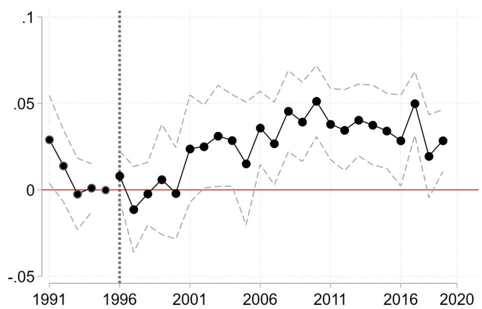
Figure 1: Event Study Analysis: Effect of Triplicate Regulation on Labor Market Flows

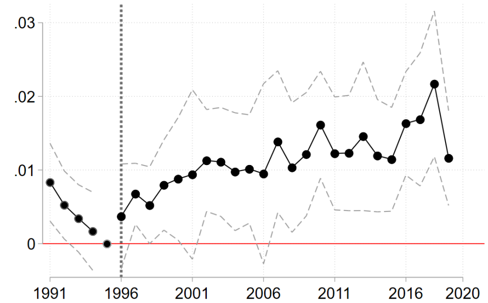
This figure shows the estimated coefficients of the triplicate regulation on the six labor market flows between employment (E), unemployment (U), and non-participation (N). The specification includes triplicate indicators for whether the state has the regulation in force by 1996, the beginning of regulatory effect due to the entry of OxyContin. The excluded (reference) year is 1995. The data span January 1991 to December 2019. The dashed horizontal lines represent 95% confidence intervals based on state-clustered standard errors.

(a) Flows to E

from U

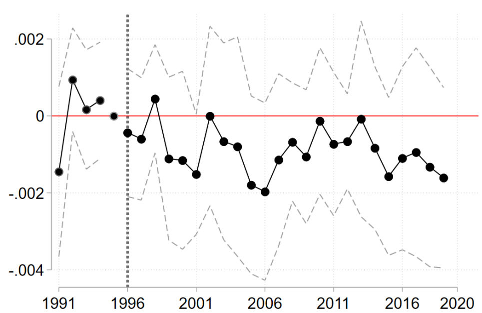


from N

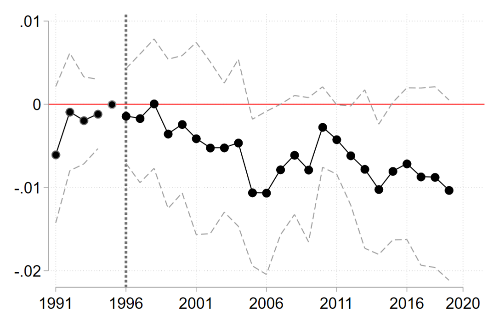


(b) Flows to U

from E

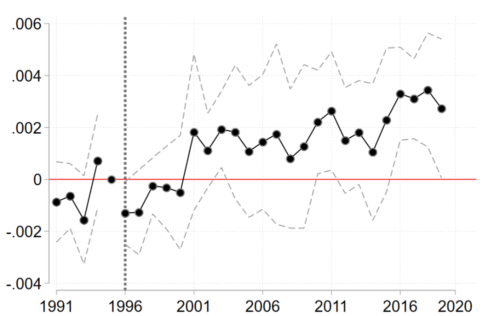


from N



(c) Flows to N

from E



from U

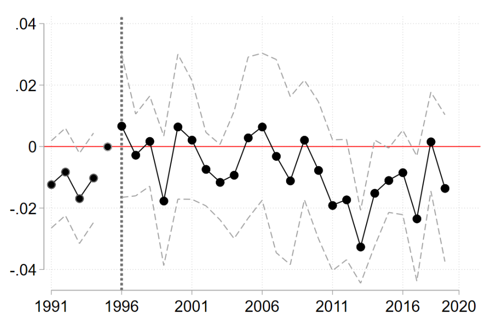


Figure 2: Adjustment Dynamics: Simulated Paths to Steady State Labor Market Activity

This figure shows the convergence paths to the steady state rates (%) of triplicate states while translating the triplicate effects on transition rates which reported in Table 4. The rates of employment-to-population, unemployment-to-population, and non-participation-to-population onset are taken from January 2001; the triplicate status effect on transition rates are taken from the second period (2001-2010) estimates.

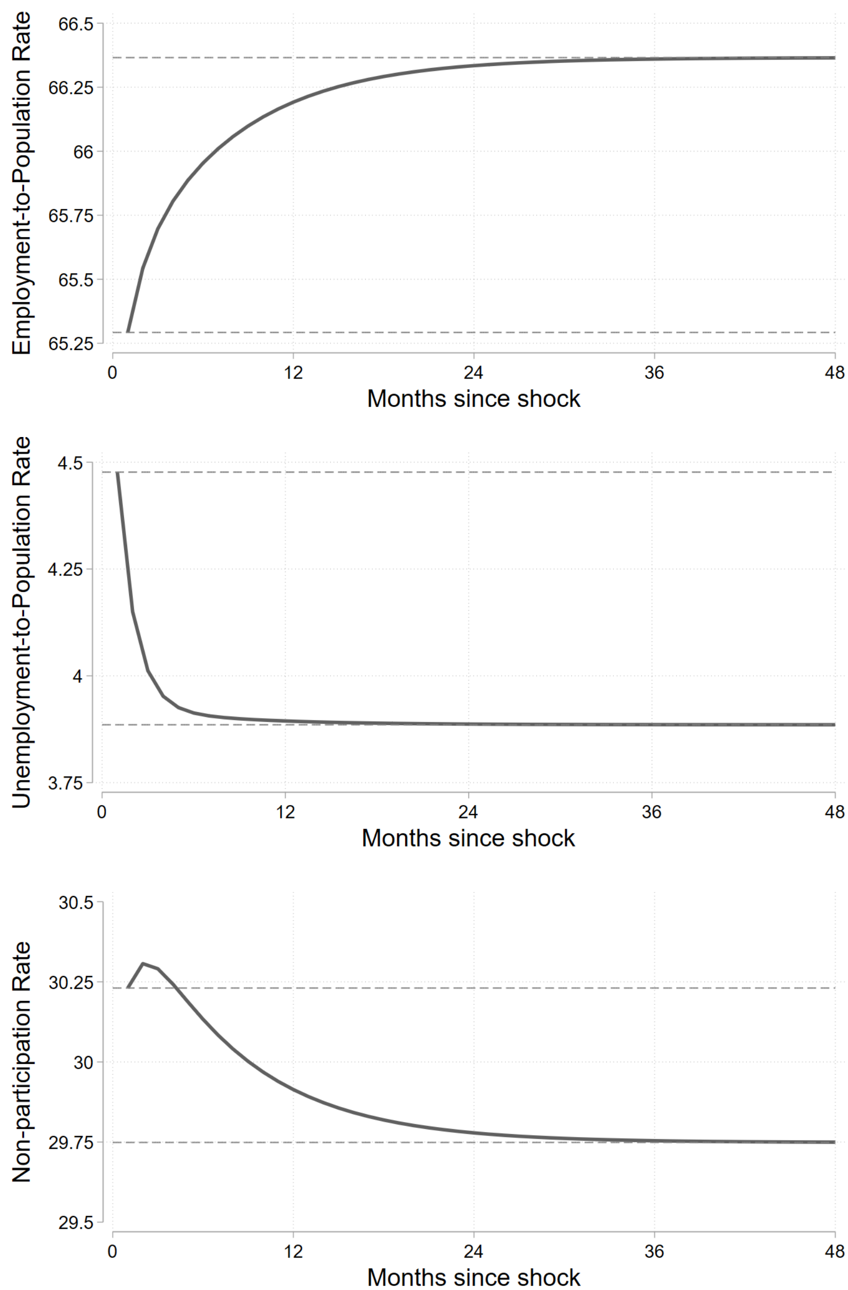


Figure 3: Adjustment Dynamics: Simulated Unemployment Recovery from the Great Recession

This figure presents the employment rate (% of population) recovery path from an unemployment shock equal in magnitude to the Great Recession for triplicate states (solid line) and their counterfactual non-triplicate path (dashed line). The vertical lines indicate the number of months of adjustment to reach 90% of baseline employment, which we consider recovery. The plot shows that states with triplicate regulation require 15.24 months of adjustment, compared to 16.86 months without triplicate regulation.

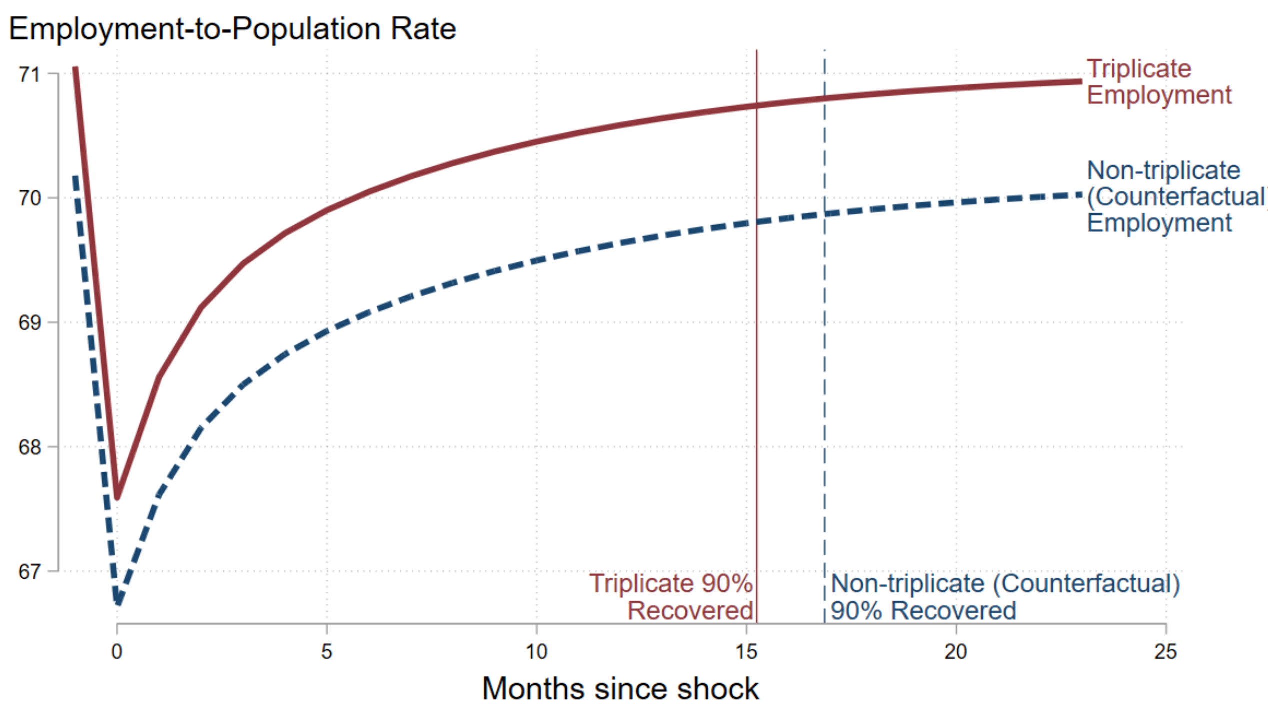
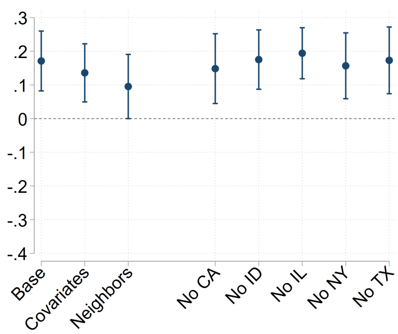


Figure 4: Robustness Checks to the Difference-in-Difference Estimates

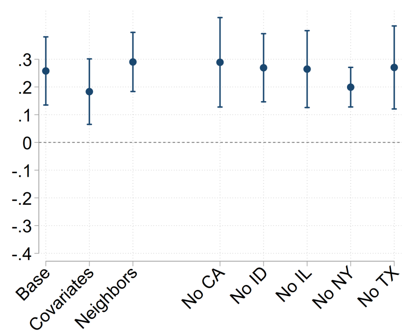
This figure plots the results from the three robustness checks described in Section 6.2. Each plot shows the coefficient from an estimation scaled by the standard deviation of the relevant outcome labor market flow for ease of comparison. Employment, unemployment, and non-participation are denoted by E, U, and N. The base case is the main specification; then we add covariates; then we estimate a stacked DID with neighboring (bordering) states; and then we drop one treated state at a time. The bars indicate 95% confidence intervals from state-clustered standard errors.

(a) Flows to E

from U

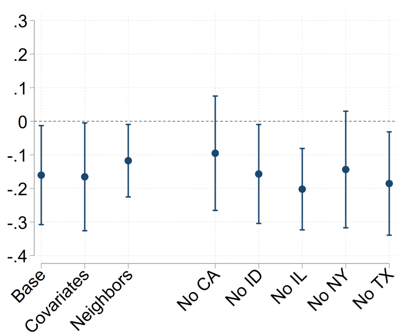


from N

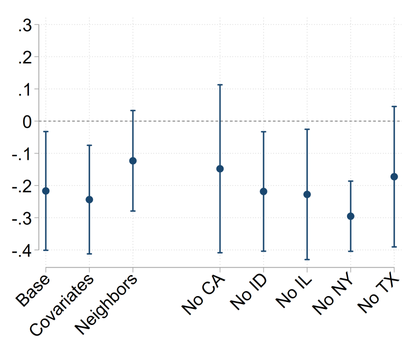


(b) Flows to U

from E

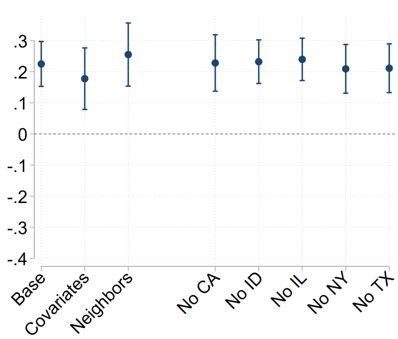


from N



(c) Flows to N

from E



from U

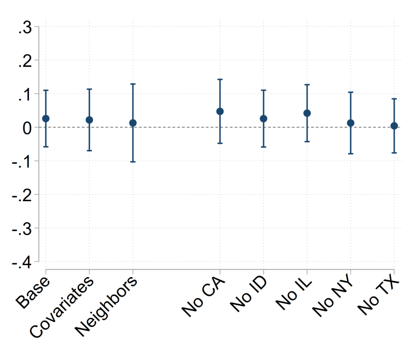


Table 1: Baseline Summary Statistics by Triplicate Regulation

This table presents summary statistics on demographic and occupational composition for each triplicate state, in the final two columns aggregated by triplicate and non-triplicate states. We aggregated data from 1991-1995, as these represent the baseline period prior to the introduction of Oxycontin. The sample includes Current Population Survey (CPS) respondents aged 15-64; we have applied the sampling weights provided in the data. Physically demanding occupations are classified using the Occupational Information Network database (O*NET) according to Acemoglu and Autor (2011) and are listed in Appendix C.3.

	CA (1)	ID (2)	IL (3)	NY (4)	TX (5)	Triplicate (6)	Non-Triplicate (7)
Race							
% White, Non-Hispanic	54	91	73	68	57	61	79
% Black, Non-Hispanic	6	0	15	14	12	10	13
% Hispanic	29	6	9	13	28	22	4
Age							
% Ages 15-24	22	23	22	21	23	22	21
% Ages 25-44	51	48	49	48	50	50	49
% Ages 45-64	27	29	29	31	27	28	30
Education							
% Some College	49	46	48	46	44	47	44
Industry							
% Manufacturing	18	17	21	15	15	17	19
% Services	44	39	42	49	42	44	42
Occupation							
% Physically Demanding	17	23	17	16	19	17	18
Population (in thousands)	31,378	1,107	11,735	18,181	18,170	80,573	176,128

Table 2: Labor Market Flows by Time Period and Triplicate Regulation

This table reports the six labor flows for triplicate states in Panel A and for non-triplicate states in Panel B. The labor market states of employed, unemployed, and not in labor force are designated by E, U, and N. Columns (1) and (2) show flows to employment, columns (3) and (4) show flows to unemployment, and columns (5) and (6) show flows to non-participation. The different time bands represent baseline (1991-1995) and subsequent waves of the opioid crisis as described in Section 2.1. Within each time period, we average the flows and scale them by 100. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-triplicate level. The data span January 1991 to December 2019.

A. Triplicate States

	Flows to E		Flows to U		Flows to N	
Time	from U (1)	from N (2)	from E (3)	from N (4)	from E (5)	from U (6)
1991 - 1995	23.3	6.5	1.7	4.9	2.7	21.9
1996 - 2000	28.1	7.2	1.4	4.3	2.8	26.4
2001 - 2010	24.2	6.9	1.5	4.1	3.0	24.2
2011 - 2019	22.3	6.4	1.3	3.6	3.1	24.5

B. Non-Triplicate States

	Flows to E		Flows to U		Flows to N	
Time	from U (7)	from N (8)	from E (9)	from N (10)	from E (11)	from U (12)
1991 - 1995	26.1	6.9	1.5	4.3	2.5	20.7
1996 - 2000	31.4	7.4	1.2	3.7	2.6	24.4
2001 - 2010	24.6	6.5	1.4	3.9	2.6	22.5
2011 - 2019	22.9	5.8	1.2	3.5	2.6	24.0

Table 3: Pooled Difference-in-Differences Analysis: Labor Market Dynamics

This table reports the coefficients of triplicate regulation (in force by 1996) interacted with the full post period (1996-2019) for each of the six labor market flows. The labor market states of employed, unemployed, and non-participation are designated by E, U, and N. The estimates are relative to the baseline period (1991-1995) and are scaled by 100. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. The data span January 1991 to December 2019. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). The counterfactual baseline provides the mean labor market transition rates netting out the triplicate effect for triplicate states over the full post-period (1996-2019).

	Flows to E		Flows to U		Flows to N	
	from U	from N	from E	from N	from E	from U
Triplicate ×	(1)	(2)	(3)	(4)	(5)	(6)
1996 - 2019	1.82	0.74	-0.10	-0.38	0.21	0.22
	(1.34, 2.30)	(0.56, 0.92)	(-0.14, -0.06)	(-0.54, -0.22)	(0.17, 0.25)	(-0.15, 0.59)
	[0.30, 2.81]	[0.40, 1.35]	[-0.18, 0.05]	[-0.68, 0.20]	[0.11, 0.29]	[-0.66, 1.14]
Counterfactual Baseline	22.47	6.04	1.48	4.31	2.79	24.55

Table 4: Difference-in-Differences Analysis with Temporal Heterogeneity: Labor Market Dynamics

This table reports the coefficients of triplicate regulation (in force by 1996) interacted with three separated time periods (1996-2000, 2001-2010, 2011-2019) for each of the six labor market flows. The labor market states of employed, unemployed, and non-participation are designated by E, U, and N. The estimates are relative to the baseline period (1991-1995) and are scaled by 100. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. The data span January 1991 to December 2019. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). The reported joint p-value tests the joint statistical significance of the three coefficients using our wild bootstrap method.

	Flows to E		Flows to U		Flows to N	
	from U	from N	from E	from N	from E	from U
Triplicate ×	(1)	(2)	(3)	(4)	(5)	(6)
1996 - 2000	-0.63	0.17	-0.06	0.01	-0.01	0.75
	(-1.32, 0.06)	(0.02, 0.32)	(-0.11, -0.01)	(-0.17, 0.19)	(-0.04, 0.02)	(0.25, 1.25)
	[-2.21, 0.88]	[-0.18, 0.62]	[-0.16, 0.06]	[-0.33, 0.69]	[-0.09, 0.06]	[-0.33, 1.93]
2001 - 2010	2.49	0.72	-0.09	-0.41	0.21	0.68
	(1.79, 3.19)	(0.49, 0.95)	(-0.13, -0.05)	(-0.58, -0.24)	(0.15, 0.27)	(0.16, 1.20)
	[0.44, 3.94]	[0.28, 1.44]	[-0.19, 0.05]	[-0.72, 0.13]	[0.07, 0.37]	[-0.43, 2.19]
2011 - 2019	2.33	1.04	-0.12	-0.50	0.31	-0.43
	(1.71, 2.95)	(0.82, 1.26)	(-0.17, -0.07)	(-0.68, -0.32)	(0.27, 0.35)	(-0.80, -0.06)
	[0.77, 3.80]	[0.52, 1.82]	[-0.22, 0.06]	[-0.91, 0.09]	[0.18, 0.40]	[-1.52, 0.37]
Joint p-value	0.04	0.05	0.49	0.03	0.00	0.13
N	17544	17544	17544	17544	17544	17544

Table 5: The Effect of Triplicate Regulation on Steady State Labor Market Outcomes

This table reports the effects of triplicate regulation (in force by 1996) on steady state labor market outcomes using the method described in Section 3.3. Column (1)-(3) provide the baseline (1991-1995) levels (in %) of employment (e), unemployment (u), and non-participation (n); note that these three levels sum to 100% by the empirical design. The triplicate regulatory effect on these levels is shown in columns (4) to (6); note that these effects sum to zero by design (imprecision is due to rounding). These estimates are calculated by netting out the counterfactual steady state rates subtracting the triplicate effect from the observed steady state transitions of triplicate states. The data are monthly and span January 1991 to December 2019. The standard errors in parentheses are estimated using the delta method and state-level clustering as detailed in Appendix G.

	Steady State Rate			Triplicate Status Effect		
	e	u	n	e	u	n
Time	(1)	(2)	(3)	(4)	(5)	(6)
A. Pooled Difference-in-Difference Analysis						
1996 - 2019	66.92	4.32	28.76	1.13	-0.59	-0.55
				(0.49, 1.77)	(-0.79, -0.39)	(-1.23, 0.13)
B. Difference-in-Difference Analysis with Three Time Periods						
1996 - 2000	70.16	3.86	25.98	0.41	-0.10	-0.31
				(-0.06, 0.88)	(-0.24, 0.04)	(-0.79, 0.17)
2001 - 2010	67.01	4.59	28.40	1.18	-0.74	-0.44
				(0.37, 1.99)	(-0.98, -0.50)	(-1.18, 0.30)
2011 - 2019	65.01	4.28	30.72	1.94	-0.76	-1.19
				(0.90, 2.98)	(-1.00, -0.52)	(-2.19, -0.19)

Table 6: Difference-in-Differences Analysis by Demographics and Occupational Subgroups (cont'd on next page)

This table follows the pooled post-period specification in Table 3 for occupational and demographic subgroups. Columns (1) and (2) show the breakdown by occupations that are physically demanding or not (the classification details are in Appendix C.3; columns (3) and (4) by sex; columns (5) to (7) by race/ethnicity; columns (8) to (10) by the span of all ages in our sample (15-24, 25-44, and 45-64); and columns (11) to (13) by education (less than high school, high school graduates, and more than high school). The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). Note that the dashed lines indicate "not applicable" because we do not observe corresponding flow for the time $t-1$ occupation categorized individuals. For example, if an individual is moving from non-participation in $t-1$ to employment in t, we would need to categorize the non-participation as physically demanding or not and do not have enough individual observations to do so.

	Occupation is Physically		Sex		Race/Ethnicity		
Triplicate×	Demanding (1)	Not Demanding (2)	Male (3)	Female (4)	White and Non-Hispanic (5)	Black and Non-Hispanic (6)	Hispanic (7)
from U to E	2.28	1.00	2.08	1.81	0.64	2.64	1.26
	(0.64, 3.92)	(0.30, 1.70)	(1.63, 2.53)	(1.05, 2.57)	(-0.18, 1.46)	(1.89, 3.39)	(-0.06, 2.58)
	[-2.06, 6.10]	[-0.67, 2.56]	[1.06, 3.18]	[-1.08, 3.30]	[-1.53, 2.41]	[0.66, 4.32]	[-2.16, 4.28]
from N to E	—	—	0.72	0.70	0.47	0.74	0.06
			(0.49, 0.95)	(0.53, 0.87)	(0.34, 0.60)	(0.38, 1.10)	(-0.43, 0.55)
			[0.28, 1.54]	[0.33, 1.23]	[0.14, 0.81]	[-0.30, 1.54]	[-1.05, 1.66]
from E to U	-0.70	-0.15	-0.13	-0.04	-0.07	-0.04	-0.16
	(-0.99, -0.41)	(-0.26, -0.04)	(-0.21, -0.05)	(-0.07, -0.01)	(-0.12, -0.02)	(-0.23, 0.15)	(-0.38, 0.06)
	[-1.30, 0.01]	[-0.37, 0.19]	[-0.28, 0.11]	[-0.13, 0.05]	[-0.16, 0.10]	[-0.51, 0.51]	[-0.66, 0.39]
from N to U	—	—	-0.99	-0.08	-0.35	0.00	-0.23
			(-1.21, -0.77)	(-0.25, 0.09)	(-0.47, -0.23)	(-0.54, 0.54)	(-0.65, 0.19)
			[-1.41, -0.36]	[-0.48, 0.54]	[-0.62, -0.05]	[-1.12, 1.70]	[-1.19, 0.76]
from E to N	0.12	0.29	0.17	0.25	0.13	0.53	-0.05
	(-0.20, 0.44)	(0.17, 0.41)	(0.13, 0.21)	(0.19, 0.31)	(0.08, 0.18)	(0.44, 0.62)	(-0.26, 0.16)
	[-0.57, 1.03]	[0.01, 0.61]	[0.09, 0.29]	[0.10, 0.41]	[0.00, 0.23]	[0.28, 0.74]	[-0.58, 0.47]
from U to N	0.73	0.73	0.18	-0.04	0.30	-0.43	-0.33
	(-2.10, 4.16)	(-0.10, 1.56)	(-0.24, 0.60)	(-0.57, 0.49)	(-0.12, 0.72)	(-1.19, 0.33)	(-0.98, 0.32)
	[-7.75, 8.73]	[-1.22, 3.18]	[-0.78, 1.20]	[-1.05, 1.76]	[-0.60, 1.60]	[-2.53, 1.40]	[-1.78, 1.13]

Triplicate×	Age			Education		
	Age 15-24 (8)	Age 25-44 (9)	Age 45-64 (10)	< HS (11)	HS (12)	> HS (13)
from U to E	1.33	1.90	2.39	2.43	2.41	1.32
	(0.73, 1.93)	(1.44, 2.36)	(1.54, 3.24)	(1.81, 3.05)	(1.59, 3.23)	(0.65, 1.99)
	[-0.24, 2.60]	[0.82, 2.97]	[-0.82, 4.03]	[0.78, 4.05]	[0.38, 4.26]	[-0.54, 2.69]
from N to E	0.69	0.92	0.83	0.31	0.82	0.93
	(0.49, 0.89)	(0.55, 1.29)	(0.68, 0.98)	(0.15, 0.47)	(0.63, 1.01)	(0.76, 1.10)
	[0.12, 1.19]	[0.16, 2.23]	[0.45, 1.17]	[-0.01, 0.73]	[0.32, 1.28]	[0.54, 1.39]
from E to U	-0.12	-0.12	-0.01	-0.42	-0.15	-0.05
	(-0.20, -0.04)	(-0.20, -0.04)	(-0.04, 0.02)	(-0.54, -0.30)	(-0.22, -0.08)	(-0.09, -0.01)
	[-0.34, 0.06]	[-0.26, 0.16]	[-0.08, 0.08]	[-0.65, -0.03]	[-0.27, 0.17]	[-0.13, 0.05]
from N to U	-0.44	-0.35	-0.21	-0.60	-0.27	-0.24
	(-0.76, -0.12)	(-0.54, -0.16)	(-0.29, -0.13)	(-0.82, -0.38)	(-0.43, -0.11)	(-0.40, -0.08)
	[-1.24, 0.59]	[-0.71, 0.29]	[-0.37, 0.01]	[-1.02, 0.15]	[-0.84, 0.04]	[-0.53, 0.24]
from E to N	0.46	0.14	0.30	-0.56	0.22	0.25
	(0.31, 0.61)	(0.07, 0.21)	(0.25, 0.35)	(-0.85, -0.27)	(0.17, 0.27)	(0.20, 0.30)
	[0.10, 0.76]	[-0.01, 0.33]	[0.12, 0.40]	[-1.24, 0.12]	[0.11, 0.34]	[0.08, 0.36]
from U to N	-0.05	0.09	0.39	-0.63	1.09	-0.39
	(-0.66, 0.56)	(-0.52, 0.70)	(-0.14, 0.92)	(-1.28, 0.02)	(0.43, 1.75)	(-1.01, 0.23)
	[-1.46, 1.47]	[-1.55, 1.85]	[-0.74, 2.00]	[-1.95, 1.05]	[-0.68, 3.17]	[-1.62, 1.71]

A Drug misuse by employment status

We use data from the 2015-2019 waves of the National Survey on Drug Use and Health (NSDUH; Substance Abuse and Mental Health Services Administration (2015-2019)) to show that drug use is much greater among the unemployed than among the employed conditional on sex, age, and survey year. NSDUH asks respondents a large battery of questions about prescription drug misuse (defined as using a prescription drug in a way other than directed by a doctor), as well as questions about illicit drug use. The survey instruments are designed to encourage truthful reporting, but given the sensitive nature of the topic, it is likely that drug use and misuse is underreported. NSDUH also collects demographic information, including age and sex, as well as employment status in the prior week. NSDUH samples Americans aged 12 and older but, to parallel our main analytic sample, we limit the NSDUH sample to respondents aged 16-64.

We focus on five categories of misuse. Our first is any misuse, defined as any prescription drug misuse or heroin use. The next three are misuse of OxyContin, any painkiller, or any tranquilizer. Fifth is use of heroin. These categories reflect our interest in opioids, which are used and misused as both prescription painkillers and heroin. We examine tranquilizer misuse because tranquilizer use and overdose has grown alongside the opioid crisis in the United States (Bachhuber et al., 2016). In addition to measuring misuse, we measure initiation, defined as misuse beginning in the previous 12 months (according to self-reported, retrospective information about when misuse began).

We report the level of misuse and initiation among the employed, and we measure the association between misuse and unemployment/non-employment. Because age and sex differ between the employed, unemployed, and non-employed, we adjust this assocaition for age and sex differences (as well as survey year differences) with the following regression for outcome y of person i in survey year t:

$$y_{it} = \beta_0 + \beta_1 Unemployed_{it} + \beta_2 NonParticipation_{it} + \beta_3 Female_{it} + \theta_{age} + \mu_t + \epsilon_{it} \quad (3)$$

Our interest is in β_1 and β_2, the association between y and employment status, adjusting for sex, age, and survey year. We report the results in Table A1. Panel A reports the rate of misuse among the employed, and the difference in misuse among the unemployed and non-participating. Panel B reports the analogous statistics for new misuse. The table shows uniformly higher rates of misuse among the unemployed than among the employed. The probability of misusing any drug is 3.6 percentage points higher among the unemployed than among the employed, a difference of 60 percent of the baseline rate of 6.1 percent. In absolute (percentage point terms) this higher drug use is concentrated in pain relievers and tranquilizers. However in relative terms, the rates of misuse of OxyContin and Heroin are particularly high among the unemployed: unemployed people are three times as likely to report misuse OxyContin, and five times as likely to report using heroin, as are employed people. We also see greater rates of drug use among the non-participating, relative to the employed, although the differences are not so large as for the unemployed. Unemployed show greater rates of initiation as well as greater overall use; the overall rate of initiation is about a third higher among the unemployed.

Table A1: Drug Misuse by Type and Employment Status

Notes: "Employed level" row reports the rate of misuse of the indicated drug among employed NSDUH respondents (2015-2019) in age 16-64. The remaining rows are the coefficients on unemployed and NILF from a regression of drug misuse on those variables plus indicators for sex, age group, and survey year. "Any" misuse is misuse of any of the four types. New misuse is misuse beginning in the prior 12 months. Robust standard errors, clustered on household, in parentheses.

Drug misuse	Any (1)	OxyContin (2)	Pain reliever (3)	Tranquilizer (4)	Heroin (5)
A. Rate of drug misuse					
Employed level	0.0609	0.0058	0.0455	0.0255	0.0025
Unemployed differential	0.0363	0.0101	0.0302	0.0139	0.0117
	(0.0037)	(0.0015)	(0.0033)	(0.0024)	(0.0014)
NILF differential	0.0062	0.0016	0.0073	0.0005	0.0035
	(0.0016)	(0.0005)	(0.0015)	(0.0011)	(0.0004)
Observations	230,599	230,297	230,599	230,599	230,553
B. Rate of new drug misuse					
Employed level	0.0128	0.0004	0.0074	0.0055	0.0002
Unemployed differential	0.0045	-0.0000	0.0026	0.0014	0.0006
	(0.0017)	(0.0002)	(0.0012)	(0.0011)	(0.0003)
NILF differential	-0.0011	-0.0002	0.0004	-0.0015	0.0002
	(0.0007)	(0.0001)	(0.0005)	(0.0004)	(0.0001)
Observations	230,590	230,238	229,973	230,131	226,220

Table A2: Drug Use and Misuse by Race/Ethnicity and Employment Status

Notes: This table reports the rate of opioid use and misuse by race/ethnicity conditional on employment status for NSDUH respondents (2015-2019) in age 15-64.

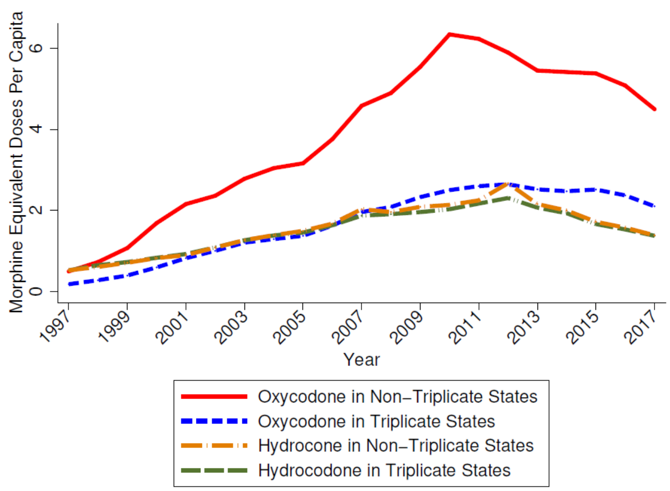
Race/Ethnicity	White and non-Hispanic (1)	Black and non-Hispanic (2)	Hispanic (3)	Others (4)
A. Total Population				
% Opioid Use	11.70	10.90	6.69	6.41
% Misuse	4.46	3.93	4.21	3.16
B. Employed				
% Opioid Use	10.46	9.98	6.31	5.95
% Misuse	4.75	3.75	4.17	3.11
C. Unemployed				
% Opioid Use	15.65	11.28	9.11	7.17
% Misuse	10.28	5.85	7.64	5.53
D. NILF				
% Opioid Use	13.51	12.34	6.88	7.13
% Misuse	3.40	3.70	3.60	2.89

B Opioid Crisis in Triplicate and Non-triplicate States

Figure B1: Impact of Triplicate Regulation on Opioid Prescriptions and Deaths

These plots are taken directly from Alpert et al. (2022) to provide context for the effect of triplicate regulation on opioid prescriptions and opioid overdose deaths. In the top panel, hydrocodone is contrasted to oxycodone because the former substance was not affected by triplicate regulation. We observe that oxycodone, which was subject to the regulation, experienced a sharp increase in non-triplicate states. In the second panel, we observe that non-triplicate states experienced a differential rise in opioid overdose deaths following the introduction of OxyContin in 1996 (indicated by the vertical line).

(a) Oxycodone and Hydrocodone Doses



(b) Opioid Overdose Deaths

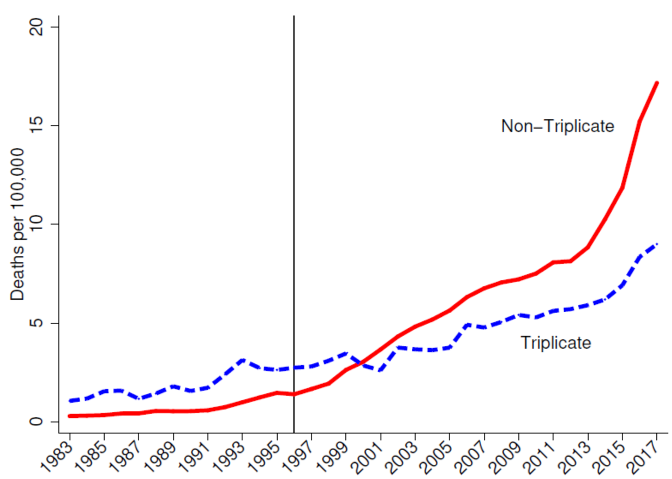
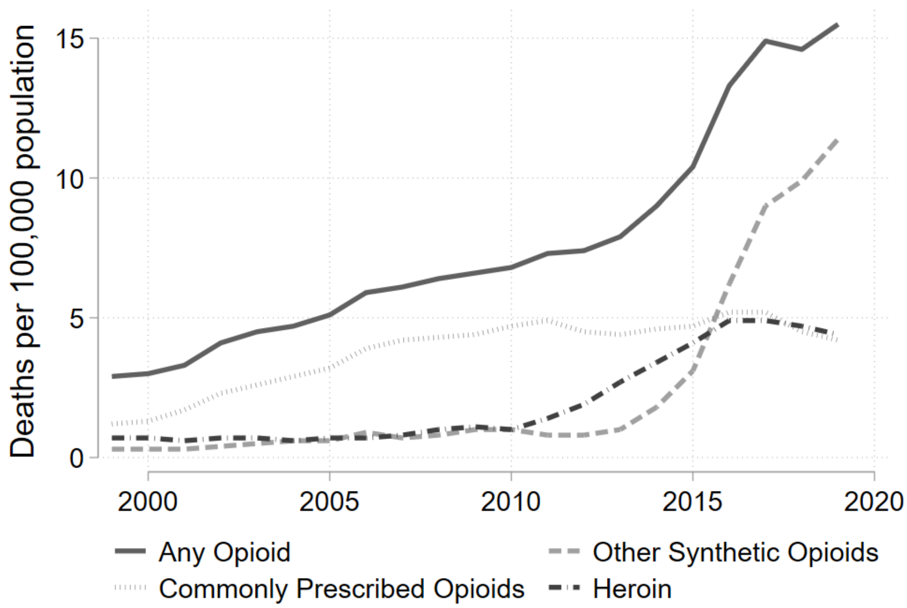


Figure B2: Temporal Patterns in Opioid-Related Deaths (1999-2019)

This figure is generated based on data provided by the Centers for Disease Control and Prevention (CDC) (`https://www.cdc.gov/drugoverdose/data/OD-deaths-2019.html`). The plot shows the pattern in opioid-related deaths between 1999 and 2019 by type of opioid. Other synthetic opioids include fentanyl and tramadol, and commonly prescribed opioids include natural and semi-synthetic opioids and methadone.



C Additional Analysis

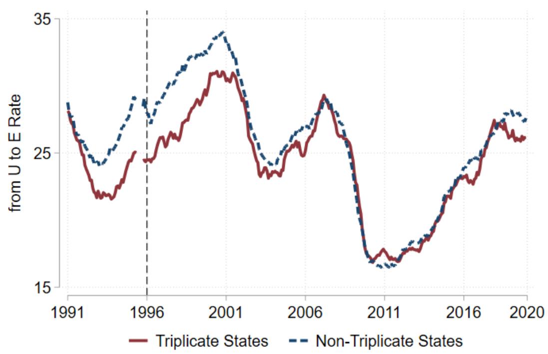
C.1 Labor Market Dynamics

Figure C1: Labor Market Flow by Triplicate Regulation Status (Seasonally Adjusted)

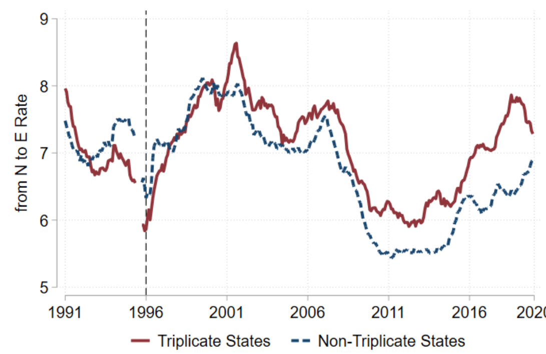
This figure shows the seasonally adjusted rates (in %) of labor market flows between employment (E), unemployment (U), and non-participation (N) by triplicate regulation. The dashed vertical line (January 1996) indicates the beginning of regulatory effect due to the entry of OxyContin. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-triplicate level. The data span January 1991 to December 2019.

(a) Flows to E

from U

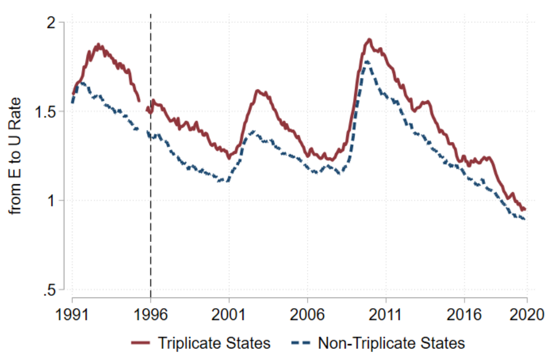


from N

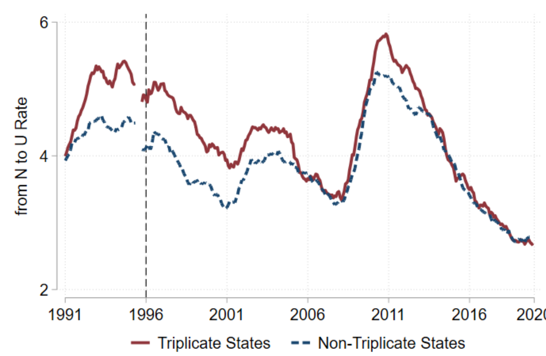


(b) Flows to U

from E

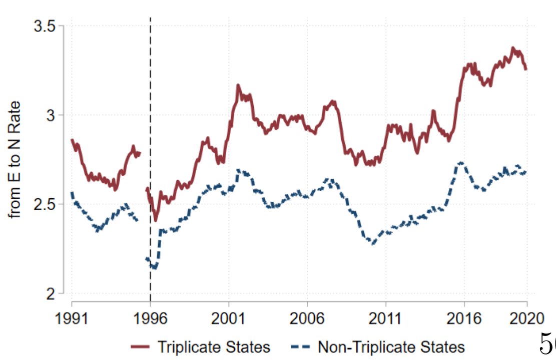


from N



(c) Flows to N

from E



from U

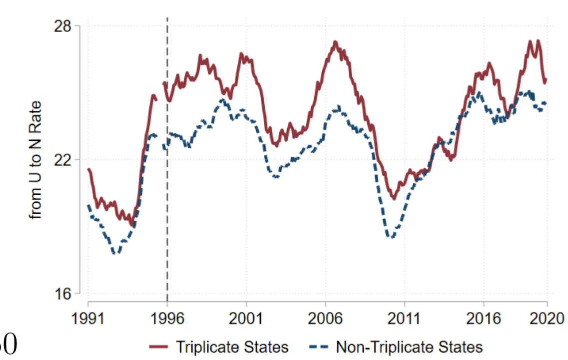
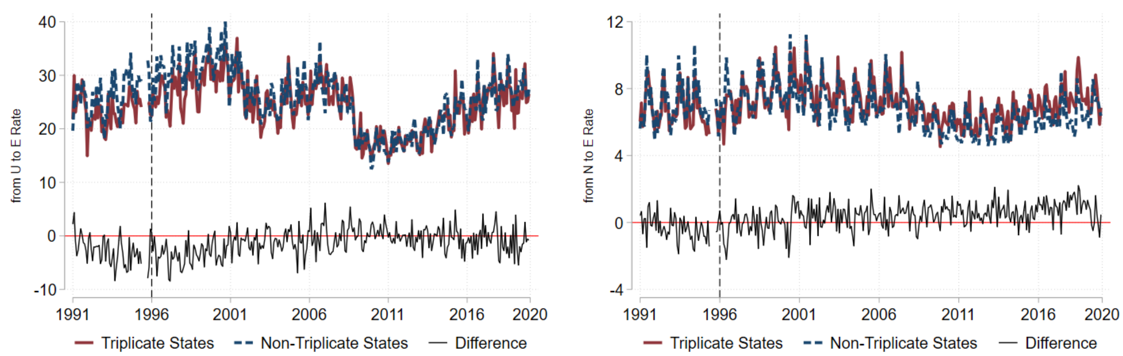


Figure C2: Labor Market Flow by Triplicate Regulation Status (Seasonally Unadjusted)

This figure shows the raw (seasonally unadjusted) rates (in %) of labor market flows between employment (E), unemployment (U), and non-participation (N) by triplicate regulation, in addition to differences between two. The dashed vertical line (January 1996) indicates the beginning of regulatory effect due to the entry of OxyContin. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-triplicate level. The data span January 1991 to December 2019.

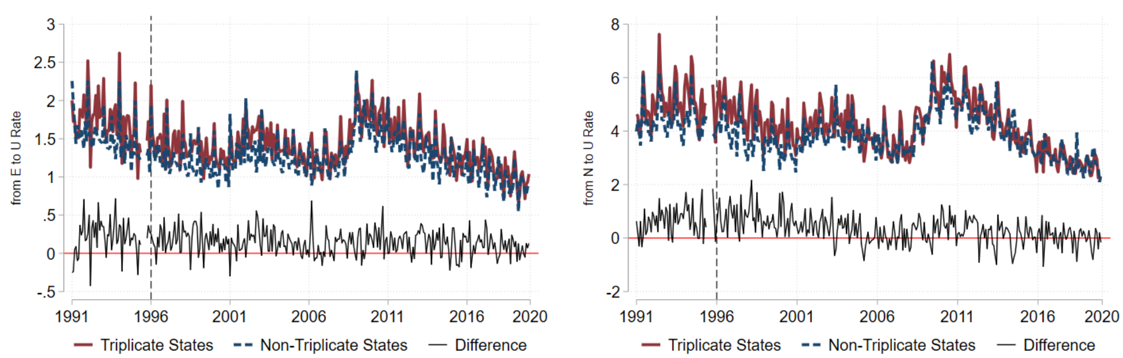
(a) Flows to E

from U | from N



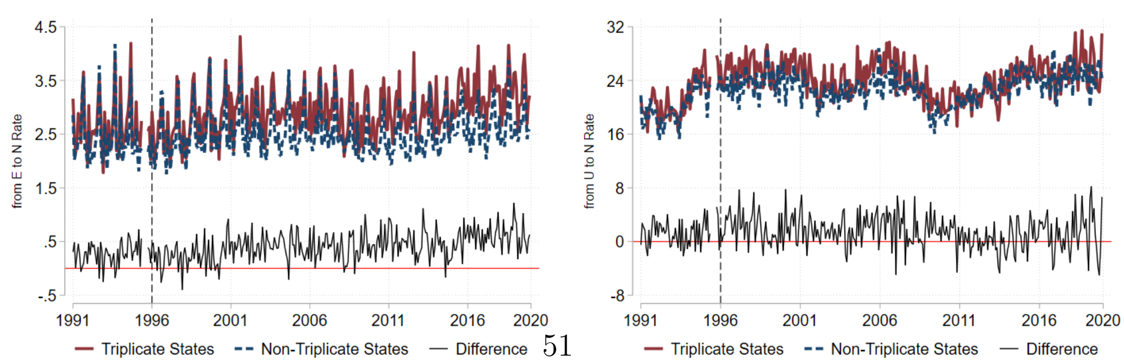
(b) Flows to U

from E | from N



(c) Flows to N

from E | from U

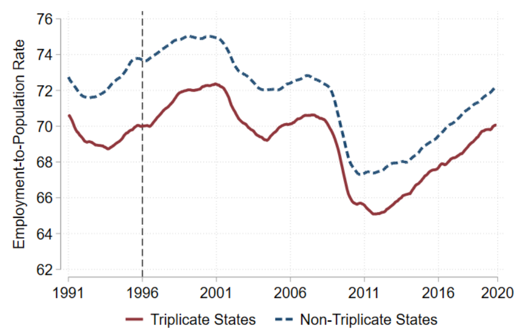


C.2 Labor Market Activity Levels

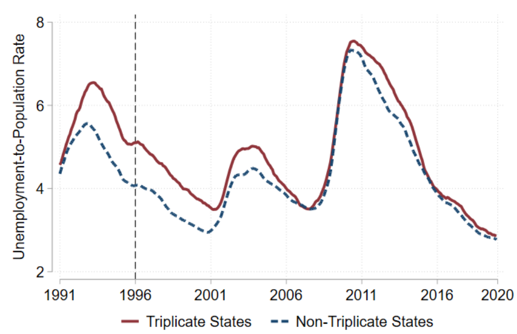
Figure C3: Labor Market Activity Levels, 1991-2019

This figure shows the seasonally adjusted rates (in % of population) of employment, unemployment, and non-participation by triplicate regulation. The dashed vertical line (January 1996) indicates the beginning of regulatory effect due to the entry of OxyContin. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-triplicate level. The data span January 1991 to December 2019.

(a) Employment-to-Population Rate



(b) Unemployment-to-Population Rate



(c) Not in Labor Force Participation-to-Population Rate

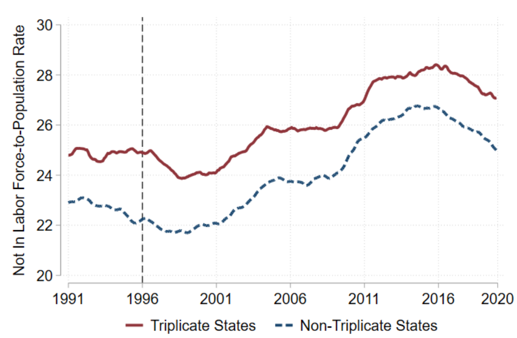
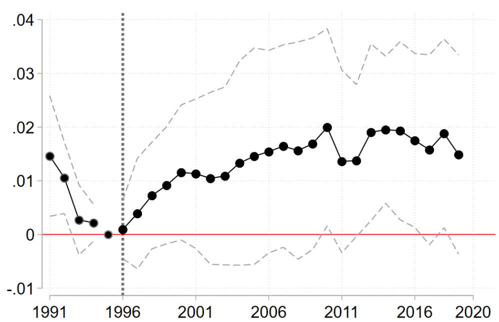


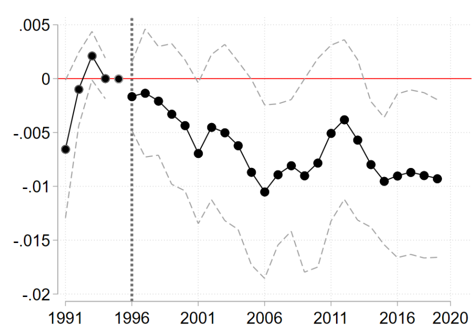
Figure C4: Event Study Analysis: Effect of Triplicate Regulation on Labor Market Levels

This figure shows the estimated coefficients of the triplicate regulation on employment (E), unemployment (U), and non-participation (N) to population rate. The specification includes triplicate indicators for whether the state has the regulation in force by 1996, the beginning of regulatory effect due to the entry of OxyContin. The excluded (reference) year is 1995. The data span January 1991 to December 2019. The dashed horizontal lines represent 95% confidence intervals based on state-clustered standard errors.

(a) Employment-to-Population Rate



(b) Unemployment-to-Population Rate



(c) Not in Labor Force Participation-to-Population Rate

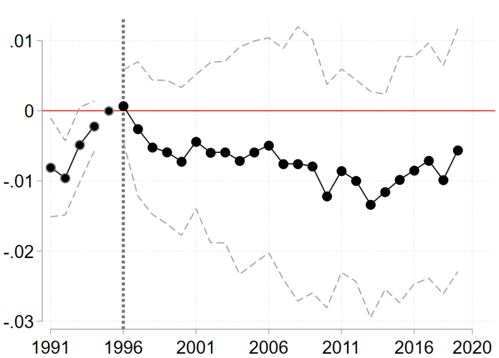


Table C1: Difference-in-Difference Analysis: Labor Market Activity Levels

This table uses labor market activity *levels* (not flows) as the outcome variable and follows the pooled post-period specification in Table 3 for Panel A, and the temporal heterogeneity specification in Table 4 for Panel B. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. The data span January 1991 to December 2019. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). The reported joint p-value in Panel B tests the joint statistical significance of the three coefficients using our wild bootstrap method.

A. Pooled Difference-in-Difference Analysis

	e	u	n
Triplicate ×	(1)	(2)	(3)
1996 - 2019	0.76	-0.55	-0.20
	(0.16, 1.36)	(-0.74, -0.36)	(-0.81, 0.41)
	[-0.84, 2.69]	[-0.93, 0.11]	[-2.38, 1.35]

B. Difference-in-Difference Analysis with Three Time Periods

	e	u	n
Triplicate ×	(4)	(5)	(6)
1996 - 2000	-0.13	-0.14	0.27
	(-0.49, 0.23)	(-0.37, 0.09)	(-0.05, 0.59)
	[-1.50, 0.52]	[-0.62, 0.42]	[-0.58, 1.36]
2001 - 2010	0.77	-0.63	-0.14
	(0.09, 1.44)	(-0.81, -0.37)	(-0.76, 0.42)
	[-1.16, 2.96]	[-1.00, 0.01]	[-2.35, 1.62]
2011 - 2019	0.95	-0.54	-0.41
	(0.30, 1.60)	(-0.74, -0.34)	(-1.09, 0.27)
	[-0.49, 3.51]	[-0.96, 0.30]	[-3.06, 1.07]
Joint p-value	0.65	0.29	0.46
N	17,544	17,544	17,544

C.3 Categorization of Physically Demanding Occupations

Table C2: List of Physically Demanding Occupations

This table lists the occupation codes and descriptions that fall into the physically demanding categorization used in certain analyses. The codes are obtained from the OCC1990 in the CPS; OCC1990 is a modified version of the 1990 Census Bureau occupational classification scheme. Physically demanding occupations are those with the largest non-routine manual physical skills among other five different skills based on task measures from the Occupational Information Network data (O*NET) following Acemoglu and Autor (2011): non-routine cognitive analytical skills, non-routine cognitive interpersonal skills, routine cognitive skills, routine manual skills, and offshorability.

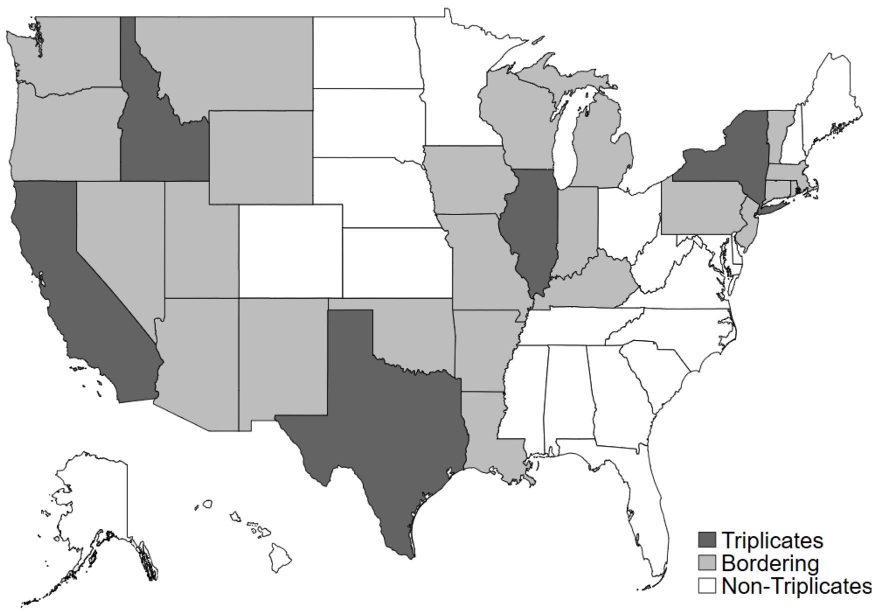
Code	Description
226	Airplane pilots and navigators
357	Messengers
364	Shipping and receiving clerks
417	Firefighting, prevention, and inspection
455	Pest control occupations
473	Farmers (owners and tenants)
474	Horticultural specialty farmers
483	Marine life cultivation workers
496	Timber, logging, and forestry workers
498	Fishers, hunters, and kindred
505	Automobile mechanics
507	Bus, truck, and stationary engine mechanics
508	Aircraft mechanics
509	Small engine repairers
514	Auto body repairers
516	Heavy equipment and farm equipment mechanics
518	Industrial machinery repairers
523	Repairers of industrial electrical equipment
526	Repairers of household appliances and power tools
527	Telecom and line installers and repairers
533	Repairers of electrical equipment, n.e.c.
534	Heating, air conditioning, and refrigeration mechanics
536	Locksmiths and safe repairers
538	Office machine repairers and mechanics
539	Repairers of mechanical controls and valves
543	Elevator installers and repairers
544	Millwrights

Code	Description
549	Mechanics and repairers, n.e.c.
563	Masons, tilers, and carpet installers
567	Carpenters
575	Electricians
577	Electric power installers and repairers
579	Painters, construction and maintenance
583	Paperhangers
585	Plumbers, pipe fitters, and steamfitters
588	Concrete and cement workers
589	Glaziers
593	Insulation workers
596	Sheet metal duct installers
597	Structural metal workers
599	Construction trades, n.e.c.
614	Drillers of oil wells
615	Explosives workers
617	Other mining occupations
643	Boilermakers
646	Lay-out workers
653	Tinsmiths, coppersmiths, and sheet metal workers
668	Upholsterers
726	Wood lathe, routing, and planing machine operators
733	Other woodworking machine operators
804	Truck, delivery, and tractor drivers
808	Bus drivers
809	Taxi cab drivers and chauffeurs
813	Parking lot attendants
823	Railroad conductors and yardmasters
824	Locomotive operators (engineers and firemen)
825	Railroad brake, coupler, and switch operators
829	Ship crews and marine engineers
853	Excavating and loading machine operators
865	Helpers, constructions
866	Helpers, surveyors
869	Construction laborers
877	Stock handlers
883	Freight, stock, and materials handlers
887	Vehicle washers and equipment cleaners
889	Laborers outside construction
905	Military

C.4 Stacked DID Analysis using Bordering States as Controls

Figure C5: Geography of Triplicate States and their Bordering States

This figure shows the geography of triplicate states and their bordering states used in the stacked difference-in-differences analysis in Table D2. The bordering states are Arizona, Nevada, and Oregon (to California); Montana, Nevada, Oregon, Utah, Washington, and Wyoming (to Idaho); Indiana, Iowa, Kentucky, Michigan, Missouri, and Wisconsin (to Illinois); Connecticut, Massachusetts, New Jersey, Pennsylvania, Rhode Island, and Vermont (to New York); and Arkansas, Louisiana, New Mexico, and Oklahoma (to Texas). Note that Oregon and Nevada are counted as bordering states to both California and Idaho.



We estimate the stacked DID analysis in Table D2 using:

$$y_{s,t} = \alpha_s + \gamma_t^{CA} + \gamma_t^{ID} + \gamma_t^{IL} + \gamma_t^{NY} + \gamma_t^{TX} + \delta_0 \times \mathbb{I}(Triplicate_s)\mathbb{I}(1996 \leq year_t \leq 2019) + \epsilon_{s,t}, \quad (4)$$

where γ is a group-time fixed effect in which each group is a triplicate state and its bordering states.

D Robustness Check Results

Table D1: Robustness Check: Including State-Specific, Time-Varying Covariates

This table follows the pooled post-period specification in Table 3 for Panel A, and the temporal heterogeneity specification in Table 4 for Panel B. All columns include the following state-specific, time-varying covariates: median age and proportion of white and non-Hispanic, black and non-Hispanic, education more than college, manufacturing industry sector, services industry sector and working in physically demanding occupations. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). The reported joint p-value in Panel B tests the joint statistical significance of the three coefficients using our wild bootstrap method.

A. Pooled Difference-in-Difference Analysis

	Flows to E		Flows to U		Flows to N	
Triplicate ×	from U	from N	from E	from N	from E	from U
1996 - 2019	1.44	0.52	-0.10	-0.42	0.17	0.19
	(0.97, 1.91)	(0.35, 0.69)	(-0.15, -0.05)	(-0.57, -0.27)	(0.12, 0.22)	(-0.21, 0.59)
	[0.21, 2.38]	[0.14, 0.95]	[-0.20, 0.05]	[-0.71, 0.09]	[0.06, 0.28]	[-0.72, 1.16]

B. Difference-in-Difference Analysis with Three Time Periods

	Flows to E		Flows to U		Flows to N	
Triplicate ×	from U	from N	from E	from N	from E	from U
1996 - 2000	-0.61	0.14	-0.06	-0.06	-0.00	0.68
	(-1.31, 0.09)	(0.00, 0.28)	(-0.11, -0.01)	(-0.24, 0.12)	(-0.04, 0.04)	(0.25, 1.11)
	[-2.16, 0.92]	[-0.17, 0.54]	[-0.17, 0.07]	[-0.39, 0.62]	[-0.10, 0.09]	[-0.25, 1.76]
2001 - 2010	2.31	0.55	-0.10	-0.51	0.19	0.52
	(1.63, 2.99)	(0.33, 0.77)	(-0.15, -0.05)	(-0.68, -0.34)	(0.11, 0.27)	(0.02, 1.02)
	[0.20, 3.70]	[0.08, 1.07]	[-0.21, 0.07]	[-0.87, -0.05]	[0.03, 0.38]	[-0.57, 1.70]
2011 - 2019	2.18	0.85	-0.12	-0.61	0.28	-0.67
	(1.50, 2.86)	(0.65, 1.05)	(-0.18, -0.06)	(-0.78, -0.44)	(0.22, 0.34)	(-1.13, -0.21)
	[0.34, 3.72]	[0.32, 1.33]	[-0.27, 0.07]	[-0.99, -0.09]	[0.14, 0.41]	[-1.68, 0.53]
Joint p-value	0.03	0.04	0.56	0.01	0.01	0.07

Table D2: Robustness Check: Stacked Difference-in-Differences Analysis with Bordering States

This table follows the pooled post-period specification in Table 3 for Panel A, and the temporal heterogeneity specification in Table 4 for Panel B. We use control states as Oregon, Arizona, and Nevada (for California), Washington, Montana, Wyoming, Oregon, Nevada and Utah (for Idaho), Iowa, Wisconsin, Missouri, Michigan, Indiana, and Kentucky (for Illinois), New Jersey, Vermont, Massachusetts, Connecticut, Pennsylvania, and Rhode Island (for New York), and Arkansas, Louisiana, New Mexico, and Oklahoma (for Texas). The detail of geography is in Appendix C.4. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets). The reported joint p-value in Panel B tests the joint statistical significance of the three coefficients using our wild bootstrap method.

A. Pooled Difference-in-Difference Analysis

	Flows to E		Flows to U		Flows to N	
Triplicate ×	from U	from N	from E	from N	from E	from U
1996 - 2019	1.01	0.83	-0.07	-0.21	0.24	0.11
	(0.49, 1.53)	(0.67, 0.99)	(-0.10, -0.04)	(-0.35, -0.07)	(0.19, 0.29)	(-0.40, 0.62)
	[-0.12, 2.88]	[0.48, 1.27]	[-0.15, 0.00]	[-0.61, 0.10]	[0.13, 0.38]	[-1.20, 1.26]

B. Difference-in-Difference Analysis with Three Time Periods

	Flows to E		Flows to U		Flows to N	
Triplicate ×	from U	from N	from E	from N	from E	from U
1996 - 2000	-0.90	0.25	-0.04	0.14	0.01	0.54
	(-1.57, -0.23)	(0.11, 0.39)	(-0.09, 0.01)	(-0.09, 0.37)	(-0.05, 0.07)	(-0.04, 1.12)
	[-2.36, 1.13]	[-0.07, 0.55]	[-0.19, 0.07]	[-0.53, 0.66]	[-0.15, 0.17]	[-0.96, 1.75]
2001 - 2010	1.50	0.87	-0.07	-0.17	0.28	0.58
	(0.92, 2.08)	(0.68, 1.06)	(-0.10, -0.04)	(-0.32, -0.02)	(0.21, 0.35)	(0.05, 1.11)
	[0.21, 3.21]	[0.48, 1.45]	[-0.15, -0.01]	[-0.58, 0.17]	[0.12, 0.49]	[-0.75, 1.83]
2011 - 2019	1.46	1.08	-0.08	-0.41	0.30	-0.53
	(0.82, 2.10)	(0.86, 1.30)	(-0.12, -0.04)	(-0.55, -0.27)	(0.24, 0.36)	(-1.20, 0.14)
	[0.22, 3.71]	[0.51, 1.59]	[-0.19, 0.01]	[-0.85, -0.14]	[0.15, 0.44]	[-2.16, 1.09]
Joint p-value	0.00	0.01	0.14	0.01	0.01	0.25

Table D3: Robustness Check: Omitting each Triplicate State

This table follows the pooled post-period specification in Table 3 and drops the listed triplicate state at a time in columns (2) through (6). Column (1) provides the result including all the states for reference, thus replicating the estimates in Table 3. The underlying data are individual observations from the entire US aggregated (using sample weights) to the month-year-state level. Below each coefficient, we report the 95% coefficient confidence intervals estimated by state-clustered standard errors (parentheses) and by wild bootstrap with 9,999 replications and a six-point weight distribution as in Webb (2013) (brackets).

Transition Rate	Nothing (1)	CA (2)	ID (3)	IL (4)	NY (5)	TX (6)
from U to E	1.82	1.57	1.86	2.06	1.66	1.84
	(1.34, 2.30)	(1.01, 2.13)	(1.38, 2.34)	(1.65, 2.47)	(1.13, 2.19)	(1.30, 2.38)
	[0.30, 2.81]	[-0.19, 2.88]	[0.36, 2.87]	[0.83, 3.10]	[-0.13, 2.83]	[-0.09, 3.08]
from N to E	0.74	0.83	0.77	0.75	0.57	0.77
	(0.56, 0.92)	(0.60, 1.06)	(0.59, 0.95)	(0.55, 0.95)	(0.47, 0.67)	(0.55, 0.99)
	[0.40, 1.35]	[-1.40, 1.42]	[0.44, 1.40]	[0.35, 1.43]	[0.09, 0.81]	[-1.16, -0.21]
from E to U	-0.10	-0.06	-0.09	-0.12	-0.09	-0.11
	(-0.14, -0.06)	(-0.11, -0.01)	(-0.14, -0.04)	(-0.16, -0.08)	(-0.14, -0.04)	(-0.16, -0.06)
	[-0.18, 0.05]	[-0.18, 0.10]	[-0.18, 0.06]	[-0.21, -0.01]	[-0.19, 0.09]	[-0.21, 0.09]
from N to U	-0.38	-0.26	-0.38	-0.40	-0.52	-0.30
	(-0.54, -0.22)	(-0.49, -0.03)	(-0.54, -0.22)	(-0.58, -0.22)	(-0.62, -0.42)	(-0.49, -0.11)
	[-0.68, 0.20]	[-0.78, 0.25]	[-0.68, 0.21]	[-0.76, 0.27]	[-0.78, -0.16]	[-0.63, 0.26]
from E to N	0.21	0.22	0.22	0.23	0.20	0.20
	(0.17, 0.25)	(0.18, 0.26)	(0.19, 0.25)	(0.20, 0.26)	(0.16, 0.24)	(0.16, 0.24)
	[0.11, 0.29]	[0.06, 0.30]	[0.12, 0.30]	[0.14, 0.30]	[0.06, 0.29]	[0.06, 0.29]
from U to N	0.22	0.41	0.22	0.36	0.11	0.04
	(-0.15, 0.59)	(-0.01, 0.83)	(-0.15, 0.59)	(-0.01, 0.73)	(-0.29, 0.51)	(-0.32, 0.40)
	[-0.66, 1.14]	[-1.02, 1.48]	[-0.67, 1.13]	[-0.47, 1.33]	[-1.12, 1.27]	[-1.07, 0.99]

E Earlier pre-periods necessitate covariate adjustment

This section discusses the role of time-varying covariates in establishing parallel (pre)trends in our context, a key assumption in the difference-in-difference framework. We begin with Figure E1 to show the descriptive evidence of differential time trends in four covariates across triplicate regulation: shares of White and non-Hispanic, Hispanic, College$^+$, and age 45-64. We estimate the following regression and present the $\delta_t s$ in Figure E1.

$$y_{s,t} = \alpha_s + \gamma_t + \sum_{t=1981,\, t\neq 1995}^{2019} \delta_t \times \mathbb{I}(Triplicate_s)\mathbb{I}(year_t) + \epsilon_{s,t}.$$

We observe that triplicate states and non-triplicate states show the diverging differential pretrends in four covariates and they are more distinctive in 1981-1990 than in 1991-1995.[18]

Given this evidence, we investigate how addressing covariates changes our coefficients. Table E1 displays three specification results: no covariates adjustment in Panel A as a baseline, the Callaway and Sant'Anna (2021) method in Panel B, and the Powell (2021) method in Panel C. Callaway and Sant'Anna (2021) uses a matching to balance covariates between a treated group and a control group. On the other hand, Powell (2021) allows a rich set of fixed effects (sex-triplicate and sex-year-month fixed effects) as well as triplicate varying covariates coefficients and uses a residualization method.

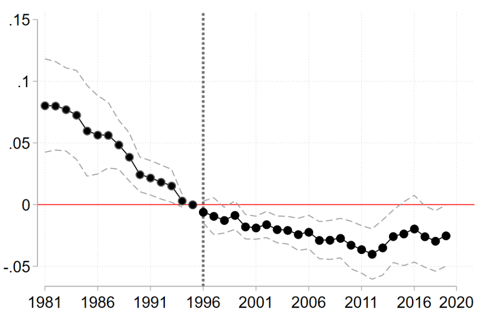
Table E1 and Figure E2 show the results from these exercises. Table E1 shows the triplicate effect in aggregated time bands as in Tables 3 and 4. The triplicate effect size is larger in Callaway and Sant'Anna (2021) and Powell (2021) methods than in no covariate adjustment specification, and even larger in Powell and Pacula (2021) than in Callaway and Sant'Anna (2021). This implies that incorporating the differential time trends in covariates enlarges the triplicate treatment effect. Second, Figure E2 shows the year-by-year triplicate effect corresponding to Figure 1, consistent with Table E1 specification but with the granular level of triplicate-time interaction dummies. Noticeably, the specification without covariate adjustment shows pretrends in all six labor dynamic outcomes. We leave in-depth investigation for time-varying covariates in difference-in-difference framework to the future research.

[18] In all analyses in the main paper, our sample begins in 1991.

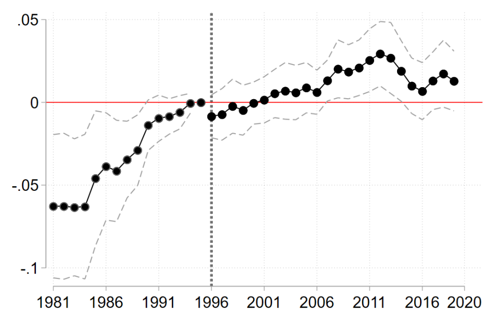
Figure E1: Event Study Analysis: Covariates

This figure shows the estimated coefficients of the triplicate regulation on the four main covariates: share of White and non-Hispanic, Hispanic, college^{+}, and age 45-64 to see the differential trends in covariates for triplicate and non-triplicate states. The specification includes triplicate indicators for whether the state has the regulation in force by 1996, the beginning of regulatory effect due to the entry of OxyContin. The data span January 1981 to December 2019. The dashed horizontal lines represent 95% confidence intervals based on state-clustered standard errors.

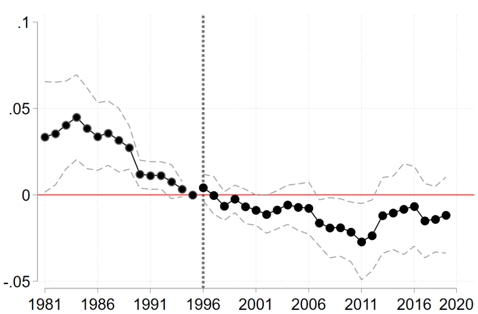
(a) White and Non-Hispanic



(b) Hispanic



(c) College^{+}



(d) Age 45-64

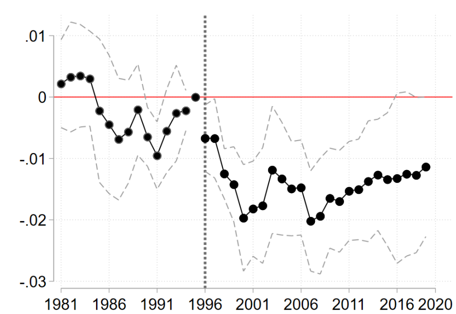


Table E1: Difference-in-Differences Analysis with Covariates Adjustment and Different Time Samples

This table provides three panels with different specifications, all using 1981-2019 data. Panel A follows the same specification in Table 3. Panel B follows Callaway and Sant'Anna (2021), doubly robust method using covariates of White and non-Hispanic share, Hispanic share, college^{+} share, and age 45-64 share. Sex dummy is included being fully interacted with each covariate. Panel C follows Powell (2021) specification with triplicate varying covariates. The statefip clustered robust standard errors are in parentheses.

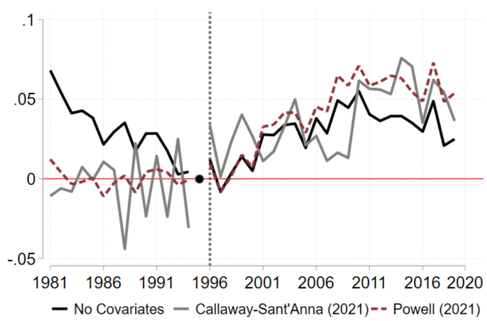
	Flows to E		Flows to U		Flows to N	
	from U	from N	from E	from N	from E	from U
Triplicate ×	(1)	(2)	(3)	(4)	(5)	(6)
A. No Covariates						
1996 - 2019	0.14	0.38	0.06	0.00	0.19	0.10
	(0.70)	(0.22)	(0.05)	(0.20)	(0.04)	(0.39)
1996 - 2000	-2.28	-0.13	0.09	0.43	-0.02	0.62
	(0.85)	(0.24)	(0.04)	(0.21)	(0.05)	(0.36)
2001 - 2010	0.78	0.34	0.06	-0.01	0.18	0.49
	(0.59)	(0.26)	(0.05)	(0.17)	(0.05)	(0.50)
2011 - 2019	0.68	0.68	0.04	-0.16	0.28	-0.48
	(1.05)	(0.23)	(0.06)	(0.23)	(0.05)	(0.48)
B. Callaway and Sant'Anna (2021), Doubly Robust Method						
1996 - 2019	3.69	0.93	0.07	-0.98	-0.17	1.18
	(1.49)	(0.51)	(0.08)	(0.40)	(0.13)	(1.44)
1996 - 2000	2.50	0.41	-0.03	-0.92	-0.42	2.24
	(1.55)	(0.54)	(0.07)	(0.51)	(0.09)	(1.30)
2001 - 2010	2.62	0.85	0.13	-0.82	-0.16	1.29
	(1.98)	(0.51)	(0.08)	(0.39)	(0.15)	(1.38)
2011 - 2019	5.61	1.31	0.09	-1.07	-0.08	0.36
	(1.30)	(0.54)	(0.10)	(0.35)	(0.15)	(1.52)
C. Powell (2021) Method, Triplicate Varying Covariates						
1996 - 2019	4.30	1.94	-0.23	-1.10	0.00	-0.85
	(0.47)	(0.12)	(0.04)	(0.19)	(0.02)	(0.27)
1996 - 2000	0.51	0.73	-0.14	-0.30	-0.12	0.16
	(0.69)	(0.25)	(0.02)	(0.16)	(0.05)	(0.11)
2001 - 2010	4.61	1.73	-0.20	-1.06	0.01	-0.30
	(0.22)	(0.16)	(0.05)	(0.16)	(0.04)	(0.32)
2011 - 2019	5.83	-1.79	-0.31	-1.49	0.04	-1.79
	(0.69)	(0.37)	(0.05)	(0.24)	(0.03)	(0.37)

Figure E2: Event Study Analysis with Different Covariates Adjustment

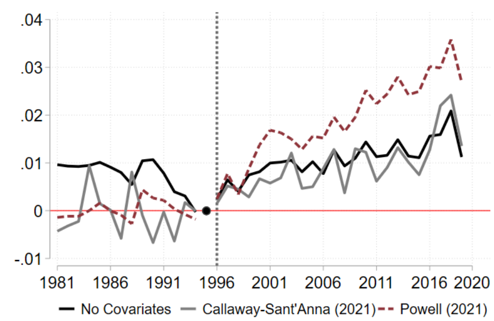
This figure shows the point estimates of the triplicate regulation effect on the six labor market flows between employment (E), unemployment (U), and non-participation (N) with three different specifications regarding covariates adjustment. The specification includes triplicate indicators for whether the state has the regulation in force by 1996, the beginning of regulatory effect due to the entry of OxyContin. The data span January 1981 to December 2019.

(a) Flows to E

from U

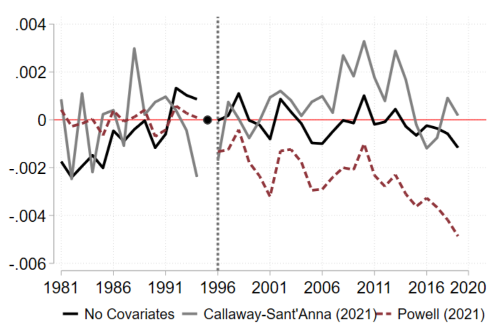


from N

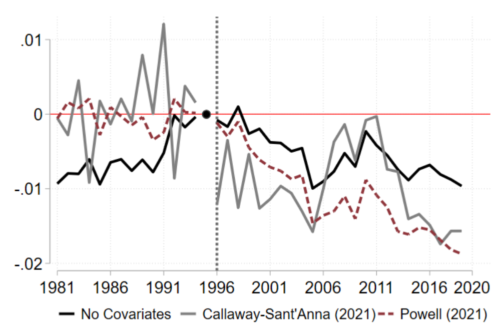


(b) Flows to U

from E

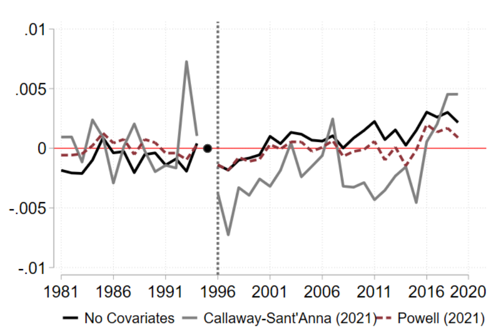


from N

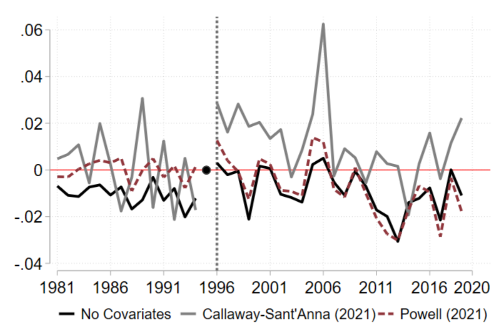


(c) Flows to N

from E



from U



F Method for steady state effects

We begin by introducing notation for the Markov process between one of three labor market states: e (employed), u (unemployed), and n (not in labor force). Let $P_{s,t}$ be the transition matrix between these states, with each matrix element denoted by p. For example, $p_{s,t}^{e|u}$ denotes the transition probability from unemployment (u) to employment (e) for state (s) in a given year-month-of-sample (t). The matrix $P_{s,t}$ contains nine elements, as shown below, from which we estimate the six unique transition probabilities (transitions that keep an individual in the same state are defined by 1 minus the probabilities they exit the state):

$$P_{s,t} = \begin{bmatrix} 1 - p_{s,t}^{u|e} - p_{s,t}^{n|e} & p_{s,t}^{u|e} & p_{s,t}^{n|e} \\ p_{s,t}^{e|u} & 1 - p_{s,t}^{n|u} - p_{s,t}^{e|u} & p_{s,t}^{n|u} \\ p_{s,t}^{e|n} & p_{s,t}^{u|n} & 1 - p_{s,t}^{u|n} - p_{s,t}^{e|n} \end{bmatrix}. \tag{5}$$

We proceed with the first step of our methodology. The Markov chain enables calculation of the steady state distribution of employment for each individual, $\pi_{s,t}$, by solving $\pi_{s,t} = \pi_{s,t}\mathbf{P}_{s,t}$ — steady state is unchanging in time. The vector $\pi_{s,t}$ contains the steady state distributions of each employment status, i.e., $\pi_{s,t} = \left[\pi_{s,t}^e, \pi_{s,t}^u, \pi_{s,t}^n\right]$ where $\pi_{s,t}^e$ is the steady state density of the individuals being employed. For a three-state Markov chain with $\pi_{s,t}^e + \pi_{s,t}^u + \pi_{s,t}^n = 1$, the steady state levels of each element are given by the following:

$$\pi_{s,t}^e = \frac{\lambda_e}{\lambda_e + \lambda_u + \lambda_n}, \qquad \pi_{s,t}^u = \frac{\lambda_u}{\lambda_e + \lambda_u + \lambda_n}, \qquad \pi_{s,t}^n = \frac{\lambda_n}{\lambda_e + \lambda_u + \lambda_n}, \tag{6}$$

where

$$\lambda_e = p_{s,t}^{e|n} \cdot p_{s,t}^{n|u} + p_{s,t}^{e|u} \cdot p_{s,t}^{u|n} + p_{s,t}^{e|n} \cdot p_{s,t}^{e|u}, \tag{7}$$

$$\lambda_u = p_{s,t}^{u|n} \cdot p_{s,t}^{n|e} + p_{s,t}^{u|e} \cdot p_{s,t}^{e|n} + p_{s,t}^{u|n} \cdot p_{s,t}^{u|e}, \tag{8}$$

$$\lambda_n = p_{s,t}^{n|u} \cdot p_{s,t}^{u|e} + p_{s,t}^{n|e} \cdot p_{s,t}^{e|u} + p_{s,t}^{n|u} \cdot p_{s,t}^{n|e}. \tag{9}$$

We use the observed labor market flows for triplicate states to plug in for $\mathbf{P}_{s,t}$ and solve for $\pi_{s,t}$. We use the notation "obs" to denote these values as they are calculated based on observed data, e.g., $\pi_{s,t}^{e,obs}$ represents the steady state employment level based on observed data.

In the second step of our methodology, we subtract (ii) counterfactual steady state levels without triplicate effect from (i) the realized steady state levels in triplicate states:

$$\underbrace{\Delta SS_{s,t}}_{\text{steady state effect}} = \underbrace{SS_{s,t}^{obs}}_{(i)\text{ observed steady state in triplicate states}} - \underbrace{SS_{s,t}^{cf}}_{(ii)\text{ counterfactual steady state without triplicate regulation effect}}$$

To obtain (ii), we adjust the observed transition rates of triplicate states by the regression coefficients in Panel B of Table 4 (estimated from equation 2). These are estimated at the year-month-of-sample level to be consistent with our regressions. We use the superscript "cf" to denote these counterfactual values, e.g., the counterfactual transition rates from employed to unemployed are calculated as $p_{s,t}^{u|e,cf} = p_{s,t}^{u|e,obs} - \delta_1$ where $1996m1 \leq t \leq 2000m12$ for $\forall s$. Once we have counterfactual transition rates for each state s in each time period t, we follow the same procedure as in our first step and obtain the counterfactual steady state rates $\pi_{s,t}^{e,cf}$, $\pi_{s,t}^{u,cf}$, and $\pi_{s,t}^{n,cf}$ of triplicate states, e.g. the steady state rates of triplicate states if they were not triplicate states.

The third step of our methodology results in the subtraction of the results from our interim steps, notated as $\Delta SS_{s,t}$ ($\Delta\pi_{s,t}^{e}$, $\Delta\pi_{s,t}^{u}$, and $\Delta\pi_{s,t}^{n}$). Our final results are the average of $\Delta\pi_{s,t}^{e}$, $\Delta\pi_{s,t}^{u}$, and $\Delta\pi_{s,t}^{n}$ for the five triplicate states and the relevant time bands in our analysis (1996−2019, 1996−2000, 2001−2010, and 2011−2019).

G Inference method for the steady state effects

G.1 Delta method

We use a delta method as an inference method for the steady state effects for two main reasons: (i) the availability of a closed form solution and (ii) the property of asymptotic normality. With the central limit theorem and the assumption that a consistent estimator B converges in probability to its true value β, the following asymptotic normality is obtained:

$$\sqrt{n}(B - \beta) \xrightarrow{d} N(0, \Sigma),$$

where n is the number of observations and Σ is a symmetric positive semi-definite covariance matrix. By taking the first two terms of the Taylor series, the delta method implies:

$$\sqrt{n}(h(B) - h(\beta)) \xrightarrow{D} N(0, \nabla h(\beta)^T \Sigma \nabla h(\beta)).$$

G.2 Steady state estimators

Let us denote the coefficients obtained from transition rate ($e|u, n|u, e|n, u|n, u|e, n|e$) regressions $\hat{\beta}^{e|u}, \hat{\beta}^{n|u}, \hat{\beta}^{e|n}, \hat{\beta}^{u|n}, \hat{\beta}^{u|e}, \hat{\beta}^{n|e}$. As we explained in Section 5.3, the steady state effect estimate of $L \in \{u, n, e\}$ can be expressed as:

$$\begin{aligned}
\Delta\hat{\pi}_{L,t} &= \underbrace{\pi_{L,t}^{*}}_{\text{observed}} - \underbrace{\pi_{L,t}^{cf}}_{\text{counterfactual}} \\
&= \frac{\lambda_{L,t}^{*}}{\underbrace{\lambda_{e,t}^{*} + \lambda_{u,t}^{*} + \lambda_{n,t}^{*}}_{\text{observed}}} - \frac{\lambda_{L,t}^{cf}}{\underbrace{\lambda_{e,t}^{cf} + \lambda_{u,t}^{cf} + \lambda_{n,t}^{cf}}_{\text{counterfactual}}} \\
&= f(\hat{\beta}_t^{e|u}, \hat{\beta}_t^{n|u}, \hat{\beta}_t^{e|n}, \hat{\beta}_t^{u|n}, \hat{\beta}_t^{u|e}, \hat{\beta}_t^{n|e})
\end{aligned}$$

G.3 Standard error calculation

By asymptotic normality assumption on coefficients and delta method,

$$\sqrt{n}(\Delta\pi_{L,t} - \Delta\hat{\pi}_{L,t}) \xrightarrow{d} N\big(0, \hat{Var}(\Delta\hat{\pi}_{L,t})\big)$$

where $\hat{Var}(\Delta\hat{\pi}_{L,t}) = \nabla f^T_{L,t}(\hat{\beta}^{e|u}_t, \hat{\beta}^{n|u}_t, \hat{\beta}^{e|n}_t, \hat{\beta}^{u|n}_t, \hat{\beta}^{u|e}_t, \hat{\beta}^{n|e}_t)\hat{\Sigma}_{L,t}\nabla f_{L,t}(\hat{\beta}^{e|u}_t, \hat{\beta}^{n|u}_t, \hat{\beta}^{e|n}_t, \hat{\beta}^{u|n}_t, \hat{\beta}^{u|e}_t, \hat{\beta}^{n|e}_t)$.

By using chain rule, each element of the gradient of $\Delta\hat{\pi}_{L,t}$ with respect to $\hat{\beta}^{e|u}_t, \hat{\beta}^{n|u}_t, \hat{\beta}^{e|n}_t, \hat{\beta}^{u|n}_t, \hat{\beta}^{u|e}_t$, and $\hat{\beta}^{n|e}_t$,

$$\nabla f^T_{L,t}(\hat{\beta}^{e|u}_t, \hat{\beta}^{n|u}_t, \hat{\beta}^{e|n}_t, \hat{\beta}^{u|n}_t, \hat{\beta}^{u|e}_t, \hat{\beta}^{n|e}_t) = \Big[\frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{e|u}_t}\ \frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{n|u}_t}\ \frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{e|n}_t}\ \frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{u|n}_t}\ \frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{u|e}_t}\ \frac{\partial\Delta\hat{\pi}_{L,t}}{\partial\hat{\beta}^{n|e}_t}\Big],$$

can be acquired. For example, $\frac{\partial\Delta\hat{\pi}_u}{\partial\hat{\beta}^{e|u}}$[19] is as follows:

$$\begin{aligned}
\frac{\partial\Delta\hat{\pi}_u}{\partial\hat{\beta}^{e|u}} &= \frac{\partial\Delta\hat{\pi}_u}{\partial\lambda^{cf}_e}\cdot\frac{\partial\lambda^{cf}_e}{\partial\hat{\beta}^{e|u}} + \frac{\partial\Delta\hat{\pi}_u}{\partial\lambda^{cf}_u}\cdot\frac{\partial\lambda^{cf}_u}{\partial\hat{\beta}^{e|u}} + \frac{\partial\Delta\hat{\pi}_u}{\partial\lambda^{cf}_n}\cdot\frac{\partial\lambda^{cf}_n}{\partial\hat{\beta}^{e|u}} \\
&= \frac{\lambda^{cf}_u}{(\lambda^{cf}_e + \lambda^{cf}_u + \lambda^{cf}_n)^2}\cdot\{-(\overline{p^{u|n}_s}^* - \hat{\beta}^{u|n}) - (\overline{p^{e|n}_s}^* - \hat{\beta}^{e|n})\} + \frac{\lambda^{cf}_u}{(\lambda^{cf}_e + \lambda^{cf}_u + \lambda^{cf}_n)^2}\cdot\{-(\overline{p^{n|e}_s}^* - \hat{\beta}^{n|e})\}.
\end{aligned}$$

Next, each element of the covariance matrix consists of as follows, where the square roots of the diagonal elements yield the standard errors:

$$\hat{\Sigma}_{L,t} = \begin{bmatrix}
\hat{Var}(\hat{\epsilon}^{e|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{e|u}_t, \hat{\epsilon}^{n|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{e|u}_t, \hat{\epsilon}^{e|n}_t|X) & \hat{Cov}(\hat{\epsilon}^{e|u}_t, \hat{\epsilon}^{u|n}_t|X) & \cdots & \hat{Cov}(\hat{\epsilon}^{e|u}_t, \hat{\epsilon}^{n|e}_t|X) \\
\hat{Cov}(\hat{\epsilon}^{n|u}_t, \hat{\epsilon}^{e|u}_t|X) & \hat{Var}(\hat{\epsilon}^{n|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{n|u}_t, \hat{\epsilon}^{e|n}_t|X) & \hat{Cov}(\hat{\epsilon}^{n|u}_t, \hat{\epsilon}^{u|n}_t|X) & \cdots & \hat{Cov}(\hat{\epsilon}^{n|u}_t, \hat{\epsilon}^{n|e}_t|X) \\
\hat{Cov}(\hat{\epsilon}^{e|n}_t, \hat{\epsilon}^{e|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{e|n}_t, \hat{\epsilon}^{n|u}_t|X) & \hat{Var}(\hat{\epsilon}^{e|n}_t|X) & \hat{Cov}(\hat{\epsilon}^{e|n}_t, \hat{\epsilon}^{u|n}_t|X) & \cdots & \hat{Cov}(\hat{\epsilon}^{e|n}_t, \hat{\epsilon}^{n|e}_t|X) \\
\hat{Cov}(\hat{\epsilon}^{u|n}_t, \hat{\epsilon}^{e|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{u|n}_t, \hat{\epsilon}^{n|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{u|n}_t, \hat{\epsilon}^{e|n}_t|X) & \hat{Var}(\hat{\epsilon}^{u|n}_t|X) & \cdots & \hat{Cov}(\hat{\epsilon}^{u|n}_t, \hat{\epsilon}^{n|e}_t|X) \\
\vdots & \vdots & \vdots & \vdots & & \vdots \\
\hat{Cov}(\hat{\epsilon}^{n|e}_t, \hat{\epsilon}^{e|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{n|e}_t, \hat{\epsilon}^{n|u}_t|X) & \hat{Cov}(\hat{\epsilon}^{n|e}_t, \hat{\epsilon}^{e|n}_t|X) & \hat{Cov}(\hat{\epsilon}^{n|e}_t, \hat{\epsilon}^{u|n}_t|X) & \cdots & \hat{Var}(\hat{\epsilon}^{n|e}_t|X)
\end{bmatrix}.$$

[19] The time period subscript t is omitted for convenience.