# Opioids and Post-COVID Labor-Force Participation*

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**Abstract**

At the onset of COVID-19, U.S. labor-force participation fell by about 3 percentage points and remained below pre-pandemic levels three years later. Recovery was slower in states hit harder by the pre-pandemic opioid crisis, measured by age-adjusted overdose death rates. An event study shows that a one-standard-deviation increase in pre-COVID opioid deaths led to a 0.9 percentage point drop in post-COVID labor participation. This effect wasn't due to differences in overall health across states and was stronger among those without a college degree. In high-opioid states, slower recovery was linked to more people leaving the workforce due to disability.

**Keywords:** Labor-Force Participation, Opioids, COVID-19, Health, Disability
**JEL Codes:** I12, I14, J11, J12, J21

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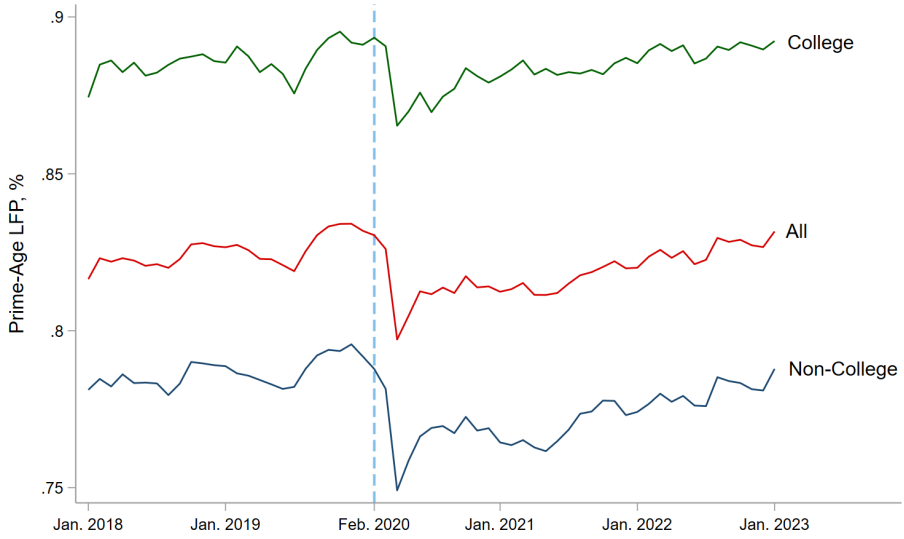
# 1 Introduction

The COVID-19 pandemic caused a sharp drop in US labor-force participation, and the recovery was slow. As Figure 1 shows, the labor-force participation among prime-age workers (25–54) declined by about 3 percentage points. The decline was more pronounced for those without a college degree. Furthermore, the pace of recovery in the labor supply varied significantly across US states. In December 2022, the LFP among prime-age workers was still 5 percentage points below the trend in Michigan, while the LFP gap relative to the trend had disappeared in Virginia. On average, LFP was around 2 percentage points below the trend in December 2022, with a standard deviation of 2 percentage points across US states.

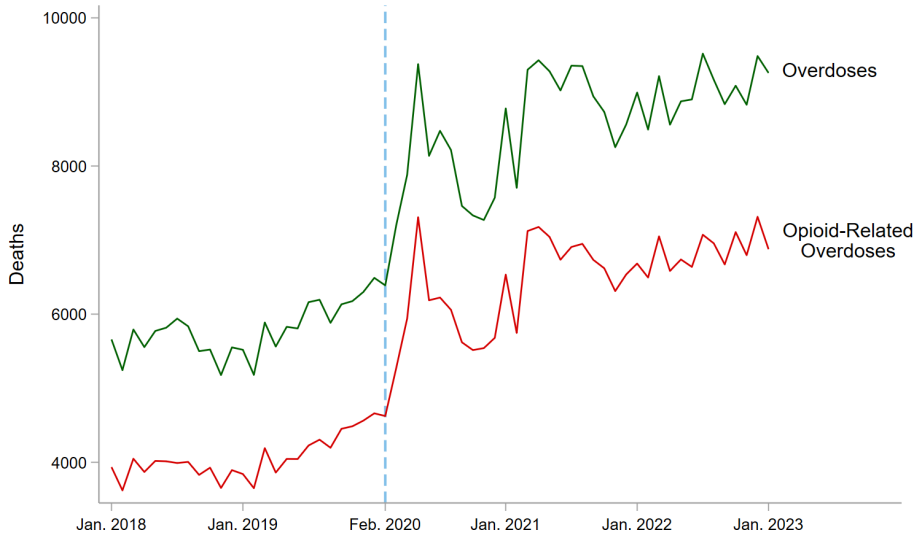
**Figure 1:** Labor-Force Participation Rate, Ages 25-54




**Notes:** *Monthly labor-force participation rates among 25-54-year-olds by education. Source: Current Population Survey (CPS).*

The COVID-19 pandemic struck the U.S. in the midst of another unfolding epidemic: the opioid crisis (Cutler and Glaeser, 2021; Alpert et al., 2022; Greenwood, Guner, and Kopecky, 2024). Between 2000 and 2019, nearly half a million people died from an opioid overdose. Deaths from opioid overdoses increased significantly during COVID-19, as shown in Figure 2. The number of deaths rose from around 50,000 in 2019 to more than 80,000 in 2021 and 2022. Deaths from other illicit drugs, mostly methamphetamine, also increased during COVID-19. The total number of drug overdose deaths increased from about 70,000 in 2019 to over 106,000 in 2021 and 2022. The increase in drug overdose deaths during this period is also documented by Mulligan (2022) and Greenwood, Guner, and Kopecky (2022).

**Figure 2:** Monthly Deaths from Overdose and Opioid Overdose




**Notes**: *Monthly deaths due to overdoses and opioid-related overdoses. Source: CDC-Multiple Cause of Death data.*

Individuals who misuse opioids or use other illicit drugs are significantly less likely to participate in the labor force compared to those who do not use opioids at all or use them strictly as prescribed (Greenwood, Guner, and Kopecky, 2022). It is shown here that labor supply recovery after the COVID-19 pandemic was slower in US states with higher pre-pandemic exposure to the opioid crisis, using an event study approach. The identification strategy utilizes a treatment effect influenced by an initial moderating condition: the onset of COVID-19 serves as the treatment and pre-COVID opioid exposure acts as the moderator. The central hypothesis posits that while COVID-19 led to increased use of illicit drugs nationwide, states with higher pre-existing opioid exposure experienced greater increases, further delaying their labor-force recovery.

Pre-COVID opioid exposure is measured using age-adjusted opioid overdose death rates from 2017, serving as a comprehensive indicator of the opioid crisis's intensity in each state. Additionally, this measure suffers less from measurement error than other measures. Our identification strategy rests on the idea that COVID-19 led to larger increases in illicit drug use in states with higher pre-pandemic opioid exposure, generating differential labor supply responses. We therefore first examine whether overdose deaths rose more in high-exposure states after the onset of COVID-19. We find that a one-standard-deviation increase in pre-COVID opioid exposure is associated with a 0.5-point increase in the monthly overdose death rate by May 2020 and a 1.7-point increase in the annual overdose death rate in 2020. We

also show that after COVID-19, there was a larger increase in the prevalence of illicit drug use disorder in states with higher pre-COVID opioid exposure.

The analysis then shows that a one-standard-deviation increase in pre-COVID opioid exposure reduces labor-force participation by approximately 0.9 percentage points below the trend following the COVID-19 shock. For instance, moving from a state at the 25th percentile of opioid exposure, such as Minnesota, to one at the 75th percentile, such as Michigan or Pennsylvania, corresponds to a 1.1 percentage-point decline in labor-force participation. This is substantial, given that the standard deviation in prime-age labor-force participation throughout the post-COVID period across US states was 3.4 percentage points.

The finding is robust to multiple event-study specifications with flexible state-specific trends and longer pre-periods. The results are also robust to alternative measures of pre-COVID opioid exposure, including the average age-adjusted opioid overdose death rates for the 2010-2017 period, the fraction of individuals with an opioid use disorder, shipments of medications for opioid addiction treatment, and pre-2010 OxyContin exposure. An alternative synthetic control group approach, as outlined in Abadie and Gardeazabal (2003), also yields results comparable to those obtained with the original approach.

We show that the finding is not driven by general health differences across states. States with higher initial opioid exposure may have poorer overall health or weaker healthcare systems, potentially explaining the observed effects. To test this, we conduct a placebo analysis, replacing opioid exposure with age-adjusted death rates from leading non-opioid-related causes. The results show no significant differences in post-COVID labor-force participation between states with higher and lower non-opioid-related death rates.

Furthermore, we find that the effects vary by demographics. While the results are similar for both men and women, differences emerge by educational attainment and age. The effects are significant for labor-force participation among non-college-educated people, who tend to have higher rates of opioid use, but not for the college-educated. Moreover, the impact of opioids on labor-force participation is more pronounced among individuals aged 44 to 54.

Finally, we explore a possible mechanism to help interpret the results. Individuals with a drug use disorder typically have lower labor-force participation, are more likely to leave the labor force due to disability, and experience poorer health. During COVID-19, the differences in labor-force participation and disability rates between nonusers and individuals with drug use disorders widened. Empirical analysis reveals that states with higher pre-COVID opioid

exposure saw a greater post-COVID increase in the share of individuals exiting the labor force due to disability, highlighting the critical role of health-related factors in the sluggish labor supply recovery.

**Related Literature** This study adds to the recent literature on the opioid epidemic's impact on labor market outcomes. The link between worsening labor market conditions and increased opioid use has been highlighted by Hollingsworth, Ruhm, and Simon (2017), Carpenter, McClellan, and Rees (2017), Pierce and Schott (2020), and Venkataramani et al. (2020). Others, such as Krueger (2017), Harris et al. (2020), Powell (2022), and Aliprantis, Fee, and Schweitzer (2023), have examined how opioid use reduces labor-force participation and employment by leveraging geographic variations in opioid exposure. This paper adds to the literature by examining how pre-existing opioid exposure shaped labor-force participation in response to a large health shock.

Within this literature, exogenous changes in prescription drug formulas or their availability have been explored to tease out the causal effects of opioid use on labor market outcomes. The introduction of an abuse-deterrent version of OxyContin in 2010 is used by Alpert, Powell, and Pacula (2018) to show that it led to a higher number of heroin deaths in states with higher initial OxyContin exposure. The same strategy is also used by Cho et al. (2021) to estimate the negative effects of heroin use on employment and labor-force participation. Beheshti (2023) performs a similar analysis using regulatory changes that made prescribing hydrocodone more difficult in 2014 as an exogenous shock. Like Alpert, Powell, and Pacula (2018), he compares units affected differently by a treatment (changes in drug availability) and shows that areas with larger reductions in hydrocodone prescriptions saw relative improvements in labor-force participation and employment.

The findings are also related to the literature on the effects of the COVID-19 pandemic on the labor market. Several studies focused on differences across demographic or socioeconomic characteristics, occupations, and industries in their suitability to remote work and, as a result, on how they are impacted by the epidemic. Alon et al. (2020) and Albanesi and Kim (2021) focus on gender, while Bartik et al. (2020), Dingel and Neiman (2020), Adams-Prassl et al. (2022*b*), and Mongey, Pilossoph, and Weinberg (2021) highlight the role of occupations and their task contents. Another strand of the literature, which is more closely related to the current analysis, documents labor market dynamics during and after the epidemic. The labor market after the epidemic was surprisingly tight, with low unemployment and labor-force participation rates (Coibion, Gorodnichenko, and Weber, 2020; Forsythe et al., 2022). At the

same time, quits and the number of workers looking for new jobs increased (Gittleman, 2022; Barlevy et al., 2024). There was also a decline in desired work hours that persisted through the end of 2021, as shown in Faberman, Mueller, and Şahin (2022). Bagga et al. (2023) suggest that the post-COVID period was characterized by a shift in workers' valuation of specific job amenities, particularly remote work, leading to persistent labor reallocation. The current analysis contributes to the literature by focusing on opioid use, a factor influencing labor-force participation, that was significantly impacted during the COVID-19 pandemic.

The rest of the paper is structured as follows: Section 2 discusses the data. Section 3 presents the empirical methodology. Section 4 shows the results from the main specification. Robustness checks are presented in Section 5, while Section 6 highlights potential mechanisms. Section 7 concludes.

## 2 Data and Motivating Evidence

The empirical analysis is conducted at the US state level, covering each month from January 2018 to January 2023. The primary outcome variable is the *prime-age labor-force participation rate* (LFP), defined as the share of the civilian noninstitutional population aged 25 to 54 who are in the labor force. We compute monthly state-level LFP values from the Current Population Survey (CPS) (Flood et al., 2025).[1] CPS data on gender and educational attainment are also used to analyze outcomes of different socioeconomic groups. Respondents are classified as college-educated if they have attained at least a bachelor's degree.

The primary measure of pre-COVID opioid exposure is the age-adjusted mortality rate from opioid overdoses in 2017, sourced from the Centers for Disease Control's *Multiple Cause of Death* database (CDC-MCOD).[2] An opioid overdose death is identified when the under-

[1] Using the rate rules out a mechanical link from opioid or COVID mortality to the outcome. In addition, prime-age mortality is small relative to the prime-age population, so changes in deaths do not mechanically generate the LFP changes we study. The COVID-19 pandemic temporarily disrupted CPS data collection, particularly due to the suspension of in-person interviewing in spring 2020. While response rates declined, existing evidence suggests that the resulting bias in aggregate labor-market statistics was small and diminished as standard operations resumed (Ward and Edwards, 2021; McIllece, 2020). In our setting, nonresponse would bias the estimates only if changes in survey response were systematically correlated with pre-2017 opioid mortality across states. We are not aware of evidence supporting such a pattern.

[2] Available at `https://wonder.cdc.gov/mcd.html`. Causes of death are coded using ICD-10, the 10th revision of the International Classification of Diseases by the World Health Organization.

lying cause of death is a drug overdose, and opioids are listed among the multiple causes. For underlying causes, the following ICD-10 codes are included: X40-X44 (accidental drug poisonings), X60-X64 (intentional self-poisoning by drugs), X85 (assault by drug poisoning), and Y10-Y14 (drug poisoning of undetermined intent). Opioids as a multiple cause of death are identified using codes T40.0 (Opium), T40.1 (Heroin), T40.2 (Other opioids), T40.3 (Methadone), T40.4 (Other synthetic narcotics), and T40.6 (Other and unspecified narcotics).

Opioid-related deaths are used as a comprehensive measure of opioid exposure, encompassing both legal and illegal opioid consumption. Thus, overdose mortality captures the cumulative impact of opioid prevalence in each state. We use overdose mortality in 2017 as our baseline exposure measure to ensure that opioid exposure is predetermined with respect to the COVID-19 pandemic and measured immediately prior to its onset. The results with overdose mortality for the 2010-2017 period are also presented. Other measures, such as self-reported misuse rates in surveys, are not available at a monthly frequency and can be subject to under-reporting, while prescription volume measures exclude illicit opioids. Summary statistics for the age-adjusted opioid overdose mortality rates across US states are presented in Table 1. Notably, the age-adjusted opioid death rate rose by over 50 percent between 2019 and 2021. Figure A1 in Appendix A.1 shows that this large increase is primarily due to a large increase in synthetic-opioid-related deaths after the onset of COVID-19. Synthetic-opioid-related deaths are mainly due to fentanyl and have been rising in the U.S. since 2013.[3] There is also considerable variation in death rates across states, as shown in Figure A2 in Appendix A.1. In 2017, opioid death rates ranged from approximately 3 deaths per 100,000 people in Nebraska and Hawaii to 39 in Ohio and 50 in West Virginia. Furthermore, this disparity across states has widened since the COVID-19 pandemic, as evidenced by the increase in the standard deviation between 2019 and 2021, from 10 to 13.[4]

The relationship between pre-COVID opioid exposure and post-COVID labor supply recovery across US states is presented in Figure 3. As a measure of recovery, the gap between the observed and predicted labor-force participation for prime-age individuals during the

[3]The rise of overdose deaths involving fentanyl starting from 2013, referred to as the third wave of opioid overdose deaths, has been documented by Ciccarone (2019) and the CDC (2023). Friedman and Shover (2023) document the increasing importance of polysubstance overdose deaths involving fentanyl and stimulants.

[4]Further details on the demographic and geographic variation in fatal drug overdoses over time can be found in Monnat (2022).

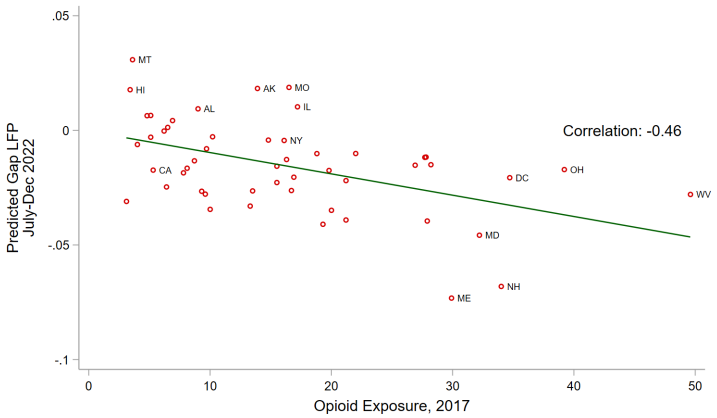
**Table 1:** Age-Adjusted Opioid Overdose Death Rate

| | Mean | *sd* | Min | Max |
|---|---|---|---|---|
| 2017 | 16.26 | 10.52 | 3.1 | 49.6 |
| 2019 | 16.66 | 10.16 | 3.5 | 43.0 |
| 2021 | 26.26 | 13.77 | 5.7 | 77.2 |
| 2022 | 26.82 | 13.03 | 5.6 | 72.5 |

**Notes**: *Summary statistics for the age-adjusted opioid-related overdose death rate for 2017, 2019, 2021, and 2022. Death rates are computed per 100,000 people. Source: CDC-MCOD.*

last six months of 2022 is used.[5] The predicted post-COVID values are based on state-level regressions of labor-force participation on a linear time trend.[6] More than two years after the onset of COVID-19, states with higher initial opioid death rates in 2017, such as West Virginia and Ohio, had labor-force participation rates that were well below the predicted values.

**Figure 3:** Relationship Between Opioid Exposure and Prime-Age LFP Gap




**Notes**: *Correlation between the predicted gap in prime-age LFP in July-December 2022 and the age-adjusted opioid-related overdose death rate for 2017. Source: CDC-MCOD and CPS.*

Three alternative measures of pre-COVID opioid exposure are also considered in the analysis. The first measure is the percentage of individuals with opioid use disorder in 2017–2018, derived from the National Survey on Drug Use and Health (NSDUH). This annual survey provides national and state-level data on tobacco, alcohol, illicit drug use (including non-medical prescription drug use), and mental health. Opioid use encompasses prescription

[5]This period was chosen to capture differences across states in LFP recovery, away from the immediate impact of the COVID-19 shock.

[6]Further details on the estimation of trends are provided in Appendix A.2.

pain relievers and heroin, assessed based on usage within the past 12 months. Prescription misuse includes any use not directed by a doctor, such as use without a prescription or in greater amounts or frequency than prescribed. All heroin users are classified as misusers. Misusers are further screened for opioid use disorder, identified by health issues, disabilities, or significant life disruptions caused by recurring use. The NSDUH is also used to identify individuals who suffer from any drug use disorder, including cocaine, hallucinogens, heroin, inhalants, methamphetamine, and psychotherapeutics (pain relievers, tranquilizers, sedatives, stimulants). This disorder measure captures sustained and clinically defined dependence and abuse, and therefore reflects more intensive usage than simple indicators of past-year drug use.[7]

The second measure of opioid exposure is the total shipment of medications used to treat opioid addiction in each state, primarily methadone and buprenorphine. Data are sourced from the Drug Enforcement Administration's (DEA) Automation of Reports and Consolidated Orders System (ARCOS).[8] The DEA provides the quantity (in grams) of these drugs distributed to each 3-digit zip code area, which is then aggregated at the state level. Morphine milligram equivalents (MME) are used to calculate a per capita MME amount for each state in 2017.[9]

The last measure uses cross-state variation in pre-2010 exposure to OxyContin misuse. This measure, developed by Alpert, Powell, and Pacula (2018), captures the rate of nonmedical OxyContin use in each state using NSDUH waves from 2004–2005 through 2008–2009, prior to the 2010 reformulation of OxyContin into an abuse-deterrent version. By focusing on nonmedical use of OxyContin before 2010, a drug that is considered one of the major drivers of the opioid crisis in the US, this measure isolates early patterns of opioid misuse that are not confounded by later shifts in the illicit drug market.[10]

[7]State-level data on drug and opioid use disorder are obtained from the Restricted-Use Data Analysis System (RDAS) of the Substance Abuse and Mental Health Services Administration, as the NSDUH public file lacks geographic indicators. For opioid disorder we use the variable UDPYOPI, which measures past-year opioid use dependence or abuse under DSM-IV criteria.

[8]Available at `https://www.deadiversion.usdoj.gov/arcos/arcos.html`. For details on opioid-treatment medications, see (Mutter and Duchovny, 2022). Naltrexone is excluded from ARCOS as it is not a scheduled drug.

[9]MME standardizes opioid potency relative to morphine, facilitating cross-opioid comparisons. Conversion is based on Cutler and Glaeser (2021).

[10]The measure is taken from the replication package for Alpert, Powell, and Pacula (2018), available at `https://www.openicpsr.org/openicpsr/project/114713/version/V1/view`. The measure calculates the share of the population above age 12 with non-medical OxyContin use. Population weights are used to aggregate shares across waves.

Summary statistics for these alternative exposure measures and their Spearman's correlations with age-adjusted death rates are presented in Table 2. All three alternative measures are positively and significantly correlated to the age-adjusted death rate.

**Table 2:** Alternative Opioid Exposure Measures

| | Mean | *sd* | Min | Max | Correlations |
|---|---|---|---|---|---|
| Death Rate, 2017 | 16.26 | 10.52 | 3.10 | 49.60 | 1.00 |
| Death Rate, 2010-2017 | 10.83 | 5.75 | 2.90 | 33.90 | 0.91 |
| Disorder, 2017-18 (%) | 0.84 | 0.30 | 0.32 | 1.60 | 0.40 |
| Treatment MME pc, 2017 | 925.43 | 601.09 | 157.06 | 2,809.07 | 0.77 |
| Pre-2010 OxyContin misuse (%) | 0.67 | 0.23 | 0.25 | 1.15 | 0.38 |

**Notes**: *Summary statistics for measures of state opioid exposure. The second row: age-adjusted opioid death rate for the combined period 2010-2017, obtained from CDC-MCOD. Third row: percentage of people with an opioid use disorder in 2017-2018, obtained from the NSDUH. Fourth row: quantity of drugs to treat opioid use disorder distributed to the state in 2017, measured as MME per capita, obtained from ARCOS. Last row: pre-2010 OxyContin exposure, taken from Alpert, Powell, and Pacula (2018). The last column shows Spearman's rank correlation between these three measures and the age-adjusted death rate in 2017.*

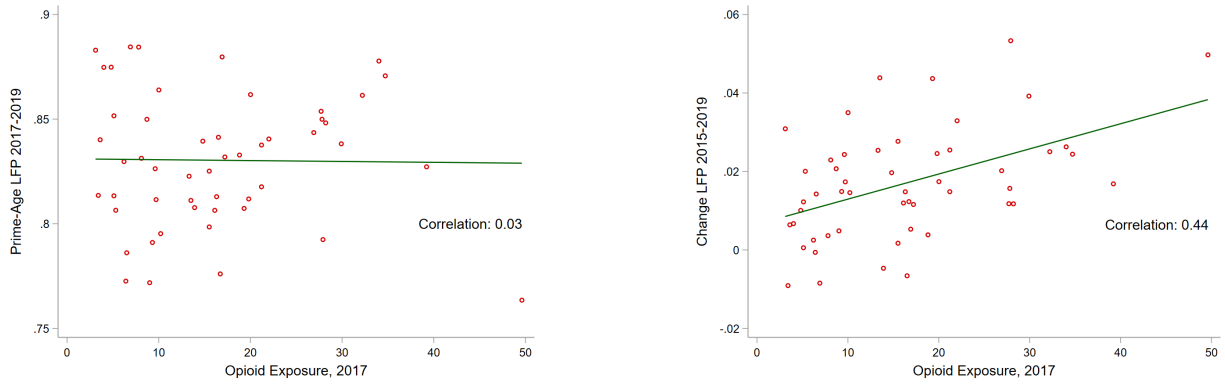
A potential concern with our exposure measure is that opioid exposure in 2017 may reflect pre-COVID labor market conditions, suggesting that both post-COVID recovery differences and opioid exposure could stem from shared pre-COVID factors. As indicated above, the interaction between opioids and labor-force participation can be complex, with effects going both ways (Abraham and Kearney, 2020). As shown in Figure 4a, there is no significant relationship between 2017 opioid exposure, measured by opioid-related deaths, and labor-force participation during the 2017–2019 period across states. Moreover, Figure 4b shows that the prime-age LFP of states with higher opioid exposure grew faster than that of lower exposure between 2015 and 2019.[11] There is no significant correlation between the measures and LFP between 2017 and 2019. Hence, the COVID-19 shock reversed an upward LFP trend in high-exposure states rather than reinforcing a downward one.

A further concern is that post-COVID labor-force participation differences across states with higher or lower initial opioid exposure might reflect differences in the sectoral composition of employment. Although we control for industry composition in the main analysis, we show here that high- and low-exposure states had broadly similar employment structures.[12]

[11] Figures A4 in Appendix A.3 analyze this relationship using the three alternative measures of opioid exposure.

[12] High exposure states are states with opioid exposure in 2017 above or equal to the median.

**Figure 4:** Relationship Between Opioid Exposure and Prime Age Labor Force Participation




**(a)** Prime Age LFP in 2017-2019

**(b)** Prime Age LFP Growth

**Notes**: *Left: Correlation between the average prime-age LFP between 2017 and 2019 and the age-adjusted opioid-related overdose death rate for 2017. Right: Correlation between prime-age LFP growth between 2015 and 2019 and age-adjusted opioid-related overdose death rate for 2017. Source: CDC-MCOD and CPS.*

As shown in Figure 5, employment shares across industries were very similar in high- and low-exposure states. Indeed, high-exposure states had a slightly lower share of employment in the two sectors most adversely affected by COVID-19: leisure and hospitality, and wholesale and retail trade. Moreover, Figure A5 in Appendix A.3 highlights that there were no diverging industry composition trends prior to 2020.

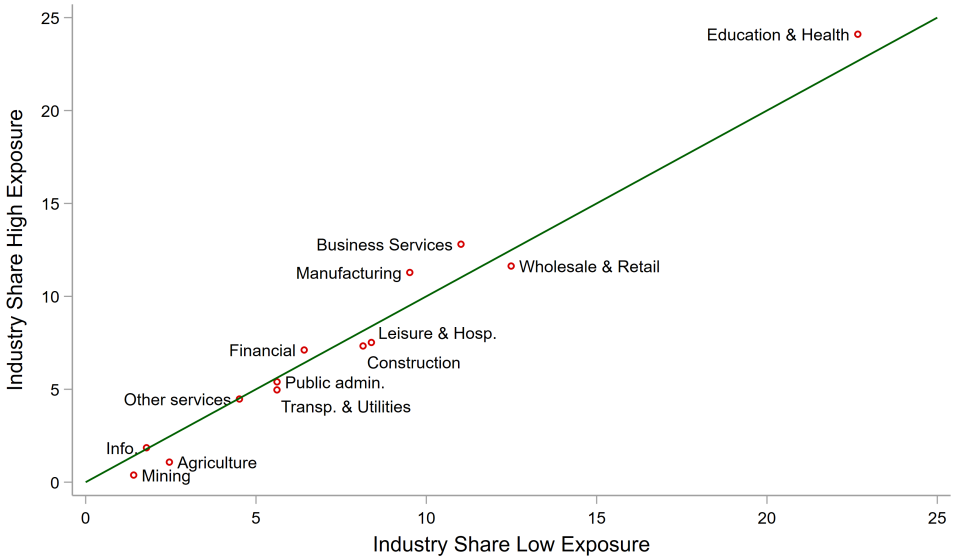
Finally, the empirical analysis also includes control variables that reflect differences in COVID-19 intensity and policy responses across states. State-level COVID-19 cases and deaths are taken from the CDC's COVID-19 Tracker.[13] Policy data are obtained from the Oxford Tracker Dataset (Hale et al., 2021); specifically, the Stringency Index (covering school and workplace closures and stay-at-home orders) and the Economic Support Index (summarizing income support policies during COVID-19) are used.[14] The average value of these two indexes and their standard deviations across US states are shown in Figure A6 in Appendix A.4.[15] Given the varied impact of COVID-19 across industries, time-varying state-level employment shares by industry are also included as a control in the empirical

[13] Available at https://covid.cdc.gov/covid-data-tracker/#datatracker-home.

[14] The Oxford Tracker Dataset categorizes various COVID-19 policies into indexes representing different policy strengths, normalized between 0 and 100.

[15] Ruhm (2024) finds that if all states had imposed COVID-19 restrictions similar to those used in the 10 most restrictive states, excess deaths would have been 10 percent to 21 percent lower.

**Figure 5:** Industry Shares in High and Low Opioid Exposure States




**Notes**: *Employment shares across major industries in 2017 for states with above- and below-median opioid exposure, where exposure is defined using the 2017 age-adjusted opioid death rate. Shares sum to 100 percent within each group. Source: CPS.*

analysis, using CPS data to construct these measures.

# 3 Empirical Strategy

The simultaneous impact of COVID-19 across all states makes a standard difference-in-differences estimator unsuitable for analyzing why some states experienced a slower recovery in labor-force participation. The analysis here uses an approach that allows the COVID-19 shock to have varying effects across states, depending on their pre-COVID opioid exposure. This approach relies on three assumptions: (1) the treatment affects all units (states) simultaneously, (2) the treatment effect depends on initial, predetermined conditions unrelated to the treatment, and (3) these initial conditions vary across units. The core idea is that states with higher pre-COVID opioid exposure experienced a slower labor force recovery, potentially due to greater availability of illicit drugs or a larger population with opioid use experience, which became more salient post-COVID. Importantly, the COVID-19 onset is assumed to be independent of initial opioid exposure levels.

This empirical strategy is used by Alpert, Powell, and Pacula (2018) to study the effects of OxyContin's reformulation, which made it harder to abuse. The reformulation, an event that affects everyone, is found to lead to greater increases in heroin deaths in states with higher pre-reformulation OxyContin exposure. A similar approach is also used by Beheshti (2023) to assess the impact of hydrocodone rescheduling by the Drug Enforcement Agency (DEA), which made it harder for doctors to prescribe. Areas with higher initial hydrocodone prescriptions show greater improvements in labor-force participation and employment.

The empirical approach employs a dynamic two-way fixed effects model. The main dependent variable is the labor-force participation rate. The key explanatory variable is the interaction between the COVID-19 outbreak (February 2020) and pre-COVID opioid exposure. Pre-COVID exposure is measured in two ways using 2017 age-adjusted opioid death rates. The first measure is a binary variable categorizing states into "high" or "low" exposure groups based on whether their death rates are above or below the median. This method enables straightforward, difference-in-difference-like comparisons but sacrifices granularity. For example, Virginia, with 15 opioid deaths per 100,000, falls just below the median in the low-exposure group, while Utah, with 16 deaths per 100,000, is just above the median in the high-exposure group alongside states like Ohio and West Virginia, which have much higher death rates (39 and 50 per 100,000, respectively). The second measure treats death rates as a continuous variable, preserving finer distinctions in exposure. To facilitate interpretation, this continuous variable is standardized to a mean of 0 and a standard deviation of 1.

The event study analysis is based on the following regression:

$$Y_{s,t} = \alpha_s + \alpha_t + \sum_{T \neq 2020m2} \beta_T \times \mathbb{1}\{t = T\} \times Op.\ Exp_s + \delta X_{s,t} + \varepsilon_{s,t}, \tag{1}$$

where $Y_{s,t}$ is the outcome of interest, LFP in state $s$ at time $t$ for the main analysis. The coefficients $\alpha_s$ and $\alpha_t$ are the state and time fixed effects. The variable $Op.\ Exp_s$ is the pre-COVID opioid exposure for state $s$, which can be either binary, taking the value of 1 if pre-COVID opioid exposure is higher than the median exposure and 0 otherwise, or continuous. The variable $X_{s,t}$ represents time-varying controls.

The coefficients of interest in equation (1) are the $\beta_T$, with $\beta_{Feb.2020}$ normalized to 0. These coefficients represent changes in the outcome variable $Y_{s,t}$ relative to the month before COVID-19, based on differing levels of prior opioid exposure. For instance, when $Op.\ Exp_s$ is the binary exposure measure, a value of $\beta_{March.2020} = -0.1$ indicates that, compared to low-

exposure states, labor-force participation in high-exposure states was 10 percentage points lower in March 2020 after controlling for their fixed effects. For the continuous exposure measure, $\beta_{March.2020}$ captures the difference in LFP associated with a one-unit increase in the standardized pre-COVID exposure measure, equivalent to a one-standard-deviation increase in the 2017 age-adjusted opioid death rate.

The control vector $X_{s,t}$ includes monthly COVID-19 case rates (set to zero prior to March 2020) to capture cross-state differences in pandemic severity. We also include the Oxford Stringency Index and the Economic Support Index, which summarize state-level containment and income-support policies that may directly affect labor supply and economic activity. These variables account for contemporaneous differences in public-health conditions and policy responses across states.[16] Lastly, a Bartik-style control variable is added to account for time-varying differences in employment structure across states. Following Di Maggio and Kermani (2016), this control is constructed as

$$B_{st} = \sum_{k} \phi_{s,k,\tau} \frac{\nu_{-s,k,t} - \nu_{-s,k,t-1}}{\nu_{-s,k,t-1}},$$

where $\nu_{-s,k,t}$ are the national employment shares in industry $k$ at time $t$ computed by excluding the state $-s$. Meanwhile, $\phi_{s,k,\tau}$ is the employment share in industry $k$, in state $s$, at fixed time $\tau = 2017$. Hence, while national employment has declined everywhere, states with larger employment in certain sectors, such as tourism, might be more intensely affected.

The identifying assumption underlying equation (1) is that, absent the COVID-19 pandemic, states with different levels of pre-pandemic opioid exposure would have experienced parallel trends in prime-age labor-force participation. This parallel trends assumption allows post-COVID differences to be interpreted as difference-in-difference outcomes. Consequently, the estimated values of $\beta_T$ should be close to 0 during the pre-COVID period. However, this assumption may be too strong and could be violated in the data. Following Beheshti (2023) and Dobkin et al. (2018), a more flexible specification, which allows for preexisting differential linear time trends across states, is also considered, given by

$$Y_{s,t} = \tilde{\alpha}_s + \tilde{\alpha}_t + \sum_{T>2020m2} \theta_T \mathbb{1}\{t = T\} \times Op.\ Exp_s + \sum_{S} \phi_S \mathbb{1}\{s = S\} \times t + \tilde{\delta} X_{s,t} + \varepsilon_{s,t}, \quad (2)$$

[16] Conditioning on post-onset variables may raise concerns if pandemic severity or policy responses lie on the causal path between pre-pandemic opioid exposure and labor-force participation, or if they induce collider bias. We therefore report the main results both with and without COVID-related controls.

where the term $\sum_S \phi_S \mathbb{1}\{s = S\} \times t$ is the term that allows for state-specific linear time trends. The identifying assumption no longer requires parallel trends across states; instead, it relies on similar deviations from potentially different linear trends in each state. In this specification, the coefficients $\theta_T$ are estimated only for the post-COVID period. However, the $\beta_T$ estimates from equation (1) can be used to derive $\theta_T$ for the pre-COVID period (Dobkin et al., 2018). If these constructed pre-COVID estimates are close to 0, the linear trend assumption is a good fit to the data. Following Beheshti (2023), equation (1) is referred to as a non-parametric event study specification, while equation (2) is called a parametric event study specification.

# 4 Results

## 4.1 Opioid Exposure and Deaths

The empirical strategy builds on the hypothesis that the COVID-19 shock generated differential increases in illicit drug use across states, depending on their pre-COVID opioid exposure, and that these differences may have contributed to subsequent divergence in labor supply behavior. More specifically, we expect states with higher pre-COVID opioid exposure to experience larger increases in opioid-related mortality following the COVID-19 shock, or more broadly, higher overdose deaths from any illicit drugs.[17] To document this "first-stage" relationship, we estimate equation (1) using death rates from any illicit drugs as the dependent variable, $Y_{s,t}$, and the continuous age-adjusted opioid death rates as our measure of pre-COVID opioid exposure, $Op.\ Exp_s$.[18] In this "first-stage" estimation, our objective is to isolate the effect of opioid exposure on illicit drug use, proxied by drug overdose deaths. For this reason, we include an additional control: the share of deaths involving fentanyl. Fentanyl is more lethal than other opioids. Moreover, its share of overdose deaths increased more rapidly between 2018 and 2023 in low-exposure states than in high-exposure states (see Figure B3 in Appendix B.1). Consequently, this control helps account for differential

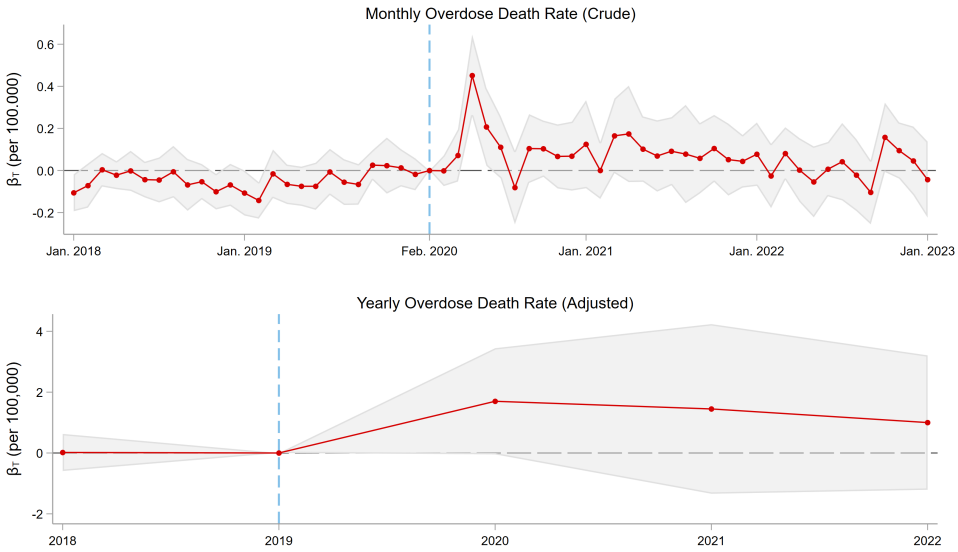
[17] For drug overdose deaths, the following ICD-10 codes are included: X40-X44 (accidental drug poisonings), X60-X64 (intentional self-poisoning by drugs), X85 (assault by drug poisoning), and Y10-Y14 (drug poisoning of undetermined intent).

[18] For results using binary exposure, where states are categorized above or below the median 2017 age-adjusted death rate, see Figure B1 in Appendix B.1. For results using opioid overdose deaths, rather than all overdoses, see Figure B2.

lethality that is not directly related to differences in drug use.[19]

The findings, shown in Figure 6, reveal that a one-standard-deviation increase in initial opioid exposure is associated with a 0.5-point rise in the monthly overdose death rate by May 2020 and a 1.7-point increase in the yearly death rate for 2020. These effects are substantial and persistent, representing 24 percent of the monthly and 8 percent of the yearly pre-pandemic death rates. Only the non-parametric event study results are presented, as the pre-COVID coefficients $\beta_T$ do not indicate the presence of pre-trends. Furthermore, Figure B6 in Appendix B.1 shows the first-stage results using the opioid death rate averaged for 2010 to 2017, and the three alternative opioid exposure measures described in Section 2. In all three cases, there is a significant and persistent increase in death rates associated with the onset of COVID-19.

**Figure 6:** Non-Parametric Event Study with Continuous Exposure - Drug Overdose Deaths



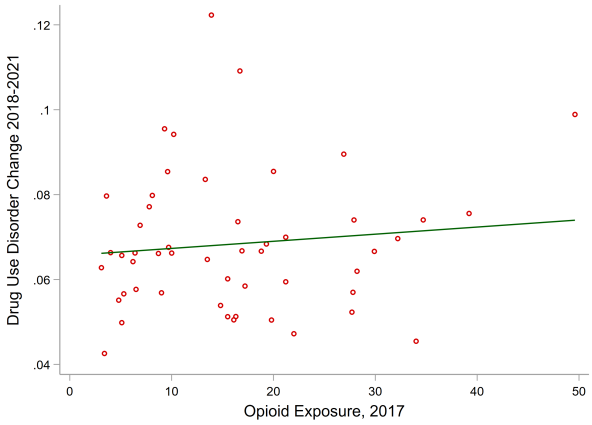

**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in drug overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom uses the yearly age-adjusted rate. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

An alternative way to highlight the hypothesis that the COVID-19 shock generated differential increases in illicit drug use across states would be to redo the analysis in Figure 6 using indicators for illicit drugs use as the dependent variable. Such an analysis is not

[19]For results without the fentanyl share control, see Figures B4 and B5 in Appendix B.1

feasible since direct measures of illicit drug consumption are not available at the state level. The closest proxy comes from the National Survey on Drug Use and Health (NSDUH), which, as described in Section 2, reports the share of individuals with an illicit drug use disorder.[20] However, NSDUH data are only available annually, and publicly released state-level estimates are reported for pooled two-year periods (e.g., 2018–2019). Furthermore, the 2020 NSDUH survey, conducted during the COVID-19 pandemic, was administered online and is not directly comparable with the 2019 survey. Still, we can compare the incidence of drug use disorder before COVID-19 (2018–2019) and after the onset of the pandemic (2021–2022) and examine whether the increase is larger in states with higher pre-COVID opioid exposure. The results are shown in Figure 7. Consistent with our hypothesis, states with higher initial opioid exposure exhibit a larger increase in the share of individuals with a drug use disorder.

**Figure 7:** Relationship Between Opioid Exposure and Changes in Drug Use Disorder




**Notes**: *Correlation between the age-adjusted opioid-related overdose death rate for 2017 and the change in drug use disorder. The change is computed as the difference in the drug use disorder rate using the Restricted NSDUH data for the period 2018-2019 and 2021-2022. Source: CDC-MCOD and NSDUH.*

Another assumption underlying the empirical analysis is that COVID-19 cases—the treatment—are uncorrelated with pre-COVID opioid exposure, which is predetermined. Fig-

[20]Measures of illicit drug use disorder constructed from the NSDUH are not fully consistent across waves due to changes in diagnostic definitions. For 2018–2019, we use state-level estimates of past-year illicit drug use disorder based on DSM-IV criteria, which combine abuse and dependence. For 2021–2022, we use state-level estimates of past-year drug use disorder based on DSM-5 criteria. Both measures capture clinically meaningful substance use disorders and are broadly comparable indicators of severe illicit drug misuse. Data are obtained from the Interactive NSDUH State Estimates portal: https://datatools.samhsa.gov/saes/state

ure B7 in Appendix B.2 reports estimates from the non-parametric event study in equation (1), where monthly COVID-19 cases per 100,000 people serve as the dependent variable and the continuous measure of initial opioid exposure is the independent variable. None of the estimated event-time coefficients, $\beta_T$, are statistically significant.

## 4.2 Opioid Exposure and Slow Labor Supply Recovery

The results for labor-force participation are presented next. The $\beta_T$ estimates from the non-parametric event study are shown in Figure 8. To ease interpretation, coefficients are already multiplied by 100 and presented as percentage points. The upper panel presents results from equation (1) using a binary measure of exposure, categorizing states as above or below the median 2017 age-adjusted opioid death rate.[21] The lower panel shows results with the continuous measure of opioid exposure.

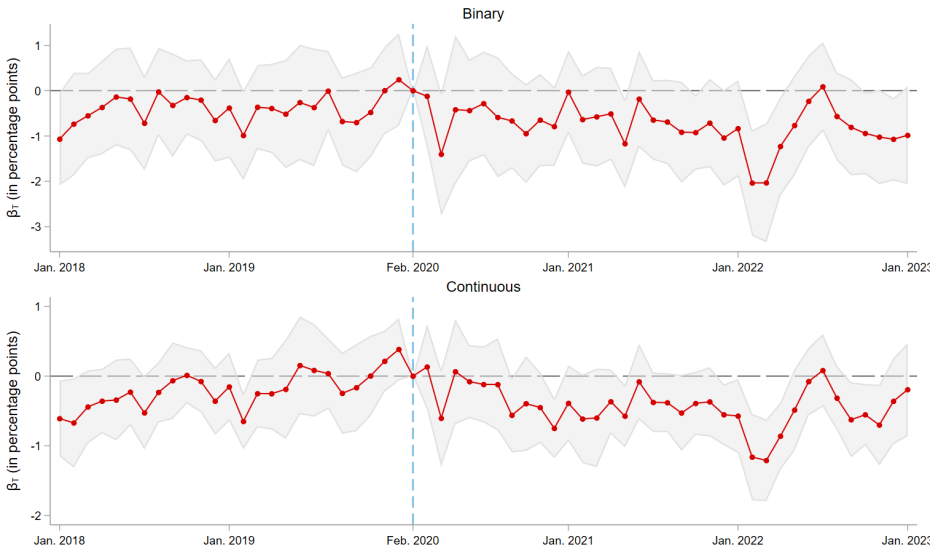
In both cases, the estimated effects are primarily negative, suggesting lower labor-force participation post-COVID in states with higher pre-COVID opioid exposure. However, these estimates are generally not statistically significant, except for February and March 2022. Additionally, the consistently negative $\beta_T$ coefficients in the pre-COVID period indicate differential pre-trends in prime-age labor-force participation.

The estimates from the parametric event study, based on equation (2), which account for state-specific linear trends, are presented in Figure 9. The upper panel displays results using the binary measure of opioid exposure, while the lower panel shows results using the continuous measure. To ease interpretation, coefficients are again multiplied by 100 and presented as percentage points. For both specifications, the pre-COVID estimated coefficients are approximately 0, supporting the identifying assumption that states experienced similar deviations from linear trends. For this reason, this is our preferred specification.

Post-COVID, the estimated $\theta_T$ coefficients indicate that labor-force participation rates in states with higher pre-COVID opioid exposure consistently fell below their linear trend compared to less-exposed states. The gap in labor-force participation between states with higher and lower opioid exposure emerged immediately after COVID-19 and continued to

[21] States classified as having median or above pre-COVID opioid exposure are: Connecticut, Delaware, District of Columbia, Florida, Illinois, Indiana, Kentucky, Maine, Maryland, Massachusetts, Michigan, Missouri, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Utah, Vermont, West Virginia, and Wisconsin.

**Figure 8:** Non-Parametric Event Study, Labor-Force Participation




**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in prime-age LFP relative to February 2020 between states with different levels of prior opioid exposure. For the top panel, the initial opioid exposure measure has been dichotomized: states with an age-adjusted opioid death rate in 2017 above the median are given a value of 1, and the others a value of 0. For the bottom panel, the initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

widen, reaching nearly 1 percentage point by January 2021 and approximately 2 percentage points by January 2022. The upper panel estimates indicate that from March 2020 to January 2023, LFP in states with above-median opioid exposure averaged 1.1 percentage points below its trend.

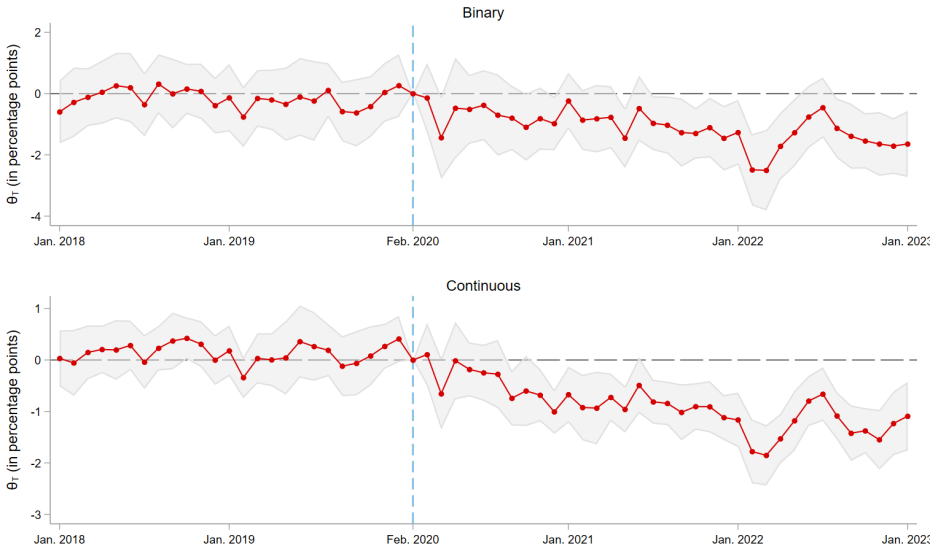
Similarly, the continuous measure in the lower panel shows that a one-standard-deviation increase in 2017 age-adjusted opioid death rates corresponds to an additional 0.9 percentage point drop in LFP during the post-COVID period. This implies that moving from the 25th percentile of initial opioid exposure (e.g., Minnesota) to the 75th percentile (e.g., Pennsylvania or Michigan) leads to an average LFP decline of 1.1 percentage points—a substantial effect, given that the standard deviation in prime-age LFP across states during the post-COVID period was 3.4 percentage points.[22]

How do these magnitudes compare with earlier studies on the labor-supply effects of opi-

[22]Re-estimating equation 2 without COVID-19 case rates and policy controls yields very similar dynamic patterns. See Figure B8 in Appendix B.2

oids? Cho et al. (2021) find that a one-standard-deviation increase in pre-2010 oxycodone exposure is associated with roughly a 0.3 percentage-point decline in both the prime-age employment-to-population ratio and the labor force participation rate. Park and Powell (2021) estimate that a 10 percent increase in exposure to the OxyContin reformulation—about one quarter of a standard deviation in their measure—reduces the employment-to-population ratio by approximately 0.14 percentage points. While our estimates are not directly comparable, as we use opioid-related mortality rather than prescription-based exposure, they are somewhat larger in magnitude but remain broadly consistent with these earlier findings.[23]

**Figure 9:** Parametric Event Study, Labor-Force Participation




**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. In the top panel, the initial opioid exposure measure has been dichotomized: states with an age-adjusted opioid death rate in 2017 above the median are given a value of 1, and the others a value of 0. For the bottom panel, the initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

[23] Using pre-2010 OxyContin exposure in our robustness analysis yields an estimated decline in labor force participation of about 0.4 percentage points per one-standard-deviation increase, closely aligned with the magnitudes reported in prior work.

## 4.3 The Effect of Overall Health

A potential concern with the analysis is that states with higher initial opioid exposure may also have weaker healthcare systems or poorer overall health. If this were the case, it could imply that the slower LFP recovery in more exposed states is not associated with higher exposure to the opioid epidemic but simply with worse overall health.

To address this concern, we conducted a placebo analysis by replacing initial pre-COVID opioid exposure in the main analysis with age-adjusted death rates from non-opioid-related causes. If these placebo results are significant, it would suggest that the larger drop in labor-force participation in states with higher opioid exposure may be attributed to broader health problems in the population or state-specific characteristics of the healthcare sector that influence both pre-COVID opioid mortality and post-COVID labor supply decisions.

For the placebo analysis, we use data from the CDC Underlying Cause of Death dataset on the 15 leading causes of death in the United States.[24] Table B1 in Appendix B.3 lists these causes along with their age-adjusted death rates for 2017. Deaths due to accidents and intentional self-harm (causes 3 and 8) may overlap with opioid-related deaths, and hence are excluded.[25] Figure 10 presents the estimated coefficients from the parametric event study where the initial exposure measure is based on the total death rate from the remaining 13 causes.[26] The coefficients for the post-COVID period are not statistically significant.[27]

## 4.4 Heterogeneity

The results for different demographic and socioeconomic groups are presented using the parametric event study with a continuous exposure measure, as specified in equation (2). Effects across gender and age groups are shown in Figure 11. The estimates are similar for men and women but show stronger effects among older individuals (ages 45–54) compared to younger ones (ages 25–44). Specifically, a one-standard-deviation increase in pre-COVID

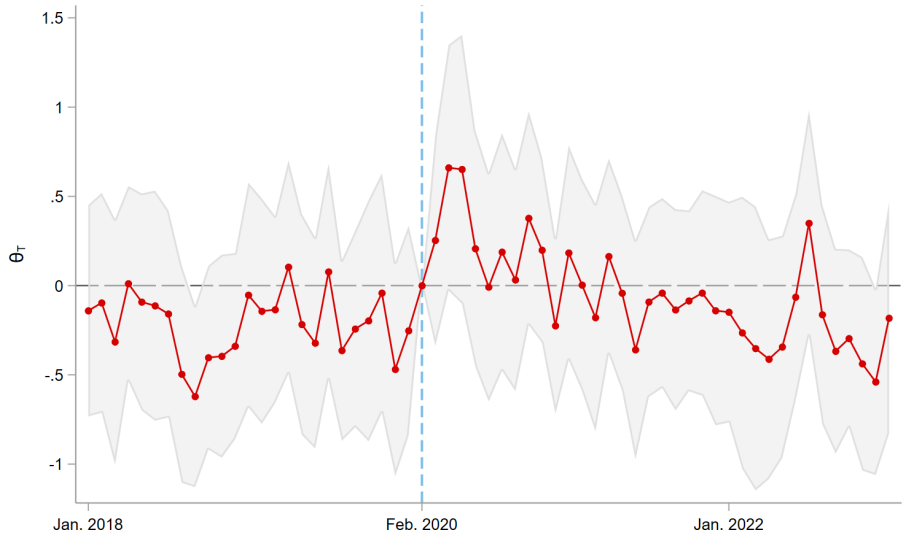
[24] https://wonder.cdc.gov/ucd-icd10.html.

[25] Accidents include underlying causes with ICD-10 codes X40-X44, and intentional self-harm includes ICD-10 codes X60-X64.

[26] Using the non-parametric event study specification leads to similar results.

[27] Results using the top 10 leading causes of death excluding accidents and intentional self-harm deaths, and each of the 13 underlying causes as the initial exposure measure, are shown in Figures B9 and B10 in Appendix B.3.

**Figure 10:** Parametric Event Study with Placebo Exposure




**Notes**: *Placebo parametric event study coefficients $\theta_T$ and the 95% confidence interval when using prime-age LFP as dependent. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior placebo exposure. The measure of prior placebo exposure is the total age-adjusted death rate in 2017 due to the top 15 underlying causes of death. The death rate has been normalized to have a standard deviation equal to 1.*

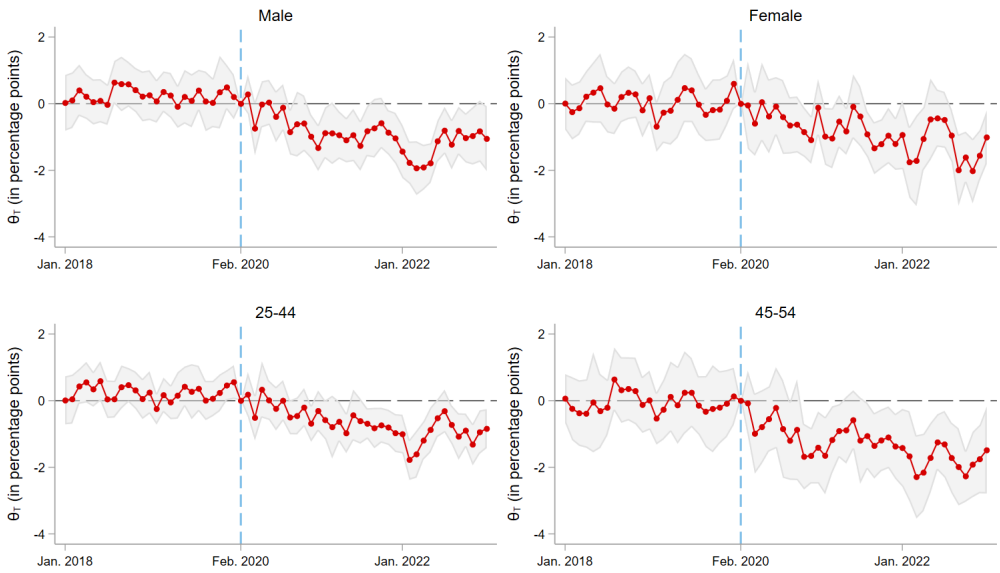
opioid exposure is associated with a 1.3 percentage point decline in labor-force participation for the 45–54 age group, compared to 0.7 percentage point for the 25–44 age group. Results for age groups 18–24 and 55–64, which are not statistically significant, are shown in Figure B11 in Appendix B.4.[28]

Effects by educational attainment are illustrated in Figure 12 and show notable differences. For non-college-educated individuals, a one-standard-deviation increase in pre-COVID opioid exposure corresponds to a 1.1 percentage point decline in LFP relative to the linear trend. In contrast, the estimates for college graduates are not significantly different from zero. This result is in line with evidence that labor market outcomes for non-college-educated individuals are more affected by opioids (Aliprantis, Fee, and Schweitzer, 2023).

[28]In principle, the analysis in the previous section can be replicated at the county level. The main source of data, the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS), however, only provides an aggregate measure of the labor force. Hence, it is not possible to focus the analysis on prime-age workers. The results, while consistent with state-level analysis, are less precise, which is not surprising given our findings for different age groups.

**Figure 11:** Parametric Event Study with Continuous Exposure

Labor-Force Participation by Gender and Age




**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. The top panel divides the sample by gender, displaying the prime-age labor force participation (LFP) among males and females. The bottom panels show LFP by age groups: 25-44 in the left panel and 45-54 in the right. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

# 5 Robustness

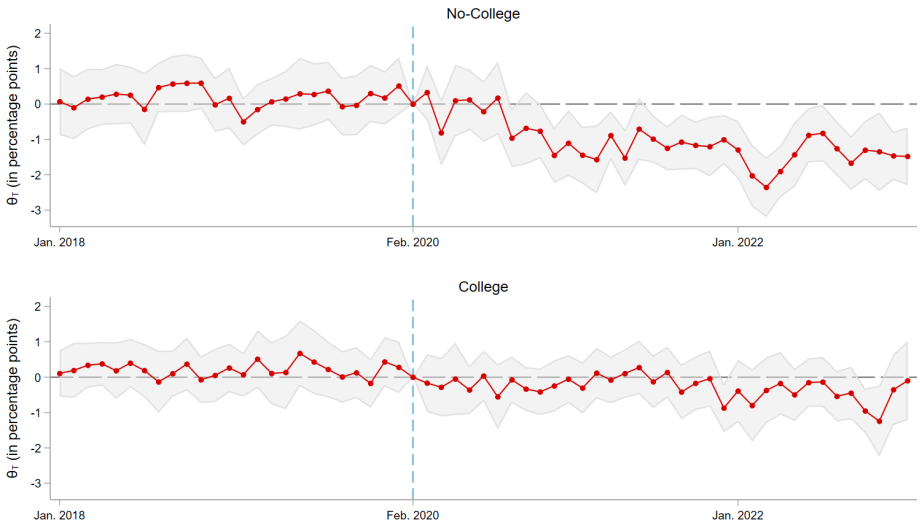
Several sensitivity and robustness checks are conducted. First, alternative measures of pre-COVID opioid exposure are used to validate the results obtained with our preferred specification, i.e., the parametric event study given in equation (2). Second, alternative empirical approaches are employed to verify that the results are not driven by modeling choices.

## 5.1 Alternative Exposure Measures

The analysis uses age-adjusted opioid death rates from 2017 as a measure of opioid exposure. In this section, we show the results using four alternative measures of opioid

**Figure 12:** Parametric Event Study with Continuous Exposure

Labor-Force Participation by Education



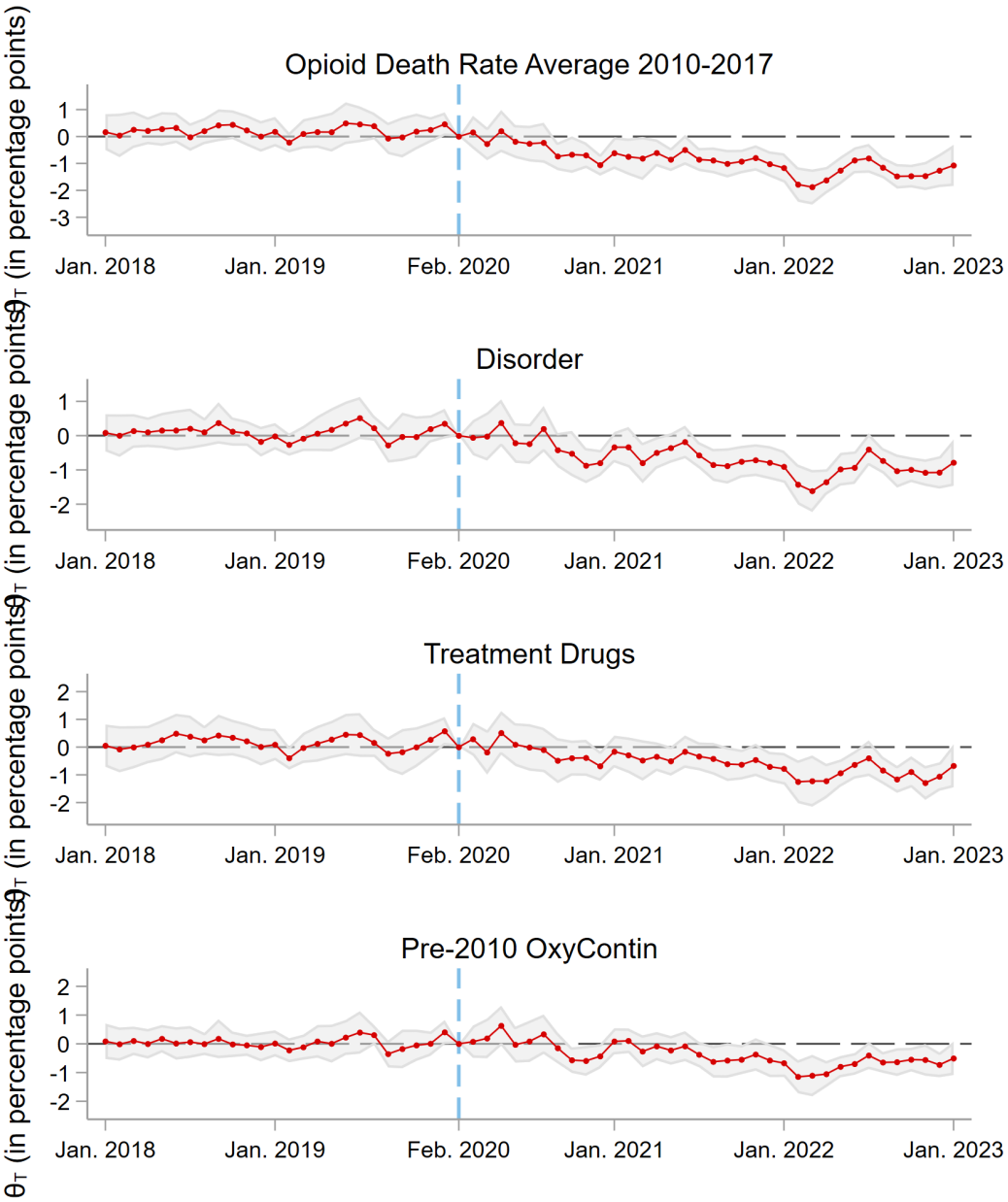

**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. The upper panel focuses on prime-age LFP for non-college-educated individuals, while the bottom panel focuses on college-educated individuals. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

exposure. First, the single-year measure is replaced with the average age-adjusted death rates from 2010 to 2017. The second alternative measure is the percentage of individuals aged 12 and above with an opioid use disorder, as reported in the NSDUH. The third one is the per-capita shipment of medications for opioid addiction treatment, sourced from ARCOS. The final one is the pre-2010 OxyContin exposure, obtained from Alpert, Powell, and Pacula (2018).

Figure 13 reports estimates from the parametric event study using these continuous exposure measures. Although the magnitudes are somewhat smaller for the last three measures, the patterns are very consistent with those in Figure 9. A one-standard-deviation increase in the share of individuals with an opioid use disorder is associated with roughly a 0.7 percentage point larger decline in labor-force participation relative to trend. For the per-capita supply of opioid treatment medications, the corresponding effect is about 0.5 percentage points, and for pre-2010 OxyContin exposure, it is approximately 0.4 percentage points.[29]

[29]Given the slightly right-skewed distribution of 2017 death rates across states, estimates are also provided using the logarithm of death rates as the exposure measure. Figure B12 in Appendix B.5 presents results using

**Figure 13:** Parametric Event Study - Alternative Measures of Opioid Exposure




**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from a state trend in prime-age LFP between states with different levels of prior opioid exposure. Top panel uses opioid death for 2010-2017. Second panel uses percentage of people with opioid use disorder in 2017-2018. Third panel uses amount of MME per capita of drugs to treat opioid use disorder distributed to the state in 2017. The bottom panel uses pre-2010 OxyContin exposure. In all three cases the measure has been normalized to have a standard deviation equal to 1.*

these variants of pre-COVID opioid exposure in the parametric event-study specification with continuous exposure. The post-COVID gap in labor-force participation closely aligns with the estimates in Figure 9.

## 5.2 Alternative Empirical Approaches

The parametric event study approach assumes that states with different initial opioid exposure experienced similar deviations from their state-specific linear trends. We assess this assumption in four ways. First, the parametric event study is re-estimated, allowing for quadratic rather than linear state-specific trends. Second, the main analysis is repeated using a longer pre-period. Instead of estimating trends over 2018–2023 and using the 2017 opioid death rate as the exposure measure, the specification is re-estimated using data beginning in 2015 and using the 2015 opioid death rate as the measure of pre-COVID exposure. Figures B13 and B14 in Appendix B.6 show that, under these alternative specifications, the results remain consistent with the main findings: states with higher opioid exposure experience a persistently larger decline in LFP after COVID-19.

Next, we estimate the effects using a *synthetic control* approach, following Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). For this method, we treat as treated those states with pre-COVID opioid exposure above or equal to the median value, and as controls the remaining states. Weights are assigned to the untreated states to construct a synthetic control group that closely matches the pre-COVID labor-force participation of the treated group. These weights are time-invariant and chosen to minimize the difference in pre-COVID LFP between the synthetic control and the treated group. By construction, the synthetic control reproduces the pre-COVID trend in LFP of the treated states. The treatment effect is then estimated as the post-COVID difference in average LFP between the treated group and its synthetic control.

Figure 14 illustrates the average prime-age labor-force participation in the treated states and in the synthetic control group. In the post-COVID period, LFP diverges between the two groups, with treated states—those with higher opioid exposure—showing systematically lower LFP. The average treatment effect over the post-COVID period is -0.7 percentage points and statistically significant at the 5 percent level.[30]

Finally, we implement an alternative parametric event study proposed by Alpert, Powell, and Pacula (2018). This specification allows for a linear pre-existing trend in LFP, a shift in its level at the onset of COVID-19, and a post-COVID trend break. Following the notation

[30]Significance is based on bootstrapped standard errors for the average treatment effect, using 5000 repetitions.

**Figure 14:** Synthetic Control and Treatment Group – LFPR


**Notes**: *Labor-force participation in treatment and synthetic control groups. The treatment group includes states with initial opioid exposure above the median. The synthetic control is a weighted average of states with below-median exposure, with weights selected to reproduce the pre-COVID LFP path of the treated group.*

specified above, we have:

$$LFP_{s,t} = \alpha_s + \alpha_t + \delta_1[t \times Op.\,Exp_s] + \delta_2[Post_t \times Op.\,Exp_s] + \delta_3[Post_t \times (t - 2020\text{m2}) \times Op.\,Exp_s] + \delta X_{s,t} + \varepsilon_{s,t}, \quad (3)$$

where $Post_t = 1$ for $t \geq$ March 2020. The coefficients $\delta_1, \delta_2$, and $\delta_3$ represent the pre-trend, the shift, and the post-trend, respectively. The variable $X_{s,t}$ contains the same state and time level controls as in the main analysis. Table 3 reports the results using our main opioid exposure measure in column 1 (the 2017 age-adjusted opioid death rate) and the three alternative exposure measures in columns 2–4. When using the main opioid exposure measure, the pre-trend coefficient is slightly positive, consistent with the non-parametric event study. Both the shift and the post-COVID trend-break coefficients are negative, indicating that high-exposure states experienced a persistent deterioration in LFP relative to low-exposure states. The cumulative effects after 6, 12, and 24 months closely match the magnitudes obtained in the baseline specification. Furthermore, for all three alternative opioid exposure measures, we obtain a significant and post-trend coefficient with similar magnitudes.

**Table 3:** Alternative Parametric Event Study, Labor Force Participation

| | (1) Death Rate | (2) Disorder | (3) Treatment | (4) OxyContin |
|---|---|---|---|---|
| Pre-trend | 0.0248*** | 0.0198*** | 0.0162 | 0.0100 |
| | (0.004) | (0.006) | (0.130) | (0.194) |
| Shift | -0.491** | -0.196 | -0.145 | 0.0688 |
| | (0.027) | (0.301) | (0.565) | (0.735) |
| Post-trend | -0.0307** | -0.0300*** | -0.0334** | -0.0299*** |
| | (0.013) | (0.003) | (0.032) | (0.009) |
| Effect 6 Months | -0.675*** | -0.376** | -0.345 | -0.110 |
| | (0.001) | (0.040) | (0.176) | (0.575) |
| Effect 12 Months | -0.860*** | -0.556*** | -0.545* | -0.290 |
| | (0.000) | (0.005) | (0.061) | (0.175) |
| Effect 24 Months | -1.228*** | -0.916*** | -0.946** | -0.648** |
| | (0.000) | (0.001) | (0.024) | (0.029) |
| Observations | 3111 | 3111 | 3111 | 3111 |
| Adj. R2 | 0.771 | 0.770 | 0.770 | 0.771 |

**Notes**: *The table shows the estimates obtained from the regression equation (3) using different initial opioid exposure measures. Column 1 uses the age-adjusted opioid death rate. Column 2 the percentage of people with opioid use disorder in 2017-2018. Column 3 the amount of MME per capita of drugs to treat opioid use disorder distributed to the state in 2017. Column 4 pre-2010 OxyContin exposure. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1. In parenthesis p-values, * p<0.10, ** p<0.05, *** p<0.01. State clustered S.E. used. States have been weighted by their population size.*

# 6 Mechanisms

The COVID-19 pandemic coincided with an unprecedented rise in deaths from opioids and other illicit drugs, driven by multiple factors. Pandemic-induced isolation worsened mental health (Panchal et al., 2020; Adams-Prassl et al., 2022*a*), while disruptions in medical services reduced access to medications for opioid use disorder (Currie et al., 2021*a*; Russell et al., 2021). This period also saw a shift in opioid consumption toward fentanyl, a more potent opioid (Currie et al., 2021*b*). Additionally, increased free time, lower drug prices, and government income support may have exacerbated the use of illicit drugs (Mulligan, 2022).

Although drug overdose deaths rose nationwide during COVID-19, the impact was most severe in states with higher pre-COVID opioid exposure, as shown in Figure 6. This differen-

tial effect may reflect established illegal opioid distribution networks, more lenient prescribing practices, or a larger population experienced with opioids in highly affected states.

The greater increase in illicit drug use, proxied by opioid overdose deaths in states with higher initial opioid exposure, may have contributed to slower labor-force participation recovery in the post-COVID period, as drug users typically have lower participation rates and poorer health. Table 4 highlights these disparities. In 2019, before COVID-19, 17 percent of individuals aged 24–49 who did not misuse any drugs (excluding marijuana) were out of the labor force, compared to 26 percent of those with a drug use disorder.[31]

Individuals with a drug use disorder are more likely to report disability as the reason they are not participating in the labor market. This mechanism is consistent with evidence from Park and Powell (2021), who show that increased exposure to the OxyContin reformulation led to higher rates of disability-related nonemployment and worsening health among affected populations. As Table 4 reports, in 2019, 17 percent of nonparticipants who did not use any drugs reported disability as the reason compared with 25 percent of those with a drug use disorder. Drug misuse also impacts job attendance; those with drug use disorders missed an average of 4.1 workdays per month, compared to 1.1 days among nonusers. Self-reported health outcomes were similarly much poorer: 26 percent of individuals with drug use disorders reported fair or poor health, compared to 10 percent among nonusers. Finally, comparing 2019 data with the 2021–2022 period, Figure 15 shows that the LFP and disability gaps between nonusers and those with drug use disorders widened after COVID-19.[32]

If higher rates of drug use were indeed having a greater impact on the labor market in states with higher pre-COVID opioid exposure, we should see a larger increase in the share of individuals out of the labor force due to disability in those states. This follows from the correlation between drug use disorder and reporting disability as the reason for nonparticipation. To explore the relationship between pre-COVID opioid exposure and nonparticipation due to disability, information is obtained from the CPS, where individuals not participating in the labor force select a reason. This can be being "retired," being "unable to work" (for medical conditions that prevent work for six months or more), or "other." Those who selected the "other" category may specify a reason, including "disability," "illness," being "in school," or "taking care of house or family."

[31] This age group is used because NSDUH reports age in brackets.

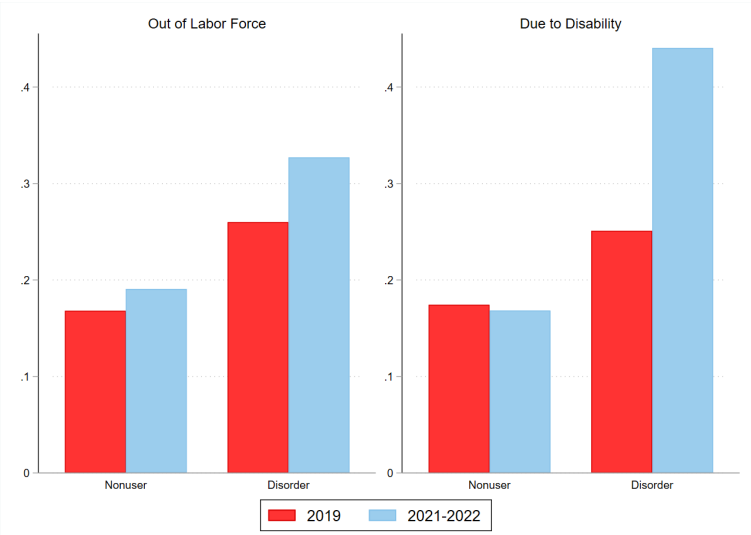
[32] Tables C1 and C2 in Appendix C.1 provide detailed employment and health outcomes by drug use in 2019 and 2021.

**Table 4:** Employment and Health by Drug Use, Ages 24-49

| ***Panel A: Employment*** | Out of LF | Disability | Total Skipped Days |
|---|---|---|---|
| Nonuser | 17% | 17% | 1.1 |
| Disorder | 26% | 25% | 4.1 |
| ***Panel B: Self-Reported Health*** | Very Good | Good | Fair/Poor |
| Nonuser | 61% | 29% | 10% |
| Disorder | 40% | 35% | 26% |

**Notes**: *Employment-related variables in Panel A, and self-reported health status in Panel B, by type of drug user in 2019. In Panel A, the first column displays the share of individuals who are not in the labor force. The second column shows the share of individuals out of the labor force who report disability as the reason for not participating. The third column displays the average number of workdays missed in the past 30 days due to illness or other reasons. Self-reported health status is a categorical variable with 4 options: very good, good, fair, and poor. Fair and poor categories are aggregated. Nonusers did not use any drug (excluding marijuana) in the past 12 months. Disorder indicates a drug use disorder (excluding marijuana) in the last 12 months. Source: NSDUH.*

**Figure 15:** Changes in Labor-Force Participation by Drug Use, Ages 24-49



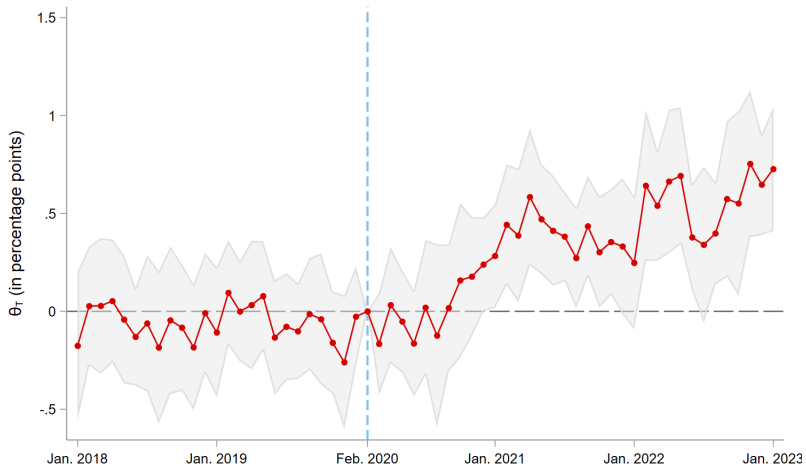

**Notes**: *Out of labor force variables by type of drug use in 2019 and 2021–2022. Left panel: share of individuals who are out of the labor force. Right panel: share of individuals out of the labor force who report disability as the reason they are not participating. Nonusers did not use any drug (excluding marijuana) in the past 12 months. Disorder indicates a drug use disorder (excluding marijuana) in the last 12 months. Source: NSDUH.*

Using this information, the prime-age out-of-labor-force due to disability rate is defined as the share of prime-age individuals who do not participate in the labor force and have as

their reason "unable to work" or "disability." The out-of-labor-force due to disability rate has been on a declining trend since 2015, driven mainly by those without a college degree. The decline, however, stopped with the pandemic, stabilizing at around 0.05 (Figure C1 in Appendix C.2).

To analyze the post-COVID trajectory across states of the share of prime-age individuals who are not working due to disability, a parametric event study using a continuous measure of initial opioid exposure, as specified in equation (2), is estimated with the out-of-labor-force due to disability rate as the dependent variable. The estimated coefficients are presented in Figure 16. The results indicate that pre-COVID, states with varying opioid exposure levels exhibited similar deviations from their trends. Post-COVID, however, states with higher preexisting opioid exposure deviated more positively from these trends. Specifically, a one-standard-deviation increase in opioid exposure is associated with a 0.4 percentage point rise in the post-COVID out-of-labor-force due to disability rate. This is a substantial effect, given that the standard deviation of the out-of-labor-force due to disability rate during the post-COVID period was 2.0 percentage points.[33]

**Figure 16:** Parametric Event Study Estimates - Out of LF due to Disability




**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state trends in prime-age out-of-labor-force due to disability rates between states with different levels of prior opioid exposure. The out-of-labor-force due to disability rate is defined as the proportion of individuals out of the labor force due to disability to the total population. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

[33] As with labor-force participation, these results are primarily driven by the non-college-educated population, as shown in Figure C2 in Appendix C.3.

# 7 Conclusions

The COVID-19 pandemic caused substantial and persistent declines in labor force participation across the United States, particularly among prime-age individuals without a college degree. This paper shows that these disruptions were significantly amplified in states with greater pre-pandemic exposure to the opioid epidemic. States with higher opioid-related mortality prior to COVID-19 experienced markedly slower labor force recoveries following the pandemic shock. Our event-study estimates indicate that a one-standard-deviation increase in pre-pandemic opioid mortality is associated with an additional decline of nearly 1 percentage point in the prime-age labor force participation rate relative to trend.

Importantly, this relationship is not driven by broader differences in state health or economic conditions. We find no comparable association between non-opioid mortality and post-pandemic labor outcomes, suggesting that generally poorer health alone cannot explain the weaker recoveries observed in high-opioid states. We further provide evidence on a potential mechanism: in these states, exits from the labor force due to disability or illness rose disproportionately after COVID-19. This pattern is consistent with individual-level evidence showing that individuals with drug use disorders are substantially more likely to leave the labor force and to report health-related work limitations.

Taken together, our findings highlight how overlapping public health crises can interact to produce lasting labor market effects. More broadly, they suggest that the labor force consequences of future economic shocks may depend critically on pre-existing public health vulnerabilities.

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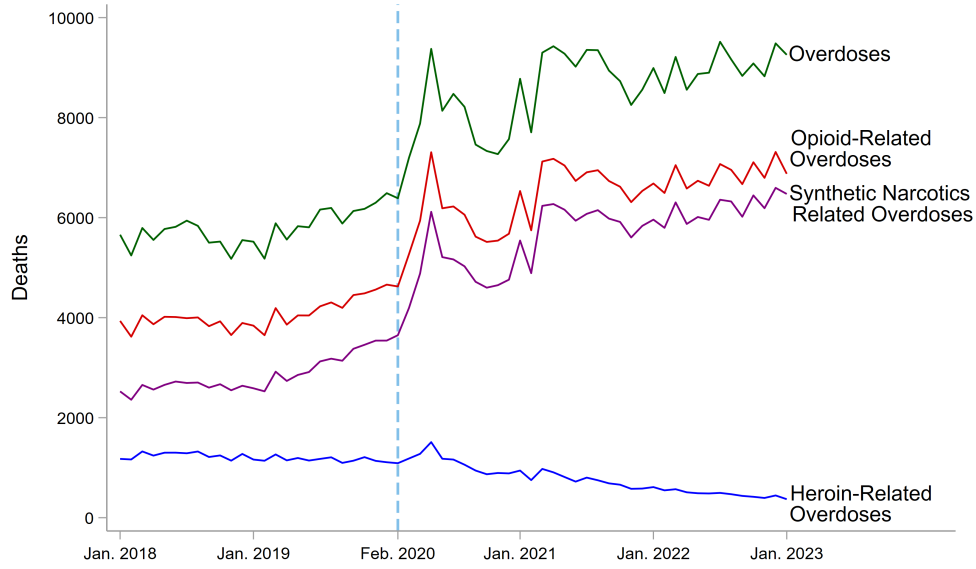
# Online Appendix

# A Data

## A.1 Opioid Deaths Across the United States

Figure A1 shows monthly deaths from all overdoses, opioid-related overdoses, and specific opioid-related categories (synthetic opioids and heroin) from 2018 to 2023 in the United States. The figure highlights that the post-COVID rise in opioid-related mortality has been driven primarily by synthetic opioids.

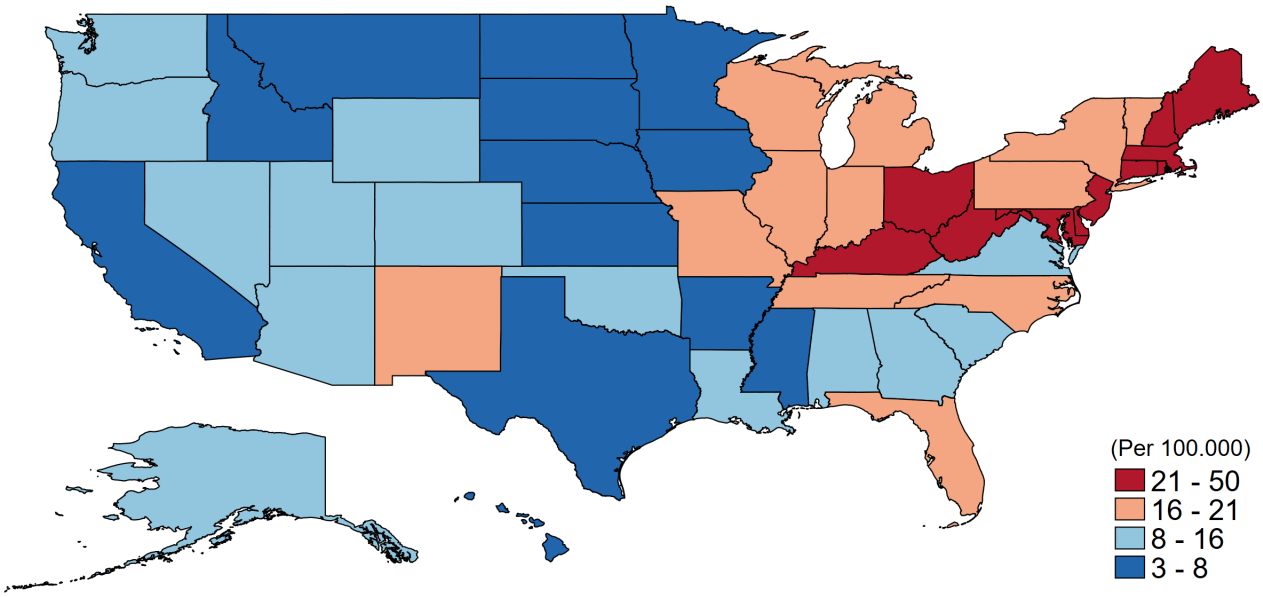
**Figure A1:** Monthly Overdose Deaths by Drug Type




**Notes**: *Monthly deaths involving all overdoses, opioid-related overdoses, synthetic opioids, and heroin from 2018 to 2023. Source: CDC-MCOD.*

Figure A2 shows the age-adjusted opioid-related overdose death rates in 2017 across the United States. The regions with the highest death rates, ranging from 16 to 50 per 100,000, are the Appalachian region, the Rust Belt, and New England.

**Figure A2:** Age-Adjusted Death Rates from Opioid Overdose, 2017




**Notes**: *Age-adjusted opioid-related overdose death rate for 2017 across the US. Death rates are computed per 100,000 people. Source: CDC-MCOD.*

## A.2 Labor Force Participation Trends

To obtain the difference between the actual and predicted prime-age labor-force participation shown in Figure 3, we do the following steps. First, we estimate a linear time trend in prime-age LFP for each state from January 2010 to February 2020. Second, we use the estimated coefficients to forecast the prime-age LFP from March 2020 to December 2022. Lastly, we determine the difference between the observed values and the trend.

To predict the trend, we first HP filter the time series for each state for the period January 2010 - February 2020 (default smoothing parameter, $1600 \times 34$). Then, we run on the obtained trend the following state-level regression:

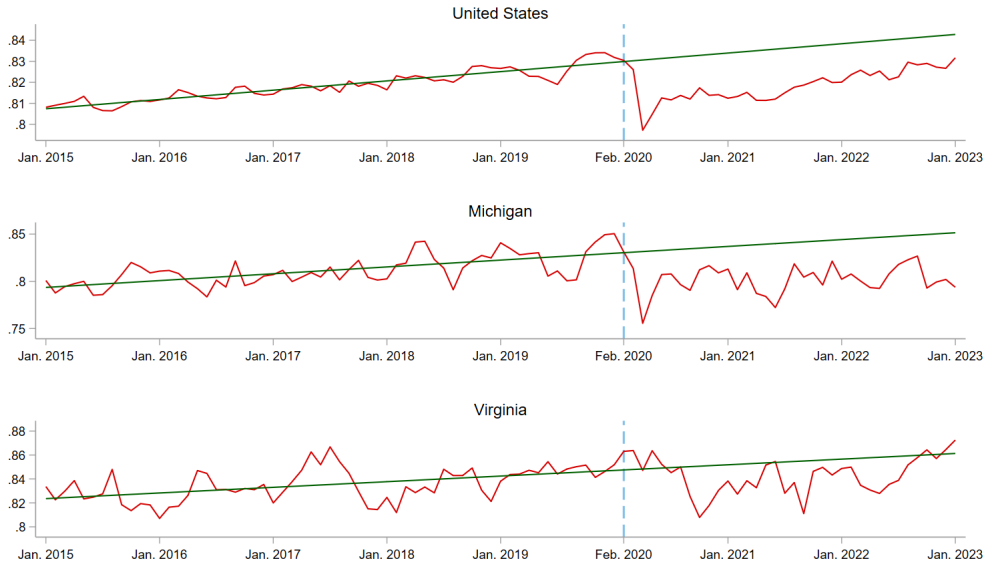
$$LFP_{i,t} = \alpha + \beta_1 t$$

where $LFP_{i,t}$ is prime-age labor-force participation for state $i$ at date $t$ (monthly).

Using the estimated coefficient, we predict the prime-age LFP for each state for March 2020 to December 2022, $\widehat{LFP_{i,t}}$. Figure A3 shows the actual (red line), as well as the

estimated and predicted (green line) prime-age LFP for the US, Michigan (a state where LFP stays below trend during the sample), and Virginia (where LFP recovers rather quickly), as examples.

**Figure A3:** Estimated and Predicted LFP for the US, Michigan, and Virginia




**Notes**: *Actual (red line) and predicted (green line) prime-age LFP. The figure shows the results for the entire United States and the states of Michigan and Virginia. Source: CDC.*

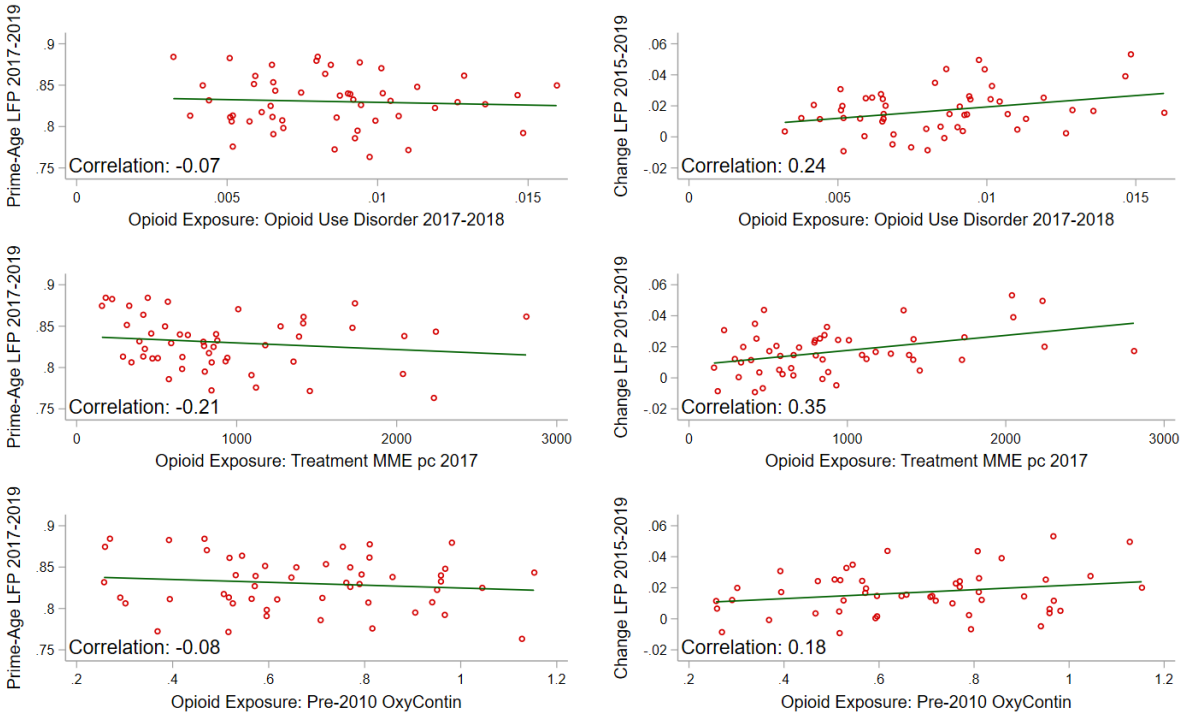
Then we obtain the difference as $Diff_{i,t} = LFP_{i,t} - \widehat{LFP_{i,t}}$. Negative values imply a lower labor-force participation than what has been predicted. To obtain the measure presented on the vertical axis of Figure 3, we average the difference for the period of July to December 2022.

## A.3 Labor-Force Participation and Opioids

The left figures in Figure A4 show the correlation between the average prime-age LFP between 2017 and 2019, and the alternative measures of opioid exposure. Meanwhile, the figures on the right column show the correlation between the growth in prime-age LFP between 2015 and 2019, and the alternative measures of opioid exposure. The alternative measures are: the percentage of individuals with opioid use disorder in 2017-2018 in the first row, the MME per capita of shipment of medications used to treat opioid addiction in the second row, and the pre-2010 OxyContin exposure in the third row. Across the three

measures, the correlations with the average prime-age LFP between 2017 and 2019 are not statistically significant. Meanwhile, there is a positive significant correlation between the first two alternative measures and the growth in prime-age LFP between 2015 and 2019.

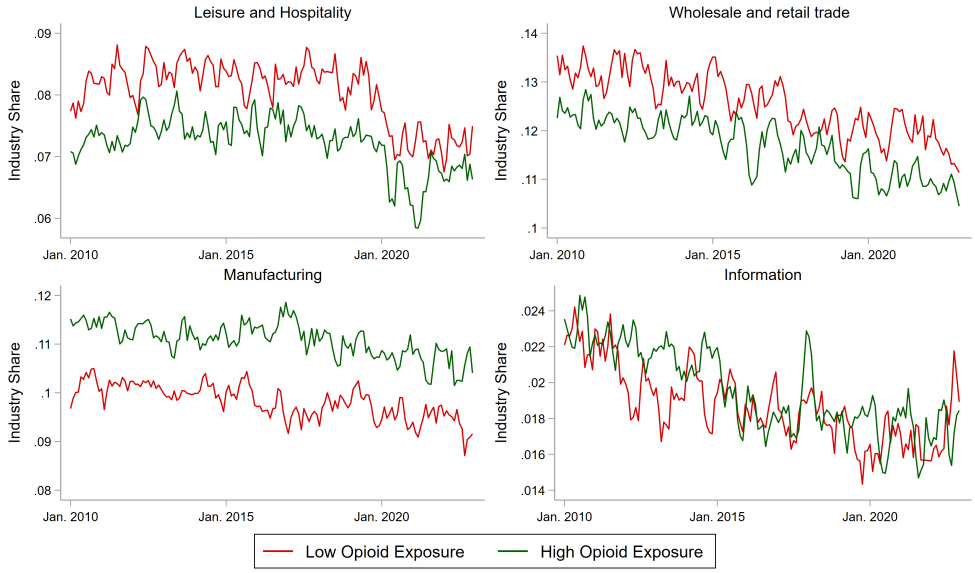
**Figure A4:** Correlations Between Prime-Age LFP and Alternative Opioid Exposure Measures




**Notes**: *Correlation between the average prime-age LFP between 2017 and 2019, and alternative measures of opioid exposure on the left. Correlation between the prime-age LFP growth between 2015 and 2019, and alternative measures of opioid exposure on the right Source: CPS, NSDUH, ARCOS.*

Figure A5 complements this by showing the time series of industry shares for selected sectors. The trajectories for both groups are parallel in the pre-COVID period, indicating no diverging industry composition trends prior to 2020.

**Figure A5:** Industry Employment Shares Over Time: Selected Sectors, 2010–2023

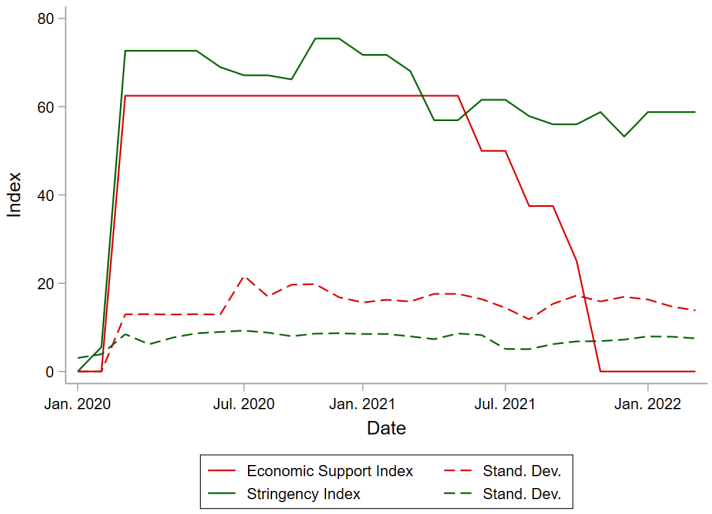



**Notes**: *Time series of employment shares in selected industries for states above and below the median opioid exposure based on 2017 age-adjusted opioid mortality. Source: CPS.*

## A.4 Stringency and Economic Support Index

Figure A6 shows the average and standard deviation of the Stringency and Economic Support index developed by the Oxford Tracker. The Oxford Tracker Dataset categorizes various COVID-19 policies into indexes representing different policy strengths, normalized between 0 and 100.

**Figure A6:** Stringency and Economic Support Index



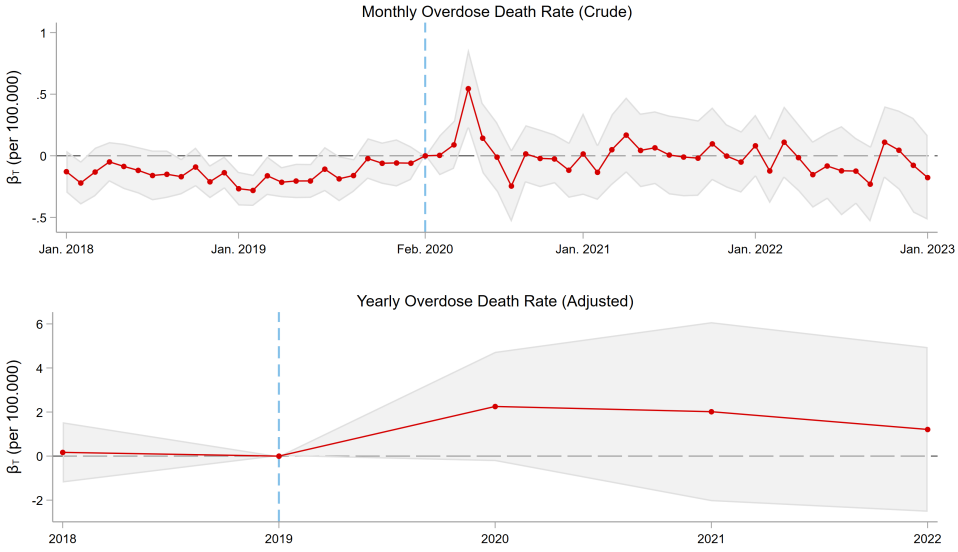

**Notes**: *Summary policy indexes for Stringency and Economic Support reported by the Oxford Tracker for the entire United States and the standard deviation of these policy indexes across states. Source: Oxford Tracker.*

# B Placebo, Heterogeneity and Robustness

## B.1 Opioid Exposure and Deaths

Figure B1 shows the results obtained by estimating equation (1) using the overdose death rate as the dependent variable. The top panel uses the monthly overdose death rate, and the bottom panel uses the yearly age-adjusted overdose death rate. Initial opioid exposure is measured as a binary variable, with states being given a value of 1 if their age-adjusted opioid death rate in 2017 was the median or above and 0 if below.

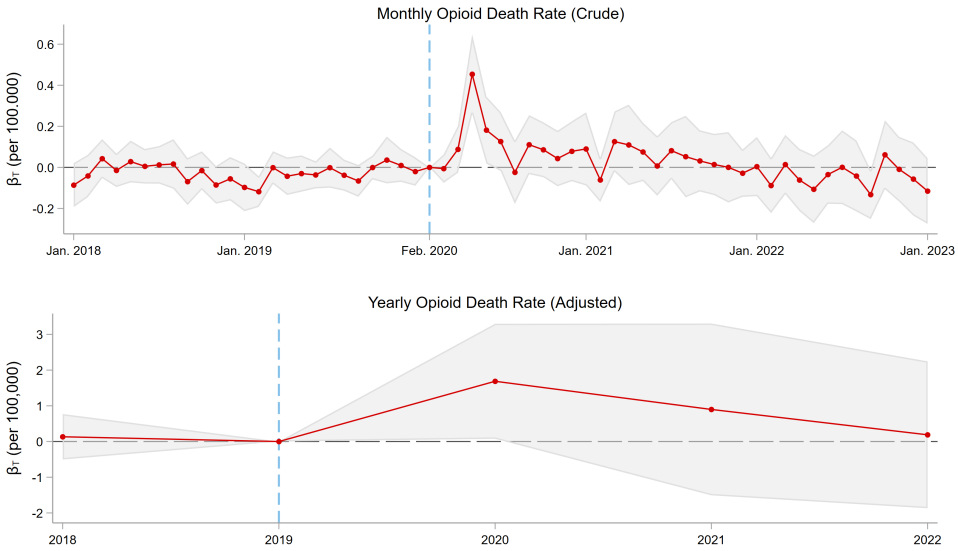
**Figure B1:** Non-Parametric Binary Event Study - Overdose Deaths




**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom panel uses the yearly age-adjusted rate. The initial opioid exposure measure has been dichotomized: states with an age-adjusted opioid death rate in 2017 above the median are given a value of 1, the others a value of 0.*

Figure B2 shows the results obtained by estimating equation (1) using the opioid death rate as the dependent variable. The top panel uses the monthly opioid death rate, and the bottom panel uses the yearly age-adjusted opioid death rate.

**Figure B2:** Non-Parametric Event Study - Opioid Deaths

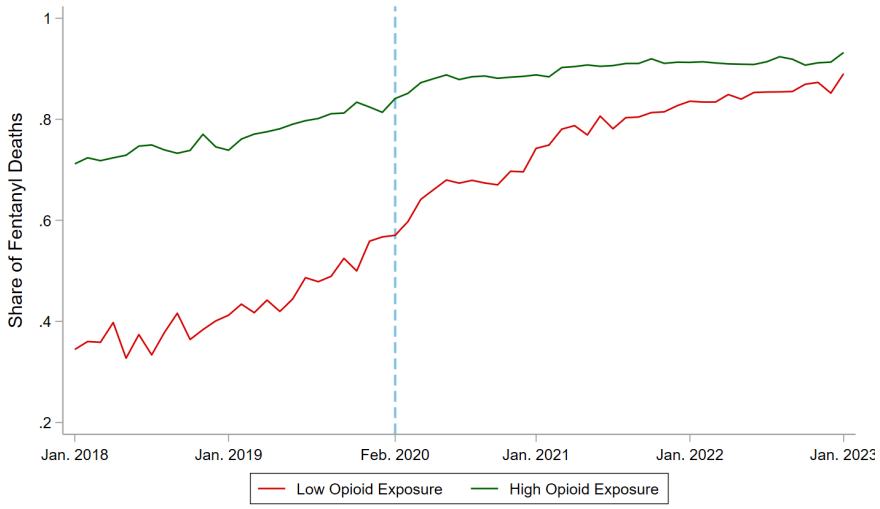



**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in opioid overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom panel uses the yearly age-adjusted rate. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

Figure B3 reports the monthly share of opioid overdose deaths involving synthetic opioids (fentanyl) for the same high- and low-exposure groups. The fentanyl share is computed as the number of deaths involving synthetic opioids (ICD–10 code T40.4) divided by total opioid-related overdose deaths. This figure highlights how the composition of opioid deaths evolved across the two groups during the pandemic.

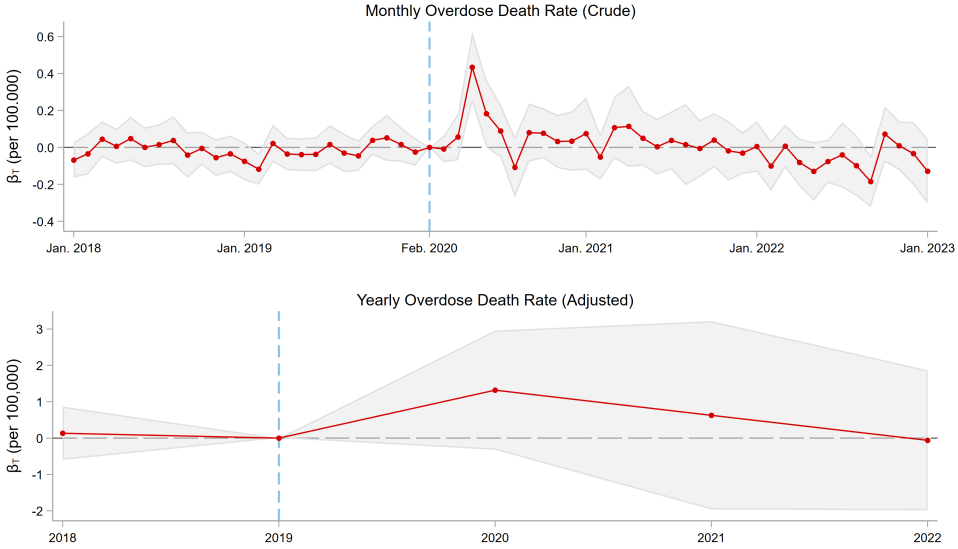
Figure B4 and Figure B5 show the results obtained by estimating equation (1) using the overdose death rate, and the opioid death rate as the dependent variable, respectively, but without including fentanyl-share as a control. The top panel uses the monthly overdose death rate, and the bottom panel uses the yearly age-adjusted overdose death rate.

**Figure B3:** Fentanyl Share - Group



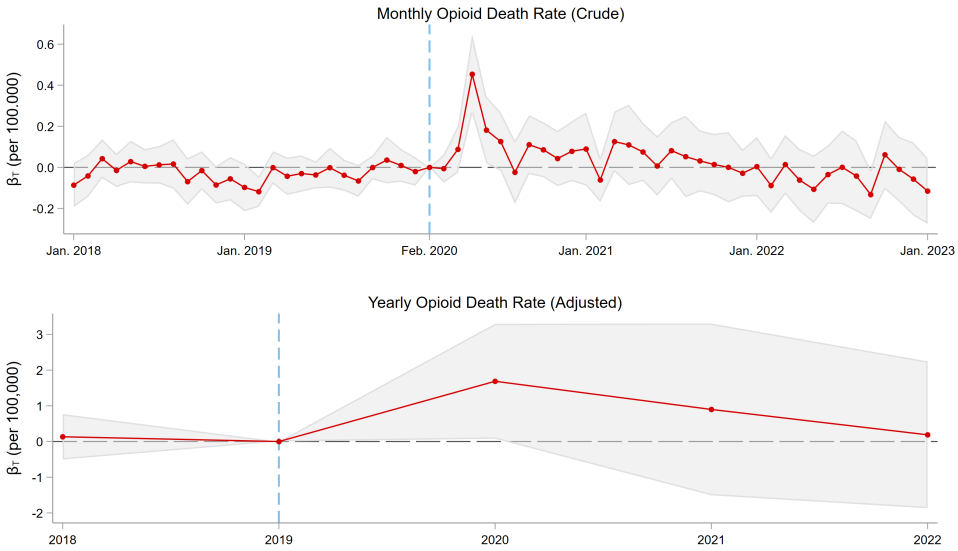

**Notes**: *Monthly share of opioid overdose deaths involving synthetic opioids (fentanyl), computed as deaths with ICD–10 code T40.4 divided by total opioid-related overdose deaths. States are grouped into high and low exposure categories based on whether their 2017 age-adjusted opioid death rate is above or below the median.*

**Figure B4:** Non-Parametric Event Study - Drug Overdose Deaths - No Fentanyl Control




**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in drug overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom uses the yearly age-adjusted rate. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

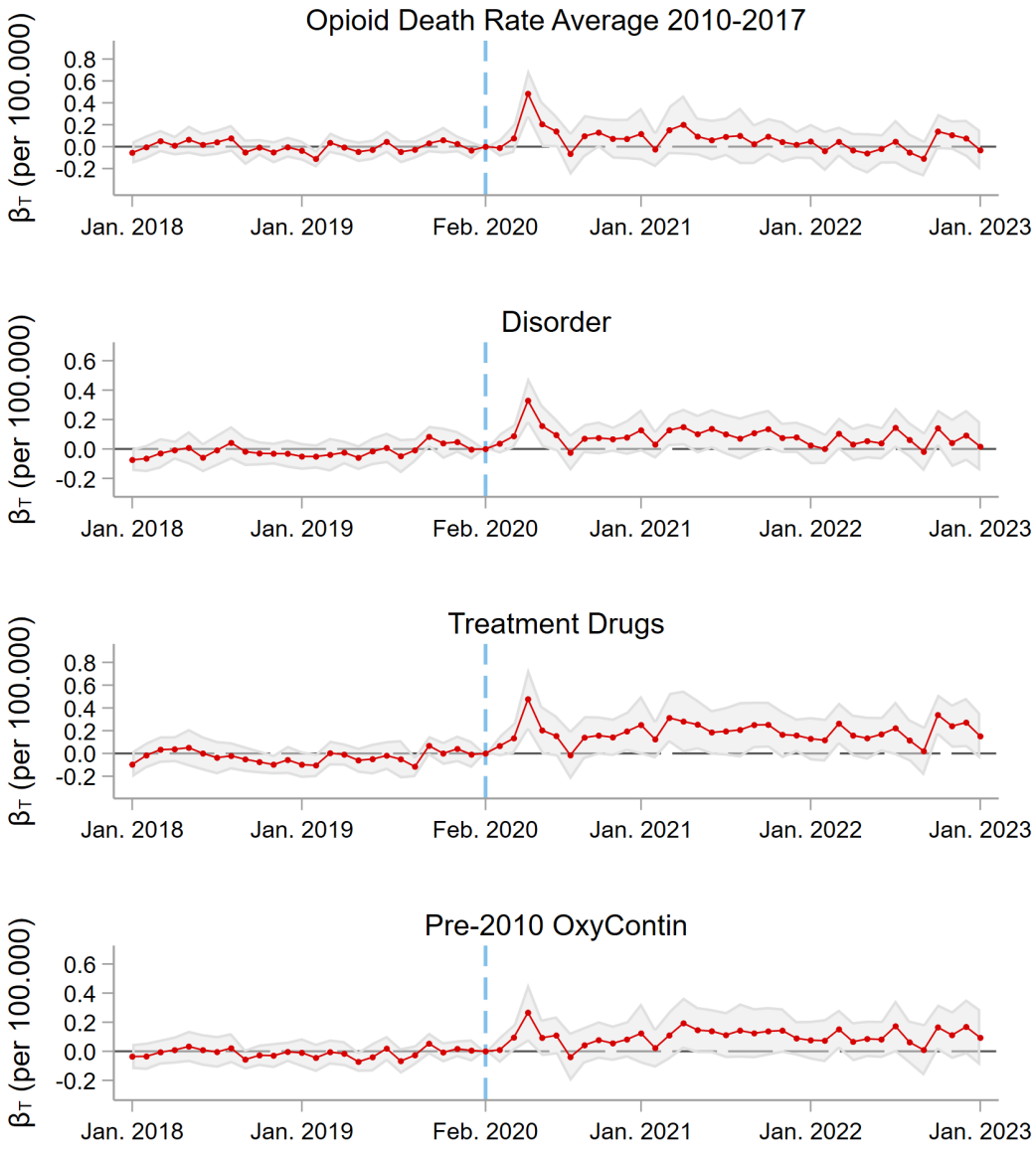
**Figure B5:** Non-Parametric Event Study - Opioid Overdose Deaths - No Fentanyl Control




**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in opioid overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom panel uses the yearly age-adjusted rate. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

Figure B6 shows the results obtained by estimating equation (1) using the monthly overdose death rate as the dependent variable and the four alternative measures of opioid exposure as independent variables. The top panel uses the combined opioid death rate between 2010 and 2017, the second panel the percentage of individuals with opioid use disorder in 2017-2018, the third panel uses the MME per capita of shipment of medications used to treat opioid addiction, and the bottom panel uses the pre-2010 OxyContin exposure.

**Figure B6:** Non-Parametric Event Study - Overdose Deaths with Alternative Measures

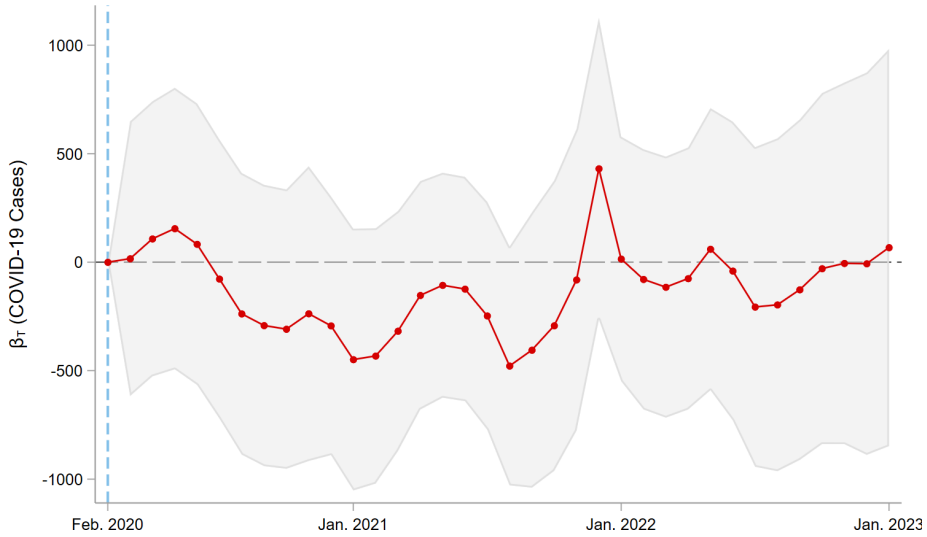



**Notes**: *Non-parametric event study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. Top panel uses opioid death for 2010-2017. Second panel uses percentage of people with opioid use disorder in 2017-2018. Third panel uses amount of MME per capita of drugs to treat opioid use disorder distributed to the state in 2017. The bottom panel uses pre-2010 OxyContin exposure. In all three cases the measure has been normalized to have a standard deviation equal to 1.*

## B.2 Opioid Exposure and COVID-19 Cases

Figure B7 highlights that COVID-19 cases across states were uncorrelated with pre-COVID opioid exposure. The figure shows the estimates obtained by running the non-parametric event study, equation (1), using monthly COVID-19 cases per 100,000 people as the dependent variable, and the continuous measure of initial opioid exposure as the independent variable. The analysis is done only for the post-COVID period.

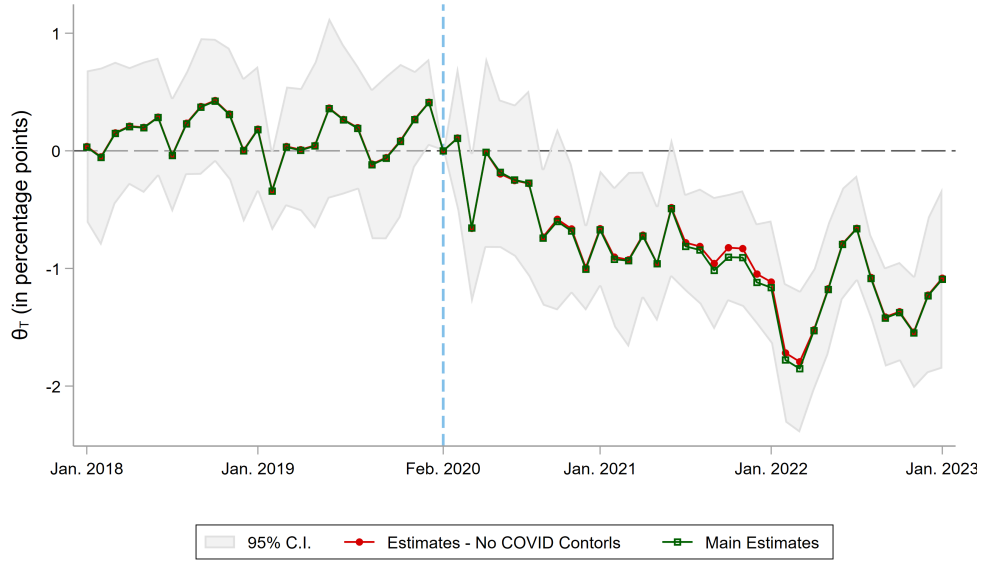
**Figure B7:** Non-Parametric Event Study - COVID-19 Cases




**Notes**: *Non-parametric event-study coefficients $\beta_T$ and the 95% confidence interval. Coefficients represent changes in total COVID-19 case rate reported relative to February 2020 between states with different levels of prior opioid exposure. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

Figure B8 reports the event-study estimates excluding COVID case rates and policy indices. The estimated post-2020 divergence across states with different pre-pandemic opioid exposure is quantitatively very similar to the baseline specification. Pre-trends remain close to zero. This indicates that the main findings are not driven by conditioning on realized pandemic severity or policy responses.

**Figure B8:** Parametric Event Study without COVID Controls




**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. Top panel, the initial opioid exposure measure has been dichotomized: states with an age-adjusted opioid death rate in 2017 above the median are given a value of 1, and the others a value of 0. For the bottom panel, the initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

## B.3 Placebo Analysis

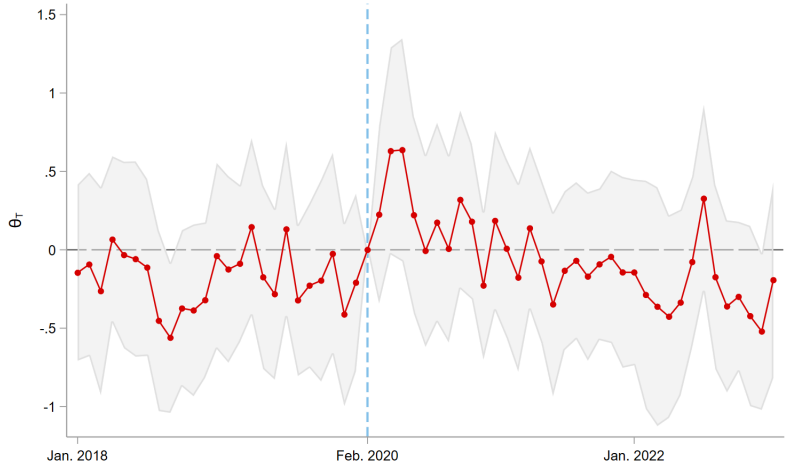
Table B1 shows the age-adjusted death rate in 2017 across the 15 leading causes of death in the US. The initial exposure measure used for the placebo analysis in the main text is constructed by aggregating the state-level death rate from these 15 causes, excluding the third (accidents) and eighth (intentional self-harm) causes of death. The reason for this is that some of the deaths in these causes are also included in the opioid death rate measure. Figure B9 presents the estimated coefficients from the parametric event study where the initial exposure measure is based on the total death rate from only the top 10 causes, excluding the third and eighth. Finally, Figure B10 shows the estimated coefficients from parametric event studies that have as the initial exposure measure the death rate of the cause highlighted in the title.

**Table B1:** Average Age-Adjusted Death Rate by Cause in 2017

| | Death Rate |
|---|---|
| 1. Diseases of heart (I00-I09,I11,I13,I20-I51) | 165.99 |
| 2. Malignant neoplasms (C00-C97) | 155.00 |
| 3. Accidents (unintentional injuries) (V01-X59,Y85-Y86) | 54.01 |
| 4. Chronic lower respiratory diseases (J40-J47) | 43.78 |
| 5. Cerebrovascular diseases (I60-I69) | 37.43 |
| 6. Alzheimer disease (G30) | 32.07 |
| 7. Diabetes mellitus (E10-E14) | 21.99 |
| 8. Intentional self-harm (suicide) (*U03,X60-X84,Y87.0) | 16.50 |
| 9. Influenza and pneumonia (J09-J18) | 14.90 |
| 10. Nephritis (N00-N07,N17-N19,N25-N27) | 12.79 |
| 11. Chronic liver disease and cirrhosis (K70,K73-K74) | 11.30 |
| 12. Septicemia (A40-A41) | 10.27 |
| 13. Parkinson disease (G20-G21) | 8.62 |
| 14. Essential hypertension and hypertensive renal disease (I10,I12,I15) | 8.44 |
| 15. Assault (homicide) (*U01-*U02,X85-Y09,Y87.1) | 6.69 |

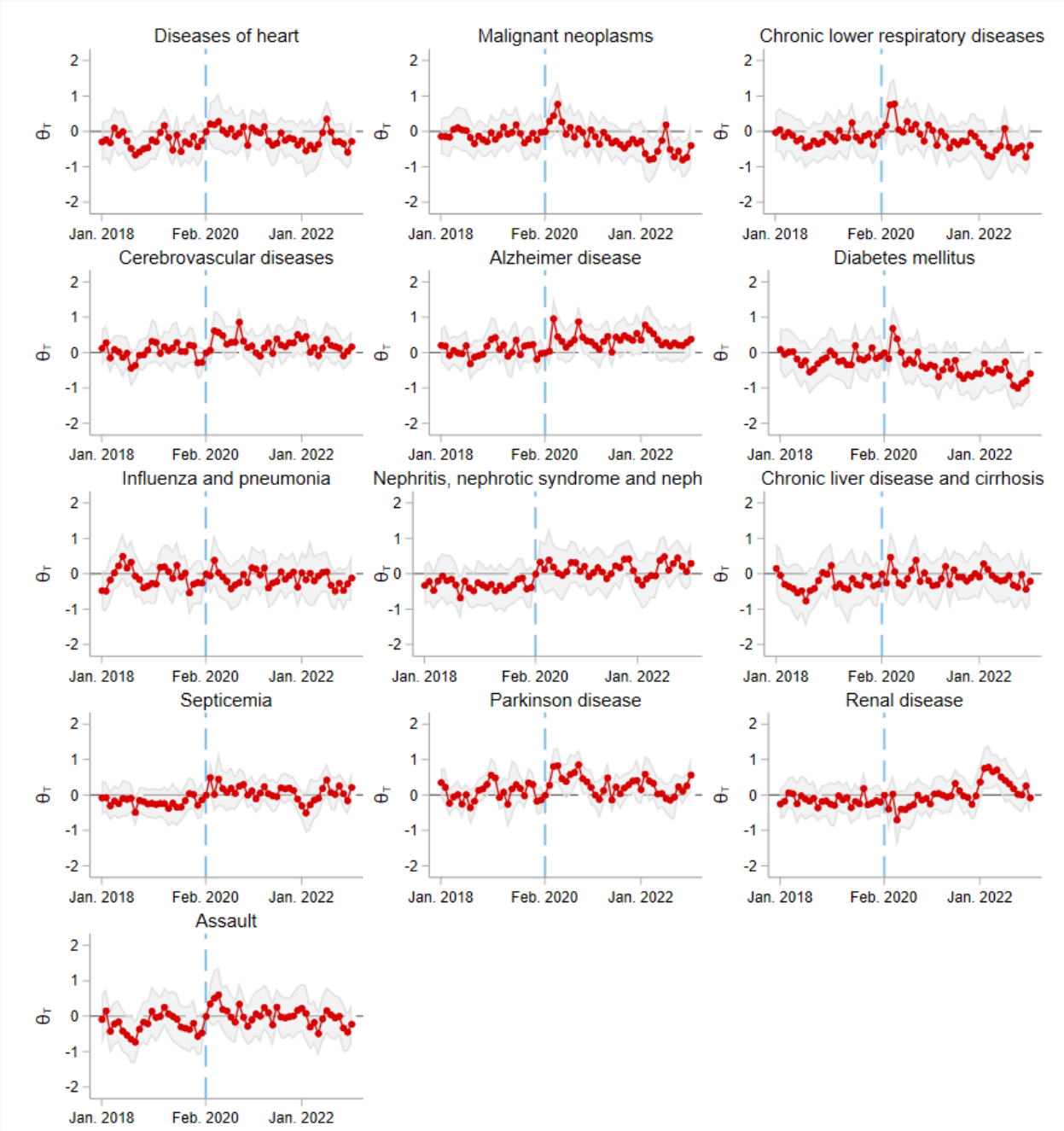
**Notes**: *The table shows the age-adjusted death rate in 2017 by the leading causes of death in the US. Source: CDC - Underlying Cause of Death.*

**Figure B9:** Parametric Event Study with Placebo Exposure




**Notes**: *Placebo parametric event study coefficients $\theta_T$ and the 95% confidence interval when using prime-age LFP as the dependent variable. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior placebo exposure. The measure of prior placebo exposure is the total age-adjusted death rate in 2017 due to the top 10 underlying causes of death. The death rate has been normalized to have a standard deviation equal to 1.*

**Figure B10:** Parametric Event Study Estimates - Placebo

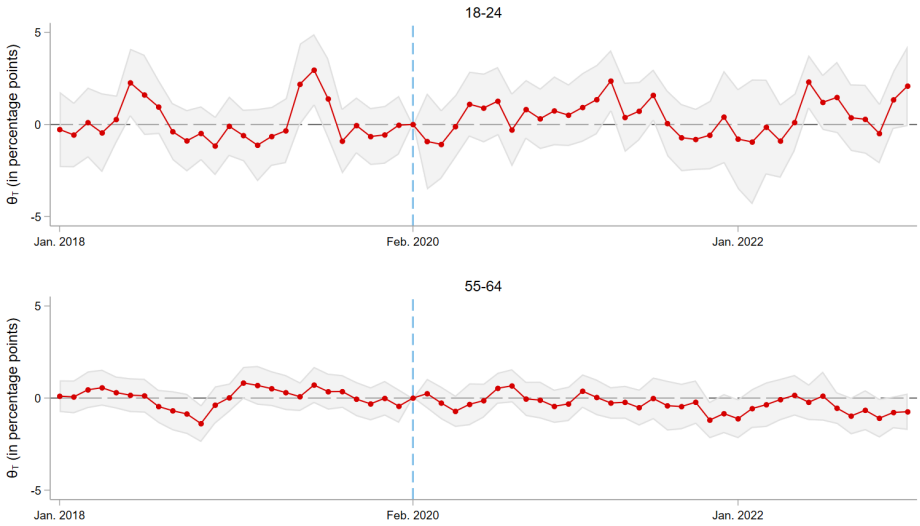



**Notes**: *Placebo parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from the state-trend in prime-age LFP between states with different levels of prior placebo exposure. The measure of prior placebo exposure is the age-adjusted death rate in 2017 due to causes indicated in the title of each panel. All death rates have been normalized to have a standard deviation equal to 1.*

## B.4 Alternative Age Groups

The main analysis focuses on the labor-force participation of prime-age workers, i.e., ages between 25 and 54. Figure B11 shows the estimated coefficients from the parametric event study analysis when focusing on the age groups 18-24 and 55-64.

**Figure B11:** Parametric Event Study Estimates - Labor Force Across Samples

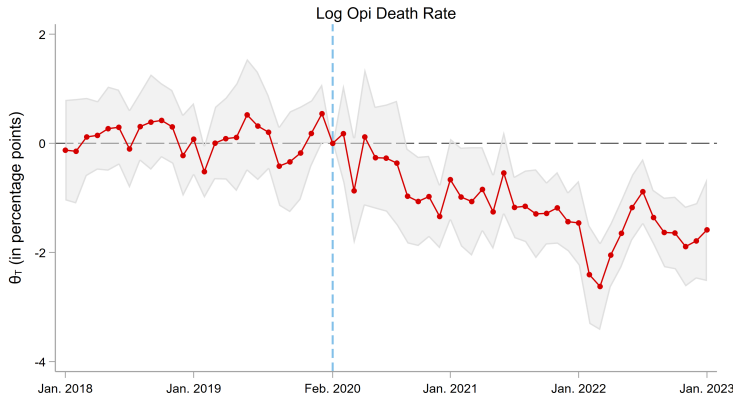



**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior opioid exposure. The upper panel restricts the sample to individuals aged 18-24, and the lower panel to those 55-64. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

## B.5 Alternative Exposure Measures

Figure B12 shows the estimated coefficients from the parametric event study analysis when using the logarithm of the age-adjusted opioid death rate in 2017.

**Figure B12:** Parametric Event Study Estimates - Measures of Opioid Death Rate

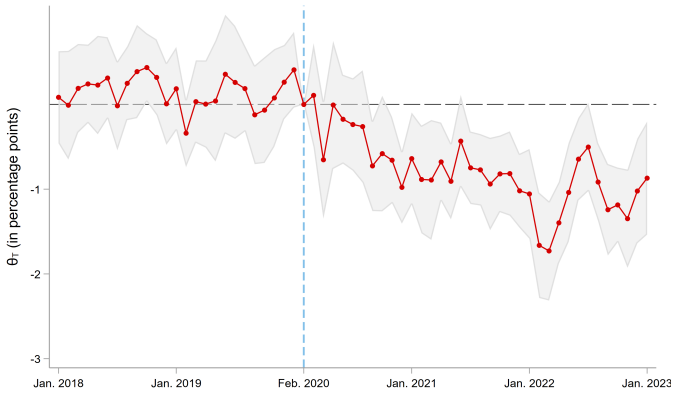



**Notes**: *Parametric event study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior opioid exposure, measured by the log of the opioid-related overdose age-adjusted death rate, normalized to have a standard deviation equal to 1.*

## B.6 Alternative Specification

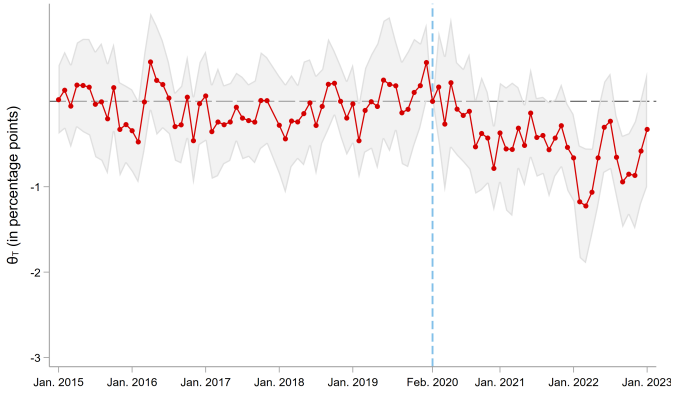
Figure B13 shows the estimated coefficients from estimating the parametric event study analysis, allowing for quadratic rather than linear state-specific trends. Figure B14 shows the estimated coefficients from estimating the parametric event study using a longer pre-period. Instead of estimating trends over 2018–2023 and using the 2017 opioid death rate as the exposure measure, the specification is re-estimated using data beginning in 2015 and using the 2015 opioid death rate as the measure of pre-COVID exposure. Under these alternative specifications, the results remain consistent with the main findings.

**Figure B13:** Parametric Event Study Estimates – Quadratic State Trends




**Notes**: *Parametric event study coefficients $\theta_T$ and 95% confidence intervals estimated from a specification that allows for quadratic state-specific trends. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. The initial opioid exposure measure has been normalized to have a standard deviation of 1.*

**Figure B14:** Parametric Event Study Estimates – Longer Pre-Period (2015 Start)




**Notes**: *Parametric event study coefficients $\theta_T$ and 95% confidence intervals estimated using data from 2015–2023 and the 2015 opioid-related overdose age-adjusted death rate as the exposure measure. Coefficients represent average differences from state trends in prime-age LFP between states with different levels of prior opioid exposure. Exposure is normalized to have a standard deviation of 1.*

# C Data on Mechanisms

## C.1 Employment and Health Statistics Across Opioid Users

Tables C1 and C2 present data on employment, health, and work disability for adults aged 24–49, based on the 2019 and 2021/2022 waves of the NSDUH. Individuals are categorized by their drug use in the past 12 months. The nonuser group includes those who reported no drug use, excluding marijuana. The disorder group comprises individuals diagnosed with a drug use disorder, excluding marijuana use disorder.

**Table C1:** Employment Status by Drug Use, Ages 24-49

| ***Panel A*** | **2019** | | | **2021/2022** | | |
|---|---|---|---|---|---|---|
| | Emp. | Unemp. | Out of LF | Emp. | Unemp. | Out of LF |
| Nonuser | 79.3% | 3.9% | 16.8% | 75.8% | 5.1% | 19.1% |
| Disorder | 58.7% | 15.3% | 26.0% | 54.3% | 13.0% | 32.7% |
| Population | 79.0% | 4.4% | 16.6% | 75.0% | 5.7% | 19.3% |
| ***Panel B*** | **2019** | | | **2021/2022** | | |
| | Very Good | Good | Fair/Poor | Very Good | Good | Fair/Poor |
| Nonuser | 61.4% | 28.7% | 9.9% | 58.1% | 31.2% | 10.7% |
| Disorder | 39.5% | 35.0% | 25.5% | 33.2% | 37.4% | 29.4% |
| Population | 60.5% | 28.9% | 10.6% | 56.5% | 31.7% | 11.8% |

**Notes**: *The table shows the employment status in Panel A, and self-reported health status in Panel B, by type of drug user in 2019 and for the 2021/2022 period. Self-reported health status is a categorical variable with 4 options: very good, good, fair, and poor. Fair and poor categories are aggregated. The type of drug user is defined by the NSDUH. See the text for further details. Nonusers do not use any drug, excluding marijuana. For each drug user type and year, the row sums up to 100. Source: NSDUH.*

## C.2 Out-of-labor-force due to Disability Rate

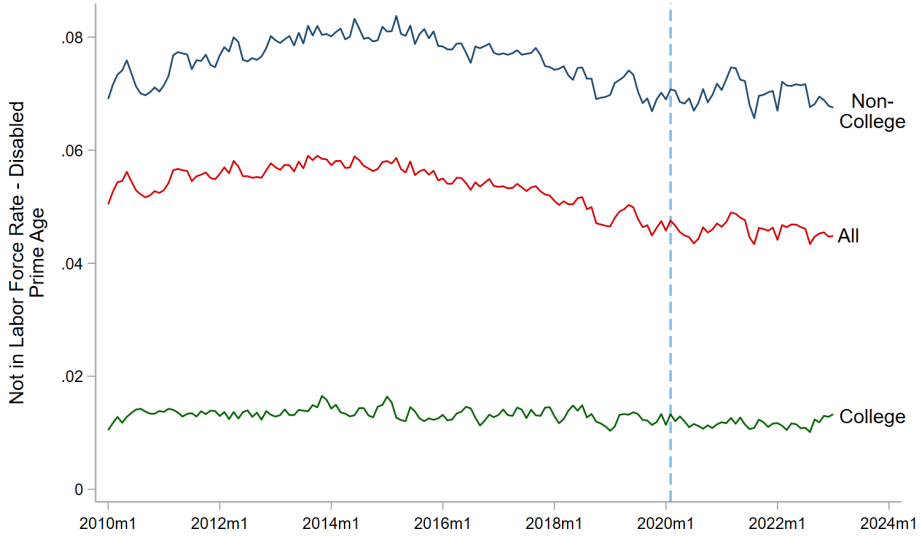
Figure C1 shows the share of prime-age individuals who do not work due to disability.

**Table C2:** Drug Use and Work Disability, Ages 24-49

| | NLF - Disability | | Work Days Sick | | Work Days Skip | |
|---|---|---|---|---|---|---|
| | **2019** | **2021/2022** | **2019** | **2021/2022** | **2019** | **2021/2022** |
| Nonuser | 17.4% | 16.8% | 0.83 | 1.00 | 0.28 | 0.32 |
| Disorder | 25.1% | 44.1% | 2.12 | 2.08 | 1.94 | 0.96 |
| Population | 18.2% | 19.4% | 0.88 | 1.05 | 0.34 | 0.38 |

**Notes**: *The table shows several measures of absence from work by type of drug user in 2019 and for the 2021/2022 period. The first two columns show the percentage of people out of the labor force due to a disability. The second two columns show the average number of work days missed in the past 30 days due to sickness. The last two columns show the average number of work days missed for other reasons. The type of drug user is defined by the NSDUH. See the text for further details. Source: NSDUH.*

**Figure C1:** Out-of-labor-force due to Disability Rate

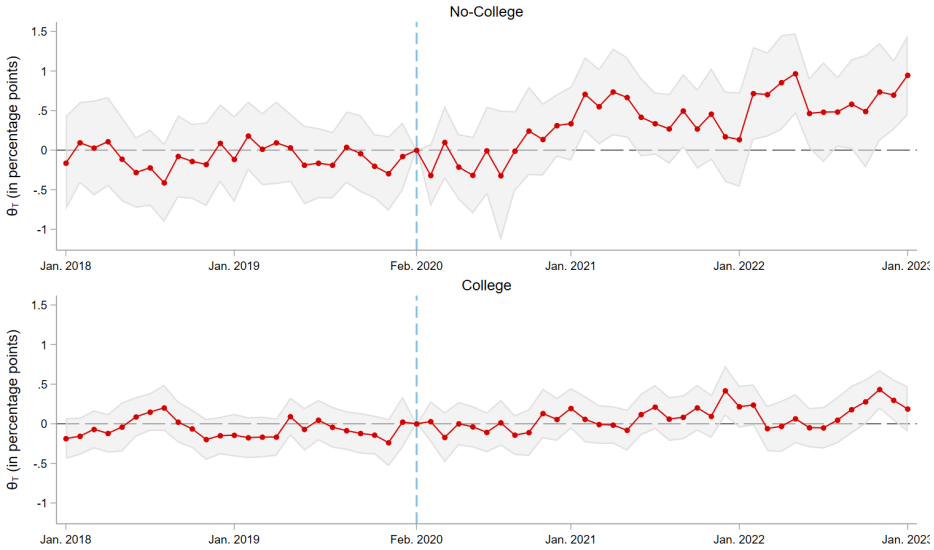



**Notes**: *Share of prime-age individuals not in the labor force for disability reasons. This measure includes individuals who report being unable to work and not working due to disability. Source: CPS*

## C.3 Out-of-labor-force due to Disability Rate across Education

Figure C2 shows the estimated coefficients from conducting separate parametric event studies for the non-college and college-educated samples using the fraction out-of-labor-force due to disability as the dependent variable.

**Figure C2:** Parametric Event Study Estimates - Out-of-LF due to Disability




**Notes**: *Parametric event-study coefficients $\theta_T$ and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age out of labor force due to disability between states with different levels of prior opioid exposure. The top panel uses prime-age out of labor force due to disability among non-college-educated, while the bottom uses college-educated. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*