The effect of ACA Medicaid expansion on drug overdose mortality rates

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Abstract

Although the Affordable Care Act's expansion of Medicaid reduced the cost of opioid addiction treatment, it also made opioid prescriptions more accessible, potentially leading to higher rates of opioid addiction and death. This study examines how the expansion affected drug overdose mortality rates. Using a difference-in-differences framework, this study finds that the expansion increased drug overdose mortality rates by 1.208 per 100,000 people at the county and quarter level, a 30.3% increase compared to the average mortality rate in the expansion counties before the expansion. Findings also suggest that the expansion fuelled the prevalence of illicitly manufactured fentanyl, a synthetic opioid, which largely accounts for the estimated effects. Moreover, the effects on mortality were lower in expansion counties with greater increases in insurance or opioid prescribing rates after the expansion, and more prominent in expansion counties with higher drug overdose mortality rates before the expansion. Taken together, these findings are consistent with the argument that stringent restrictions on prescription opioids alongside the expansion of Medicaid resulted in more people turning to illicitly manufactured opioids, increasing overdose deaths.

Keywords: Medicaid expansion, opioid epidemic, drug overdose mortality, illicitly manufactured fentanyl

Introduction

The largest health insurance program in the US, Medicaid provides free or low-cost health coverage to low-income people, the disabled, children, and pregnant women. Passed in March

2010, the Patient Protection and Affordable Care Act (ACA) expanded Medicaid coverage, extending eligibility to adults below the age of 65 (i.e. non-elderly adults) with incomes up to 138% of the Federal Poverty Level (FPL).[1] To date, 38 states as well as the District of Columbia have expanded their Medicaid programs (Kaiser Family Foundation 2022). However, the resulting increase in the health insurance rate among non-elderly adults is estimated to be no more than 1% (Black et al., 2019).

Over the past two decades, drug overdose deaths, particularly those involving opioids, have risen dramatically in the US. Indeed, opioid-related cases have prompted references to the so-called 'opioid epidemic'.[2] In principle, the expansion could either exacerbate or alleviate the opioid epidemic. On the one hand, extended coverage may help newly insured beneficiaries obtain prescription opioids, potentially resulting in addiction, drug dependence, and overdose.[3] Moreover, facilitating access to prescription opioids may lead to the nonmedical use of opioids and opioid-related deaths among individuals without prescriptions.[4] Individuals who become addicts after being prescribed drugs may be ineligible to obtain the prescriptions necessary to meet their demands, prompting them to resort to illicit drugs. This increases the risk of overdose because users cannot easily assess the safety and quality of drugs available on the underground markets (Goodman-Bacon and Sandoe 2017; Miron, Sollenberger, and Nicolae 2019).[5] As

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[1] The expansion was intended to be implemented nationwide. However, a 2012 Supreme Court decision ruled that states could decide whether to adopt the expansion policy.

[2] The unintentional opioid overdose mortality rate in the US was about 2 per 100,000 people in 1999; by 2017, this figure had increased to approximately 13 per 100,000.

[3] However, multiple studies found no increase in opioid prescriptions with the expansion of Medicaid (Saloner et al. 2018; Sharp et al. 2018; Cher, Morden, and Meara 2019).

[4] For example, using combined 2013 and 2014 data from the National Survey on Drug Use and Health (NSDUH), Lipari and Hughes (2017) found that about half of respondents who misused prescription opioid pain relievers reported obtaining them from a friend or relative. Powell, Pacula, and Taylor (2020) estimated a 10% growth in opioid supply from Medicare Part D, and a 7.1% increase in opioid-related deaths in the Medicare-ineligible population.

[5] For example, illicit opioids obtained from underground markets do not have warning labels, resulting in users being more likely to combine opioids with alcohol or other drugs, increasing the risk of respiratory depression (Miron, Sollenberger, and Nicolae 2019). Additionally, illicit opioids are produced without adherence to appropriate manufacturing measures, producing considerable variation in their potency and unpredictable results (Abouk et al. 2021).

such, policies intended to curb opioid addiction by restricting access to prescription opioids could inadvertently drive users to switch from prescription to illicit drugs, increasing drug overdose deaths. Restrictions on prescription drugs may also lead to the undertreatment of pain, reducing individual's quality of life and increasing the risk and instance of suicide (Kertesz et al., 2018).[6]

On the other hand, the ACA includes substance use disorder (SUD) services as an essential health benefit, requiring all Medicaid health insurance to cover SUD services.[7] Medicaid is the only insurance option for many SUD patients to obtain affordable treatments (Goodman-Bacon & Sandoe, 2017).[8] Health conditions requiring pain relief and drug abuse treatment are more common among Medicaid recipients than non-recipients, especially those with a disability or chronic disease. Accordingly, the expansion could reduce drug overdose deaths by increasing access to treatment.[9] It may also help curb drug overdose by increasing accessibility to prescription opioids for those susceptible to using illicit drugs.

Given the substantial number of drug overdose deaths every year and broad adoption of Medicaid expansion in the US, this study investigates how the expansion has affected drug overdose mortality rates. The literature addressing this issue reveals mixed results (Yan et al. 2021; Abouk et al. 2021; Borgschulte and Vogler 2020; Averett, Smith, and Wang 2019;

[6] Survey data indicate that regulations have tended to discourage physicians from prescribing opioids, potentially leading to the undertreatment of pain (Gilson and Joranson 2001).
[7] See https://obamawhitehouse.archives.gov/ondcp/healthcare.
[8] About 37% of respondents to the National Survey on Drug Use and Health (2010–2013) cited a lack of health insurance as their main reason for not receiving treatment (Grooms & Ortega, 2019).
[9] Currently, standard care for treating opioid addiction is medication-assisted treatment (MAT), which involves using medications (methadone, buprenorphine, and naltrexone) as well as counselling and other support services (Abouk et al., 2021). All state Medicaid programs cover at least one of these medications. Research shows that MATs are effective in reducing illicit drug use, opioid dependence, and drug and opioid-related deaths. Studies have found evidence that Medicaid expansion has improved access to SUD treatment (Maclean and Saloner 2019; Andrews et al. 2019; Cher, Morden, and Meara 2019; Clemans-Cope et al. 2019; Sharp et al. 2018; Meinhofer and Witman 2018; Saloner et al. 2018; Wen et al. 2017).

Maclean and Saloner 2019). However, many of these studies may suffer various identification issues, including a lack of statistical power.

This study contributes to the literature in two ways. First, using a difference-in-differences (DiD) framework and more granular data at the county-quarter level, this study shows that the expansion of Medicaid significantly increased drug overdose mortality rates. Granular data could account for confounding variations in mortality, which are uncontrolled for when using a broader unit. The event study plot shows the differential trend between the expansion and non-expansion states was flat in the pre-expansion period and started to rise one year after the expansion. Second, this study conducts mediation analyses to identify potential channels by which the effects emerge. In doing so, this study shows that the expansion increased insurance rates below 138% FPL, which, in turn, helped reduce the mortality rate. Moreover, there is evidence to suggest that the expansion of Medicaid exacerbated the rise in the prevalence of illicitly manufactured fentanyl, thereby increasing the mortality rate. Meanwhile, despite the expectation that the expansion would increase the demand for prescription opioids, as of 2018, states that adopted the expansion did not see a rise in opioid prescribing rates compared to those that did not adopt the expansion. Based on these findings, this study hypothesizes that restrictions on opioid prescriptions alongside the expansion of Medicaid have resulted in more people with unmet demand for prescription opioids resorting to illicit substitutes, which are more dangerous than legal versions, thus leading to more drug overdose deaths. Therefore, the results of this study are consistent with the argument of 'more restrictions, more deaths' rather than 'more prescriptions, more deaths' (Miron et al., 2019).

This study uses a stylized model based on the supply and demand of prescription drugs to illustrate that in the presence of restrictions, the expansion of insurance eligibility may drive more people to use illicit drugs. In light of this model, this study examines heterogeneous effects among counties. In doing so, this study found that the effects were less pronounced in expansion counties with higher increases in insurance or opioid prescribing rates and more pronounced in expansion counties with more severe drug overdose problems. These findings are consistent with the implications of the stylized model.

The remainder of this paper is organized as follows. The next section reviews the relevant literature, while the third section illustrates the stylized model. The fourth section describes the data used in this study, and the fifth presents the empirical methods, and the sixth section discusses the results. The final section concludes this study by considering the policy implications of its findings.

Literature review

An emerging body of literature examining the impact of Medicaid expansion under the ACA on drug-related mortality has found ambiguous or positive effects. Several studies show that drug-related deaths grew more rapidly in expansion states than in non-expansion states compared to pre-expansion figures (Goodman-Bacon & Sandoe, 2017; Yan et al., 2021) or among states east of the Mississippi river (Abouk et al., 2021). These scholars argue that evidence from difference-in-differences (DiD) models is not credible because of the pre-existing differential trend in drug-related deaths between expansion and non-expansion states, which violates the parallel trend assumption of the DiD model. However, this study's analysis reveals that the pre-trend remained positive but became statistically insignificant after controlling for confounding covariates.

More specifically, two studies found imprecise estimates (Averett et al., 2019; Borgschulte & Vogler, 2020). In the first, Averett et al. (2019) used a DiD approach with state-year level data and found a positive and insignificant effect of the expansion on opioid deaths.

However, state-year level data may be underpowered to detect a reasonable effect.[10] In the second study, Borgschulte and Vogler (2020) first used the double lasso method described in Belloni et al. (2014) and Urminsky et al. (2016) to select the variables to be included in a propensity score model that matches counties in expansion and non-expansion states. They then used propensity-score weighting DiD models with controls selected by the double lasso procedure and found no significant overall effects of the expansion on opioid overdose mortality rates. Borgschulte and Vogler (2020) also found sizeable and significant effects, namely, a 35.63% increase, among people aged 20 to 24, but no significant effects among other age groups. However, they did not account for other state policies implemented concurrently with the expansion that could affect drug overdose deaths. As such, while propensity-score matching attempts to address differences in observables between expansion and non-expansion counties, it may introduce bias to the estimate (Daw & Hatfield, 2018; King & Nielsen, 2019).

Meanwhile, two studies found positive and significant effects. Using a DiD approach, Yan et al. (2021) estimated that drug overdose mortality increased by 10.3% in expansion states compared to non-expansion states after the expansion. The authors attributed this result to the opioid epidemic, arguing that it mitigated the life-saving impact of the expansion. However, Yan et al. (2021) did not investigate potential mechanisms for the rise in mortality and their relationship with the expansion. Like Borgschulte and Vogler (2020), Yan et al. (2021) did not control for other drug-related policies. In the second study, Abouk et al. (2021) used DiD models to examine the association between the expansion and drug-related mortality separately in the east and the west of the Mississippi River.[11] In doing so, they found a statistically

[10] Using variables at the state-quarter level, this study conducts a power analysis à la Black et al. (2019), in which the minimum detectable effect (MDE) was defined as the minimum effect detectable at the 95% confidence level for a two-tailed test 80% of the time. In doing so, this study found an insignificant estimate below the MDE, indicating a lack of power to detect a significant effect. The result is available upon request.

[11] The separation attempted to account for the coinciding rise in the supply of illicitly manufactured fentanyl. According to the Abouk et al. (2021), black and brown powder heroin was primarily sold west of the Mississippi River, whereas white powder heroin was primarily sold east of the Mississippi River.

significant association between the expansion and the increase in drug-related mortality in the east, which they attributed to synthetic opioids other than methadone and heroin, and found no association in the west. Abouk et al. (2021) also argued that the estimate in the east was invalid due to the pre-trend, whereas there seemed to be no pre-trend in the west. After controlling for state-specific linear time trends to address the pre-trend in the east, the estimate for the east became insignificant. However, Meer and West (2016) illustrated that if the treatment affects the outcome gradually over time rather than immediately impacting the outcome in a discrete manner, controlling for state-specific linear time trends will mechanically attenuate the estimated treatment effect toward zero. Moreover, a power analysis à la Black et al. (2019) suggested that data restricted to the west, where the DiD design was valid, were underpowered to detect a reasonable effect.

As such, the literature mainly suffers various identification issues, including omitted variable bias (e.g. not accounting for other relevant state policies), the pre-trend, and lack of power. To address these issues, this study uses finer data at the county-quarter level and a richer set of covariates. This study also conducts mediation analyses to examine potential mechanisms for the positive and significant effects it observes.

Conceptual framework

Research indicates that the cause of overdose deaths shifts from legal to illicit drugs when access to prescription opioids is restricted (Goodman-Bacon & Sandoe, 2017). This section employs a stylized model based on supply and demand to illustrate how the expansion can exacerbate deaths from illicitly manufactured drugs in the presence of prescription opioid restrictions.

While illicitly manufactured fentanyl was white and can be easily mixed with eastern white heroin, it was difficult to mix with western black or brown heroin and thus less popular in the west.

Figure 1 illustrates the supply (S) and demand (D) of prescription opioids, where p and q denote the out-of-pocket price and quantity of prescription opioids. Suppose that the supply is restricted to be fixed at $\bar{s}$. Initially, the price and quantity demanded of prescription drugs are p_0 and q_0. As $q_0 > \bar{s}$, there is a shortage of prescription drugs of $q_0 - \bar{s}$. People with unmet demand may opt to use illicit drugs from underground markets, which are more dangerous than legally prescribed versions because the drug potency is not easily accessible, increasing the risk and incidence of overdose and death.

[Insert Figure 1 here]

After the expansion, the price decreases from p_0 to p_1 for newly insured Medicaid recipients and those who can easily obtain prescription drugs from others with Medicaid. As the demand is downward sloping, the quantity demanded increases from q_0 to q_1. As the supply is fixed, the shortage increases to $q_1 - \bar{s}$ by $q_1 - q_0$. This increase in shortage prompts more people with unmet demand to use to illicit drugs, exacerbating overdose and deaths.

Based on this model, this study proposes the following three hypotheses regarding the heterogeneous effects of Medicaid expansion across counties. More specifically, all else being equal:

1. As health insurance rates increase due to the expansion, more people receive treatment for opioid addiction, decreasing the demand for prescription opioids. This lowers the shortage increase, thereby decreasing overdose deaths. Therefore, expansion counties with higher increases in insurance rates due to the expansion are expected to experience fewer effects of the expansion in respect to increased overdose deaths.
2. As opioid prescribing rates increase, so does the supply of prescription opioids ($\bar{s}$), lowering the shortage increase. Accordingly, expansion counties with higher increases in opioid prescribing rates due to the expansion will also experience fewer effects.

3. A decrease in the price of prescription opioids results in higher shortages in counties with more severe drug problems because these counties have more elastic demand.[12] Therefore, expansion counties with more severe drug problems, proxied by higher drug overdose mortality rates before the expansion, are expected to experience greater effects of Medicaid expansion in respect to exacerbating overdose deaths.

This study empirically examines these implications by estimating heterogeneous effects among counties in a later section.

Empirical strategy

Baseline specification

This study exploits the variation in the timing of states' adoption of Medicaid expansion using a difference-in-differences (DiD) framework to identify the causal effect of the expansion on an outcome variable. This study employs the following baseline estimating equation:

$$Y_{cst} = \alpha \cdot Exp_{st} + X'_{cst}\beta + C_c + T_t + \varepsilon_{cst} \tag{1}$$

where Y_{cst} is a dependent variable (e.g. the number of drug overdose deaths per 100,000 people) in county c, state s, and quarter t (from 2010 Q1 to 2018 Q4).[13] A dummy variable for expansion status, Exp_{st}, is 1 if state s' expansion was effective in quarter t, and 0 otherwise. X_{cst} is a vector of control variables, including: (a) dummies for other state-wide drug-related policies that were implemented concurrently with the expansion and may impact the dependent variable (constructed in the same manner as the dummy for the expansion), including prescription drug monitoring program (PDMP), pain clinic law, naloxone access law, 'Good

[12] More specifically, with everything else being equal, individuals more addicted to drugs spend a larger proportion of income on drugs. Therefore, these individuals have a more elastic demand curve because the quantity of drugs they demand is more sensitive to drug price changes (Reuter, 2010). Accordingly, counties with more drug problems (i.e., with a higher number of drug addicts) have a more elastic demand curve.

[13] YYYY QX stands for quarter X of year YYYY.

Samaritan' law, medical marijuana law, and recreational marijuana law; (2) county-level and time-varying demographics, including population shares by gender, race, origin, and age groups; and (c) economic indicators, including unemployment rates, poverty rates, and median household income.[14] C_c and T_t are county and quarter fixed effects, which control for county- and year-specific fixed heterogeneity. ε_{cst} is an idiosyncratic error term.

If this model correctly captures the relationship between the expansion and the dependent variable, then the coefficient on the expansion dummy, α, represents the causal impact of the expansion on the dependent variable. Finally, in estimation, standard errors are clustered at the state level to allow for any arbitrary autocorrelation of the errors in each state.

Event study

A key assumption required for a valid DiD analysis, the parallel trends assumption holds that the dependent variable in expansion and non-expansion states would exhibit parallel trends in the absence of the expansion (after adjusting for other covariates). Otherwise, the DiD estimates would be driven by unobserved trends, making them invalid.

To examine the pre-expansion differential trend in drug overdose mortality between the expansion and non-expansion states, as well as the evolution of the treatment effect in the post-expansion period, this study performs event studies by running a leads-and-lags regression as follows:

$$Y_{cst} = \sum_{r=-16}^{19} \mathbb{1}(r(s) = r) \cdot \alpha_r + X'_{cst}\beta + C_c + T_t + \varepsilon_{cst} \qquad (2)$$

[14] Prescription Drug Monitoring Programs are state-wide electronic databases that track prescriptions of controlled substances. Pain clinic laws impose regulations on pain clinics to prevent the prescribing of controlled substances (including opioids) without medical indication. Naloxone Access Laws allow lay responders to administer naloxone, an opioid receptor antagonist. 'Good Samaritan' Laws protect people from prosecution for possessing controlled substances in the event of a drug overdose. Medical marijuana laws allow for marijuana use to treat certain medical conditions. Finally, recreational marijuana laws allow for the use of marijuana for recreational purposes.

where $\mathbb{1}(\cdot)$ is the indicator function. For expansion states, $r(s)$ is a function that returns the quarter relative to state s' expansion quarter; for non-expansion states, $r(s) = -1$. In estimation, the indicator for the quarter preceding the expansion quarter, $\mathbb{1}(r(s) = -1)$, is omitted from the model. Contingent on other variables, α_r is the difference of the dependent variable in the relative quarter r between expansion and non-expansion states relative to the difference in the quarter preceding the expansion quarter. Other variables are defined in Eq. (1).

Mediation analysis

Mediation analysis is used to estimate the role of the pathways or mechanisms by which a treatment variable (e.g. policy) affects an outcome, thus elucidating why a relationship exists between the two variables (Hicks and Tingley 2011). This study uses mediation analysis to investigate how Medicaid expansion may impact drug overdose mortality rates.

To illustrate, let T and Y denote a treatment and outcome, and let M denote a potential mechanism (i.e. mediator) that transmits the effect of T on Y. Following the steps suggested by Baron and Kenny (1986), the mediation analysis comprises three regressions as follows:

$$M = b_0 + b_1 T + \nu \tag{3}$$

$$Y = \varphi_0 + \varphi_1 T + e \tag{4}$$

$$Y = \theta_0 + \theta_1 T + \theta_2 M + u \tag{5}$$

where e, ν, and u are error terms. In Eq. (3), which relates the mediator M to the treatment T, b_1 needs to be significant for M to be a mediator; otherwise, T and M have no relationship. In Eq. (4), which relates the outcome Y to the treatment T, b_1 gauges the total effect of the treatment T on the outcome Y. In addition to Eq. (4), Eq. (5) includes the mediator M as an explanatory variable. If the magnitude of θ_1 is significantly smaller than that of φ_1, then this indicates the mediation via the mediator M because the inclusion of M explains some of the treatment effect on the outcome (VanderWeele 2016).[15]

In a later section, this study examines four sets of time-varying channels that may respond to the expansion and influence drug overdose mortality rates: namely, (1) insurance rates (below 138% FPL), (2) distributed controlled substance rates, (3) opioid prescribing rates, and (4) illicit drug seizure rates. These variables are described in the following section.

Data and variables

Data source

Table 1 lists the variables used in this study's analyses, including their unit of observation and data source.

[Insert Table 1 here]

Drug overdose mortality rates

Data regarding the number of drug overdose deaths at the county and month level between 2010 and 2013 are obtained from the Centers for Disease Control and Prevention (CDC) WONDER. Drug overdose deaths are denoted by the 10th version of the International Classification of Diseases (ICD-10) codes: X40–X44 (unintentional), X60–X64 (suicide), X85 (homicide), and Y10–Y14 (undetermined). For confidentiality purposes, CDC WONDER suppresses the number of drug overdose deaths that are less than 10. In this study, suppressed mortality data are replaced with 0; data are then aggregated to the county and quarter level. As the expansion only applies to non-elderly adults, the sample is limited to the deceased aged 20–64. In this respect, Medicare covers most elderly people aged 65 or over, while the Children's

[15] The difference between these two coefficients is often interpreted as a mediated or indirect effect (IE); i.e. $IE = \varphi_1 - \theta_1$. Meanwhile, the remaining treatment effect in Eq. (5), θ_1, is often taken as a measure of the direct effect (DE); i.e. $DE = \theta_1$ (Ibid.)

Health Insurance Program (CHIP) provides coverage to eligible individuals up to the age of 19.

Drug overdose mortality rates (per 100,000 population) are calculated by dividing the number of drug overdose deaths by the county population aged 20–64 and multiplying by 100,000. Figure 2 illustrates the trends in drug overdose mortality rates separately in expansion and non-expansion states at the county and quarter level.

[Insert Figure 2 here]

Medicaid expansion

States' ACA Medicaid expansion statutes are drawn from the Kaiser Family Foundation. Table 2 shows the number of expansion states by expansion quarter as of the end of the study period, 2018 Q4. Most expansion states implemented the expansion on January 1, 2014.

[Insert Table 2 here]

Potential mediators

Time-varying variables—namely, insurance rates, opioid prescribing rates, distributed controlled substance rates, and illicit drug seizure rates—are potential mediators by which the expansion affects drug overdose mortality rates.

In this study, insurance rates refer to countywide health insurance rates for individuals between the ages of 18 and 64 and with an income at or below 138% FPL, that is, the demographic whose health insurance coverage was most affected by the expansion. Opioid prescribing rates are the number of retail opioid prescriptions dispensed per 100 people.[16] Distributed controlled substance rates refer to state-wide retail drug distribution in terms of

[16] As opioid prescribing rates are confined to initial or refill prescriptions dispensed at retail pharmacies, they do not capture data regarding illicitly manufactured opioids (Shakya & Harris, 2022).

grams per 100,000 people, obtained from Automation of Reports and Consolidated Orders System (ARCOS) Report 3 (Quarterly Distribution in Grams per 100K Population).[17] Finally, illicit drug seizure rates are defined as the number of drug cases seized by law enforcement operations per 100,000 people, and are used to proxy the prevalence of illicitly manufactured and distributed opioids.[18] As drug seizures may vary in terms of the volume of drugs, this study constructed two illicit drug seizure rates for fentanyl and heroin.

Summary statistics

Table 3 presents the means and standard deviations of county-level outcome variables and covariates (see Table 2) aggregated annually for expansion and non-expansion counties over 2010–2013. This study's sample comprises 206 counties in 29 expansion states and 101 counties in 16 non-expansion states.

[Insert Table 3 here]

Results

Baseline specification

Table 4 shows the results from estimating variations of the baseline specification—that is, Eq. (1)—with drug overdose mortality rates as the dependent variable. To examine how robust the estimate is with various controls, this study progressively included more controls in columns (1)–(3). Estimates from columns (1)–(3) are qualitatively similar and statistically significant. In addition to column (3), column (4) weights the regression by the county population aged 20–64, obtaining a similarly significant estimate. Based on the estimate in column (4), the

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[17] ARCOS is a data collection system to which manufacturers and distributors of controlled substances report their transactions to the Drug Enforcement Administration (DEA). ARCOS data can be obtained via https://www.deadiversion.usdoj.gov/arcos/retail_drug_summary/

[18] Drug seizure data are obtained from the DEA's National Forensic Laboratory Information System (NFLIS). The NFLIS collects drug identification results from forensic laboratories that analyse drugs seized by law enforcement agencies (NFLIS).

expansion increased drug overdose mortality rates by 1.208 per 100,000 at the county and quarter levels. This represents a 30.3% increase compared to the average mortality rate (weighted by the age 20–64 population) of 3.984 in expansion states in 2013, the year before the expansion for most expansion states.

[Insert Table 4 here]

According to Goodman-Bacon (2021), estimates from traditional two-way fixed effects (TWFE) are biased if the timing of treatment varies across states, which is the case with the expansion, and if the treatment effect is heterogeneous over time. To check whether the estimate in column (4) suffers such bias, column (5) excludes expansion states implementing the expansion after January 1, 2014. The estimate in column (5) remained significant and similar to that in column (4) in magnitude, indicating that the estimate in column (4) is robust to the heterogeneity in the timing of treatment. Therefore, column (4) is the preferred specification, and its estimate on the expansion is referred to as the baseline estimate.

Event study

Figure 3 shows the estimates for the leads-and-lags regression—that is, Eq. (2)—weighted the county population aged between 20 and 64. The coefficient for the quarter before the expansion quarter is set to zero. Therefore, the estimate of a lead or lag can be interpreted as the mortality rate difference between the expansion and non-expansion states relative to that difference at the expansion quarter, adjusting for other covariates.[19] None of the estimates before the expansion were statistically significant. In a separate regression, the slope of the differential pre-expansion linear trend was estimated at 0.056 and statistically insignificant ($p = 0.136$), consistent with the parallel-trends assumption. Therefore, the DiD design is plausibly valid. In

[19] Figure 3 shows estimates for 16 lags and 20 leads. As some expansion states implemented the expansion later than January 1, 2014, these expansion states have more than 16 lags.

comparison, the slope in the post-expansion period was estimated at 0.140 and statistically significant (p = 0.007), confirming that the differential pre-trend does not drive the baseline estimate. Moreover, the positive effect began rising after a year, increasing to about two deaths per 100,000 after two years.

[Insert Figure 3 here]

Mechanisms

In the previous two subsections, the expansion was shown to have significantly increased drug overdose mortality rates. This subsection conducts mediation analysis to examine potential mechanisms that could explain the effects. More specifically, this section controls for potential mediators in addition to the preferred specification and observe how the estimated effect changes. Table 5 shows the results.

[Insert Table 5 here]

In Table 5, the first column reproduces the baseline estimate for comparison. In column (6), insurance rates (below 138% FPL), which are widely accepted to be significantly increased by the expansion (e.g., Miller & Wherry, 2017), are controlled for. The estimate for the insurance rates is negative and not statistically significant. Once the insurance rates are controlled, the estimate for the expansion increases. Therefore, the indirect effect (IE) was negative (see 'Empirical strategy: Mediation analysis'), indicating that the expansion increased the insurance rates and reduced mortality rates. This may occur if more drug addicts are insured and receive treatment for drug addiction, reducing the mortality rate.

Column (7) controls for distributed controlled substance rates. However, the point estimate for the expansion remains unchanged, and the estimate of distributed controlled substance rates is statistically insignificant, indicating that distributed controlled substance rates do not explain the baseline estimate. Column (8) includes opioid prescribing rates. The estimate for the expansion is unchanged, and the estimate on opioid prescribing rates is negative and marginally significant, suggesting that opioid prescription rates did not account for the baseline estimate. Taken together, columns (7) and (8) indicate that the effects of the expansion on drug overdose mortality rates are unlikely to be attributable to legally manufactured and prescribed opioids.

Finally, column (9) controls for illicit seizure rates for fentanyl and heroin to proxy the prevalence of illicitly manufactured heroin and fentanyl. Once these illicit seizure rates are controlled, the point estimate on the expansion is nearly halved and becomes marginally significant. While the estimate of illicit heroin seizure rates is imprecise, the estimate of illicit fentanyl seizure rates is positive and significant. This suggests that illicitly manufactured fentanyl largely accounts for the expansion's effects on drug overdose mortality.

This study also investigated whether the relationships between these sets of potential mediators and the expansion are causal, a necessary condition for these variables to act as channels by which the expansion affected the mortality rates. To this end, this study conducted event studies using the preferred specification with each potential mediator separately as the dependent variable. Figure 4 shows estimates from these leads-and-lags regressions. In Panel (a), the expansion discretely and significantly increased insurance rates. The DiD estimate with the insurance rates as the dependent variable was 3.822 and statistically significant ($p = 0.000$). In Panels (b) and (c), the expansion did not seem to influence distributed controlled substance rates and opioid prescribing rates. Panel (d) indicates that, in the pre-expansion period, the illicit fentanyl seizure rate was more or less consistent in expansion states relative to that in non-expansion states. However, it rose dramatically and steadily after the expansion. The DiD estimate with illicit fentanyl seizure rates as the dependent variable was 9.839 and statistically significant ($p = 0.005$). Together with the estimates in column (9) of Table 5, Panel (d) of

Figure 4 suggests that the expansion may have increased illicitly manufactured and distributed fentanyl, resulting in increased mortality rates.

[Insert Figure 4 here]

These results lend support to the notion that illicitly manufactured opioids are at least partially responsible for the effects of the expansion on drug overdose mortality. A possible explanation is that more stringent restrictions on prescription opioids accompanying the expansion forced people to switch from legally to illegally manufactured drugs, leading to more deaths. To summarize, the results presented in this section suggest that insurance rates and illicit manufactured fentanyl, which had opposing effects on mortality rates, were mechanisms by which the expansion impacted the drug overdose mortality rate.

Heterogeneous effects across counties

To test the implications of the stylized model presented in the section entitled 'Conceptual framework', this subsection examines heterogeneous effects among counties. In addition to the preferred specification, this section includes the interaction terms between the dummy for expansion and other variables. Table 6 shows the results.

[Insert Table 6 here]

In column (10), 'Δinsurance rates' refers to the increase in insurance rates (below 138% FPL) between 2013 and 2017 (i.e. before and after the expansion); the estimate is negative and statistically significant. This indicates that the effects of the expansion are less pronounced in expansion counties with a higher increase in insurance rates due to the expansion compared to other expansion counties. In column (11), 'Δprescribing rates' refers to the increase in opioid prescription rates between 2013 and 2017; the estimate is negative and statistically significant. This finding suggests that the effects of the expansion are lower in expansion counties with a higher increase in the prescribing rates after the expansion compared to other expansion counties. Meanwhile, in column (12), the estimate for the interaction between the expansion and drug overdose mortality rates before 2014 (i.e. before the expansion) is positive and statistically significant. This result indicates that the effects of the expansion are more pronounced in expansion counties with previously high overdose rates or more severe drug overdose problems compared to other expansion states. Finally, column (13) includes all three interaction terms. The patterns found in columns (10)–(12) remain the same in column (13), indicating that the effects of these three interaction terms were roughly independent of one another. Together, the results presented in this subsection are consistent with the implications of the stylized model proposed in 'Conceptual framework'.

Conclusion

Drug overdose deaths have risen dramatically in the US over the past two decades. The conventional explanation attributes this growth to the increase in the prescribing and advertising of opioids in the 1990s. Known as 'more prescribing, more deaths', this explanation has spurred federal and state governments to adopt measures aimed at curtailing opioid prescriptions and increasing the costs of opioid production. Proponents of this view argue that policies restricting the supply of prescription opioids will reduce overdose deaths. However, a competing view known as 'more restrictions, more deaths' holds that stringent restrictions on prescription opioids only force people to use illicitly manufactured drugs, which are more dangerous than legally prescribed versions and thus increase the risk of overdose and death. Indeed, despite significant limitations on the prescription of heroin and synthetic drugs like fentanyl, related drug overdose mortality rates have continued to increase. Moreover, stringent restrictions on prescription opioids can result in the undertreatment of pain, negatively impacting people's quality of life and even driving some to commit suicide. According to this view, loosening access to prescription opioids will disincentivize the use of illicitly manufactured drugs, curbing overdose deaths.

This study investigated how the ACA's expansion of Medicaid affected drug overdose mortality rates. On the one hand, the expansion lowered the cost of opioid addiction treatment, helping alleviate drug opioid dependence and overdose deaths. On the other hand, it reduced the costs of opioid prescriptions, thereby making opioids more accessible, possibly leading to more addiction and deaths. Using a difference-in-differences (DiD) framework, this study estimated that the expansion of Medicaid increased drug overdose mortality rates by 1.208 per 100,000 people at the county and quarter levels. This represents a 30.3% increase compared to the average drug overdose mortality rate in the expansion counties prior to the expansion, a sizable and statistically significant effect. Event study results showed no significant differential pre-expansion trend in drug overdose mortality rates in expansion counties relative to non-expansion counties when adjusting for controls. However, the differential trend began rising a year after the expansion. The large increase in drug overdose mortality rates relative to that for insurance rates suggests that the expansion likely affected drug overdose mortality through channels beyond access to care among newly insured individuals. For example, restrictions on prescription opioids following the expansion could apply to all patients, not just Medicaid patients, as is the case in Louisiana.[20]

This study also examined the potential mechanisms connecting the expansion to the mortality rates. Results suggest that the expansion of Medicaid increased insurance rates (below 138% FPL), which reduced mortality rates. Analysis also indicated that the expansion fuelled the prevalence of illicitly manufactured fentanyl, increasing the mortality rates in

[20] In Louisiana, Governor John Bel Edwards implemented limitations on the dosage of opioid prescriptions for all people alongside the state's Medicaid expansion on July 1, 2016. Since the expansion went into effect, although Medicaid has covered many more prescriptions, the Louisiana Board of Pharmacy reported that the number of opioid prescriptions and the number of opioid prescription doses dropped by 2% and 3%, respectively (O'Donoghue, 2017).

expansion counties compared to non-expansion counties. Results also suggest that the effects of Medicaid expansion were largely driven by illicitly manufactured fentanyl.

To illustrate how the expansion can exacerbate overdose deaths from illicitly manufactured drugs in the presence of prescription opioid restrictions, this study constructed a stylized model, producing three hypotheses regarding three heterogeneous effects across counties. To test the stylized model, this study found that the effects of the expansion of Medicaid were less pronounced in expansion counties with higher increases in insurance or opioid prescribing rates after the expansion compared to other expansion counties. In contrast, the effects were more pronounced in expansion counties with higher drug overdose mortality rates or more severe drug overdose problems before the expansion. These results are consistent with the anticipated implications of the stylized model.

In sum, this study contributes to a growing body of literature on the effects of the expansion of Medicaid on drug overdose mortality. This study's findings highlight the importance of understanding how the markets for prescription and illicitly manufactured opioids may interact and support the view of 'more restrictions, more deaths' rather than the conventional argument of 'more prescribing, more deaths'. Moreover, the results of this study can inform policymakers in assessing and weighing the costs and benefits of restricting legal access to opioids. Indeed, although greater access to prescription opioids have the potential to exacerbate opioid dependence and overdose, it may also improve pain patients' quality of life and reduce the prevalence of underground drug consumption. Policymakers should also consider a mixed strategy targeting the improper use of prescription opioids while simultaneously meeting the demand for prescription opioids and increasing access to treatment for substance use disorder (SUD).

However, this study does not provide conclusive evidence indicating whether the dramatic post-expansion increase in illicit fentanyl seizure rates in expansion states relative to non-expansion states was a coincidence or a consequence of the expansion. If it was a consequence, then the question of whether the expansion fuelled drug overdose deaths through the stringent accompanying prescription restrictions remains unanswered. Moreover, this study does not rule out alternative explanations. For example, Abouk et al. (2021) found a substantial pre-expansion increase in heroin mortality in expansion states relative to non-expansion states. As heroin users may blend heroin with fentanyl, expansion states could be more susceptible to illicitly manufactured fentanyl, increasing its prevalence and resulting deaths. Moreover, the adoption of Medicaid expansion could have been motivated by drug-related mortality trends. However, the statistically insignificant differential pre-trend found in the event study renders this explanation unconvincing.

In line with this study's argument, future research needs to explore (1) whether the finding that the expansion did not increase opioid prescribing rates as expected was due to opioid prescription restrictions, (2) whether the expansion heightened the demand for prescription opioids exceeding the limits on prescriptions, and (3) whether more people with unmet demand resorted to underground markets and thus faced greater risk of overdose.

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TABLES

Table 1. Variables, units, and data sources

Variable(s)	Unit	Data Source
Mortality rates	county/quarter	CDC WONDER
Medicaid expansion	county/quarter	Kaiser Family Foundation
Other policies	county/quarter	Prescription Drug Abuse Policy System
Demographics	county/year	National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER)
Unemployment rates	county/quarter	Bureau of Labor Statistics
Poverty rates	county/year	U.S. Census Bureau Small Area Income and Poverty Estimates (SAIPE)
Median household income	county/year	U.S. Census Bureau Small Area Income and Poverty Estimates (SAIPE)
Insurance rates	county/year	Small Area Health Insurance Estimates (SAHIE)
Opioid prescribing rates	county/year	Centers for Disease Control and Prevention (CDC)
Distributed controlled substance rates	state/quarter	Automated Reports and Consolidated Ordering System (ARCOS)
Illicit drug seizure rates	state/year	National Forensic Laboratory Information System (NFLIS)

Table 2. The number of expansion states by expansion quarter

Expansion quarter	Number of states
2014 Q1	25
2014 Q2	1
2014 Q3	1
2015 Q1	2
2015 Q3	1
2016 Q1	1
2016 Q3	1

Table 3. Summary statistics for county-level variables

Variable	Expansion counties Mean (SD)	Non-expansion counties Mean (SD)
Drug overdose mortality rates per 100,000	7.94 (10.83)	6.38 (10.25)
% Male	0.49 (0.01)	0.49 (0.01)
% White	0.81 (0.14)	0.77 (0.14)
% Black	0.12 (0.12)	0.19 (0.14)
% Asian	0.06 (0.08)	0.04 (0.03)
% Hispanic	0.14 (0.14)	0.14 (0.14)
% Ages under 20	0.26 (0.02)	0.27 (0.03)
% Ages 20–24	0.07 (0.02)	0.07 (0.02)
% Ages 25–34	0.13 (0.02)	0.14 (0.02)
% Ages 35–44	0.13 (0.01)	0.13 (0.02)
% Ages 45–54	0.15 (0.01)	0.14 (0.01)
% Ages 55–64	0.13 (0.01)	0.12 (0.02)
Unemployment rate	8.73 (2.36)	8.19 (1.66)
Poverty rate	14.09 (4.97)	15.57 (4.48)
Median household income	56,834.98 (13,589.78)	50,182.31 (11,433.11)
Insurance rates (below 138% FPL)	0.65 (0.09)	0.54 (0.08)
Opioid prescribing rates (per 100)	82.69 (28.58)	93.75 (30.27)
Number of counties	206	101
Number of states	29	16

Notes: Table 3 shows the means and standard deviations of county-level variables aggregated annually in the pre-expansion period (2010–2013) in expansion and non-expansion counties, as well as the number of counties and states that did and did not adopt the expansion.

Table 4. DiD estimates of the expansion's effects on drug overdose mortality rates

	(1)	(2)	(3)	(4)	(5)
Expansion	1.592**	1.619***	1.574***	1.208***	1.302**
	(0.689)	(0.537)	(0.439)	(0.370)	(0.492)
	[2.475]	[2.475]	[2.475]	[3.984]	[3.943]
% Effect relative to baseline	64.3%	65.4%	63.6%	30.3%	33.0%
Demographics	No	Yes	Yes	Yes	Yes
Other drug-related policies	No	No	Yes	Yes	Yes
Weight by 20–64 population	No	No	No	Yes	Yes
Exclude later expansion states	No	No	No	No	Yes
Observations	11,052	11,052	11,052	11,052	9,324

Notes: Table 4 comprises panel data with the unit of observation at the county and quarter level from 2010 to 2018. The dependent variable is drug overdose mortality rates per 100,000 people aged 20–64. Standard errors are clustered at the state level and reported in parentheses. The numbers in square brackets are average mortality rates in expansion states in 2013, weighted by the age 20–64 population in columns (4) and (5). All columns include county and quarter fixed effects. Estimates of other covariates are omitted for clarity. Demographics and other drug-related policies are listed in Empirical strategy: Baseline specification. 'Later expansion states' refer to expansion states that implemented the expansion after January 1, 2014. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. DiD estimates of the expansion's effects on drug overdose mortality rates with and without controlling for potential mediators

	baseline	(6)	(7)	(8)	(9)
Expansion	1.208***	1.521***	1.206***	1.207***	0.633*
	(0.370)	(0.427)	(0.372)	(0.355)	(0.323)
Insurance rates (below 138% FPL)		-0.0819			
		(0.0659)			
Distributed controlled substance rates (grams per 100,000)			-0.00000523		
			(0.00000346)		
Opioid prescribing rates (per 100)				-0.0458*	
				(0.0256)	
Illicit fentanyl seizure rates (per 100,000)					0.0561***
					(0.00952)
Illicit heroin seizure rates (per 100,000)					0.00490
					(0.00613)
Observations	11,052	11,052	11,052	11,052	11,052

Notes: Table 5 comprises panel data with the unit of observation at the county and quarter level from 2010 to 2018. The dependent variable is drug overdose mortality rates per 100,000 people aged 20–64. Standard errors are clustered at the state level and reported in parentheses. Regressions are weighted by the age 20–64 population. All columns include controls listed in the Empirical strategy: Baseline specifications, county, and quarter fixed effects. Estimates of other covariates are omitted for clarity. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. Heterogeneous effects across counties

	Drug overdose mortality rates			
	(10)	(11)	(12)	(13)
Expansion × Δinsurance rates	-0.158***			-0.170***
	(0.0566)			(0.0441)
Expansion × Δprescribing rates		-0.124***		-0.106***
		(0.0296)		(0.0314)
Expansion × Mortality rates before 2014			0.249***	0.248***
			(0.0776)	(0.0699)
Expansion	4.077***	-1.166*	0.329	1.407
	(1.110)	(0.618)	(0.432)	(1.136)
Observations	11052	11052	11052	11052

Notes: Table 6 comprises panel data with the unit of observation at the county and quarter level from 2010 to 2018. The dependent variable is drug overdose mortality rates per 100,000 people aged 20–64. Standard errors are clustered at the state level and reported in parentheses. Regressions are weighted by the age 20–64 population. "Δinsurance rates" is the increase in insurance rates (below 138% FPL) between 2013 and 2017. "Δprescribing rates" is the increase in opioid prescription rates between 2013 and 2017. "Mortality rates before 2014" are average drug overdose mortality rates before 2014. All columns include controls listed in Empirical strategy: Baseline specification, county, and quarter fixed effects. Estimates of other covariates are omitted for clarity. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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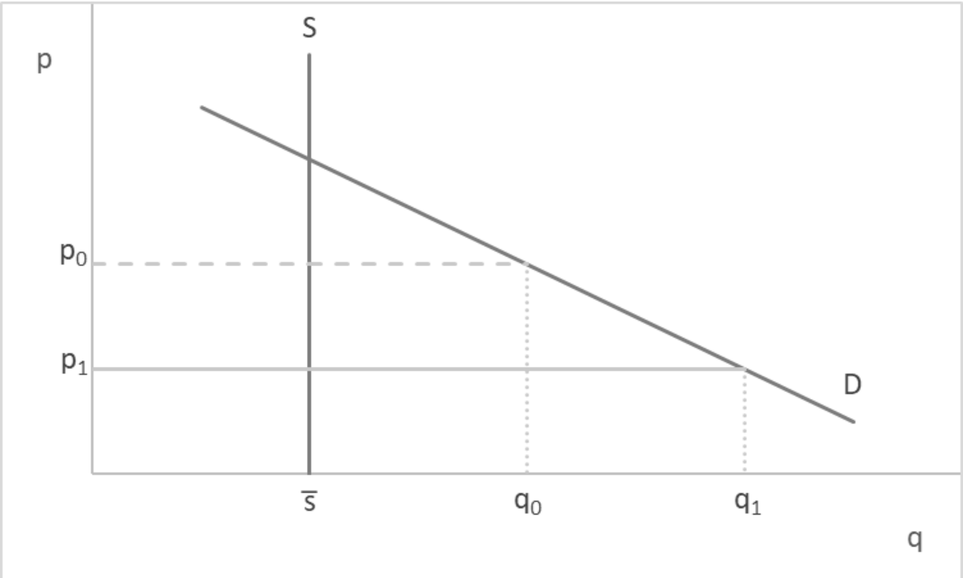
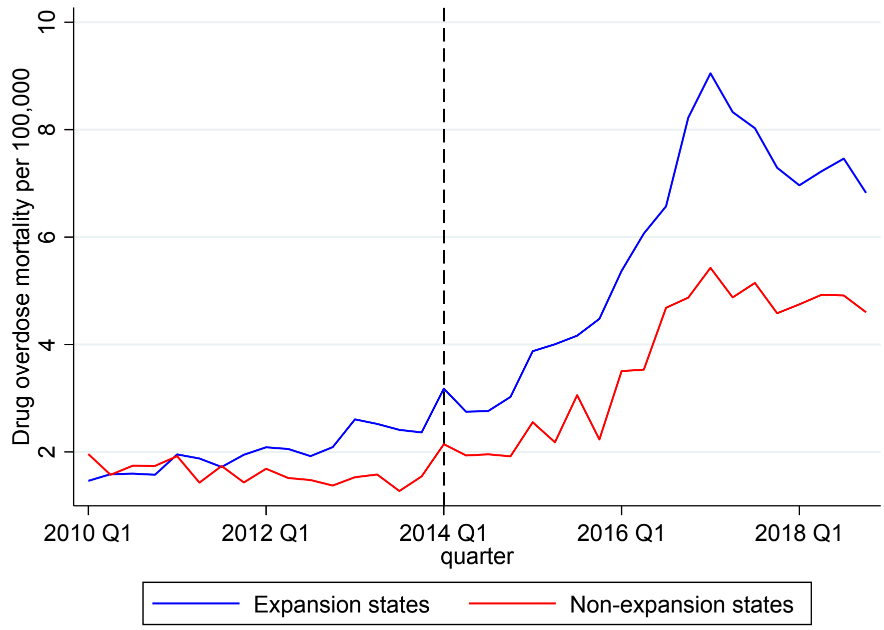
FIGURES

Figure 1. Supply and demand of prescription opioids before and after the expansion.



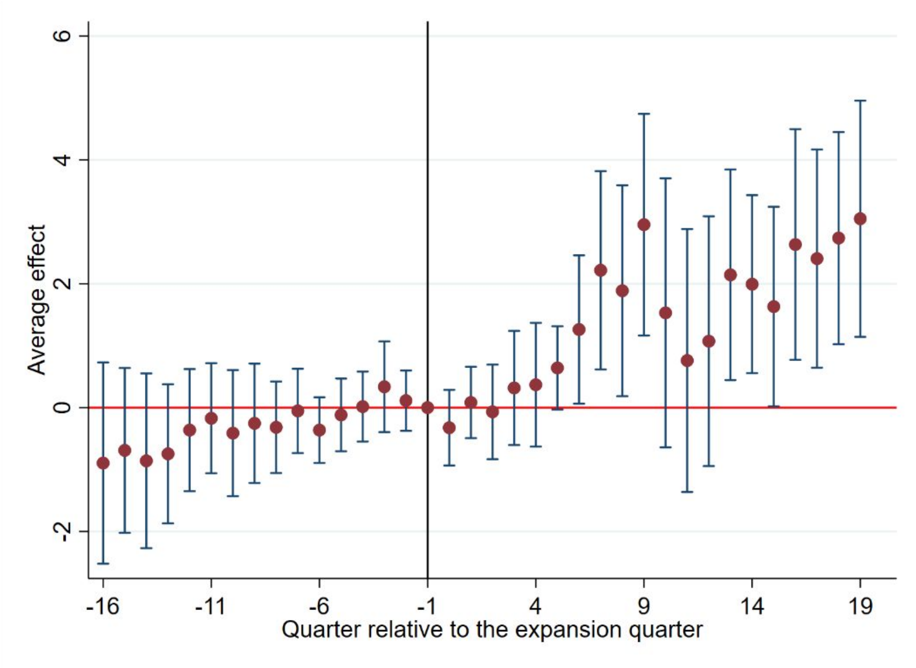
Notes: This figure illustrates the supply (S) and demand (D) of prescription drugs. After the expansion, the out-of-pocket price of prescription opioids decreases from p_0 to p_1, and the quantity demanded of prescription opioids increases from q_0 to q_1.With the supply fixed at $\bar{s}$, the shortage of prescription drugs increases from $q_0 - \bar{s}$ to $q_1 - \bar{s}$ by $q_1 - q_0$.

Figure 2. Trends in drug overdose mortality rates per 100,000 people at the county and quarter level in expansion and non-expansion states between 2010 and 2018.



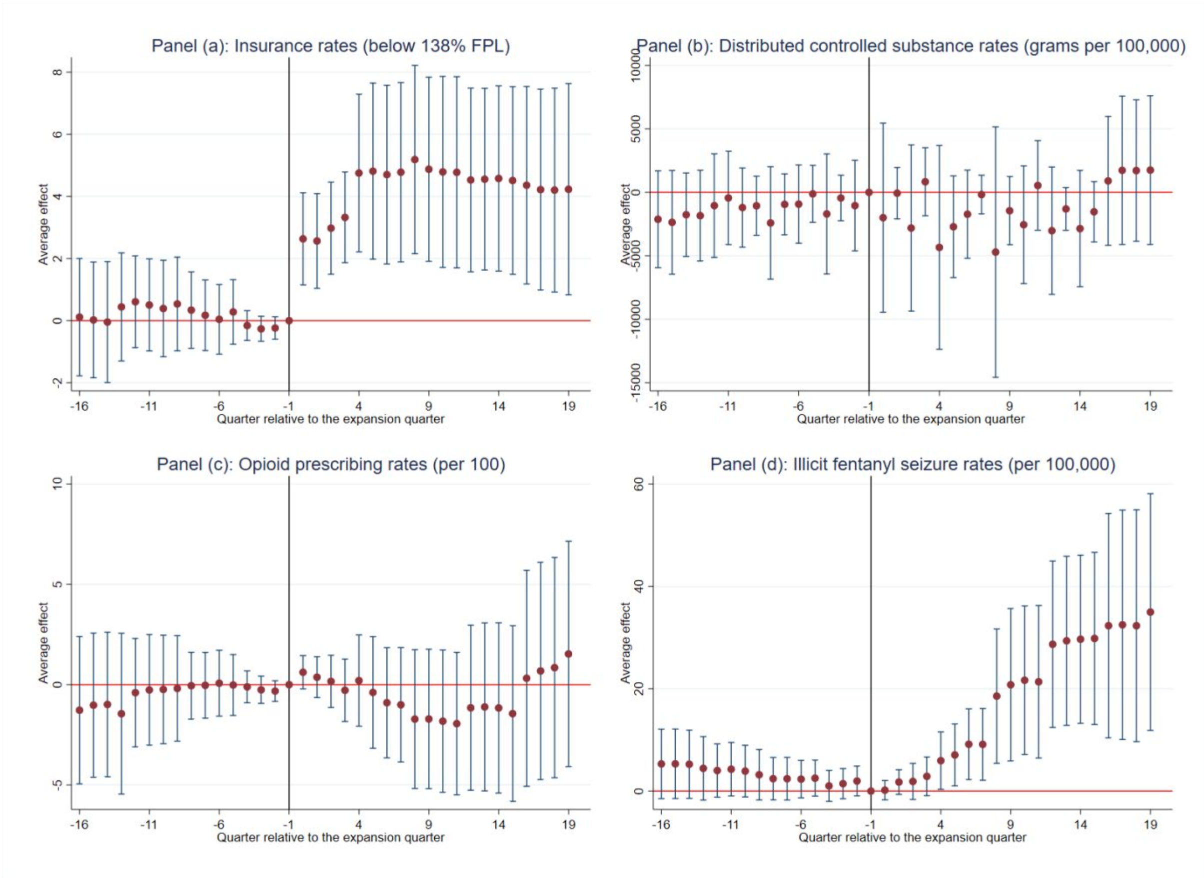
Notes: The dashed vertical line at the 2014 Q1 mark indicates the quarter in which the expansion went into effect in most expansion states.

Figure 3. Lead and lag estimates for the expansion's effects on drug overdose mortality rates.



Notes: In this figure, dots show point estimates of the lead and lag dummies in Equation (2) (Empirical strategy: Baseline specification). Vertical bars show 95% confidence intervals using standard errors clustered at the state level. The solid vertical line indicates the relative quarter -1, the coefficient of which is set at zero. The dependent variable is drug overdose mortality rates per 100,000 people aged 20–64 years. Covariates include the controls listed in Empirical strategy: Baseline specification, county, and quarter fixed effects. The regression is weighted by the age 20–64 population.

Figure 4. Lead and lag estimates with different potential mediators as the dependent variable.



Notes: In this figure, dots show point estimates of lead and lag dummies in Equation (2) (Empirical strategy: Baseline specification). Vertical bars show 95% confidence intervals using standard errors clustered at the state level. The solid vertical line indicates the relative quarter -1, the coefficient of which is set at zero. In each panel, the dependent variable is indicated by the title. Covariates include the controls listed in Empirical strategy: Baseline specification, county, and quarter fixed effects. Each regression is weighted by the age 20–64 population.