Opioids and Post-COVID Labor-Force Participation*

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Abstract

At the onset of COVID-19, U.S. labor-force participation dropped by about 3 percentage points and remained below pre-pandemic levels three years later. Recovery varied across states, with slower rebounds in those more affected by the pre-pandemic opioid crisis, as measured by age-adjusted opioid overdose death rates. An event study shows that a one-standard-deviation increase in pre-COVID opioid death rates corresponds to a 0.9 percentage point decline in post-COVID labor participation. The result is not driven by differences in overall health between states. The effect of prior opioid exposure had a more significant impact on individuals without a college degree. The slow recovery in states with more opioid exposure was characterized by an increase in individuals who are not in the labor force due to disability.

Keywords: Labor-Force Participation, Health, Opioids, COVID-19

JEL Codes: I12, I14, J11, J12, J21

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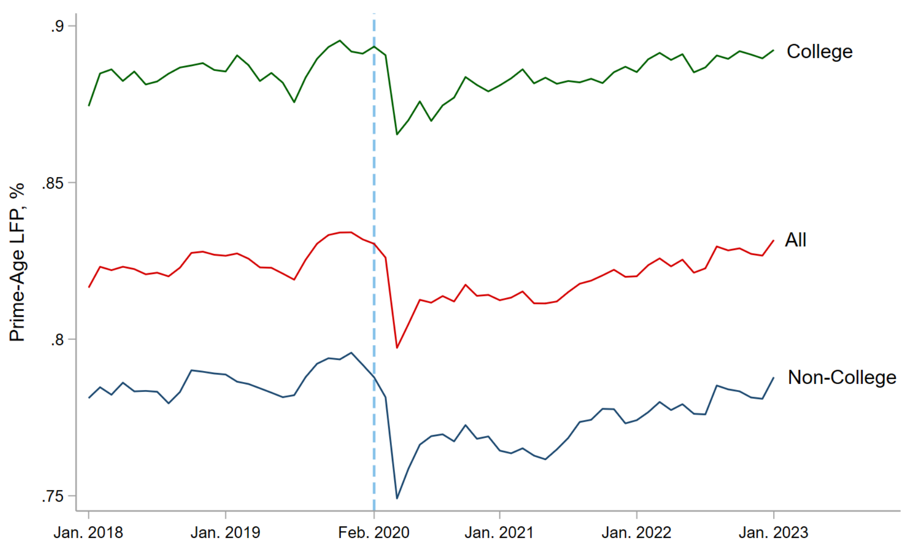
1 Introduction

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 of the labor supply was significantly different across US states. In December 2022, for example, LFP among prime-age workers was still 5 percentage points below the trend in Michigan, while the LFP gap from the trend disappeared in Georgia. On average, LFP was around 1 percentage point below the trend, with a standard deviation of 2 percentage points across US states.

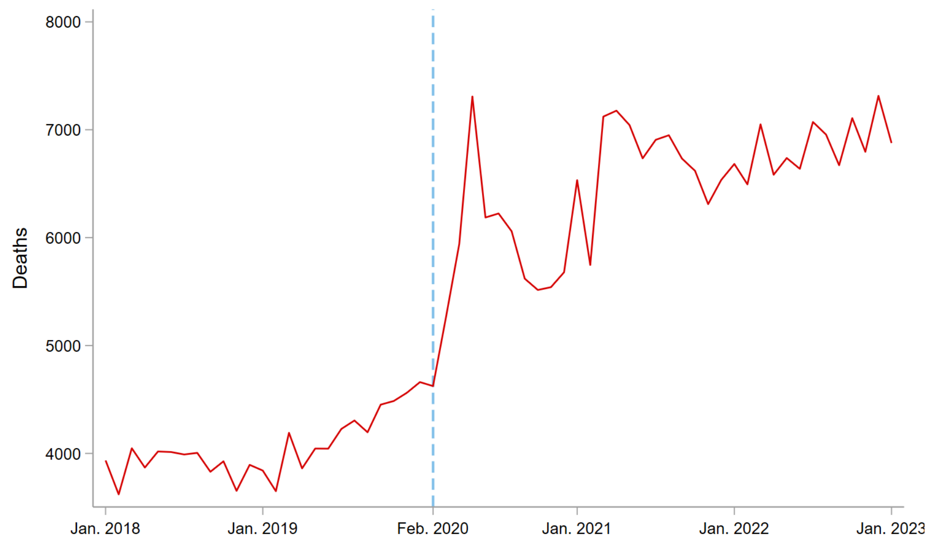
Figure 1: Labor-Force Participation Rate, Ages 25-54



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

The COVID-19 pandemic occurred in the US as another epidemic, the opioid crisis, was unfolding (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 substances, such as alcohol and methamphetamine, also increased substantially during this period (Mulligan, 2022).

Figure 2: Monthly Deaths from Opioid Overdose



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

Individuals who misuse opioids 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 following Alpert, Powell, and Pacula (2018) and Beheshti (2023). 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 opioid use 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 than other measures from measurement error. The analysis shows that a one-standard-deviation increase in pre-COVID opioid exposure reduces labor-force participation by approximately 0.9 percentage point 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, like 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 was 3.4 percentage points.

We show that this 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. The results are robust to alternative measures of pre-COVID opioid exposure, including the fraction of individuals with an opioid use disorder and shipments of medications for opioid addiction treatment. An alternative synthetic control group approach, as outlined in Abadie, and Gardeazabal (2003), also yields comparable results.

Finally, we explore a possible mechanism to help interpret the results. Individuals with opioid 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 disparities in labor-force participation and disability rates between nonusers and individuals with opioid use disorder 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 as an exogenous shock the change in regulations that made the prescription of hydrocodone more difficult in 2014. Like Alpert, Powell, and Pacula (2018), he compares units affected differently by a treatment (changes in drug availability) and shows that areas that experienced larger reductions in hydrocodone prescriptions experienced 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 has been surprisingly tight, with low unemployment and labor-force participation rates (Coibion, Gorodnichenko, and Weber, 2020; Forsythe et al., 2022). Quits and the number of workers looking for new jobs also increased (Gittleman, 2022; Barlevy et al., 2024). There has also been a decline in the 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, mainly remote work, which led to persistent labor reallocation. The current analysis contributes to the literature by focusing on opioid use, a factor influencing labor supply behavior 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 labor-force participation rate of prime-age civilians (ages 25 to 54), calculated for each state and month using data from the Current Population Survey (CPS). 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. Additionally, CPS data are used to construct state-level employment levels by industry.

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).[1] An opioid overdose death is identified when the underlying 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 and accounting for variations in the types of opioids used, such as OxyContin, Fentanyl, and heroin, each differing significantly in potency. 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. There is also considerable variation in death rates across states, as shown in Figure A1 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

[1] 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.

pandemic, as evidenced by the increase in the standard deviation between 2019 and 2021, from 10 to 13.[2]

Table 1: Age-Adjusted Opioid Overdoses 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.*

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 last six months of 2022 is used.[3] The predicted post-COVID values are based on state-level regressions of labor-force participation using a polynomial in time.[4] 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.

Two 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 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, who are further screened for substance use disorder, identified by health issues, disabilities, or significant life disruptions caused by recurring use.[5]

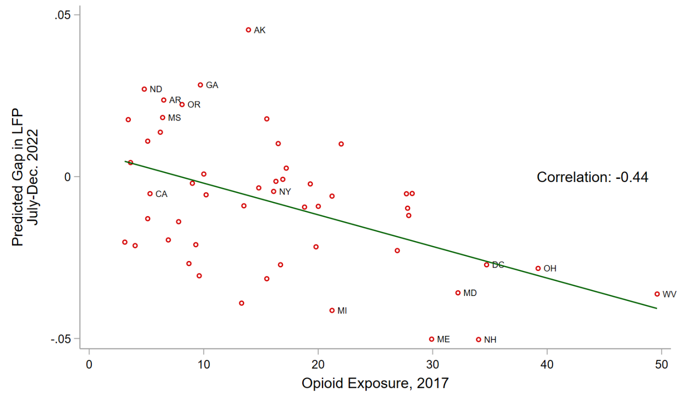
[2]Further details on the demographic and geographic variation in fatal drug overdoses over time can be found in Monnat (2022).

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

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

[5]State-level data on 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

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



Notes: *Correlation between the predicted gap in prime-age LFP and age-adjusted opioid-related overdose death rate for 2017. The predicted gap in prime-age LFP is constructed by subtracting the predicted LFP from the observed one. The average of the predicted gap between July and December 2022 is reported. Appendix A.2 describes the construction of the predicted LFP values. Source: CDC-MCOD and CPS.*

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 Agency's (DEA) Automation of Reports and Consolidated Orders System (ARCOS).[6] 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.[7] Summary statistics for these alternative exposure measures and their correlations with age-adjusted death rates are presented in Table 2. There are also significant differences in these measures across states, with a high correlation between these metrics and the age-adjusted death rates.

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

geographic indicators.

[6] 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.

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

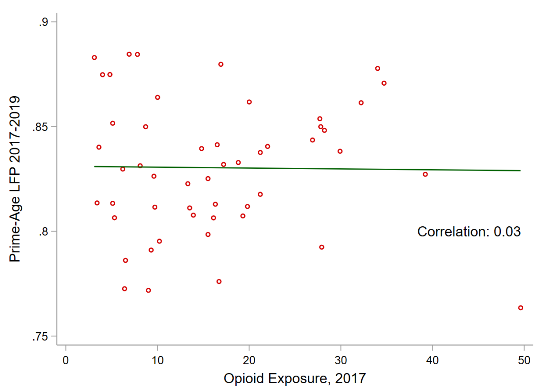
Table 2: Alternative Opioid Exposure Measures

	Mean	*sd*	Min	Max	Corr. with Death Rate
Death Rate, 2017	16.26	10.52	3.10	49.60	1.00
Disorder, 2017-18 (%)	0.84	0.30	0.32	1.60	0.41
Treatment MME pc, 2017	925.43	601.09	157.06	2,809.07	0.78

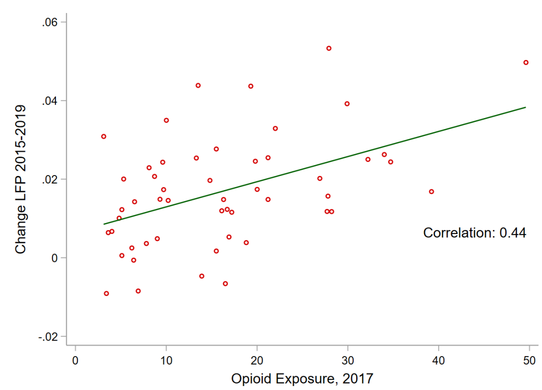
Notes: *Summary statistics for measures of state opioid exposure. The second row shows the percentage of people with an opioid use disorder in 2017-2018, obtained from the NSDUH. The third row shows the quantity of drugs to treat opioid use disorder distributed to the state in 2017, measured as MME per capita, obtained from ARCOS. The last column shows Spearman's rank correlation between these two measures and the age-adjusted death rate.*

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.[8]

Figure 5: 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 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.*

[8]Figures A3 and A4 in Appendix A.3 analyze this relationship using the two alternative measures of opioid exposure. There is no significant correlation between the measures and LFP between 2017 and 2019. Meanwhile, the relationship between the two alternative measures and the LFP growth rate between 2015 and 2019 is positive.

The empirical analysis also includes control variables reflecting 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.[9] 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.[10] The average value of these two indexes and their standard deviations across US states are shown in Figure A5 in Appendix A.4.[11]

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 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 opioid availability or a larger population with opioid use experience, which became more salient post-COVID. Importantly, 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) that made it harder for doctors to prescribe. The areas with higher initial hydrocodone prescriptions are found to have more significant improvements in labor-force participation and employment.

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

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

[11] 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.

The empirical approach employs a dynamic two-way fixed effects model, with labor-force participation as the dependent variable and the interaction between the COVID-19 outbreak (February 2020) and pre-COVID opioid exposure as the key explanatory variable. 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, and α_s and α_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 β_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 variables in $X_{s,t}$ include monthly COVID-19 case counts (set to 0 for the pre-COVID period) to account for the severity of the epidemic across states. Additionally, the Stringency and Economic Support indexes, which summarize state-level COVID-19 policies, are included as they likely influence economic activity and labor supply incentives. Lastly, a Bartik-style control variable is added to account for time-varying differences in industrial structures 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 specification in equation (1) assumes that states with different initial opioid exposure exhibited parallel trends in labor-force participation before the COVID-19 epidemic. This parallel trends assumption allows post-COVID differences to be interpreted as difference-in-difference outcomes. Consequently, the estimated values of β_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

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

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 but instead relies on similar deviations from potentially different linear trends in each state. In this specification, the coefficients θ_T are estimated only for the post-COVID period. However, the β_T estimates from equation (1) can be used to derive θ_T for the pre-COVID period (Dobkin et al., 2018). If these constructed pre-COVID estimates are close to 0, the linear trend assumption can be considered a good fit for 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 Opioid Exposure and Slow Labor Supply Recovery

The empirical strategy relies on the assumption that the COVID-19 shock led to differential increases in opioid use, depending on the pre-COVID opioid exposure. More specifically, we expect that states with higher exposure should also exhibit higher opioid death rates following the COVID-19 shock. To test this "first-stage" effect, equation (1) is estimated with opioid death rates as the dependent variable, $Y_{s,t}$, and the continuous age-adjusted opioid death rates as our measure of pre-COVID opioid exposure, $Op.\ Exp_s$.[12] Figure B2 in Appendix B.2 illustrates another assumption of the empirical analysis and shows that COVID-19 cases (treatment) were uncorrelated with pre-COVID opioid exposure (predetermined condition) across states.

Only the non-parametric event study results are presented, as the pre-COVID coefficients β_T do not indicate the presence of pre-trends. The findings, shown in Figure 6, reveal that a one-standard-deviation increase in initial opioid exposure is associated with a 0.4 point rise in the monthly opioid death rate by May 2020 and a 2 point increase in the yearly death rate for 2020. These effects are substantial, representing 35 percent of the monthly and 13 percent of the yearly pre-pandemic death rates.

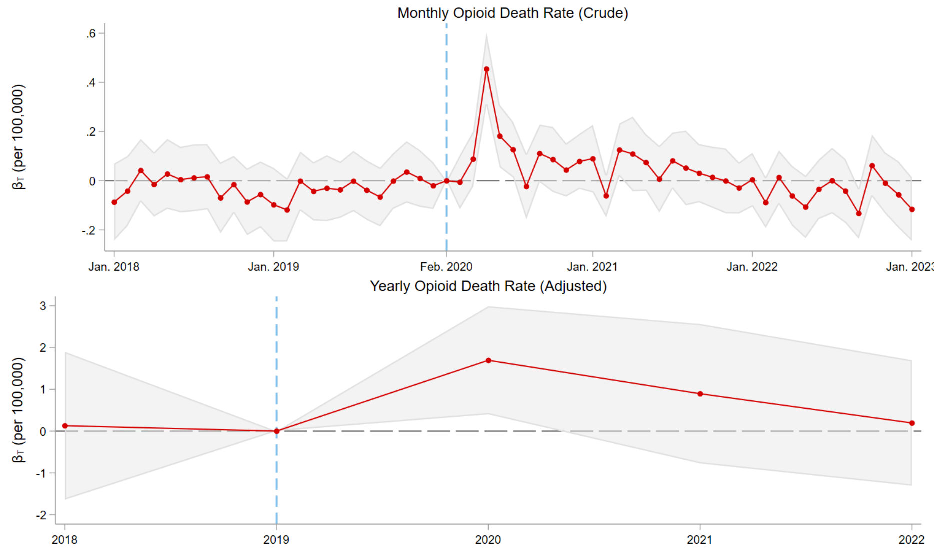
The results for labor-force participation are presented next. The β_T estimates from the non-parametric event study are shown in Figure 7. To ease interpretation, coefficients are already multiplied by 100 and presented as percentage points. The upper panel presents results from equation (1) using a binary measure of exposure, categorizing states as above or below the median 2017 age-adjusted opioid death rate. 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 consistent negativity of β_T coefficients in the pre-COVID period indicates the presence of 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 8. The upper panel displays results

[12] 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.

Figure 6: Non-Parametric Event Study with Continuous Exposure - Opioid Deaths



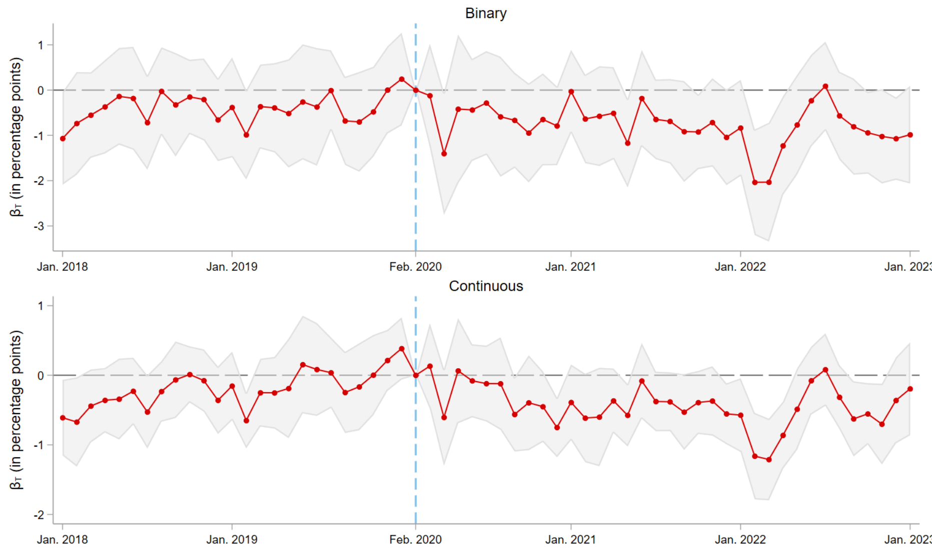
Notes: *Non-parametric event study coefficients* β_T *and the 95% confidence interval. Coefficients represent changes in the opioid overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom uses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

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 θ_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 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

Figure 7: Non-Parametric Event Study, Labor-Force Participation



Notes: *Non-parametric event study coefficients β_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.*

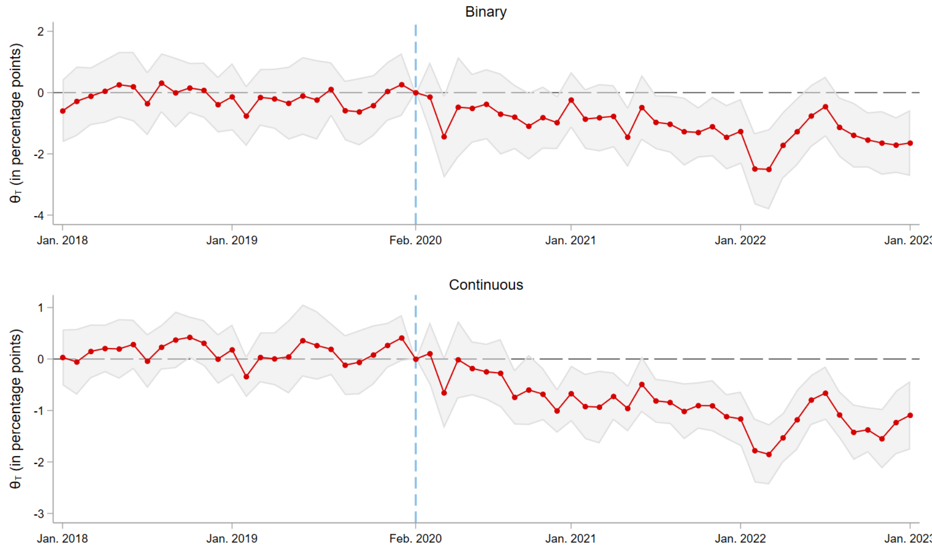
point drop in LFP during the post-COVID period. This implies that moving from the 25th percentile of initial opioid exposure (e.g., Minnesota) to the 75th percentile (e.g., Pennsylvania or Michigan) leads to an average LFP decline of 1.1 percentage points—a substantial effect, given that the standard deviation in prime-age LFP across states during the post-COVID period was 3.4 percentage points.

4.1 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

Figure 8: Parametric Event Study, Labor-Force Participation



Notes: *Parametric event study coefficients θ_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. 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.*

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.[13] 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.[14] Figure 9 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.[15] The coefficients for the post-COVID period are not statistically significant.[16]

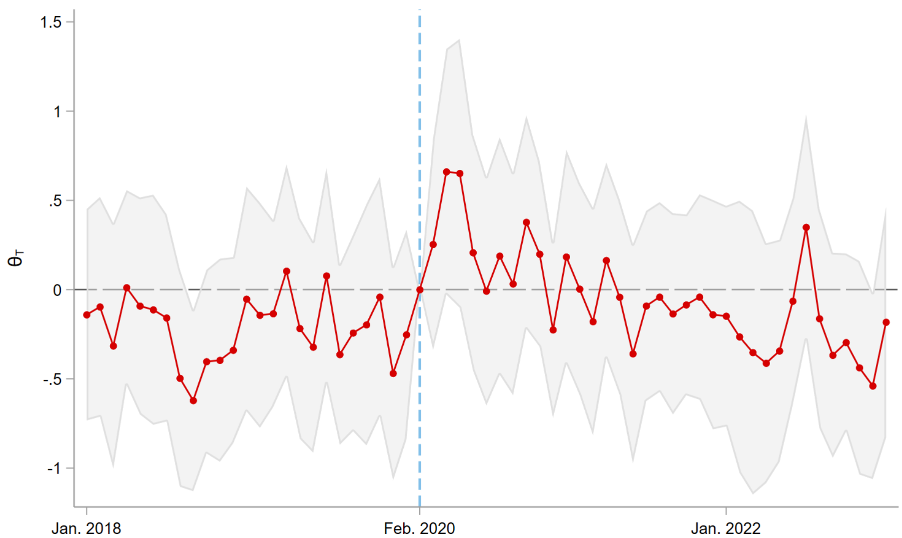
[13]https://wonder.cdc.gov/ucd-icd10.html.

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

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

[16]Results using the top 10 leading causes of death excluding accidents and intentional self-harm deaths,

Figure 9: Parametric Event Study with Placebo Exposure



Notes: *Placebo parametric event study coefficients θ_T and the 95% confidence interval when using prime-age LFP as dependent. 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 15 underlying causes of death. The death rate has been normalized to have a standard deviation equal to 1.*

4.2 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 10. The estimates are similar for men and women but show stronger effects among older workers (ages 45–54) compared to younger ones (ages 25–44). Specifically, a one-standard-deviation increase in pre-COVID 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 B5 in Appendix B.4.[17]

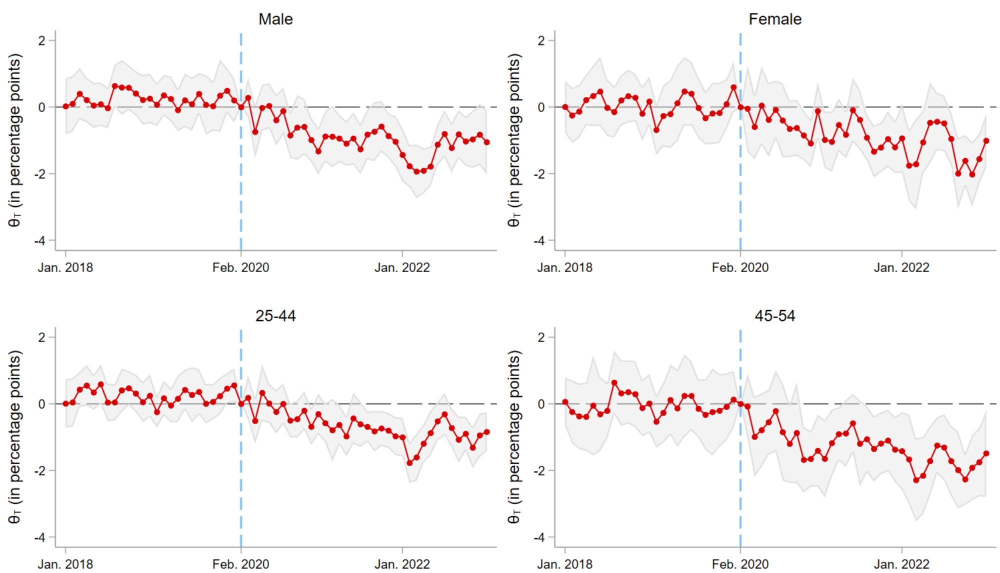
and each of the 13 underlying causes as the initial exposure measure, are shown in Figures B3 and B4 in Appendix B.3.

[17]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

Effects by educational attainment are illustrated in Figure 11 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).

Figure 10: Parametric Event Study with Continuous Exposure

Labor-Force Participation by Gender and Age



Notes: *Parametric event study coefficients θ_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 top panel splits the sample by gender, showing prime-age 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.*

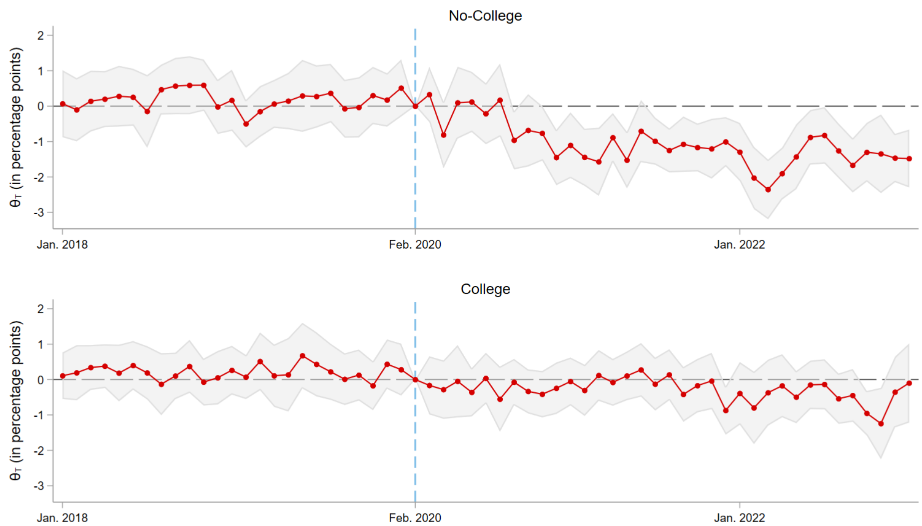
5 Robustness

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

findings for different age groups.

Figure 11: Parametric Event Study with Continuous Exposure

Labor-Force Participation by Education



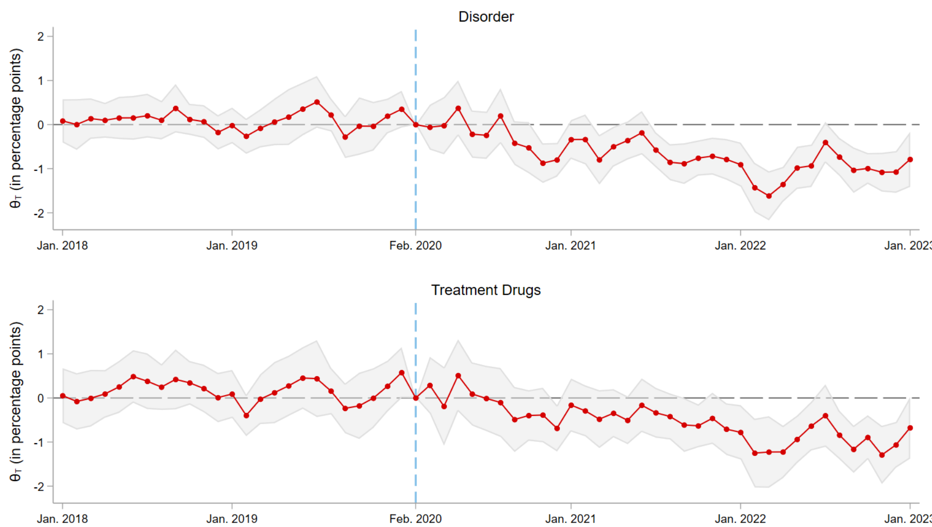
Notes: *Parametric event study coefficients* θ_T *and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior opioid exposure. The upper panel uses prime-age LFP focusing on non-college-educated, while the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

specification, i.e., the parametric event study given in equation (2). Second, a synthetic control method is applied as an alternative empirical approach.

Alternative Exposure Measures: The analysis uses age-adjusted opioid death rates from 2017 as a measure of opioid exposure. First, the single-year measure is replaced with the average age-adjusted death rates from 2010 to 2017. Second, given the slightly right-skewed distribution of 2017 death rates across states, estimates are provided using the logarithm of death rates as the exposure measure. Figure B6 in Appendix B.5 presents the results using these variants of pre-COVID opioid exposure within the parametric event study specification with continuous exposure. The post-COVID gap in labor-force participation closely aligns with the estimates in Figure 8.

Next, results using two alternative measures of opioid exposure are presented. The first is the percentage of individuals aged 12 and above with an opioid use disorder, as reported in the NSDUH. The second is the per-capita shipment of medications for opioid addiction treatment, sourced from ARCOS. Figure 12 shows estimates from a parametric event study with the continuous exposure measure. Despite a smaller magnitude, the results closely mirror those in Figure 8. States with either a one-standard-deviation higher share of individuals with an opioid use disorder or a one-standard-deviation higher per-capita supply of opioid treatment medications experienced about a 0.6 percentage point larger negative deviation from the trend in labor-force participation.

Figure 12: Parametric Event Study - Alternative Measures of Opioid Exposure



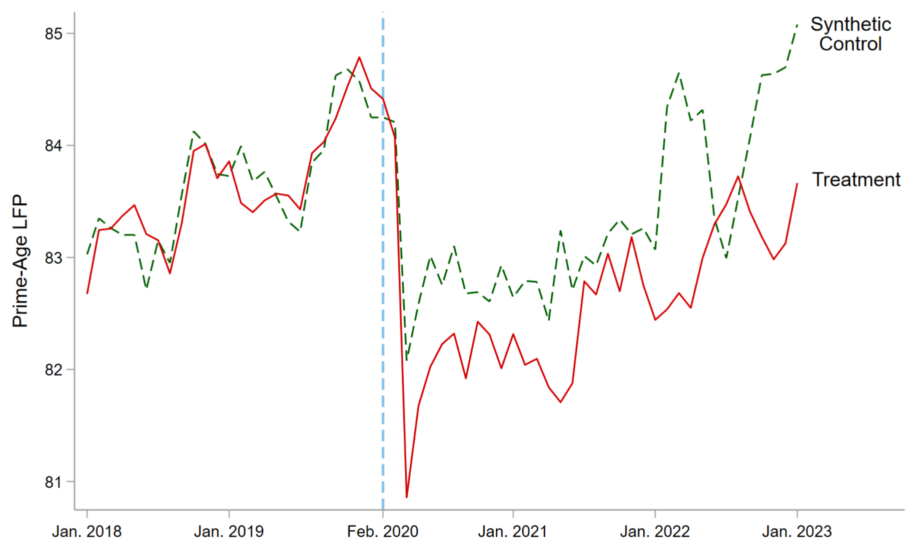
Notes: *Parametric event study coefficients θ_T and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior opioid exposure. The top panel uses as prior opioid exposure the percentage of people with a substance use disorder in 2017-2018. The bottom panel uses the amount of MME per capita of drugs to treat opioid use disorder distributed to the state in 2017. The initial opioid exposure measure has been normalized to have a standard deviation equal to 1.*

Alternative Empirical Approach: The parametric event study approach assumes that states with different initial opioid exposure experienced similar deviations from their state-specific linear trends. As an alternative empirical strategy, results using a *synthetic control* method, following Abadie, and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010), are presented.

In this method, states are classified as treated or untreated based on whether their pre-COVID opioid exposure is above or below the median. Weights are assigned to states in the untreated group to construct a synthetic control group that closely matches the average pre-COVID labor-force participation of the treated group. These weights are time-invariant and are determined by minimizing the difference in pre-COVID LFP between the synthetic control and treated groups. By design, the synthetic control group mimics the pre-COVID trend in LFP of the treated group. The treatment effect is then estimated by comparing post-COVID LFP between the treated group and the synthetic control group, calculated as the difference in average LFP during the post-COVID period.

Figure 13 illustrates the average prime-age labor-force participation in treated states and the synthetic control group. In the post-COVID period, LFP diverged between the two groups, with treated states—those with higher opioid exposure—exhibiting lower LFP. The average treatment effect in the post-COVID period is -0.7 percentage point and is statistically significant at the 5 percent level.[18]

Figure 13: Synthetic Control and Treatment Group - LFPR



Notes: *Labor-force participation in treatment and synthetic control groups. The treatment group includes states with an initial opioid exposure above the median. The LFP of the synthetic control is obtained as a weighted average of the LFP of states with an initial opioid exposure below the median.*

6 Mechanisms

The COVID-19 pandemic coincided with an unprecedented rise in opioid overdose deaths, driven by multiple factors. Pandemic-induced isolation worsened mental health (Panchal

[18]Significance is based on bootstrapped standard errors for the average treatment effect over the period, with 5000 repetitions.

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 opioid use (Mulligan, 2022).

Although opioid-related 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 differential 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 opioid use, proxied by opioid deaths in states with higher initial opioid exposure, may have contributed to a slower labor-force participation recovery in the post-COVID period, as opioid users typically have lower participation rates and poorer health. Table 3 highlights these disparities. In 2019, before COVID-19, 15 percent of individuals aged 24–49 who did not use opioids were out of the labor force, compared to 20 percent of those using opioids by prescription and 26 percent of those with opioid-use disorders.[19]

The disparity is even greater among those reporting nonparticipation due to disability, with individuals with opioid-use disorders nearly three times as likely to report a disability. Opioid misuse also impacts job attendance; those with opioid-use disorders missed an average of 3.5 workdays per month, compared to less than one day among nonusers. Self-reported health outcomes were similarly poorer: 27 percent of individuals with opioid-use disorders reported fair or poor health, compared to 8 percent among nonusers and 17 percent among prescription users. Finally, comparing 2019 data with the 2021–2022 period, Figure 14 shows that the LFP and disability gaps between nonusers and those with opioid-use disorders widened after COVID-19.[20]

The importance of disability as a potential link between opioids and labor-force participation is analyzed by focusing on the post-COVID trajectory of the share of prime-age individuals who are not working due to disability. This 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

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

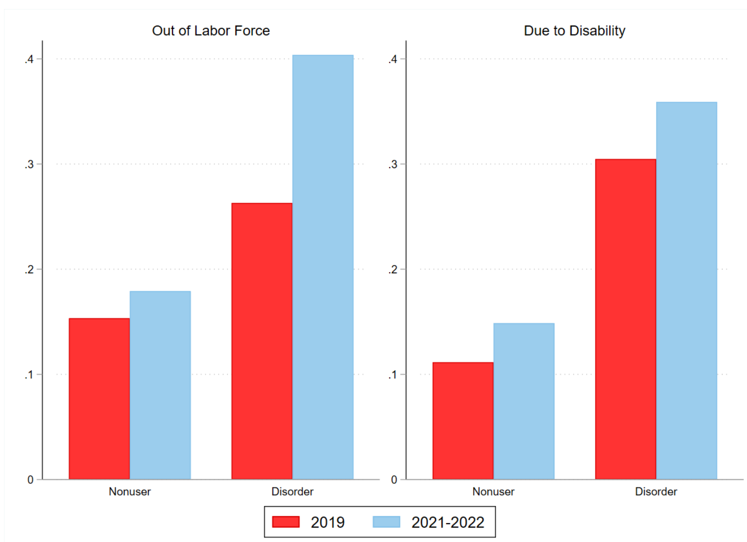
[20]Tables C1 and C2 in Appendix C.1 provide detailed employment and health outcomes by opioid use in 2019 and 2021.

Table 3: Employment and Health by Opioid Use, Ages 24-49

Panel A: Employment	Out of LF	Disability	Total Skipped Days
Nonuser	15%	11%	0.9
Prescription User	20%	32%	2.0
Disorder	26%	31%	3.5
Panel B: Self-Reported Health	Very Good	Good	Fair/Poor
Nonuser	65%	27%	8%
Prescription User	50%	33%	17%
Disorder	36%	37%	27%

Notes: *Employment-related variables in Panel A, and self-reported health status in Panel B, by type of opioid use in 2019. In Panel A, the first column shows the share of individuals who are out of the labor force. The second column shows the share of individuals out of the labor force who report disability as being the reason to not participate. The third column shows the average number of work days missed in the past 30 days due to sickness 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. The type of opioid use is defined by the NSDUH, as explained in the text. Source: NSDUH.*

Figure 14: Changes in Labor-Force Participation by Opioid Use, Ages 24-49



Notes: *Out of labor force variables by type of opioid 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 being the reason to not participate. The type of opioid use is defined by the NSDUH, as explained in the text. Source: NSDUH.*

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." 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 from 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).

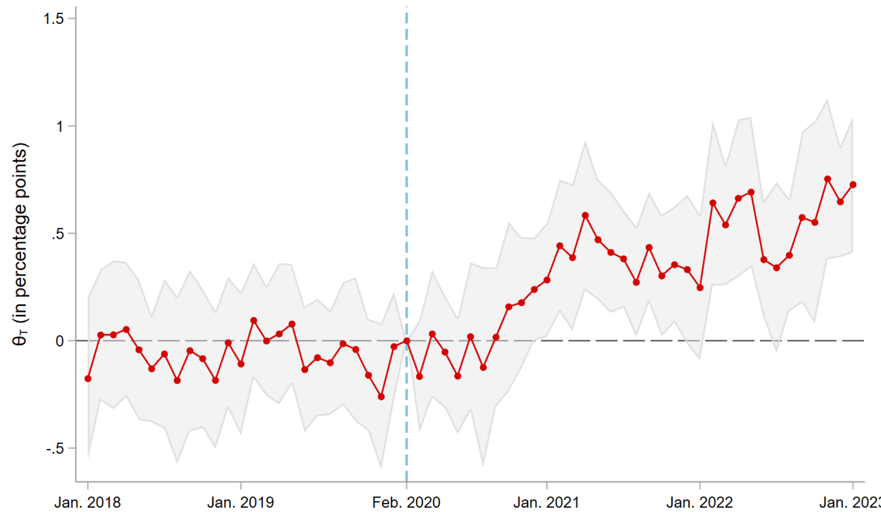
To analyze the post-COVID trajectory across states of the share of prime-age individuals who are not working due to disability, a parametric event study using a continuous measure of initial opioid exposure, as specified in equation (2), is estimated with the out-of-labor-force due to disability rate as the dependent variable. The estimated coefficients are presented in Figure 15. 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.[21]

7 Conclusions

The COVID-19 pandemic significantly disrupted labor-force participation across the United States, with prolonged declines among prime-age individuals, particularly those without a college degree. This study demonstrates that states with higher pre-pandemic opioid exposure experienced slower labor-force recovery, underscoring how public health crises can compound labor market challenges. The findings suggest that preexisting patterns of opioid misuse intensified the labor market disruptions caused by COVID-19, likely due to increased substance abuse driven by pandemic-related factors. The interaction between the COVID-19 and opioid crises highlights that the labor force impacts of future economic shocks may similarly be shaped by preexisting public health vulnerabilities.

[21] 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.

Figure 15: Parametric Event Study Estimates - Out of LF due to Disability



Notes: *Parametric event study coefficients θ_T and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age out-of-labor-force due to disability rate between states with different levels of prior placebo 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.*

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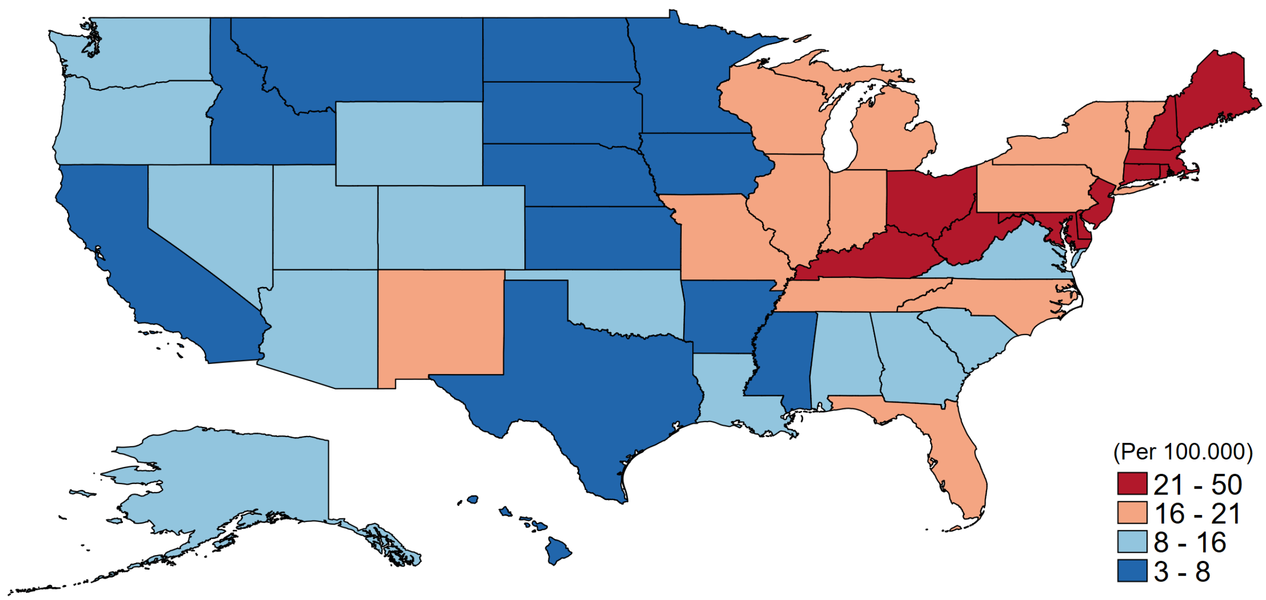
Appendix

A Data

A.1 Opioid Deaths Across the United States

Figure A1 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 A1: 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 Trends and Prediction

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 for each state a trend in prime-age LFP for the period January 2010 to February 2020. Second, we use the estimated coefficients to forecast the prime-age LFP for the period March 2020 to December 2022. Lastly, we determine the difference between the observed values and the trend.

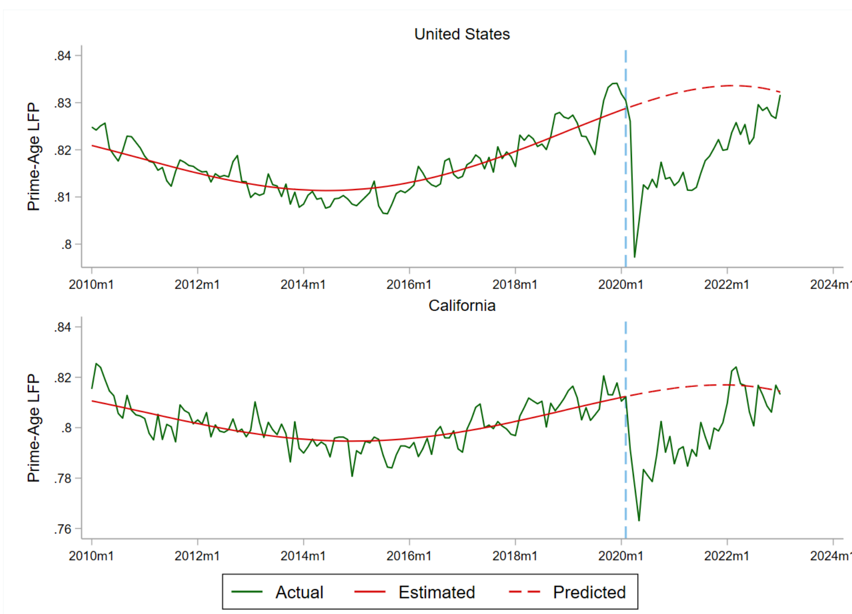
To predict the trend we first HP filter the time series for each state for the period January 2010 - February 2020 (default smoothing parameter, 1600 × 34). Then, we run on the obtained trend the following state-level regression:

$$LFP_{i,t} = \alpha + \beta_1 t + \beta_2 (t-\tau)^2 + \beta_3 (t-\tau)^3 + \beta_4 (t-\tau)^4 + \gamma_t,$$

where $LFP_{i,t}$ is prime-age labor-force participation for state i at date t (monthly). Finally, τ represents the end of the estimation period (February 2020) and γ_t are date fixed effects.

Using the estimated coefficient, we predict for each state the prime-age LFP for the period March 2020 to December 2022, $\widehat{LFP_{i,t}}$. Figure A2 shows the actual (green line), the estimated (solid red line), and the predicted (dashed red line) prime-age LFP for the US and California, as examples.

Figure A2: Estimated and Predicted LFP for the US and California



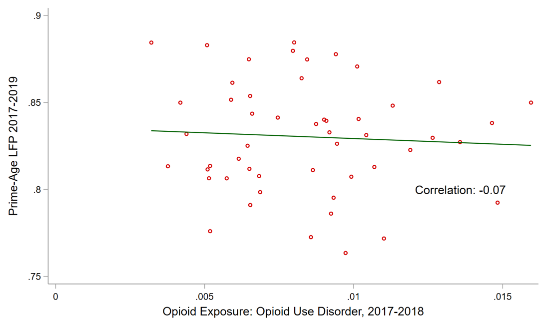
Notes: *Actual prime-age LFP, estimated trend (solid red line), and the predicted trend for the post-COVID period (dashed red line). The figure shows the results for the entire United States and the state of California.*

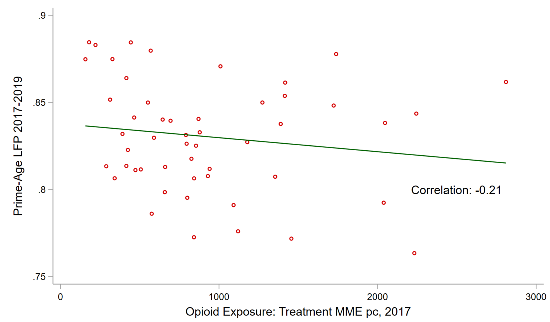
Then we obtain the difference as $Diff_{i,t} = LFP_{i,t} - \widehat{LFP_{i,t}}$. Negative values imply a lower labor-force participation than what has been predicted. To obtain the measure presented on the vertical axis of Figure 3, we average the difference for the period of December 2022.

A.3 Labor-Force Participation and Opioids

Figure A3 shows the correlation between the average prime-age LFP between 2017 and 2019, and alternative measures of opioid exposure: the percentage of individuals with opioid use disorder in 2017-2018 on the left, and the MME per capita of shipment of medications used to treat opioid addiction on the right. In both cases, the correlations are not statistically significant.

Figure A3: Relationship Between Prime-Age LFP in 2017-2019 and Alternative Opioid Exposure Measures





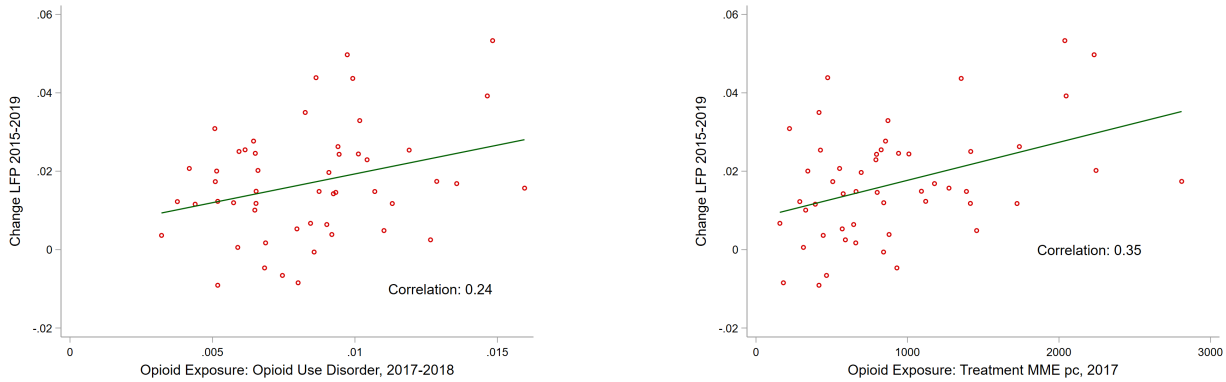
Notes: *Correlation between the average prime-age LFP between 2017 and 2019, and alternative measures of opioid exposure. Left panel: percentage of individuals with opioid use disorder in 2017-2018. Right panel: the MME per capita of shipment of medications used to treat opioid addiction. Source: CPS, NSDUH, ARCOS.*

Figure A4 shows the correlation between the average prime-age LFP between 2015 and 2019, and alternative measures of opioid exposure: the percentage of individuals with opioid use disorder in 2017-2018 on the left, and the MME per capita of shipment of medications used to treat opioid addiction on the right. In both cases, the correlations are not statistically significant.

A.4 Stringency and Economic Support Index

Figure A5 shows the average and standard deviation of the Stringency and Economic Support index developed by the Oxford Tracker. The Oxford Tracker Dataset categorizes

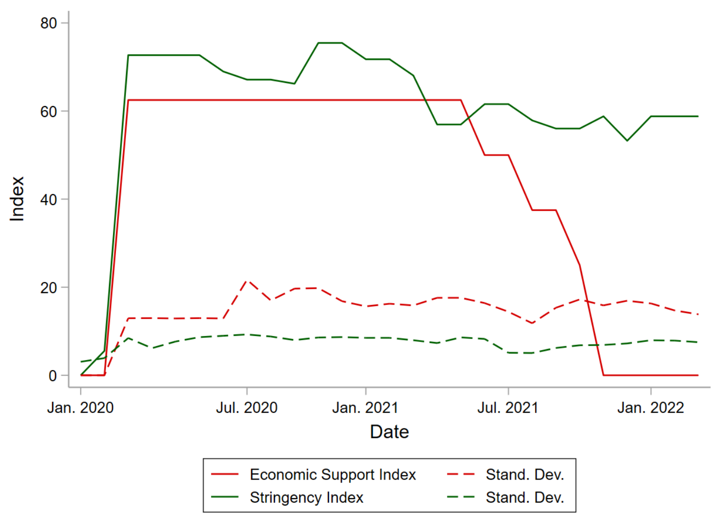
Figure A4: Relationship Between Prime-Age LFP Growth 2017-2019 and Alternative Opioid Exposure Measures



Notes: *Correlation between the prime-age LFP growth between 2015 and 2019, and alternative measures of opioid exposure. Left panel: percentage of individuals with opioid use disorder in 2017-2018. Right panel: the MME per capita of shipment of medications used to treat opioid addiction. Source: CPS, NSDUH, ARCOS.*

various COVID-19 policies into indexes representing different policy strengths, normalized between 0 and 100.

Figure A5: Stringency and Economic Support Index



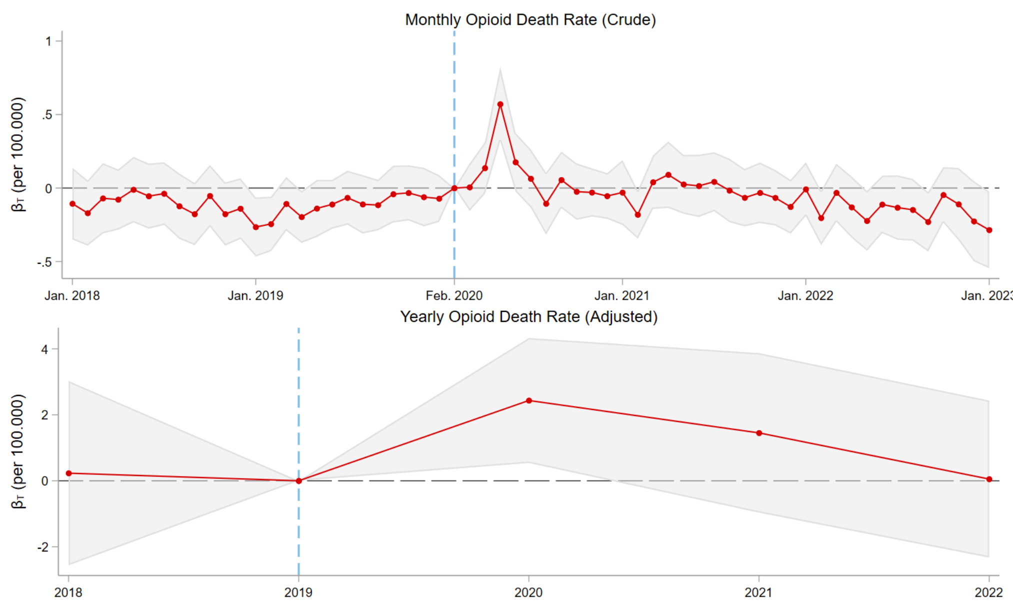
Notes: *Summary policy indexes for Stringency and Economic Support reported by the Oxford Tracker for the entire United States and the standard deviation of these policy indexes across states. Source: Oxford Tracker.*

B Placebo, Heterogeneity and Robustness

B.1 Opioid Exposure and Opioid Deaths

Figure B1 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. 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 above the median and 0 if below.

Figure B1: Non-Parametric Binary Event Study - Opioid Deaths

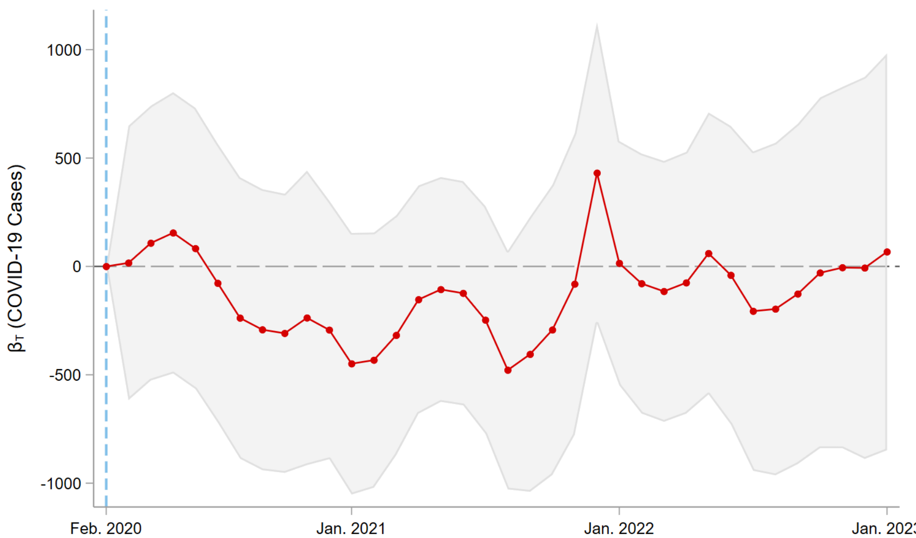


Notes: *Non-parametric event study coefficients β_T and the 95% confidence interval. Coefficients represent changes in opioid overdose deaths relative to February 2020 between states with different levels of prior opioid exposure. The top panel uses the monthly crude rate, while the bottom panel uses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

B.2 Opioid Exposure and COVID-19 Cases

Figure B2 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 B2: Non-Parametric Event Study - COVID-19 Cases



Notes: *Non-parametric event-study coefficients β_T and the 95% confidence interval. Coefficients represent changes in total COVID-19 cases 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.*

B.3 Placebo Analysis

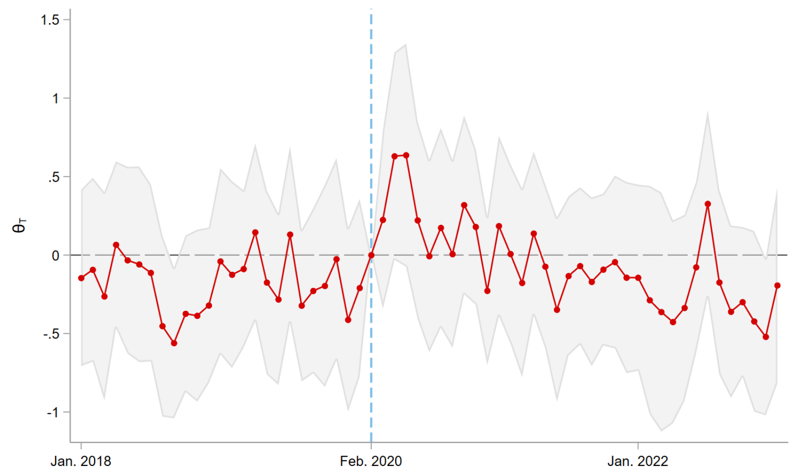
Table B1 shows the age-adjusted death rate in 2017 across the 15 leading causes of death in the US. The initial exposure measure used for the placebo analysis in the main text is constructed by aggregating the state-level death rate from these 15 causes, excluding the third (accidents) and eighth (intentional 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 B3 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 B4 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

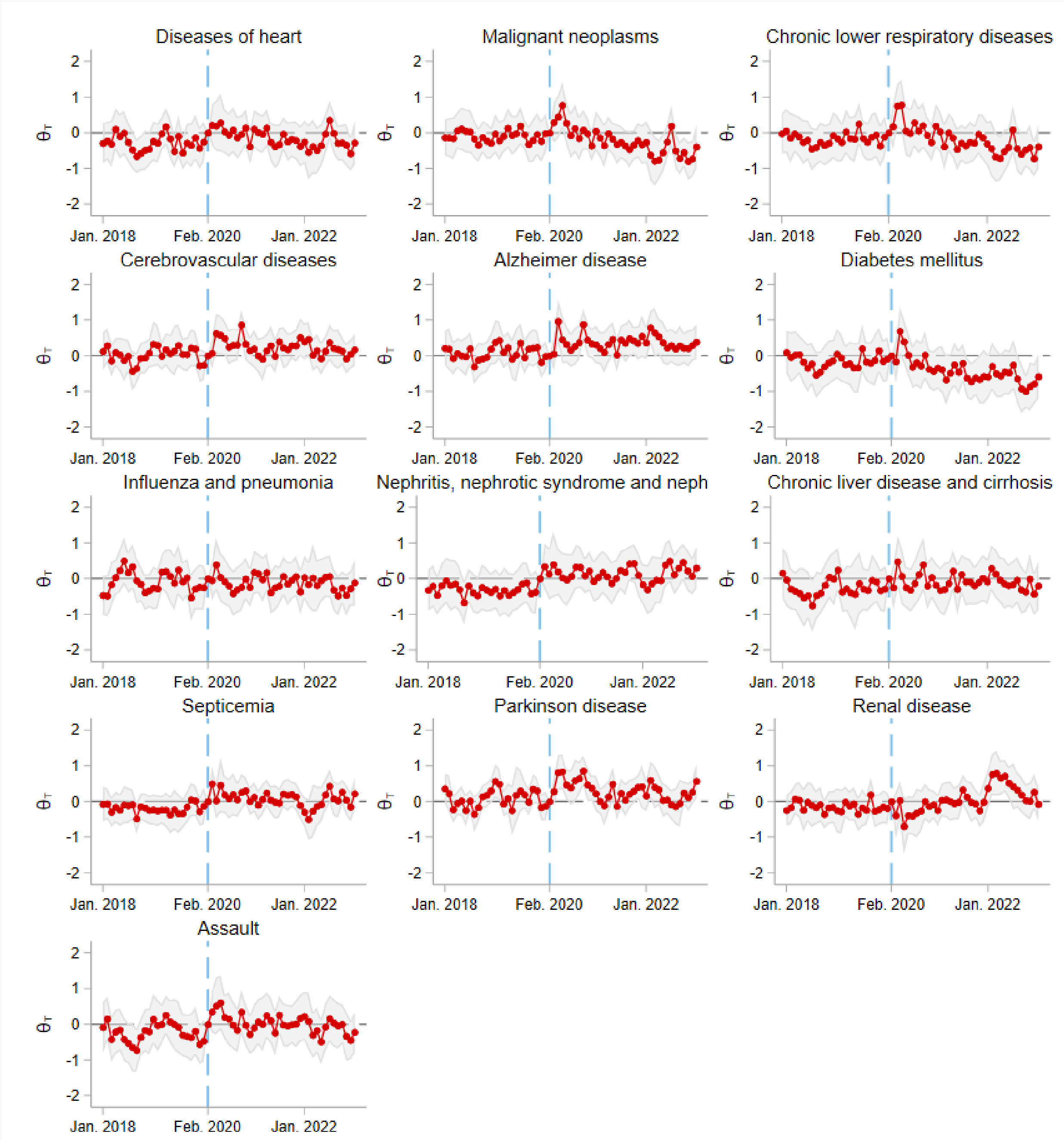
Notes: *The table shows the age-adjusted death rate in 2017 by the leading causes of death in the US. Source: CDC - Underlying Cause of Death.*

Figure B3: Parametric Event Study with Placebo Exposure



Notes: *Placebo parametric event study coefficients θ_T and the 95% confidence interval when using prime-age LFP as dependent. 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 B4: Parametric Event Study Estimates - Placebo

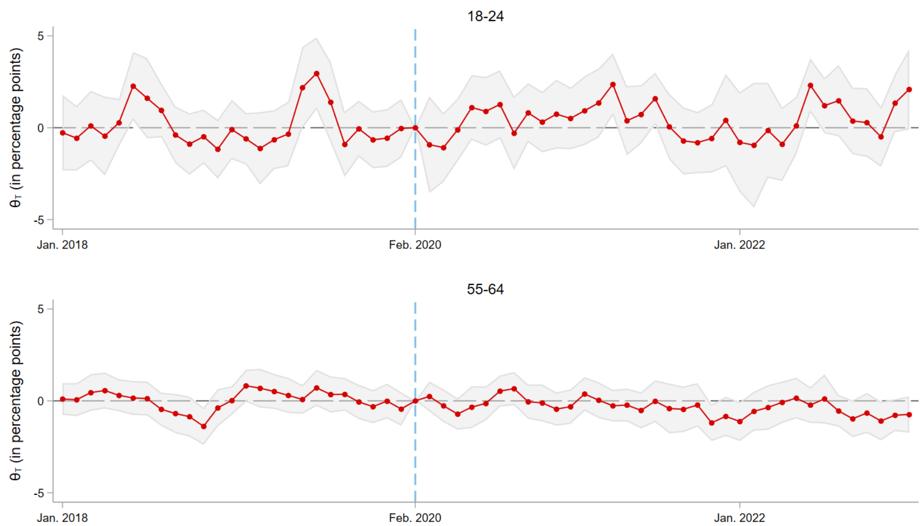


Notes: *Placebo parametric event study coefficients* θ_T *and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior placebo exposure. The measure of prior placebo exposure is the age-adjusted death rate 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 B5 shows the estimated coefficients from the parametric event study analysis when focusing on the age groups 18-24 and 55-64.

Figure B5: Parametric Event Study Estimates - Labor Force Across Samples

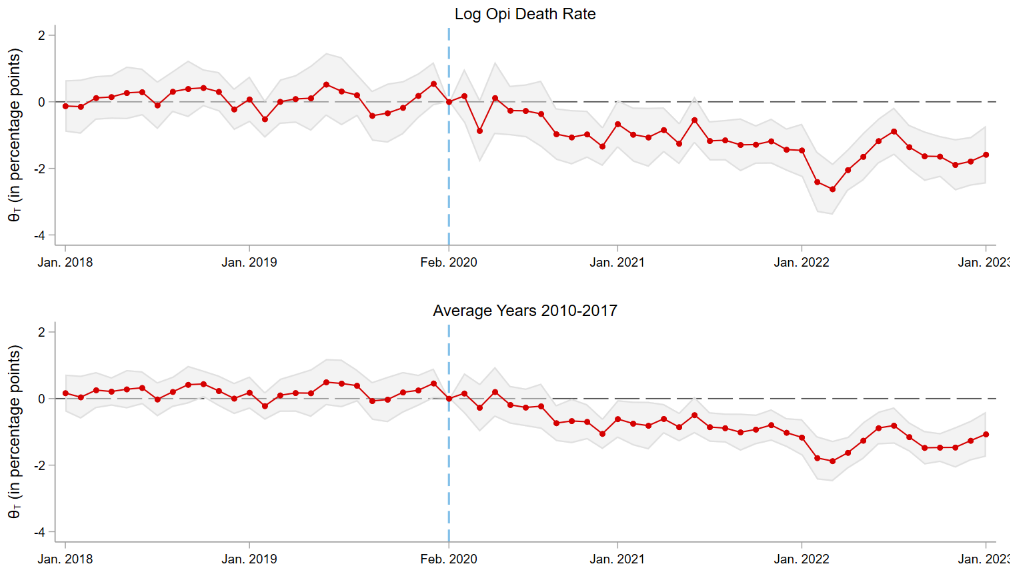


Notes: *Parametric event study coefficients θ_T and the 95% confidence interval. Coefficients represent average differences from state-trend in prime-age LFP between states with different levels of prior opioid exposure. The upper panel restricts the sample to individuals aged 18-24, and the lower panel 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 B6 shows the estimated coefficients from the parametric event study analysis when using two alternative measures of opioid exposure are presented: the logarithm of the age-adjusted opioid death rate in 2017 and the average age-adjusted opioid death rate between 2010 and 2017.

Figure B6: Parametric Event Study Estimates - Measures of Opioid Death Rate



Notes: *Parametric event study coefficients θ_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. Prior opioid exposure in the top panel is given by the log of the opioid-related overdose age-adjusted death rate. The bottom panelshows the average opioid-related overdose age-adjusted death rate between 2010 and 2017, normalized to have a standard deviation equal to 1.*

C Data on Mechanisms

C.1 Employment and Health Statistics Across Opioid Users

Tables C1 and C2 provide employment, health, and work disability information among adults in the age group 24-49 by opioid use in 2019 and 2021 using the NSDUH.

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

Panel A		**2019**			**2021**	
	Emp.	Unemp.	Out of LF	Emp.	Unemp.	Out of LF
Nonuser	80.6%	4.1%	15.3%	75.3%	5.9%	18.8%
Disorder	61.1%	12.6%	26.3%	39.5%	21.0%	39.6%
Population	79.0%	4.4%	16.6%	73.6%	6.3%	20.1%
Panel B		**2019**			**2021**	
	Very Good	Good	Fair/Poor	Very Good	Good	Fair/Poor
Nonuser	65.1%	27.2%	7.7%	60.9%	30.0%	9.1%
Disorder	36.0%	36.6%	27.4%	30.3%	39.0%	30.7%
Population	60.5%	28.9%	10.6%	57.1%	31.0%	11.9%

Notes: *The table shows the employment status in Panel A, and self-reported health status in Panel B, by type of opioid use in 2019 and 2021. 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 opioid use is defined by the NSDUH. See the text for further details. Nonusers do not use any opioids. For each opioid user type and year, the row sums up to 100. Source: NSDUH.*

Table C2: Opioids and Work Disability, Ages 24-49

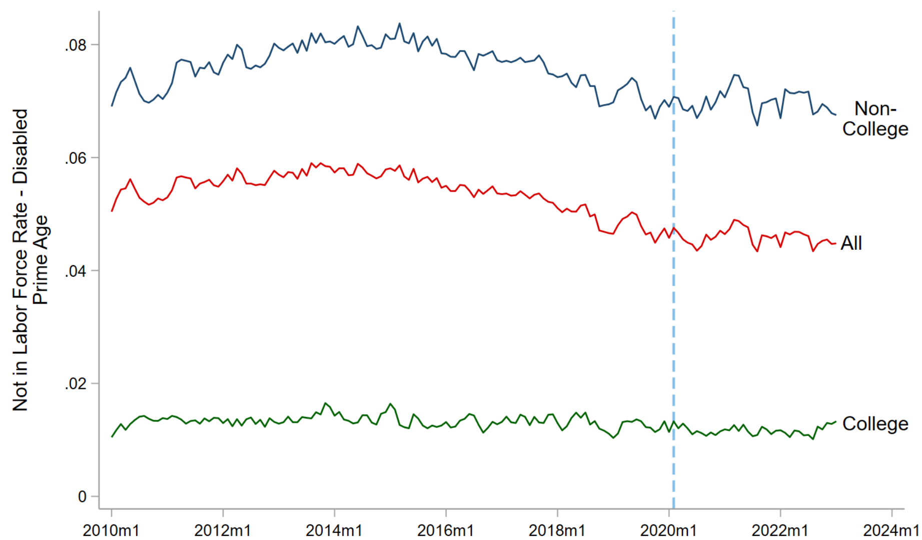
	NLF - Disability		Work Days Sick		Work Days Skip	
	2019	**2021**	**2019**	**2021**	**2019**	**2021**
Nonuser	11.1%	14.8%	0.63	0.77	0.27	0.33
Disorder	30.5%	42.8%	1.75	2.16	1.92	1.19
Population	18.2%	19.5%	0.88	0.98	0.34	0.37

Notes: *The table shows several measures of absence from work by type of opioid use in 2019 and 2021. 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 opioid use is defined by the NSDUH. See the text for further details. Source: NSDUH*

C.2 Not in Labor Force due to Disability Rate

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

Figure C1: Not in Labor Force for Disability Rate (NLDR)

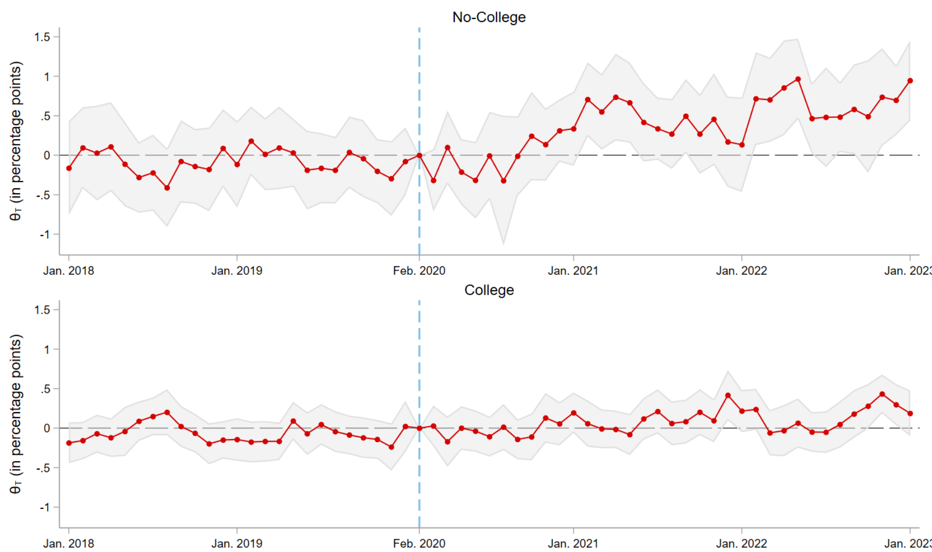


Notes: *Share of prime-age individuals not in the labor force for disability reasons. This measure includes individuals who report being unable to work and not working due to disability. Source: CPS*

C.3 Not in Labor Force due to Disability Rate Across Education

Figure C2 shows the estimated coefficients from the parametric event study analysis when using the Not in Labor Force due to Disability Rate across prime-age individuals across education groups.

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



Notes: *Parametric event-study coefficients θ_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.*