The Effect of Affordable Care Act's Medicaid Expansion on Drug Overdose Mortality Rates

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Abstract

While the Affordable Care Act (ACA)'s Medicaid expansion lowered the cost of opioid addiction treatment, it also made opioid prescriptions more accessible, possibly leading to more addiction and deaths. This study examined how the expansion affected drug overdose mortality rates. Using a difference-in-differences framework, I estimated that the expansion increased drug overdose mortality rates by 0.881 per 100,000 people at the county and quarter levels. This is a 15.4% increase compared to the average mortality rate before the expansion, and over half of these attributed to opioids. However, an additional analysis showed that the expanded insurance itself was not responsible for the increase in mortality but protects against mortality, as the effects were lower in expansion counties with greater increases in insurance rates. Rather, I found evidence that the expansion fueled the prevalence of illicitly manufactured drugs, mainly explaining the effects. Furthermore, contrary to the expectation, there is no evidence that the expansion increases opioid prescribing rates, suggesting that prescribing opioids is restricted. These findings are consistent with the notion that, coupled with restrictions on prescription opioids, the expansion exacerbates the shortage of licit prescription opioids, pushing more people to consume illicitly manufactured opioids instead and thereby increasing the risks of overdose and death. Thus, this paper highlights the importance for policymakers to consider the interaction between licit and illicit drug markets when crafting drug policies.

Keywords: *Affordable Care Act, Medicaid expansion, opioid epidemic, drug overdose mortality, illicitly manufactured fentanyl*

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1 Introduction

As one of the largest health insurance programs in the U.S., 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) Medicaid expansion extended the eligibility for Medicaid coverage to adults with incomes up to 138% of the federal poverty level (FPL).[1] To date, 39 states (including the District of Columbia) have expanded their Medicaid programs (Kaiser Family Foundation 2019). However, the increase in the health insurance rate among non-elderly adults due to the expansion is estimated to be only about 2% (Black et al. 2019).

Over the past two decades, deaths from drug overdose, particularly opioid overdose, have increased dramatically in the U.S. For opioids, this phenomenon is often dubbed as the "opioid epidemic." [2] A priori, the impact of expansion on drug overdose deaths remains unclear. On the one hand, the expansion may facilitate newly insured beneficiaries to obtain prescription opioids, which could be addictive and lead to increased drug dependence and overdose; namely, prescription opioids could be a "gateway" to addiction.[3] Patients who develop addiction using prescription drugs and can no longer obtain sufficient prescriptions to meet their demand may resort to illicit drugs. This increases the risk of overdose because users cannot easily assess drug safety and quality in the underground markets (Goodman-Bacon and Sandoe 2017; Miron, Sollenberger and Nicolae 2019).[4] Therefore, policies intended to curb opioid addiction by restricting access to prescription opioids could inadvertently drive users to switch from prescription drugs to illicit drugs and increase drug overdose deaths. Restrictions on prescription drugs may also lead to pain undertreatment, reduce users' quality of life, and result in more suicides (Kertesz, Gordon and Satel 2018).[5] Moreover, facilitating access to prescription opioids

[1] The expansion was intended to mandate all states to expand their eligibility for Medicaid. However, a 2012 Supreme Court decision effectively ruled that the expansion was optional for states.

[2] The unintentional opioid overdose mortality rate in the United States was about two per 100,000 people in 1999; by 2017, it had increased to about 13 per 100,000 people.

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

[4] For example, illicit opioids obtained from underground markets do not have warning labels, and thereby users are more likely to combine opioids with alcohol or other drugs, increasing the risk of respiratory depression (Miron, Sollenberger and Nicolae 2019). Moreover, illicit opioids are produced without adhering to appropriate manufacturing measures, leading their potency to vary considerably and unpredictably (Abouk et al. 2021).

[5] Surveys indicated that the regulations discourage physicians from prescribing opioids, potentially leading to undertreatment of pain (Gilson and Joranson 2001).

may lead to nonmedical use of opioids among individuals without prescriptions. About half of the respondents who misused prescription opioid pain relievers reported obtaining them from a friend or relative (Lipari and Hughes 2017).

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.[6] Medicaid is the only insurance option available to many patients with SUD to obtain affordable treatment (Goodman-Bacon and Sandoe 2017).[7] Health conditions requiring pain relief and demand for drug abuse treatment are more common among Medicaid recipients than among non-recipients, especially those with disabilities and chronic diseases. Therefore, the expansion could reduce drug overdose deaths by increasing access to treatment.[8] Further, the expansion may also help curb drug overdoses by increasing the accessibility to prescription opioids for individuals who are susceptible to illicit drug use.

As drug overdose claims a substantial number of lives every year, and many states have adopted the expansion, the ambiguous effect of the expansion on drug overdose mortality rates warrants empirical investigation. Therefore, this study investigated how the expansion affects drug overdose mortality rates. Existing studies examining this issue have reported mixed results (Yan et al. 2021; Abouk et al. 2021; Averett, Smith and Wang 2019; Maclean and Saloner 2019). However, much of this literature has various identification issues such as 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, I find evidence that the expansion was associated with an increase in drug overdose mortality rates. Nearly half of these effects were attributable to opioids. Second, I conducted mediation analyses to identify potential channels through which the effects occurred. I found that the expansion increased insurance rates for individuals with incomes below 138% of the FPL, which in turn helped reduce the mortality rate. This suggests that the expanded

[6] Source: https://obamawhitehouse.archives.gov/ondcp/healthcare

[7] About 37% of the respondents to the National Survey on Drug Use and Health (2010–2013) cited the lack of health insurance as their main reason for not receiving treatment (Grooms and Ortega 2019).

[8] The current standard care to treat opioid addiction is medication-assisted treatment (MAT), which involves using medications (e.g., methadone, buprenorphine, and naltrexone) along with counseling and other support services (Abouk et al. 2021). All state Medicaid programs cover at least one of these medications. Evidence shows that MATs are effective in reducing illicit drug use, opioid dependence, and drug and opioid-related deaths. Prior studies found evidence that the expansion 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).

insurance itself is not responsible for the increase in mortality but protects against mortality. However, there is suggestive evidence that the expansion exacerbated the increase in the prevalence of illicit drugs, thereby increasing the mortality rate. Furthermore, despite the expectation that the expansion would increase the demand for prescription opioids, states that adopted the expansion (hereafter, the expansion states) did not see an increase in opioid prescribing rates compared to states that never adopted the expansion as of 2018 (hereafter, the non-expansion states). This suggests that prescribing opioids is restricted. Based on these findings, I hypothesize that restrictions on opioid prescriptions along with the expansion led to more people with unmet demands for prescription opioids to resort to illicit substitutes, which are more dangerous than the legal versions, thus leading to more drug overdose deaths.[9] As such, my results are consistent with the view of "more restrictions, more deaths" rather than that of "more prescriptions, more deaths" (Miron, Sollenberger and Nicolae 2019).

In this study, I use 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, I examined the heterogeneous effects among counties. I found that the effects were less pronounced in expansion counties with higher increases in insurance or opioid prescribing rates, and they were more pronounced in expansion counties with more severe drug problems. These findings are consistent with the implications of the stylized model.

The remainder of this paper is organized as follows. Section 2 reviews the relevant literature. Section 3 illustrates the stylized model. Section 4 presents the empirical methods. Section 5 describes the data used in this study. Section 6 discusses the results. Finally, Section 7 concludes the paper with a consideration of policy implications.

2 Literature Review

An emerging body of literature examining the effect of Medicaid expansion under the ACA on drug-related mortality found ambiguous or positive effects. Several studies noted that drug-related deaths grew more rapidly in expansion states than in non-expansion

[9] 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. Although Medicaid has covered many more prescriptions since the expansion was implemented, the Louisiana Board of Pharmacy reported that the numbers of opioid prescriptions and their doses have dropped by 2% and 3%, respectively (O'Donoghue 2017).

states before the expansion among all states (Goodman-Bacon and Sandoe 2017; Yan et al. 2021) or only among states east of the Mississippi River (Abouk et al. 2021). These studies claimed that evidence from DiD models was not credible because of the pre-trend in drug-related deaths, which violates the parallel trend assumption of the DiD model. However, my analysis found that the pre-trend remained positive but became statistically insignificant after controlling for confounding covariates.

Two studies found imprecise estimates (Averett, Smith and Wang 2019; Borgschulte and Vogler 2020). In the first, Averett, Smith and Wang (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 reasonable effects.[10] In the second study, Borgschulte and Vogler (2020) first used the double-lasso method described by Belloni, Chernozhukov and Hansen (2014) and Urminsky, Hansen and Chernozhukov (2016) to select variables to be included in a propensity score model that matches the counties in expansion and non-expansion states. They then used propensity-score weighting DiD models and found no significant overall effects of the expansion on opioid overdose mortality rates. Furthermore, they found sizable and significant effects—a 35.63% increase—among people aged 20 to 24 years, but they found no significant effects among other age groups. However, they did not account for other state policies being implemented concurrently with the expansion, which could affect drug overdose deaths. While propensity-score matching attempts to address the differences in observables between the expansion and non-expansion counties, it may introduce bias to the estimate (Daw and Hatfield 2018; King and Nielsen 2019).

In contrast, 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 relative to non-expansion states after the expansion. The authors attributed this result to the opioid epidemic and concluded that it mitigated the life-saving impact of the expansion. However, the authors did not investigate the potential mechanisms for the increase in mortality and their relationship with the expansion. Like Borgschulte and Vogler (2020), the authors did not control for other drug-related policies. Abouk et al. (2021) used DiD models to separately examine the association between expansion

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

and drug-related mortality in the east and west of the Mississippi River.[11] They found a statistically significant association between the expansion and increase in drug-related mortality in the east (attributable to synthetic opioids other than methadone and heroin) and found no association in the west. The authors also argued that the estimate in the east was not valid because of the pre-trend, whereas there appeared to be no pre-trend in the west. After controlling for state-specific 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 having an immediate effect on the outcome in a discrete manner, controlling for state-specific 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 suffers various identification issues, such as omitted variable bias (e.g., not accounting for other relevant state policies), non-parallel pre-trends, and lack of power. To address these issues, I used finer data at the county-quarter level and a richer set of covariates. Moreover, I conducted mediation analyses to examine the potential mechanisms of the identified positive and significant effects.

3 Conceptual Framework

Previous studies suggest that the cause of overdose deaths shifts from legal to illicit drugs when the access to prescription opioids is restricted (Goodman-Bacon and Sandoe 2017). In this section, I used 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.

Figure 1 illustrates the supply (S) and demand (D) for prescription opioids, where p and q denote the out-of-pocket price and quantity of prescription opioids, respectively. Suppose that the supply is restricted to be fixed at $\bar{s}$. Initially, the price and quantity demanded of the prescription drugs are p_0 and q_0, respectively. 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

[11] This separation attempted to account for the coinciding rise in the supply of illicitly manufactured fentanyl. According to the authors, black and brown powder heroin was sold primarily west of the Mississippi River, whereas white powder heroin was sold primarily east of the Mississippi River. While illicitly manufactured fentanyl is white and can be easily mixed with eastern white heroin, it is difficult to mix it with western black or brown heroin; therefore, it is used much less in the west.

from underground markets, which are more hazardous than legally prescribed versions because their drug potency is not easily accessible, increasing the risk and incidence of overdose and death.

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

This model makes the following three predictions about the heterogeneous effects of the expansion across counties, given that everything else is equal. First, as health insurance rates increase due to the expansion, more people are being treated for opioid addiction, decreasing the demand for prescription opioids. This lowers the shortage increase, thereby decreasing overdose deaths. Thus, expansion counties with higher increases in insurance rates due to the expansion are expected to have fewer exacerbating effects on overdose deaths. Second, as opioid prescribing rates increase, the supply of prescription opioids ($\bar{s}$) increases, lowering the shortage increase. Therefore, expansion counties that saw higher increases in opioid prescribing rates due to the expansion are also predicted to have reduced mortality as there will be less diversion to illicit drugs in these counties. Third, a decrease in the price of prescription opioids creates a greater increase in drug shortage for counties with more severe drug problems (having more drug addicts) because these counties have a more elastic demand. The intuition is that, as individuals more addicted to drugs spend a larger proportion of their income on drugs, the quantity of drugs they demand is more sensitive to drug price changes (National Research Council 2010). Accordingly, these individuals have a more elastic demand curve. As such, expansion counties with more severe drug problems, proxied by higher drug overdose mortality rates before the expansion, are expected to experience greater effects of exacerbation in overdose deaths. In a later section, I empirically examine these implications by estimating the heterogeneous effects among counties.

4 Empirical Strategy

4.1 Baseline Specification

I exploit the variation in the timing of states' adoption of the Medicaid expansion using a DiD framework to identify the causal effect of the expansion on an outcome variable. The baseline estimation equation is as follows:

$$Y_{cst} = \alpha \cdot \mathrm{Exp}_{st} + X'_{cst}\beta + C_c + T_t + \epsilon_{cst}. \tag{1}$$

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 2010Q1 to 2018Q4).[12] A dummy variable for the expansion status, Exp_{st}, is 1 if state s implemented the expansion in quarter t, and 0 otherwise. X_{cst} is a vector comprising the following control variables: (1) dummies for other statewide 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 the prescription drug monitoring program, pain clinic law, naloxone access law, "Good Samaritan" law, medical marijuana law, and recreational marijuana law; (2) county-level and time-varying demographics, including shares by race, gender, origin, or age group; and (3) economic indicators, including unemployment rates, poverty rates, and median household income.[13] C_c and T_t are county and quarter fixed effects, which control for county- and year-specific fixed heterogeneity, respectively. ϵ_{cst} is an idiosyncratic error term. α captures the causal impact of the expansion on the dependent variable. Finally, in the estimation, standard errors are clustered at the state level to allow for arbitrary autocorrelation of the errors in each state.

4.2 Event Study

A key assumption of the DiD model—the parallel trends assumption—holds that the dependent variable in expansion states would trend in a way similar to that in non-

[12] YYYYQX stands for quarter X of year YYYY.

[13] Prescription drug monitoring programs involve statewide electronic databases that track prescriptions of controlled substances. Pain clinic laws impose regulations on pain clinics to restrict prescriptions of controlled substances (including opioids) without medical indication. Naloxone access laws allow lay responders to administer naloxone, an opioid 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. Recreational marijuana laws legalize marijuana use for recreational purposes.

expansion states in the absence of expansion (after controlling for covariates). Otherwise, the DiD estimates are driven by unobserved trends and are thereby 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, I perform event studies by running a leads-and-lags regression as follows:

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

where, for expansion states, $r(s)$ is a function that returns the quarter relative to state s' expansion quarter in quarter t; for non-expansion states, $r(s) = -1$. In the estimation, the indicator for the quarter preceding the expansion quarter, $\mathbb{1}[r(s) = -1]$, is omitted from the model. Conditional on other variables, α_r is the difference in the dependent variable in relative quarter r between the expansion and non-expansion states relative to the difference in the quarter preceding the expansion quarter. The other variables are defined in Equation (1).

4.3 Mediation Analysis

Mediation analysis is used to estimate the role of pathways or mechanisms by which a treatment variable (e.g., policy) affects an outcome; it explains why a relationship exists between two variables (Hicks and Tingley 2011). Here, I further investigated how the expansion may affect drug overdose mortality rates by performing mediation analyses.

To illustrate, let T and Y denote the treatment and outcome, respectively. Let M denote a potential mechanism (called 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:

$$M = b_0 + b_1 \cdot T + v, \tag{3}$$

$$Y = \phi_0 + \phi_1 \cdot T + e, \tag{4}$$

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

where v, e, and u are the error terms. In Equation (3), which relates mediator M with treatment T, b_1 needs to be significant for M to be a mediator; otherwise, T and M have no relationship. In Equation (4), which relates outcome Y to treatment T, ϕ_1 gauges the total effect of treatment T on outcome Y. In addition to Equation (4), Equation (5) includes mediator M as an explanatory variable. If the magnitude of θ_1 is significantly smaller than that of ϕ_1, this indicates mediation via mediator M because the inclusion of M explains some of the treatment effect on the outcome (VanderWeele 2016). The difference between these two coefficients is often interpreted as a mediated or indirect effect (IE), that is, IE = $\phi_1 - \theta_1$. The remaining treatment effect in Equation (5), θ_1, is often taken as a measure of the direct effect (DE), that is, DE = θ_1 (VanderWeele 2016).

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

5 Data and Variables

5.1 Data Source

Table 1 lists variables used in the analyses along with their units of observation and data sources.

5.2 Drug Overdose Mortality Rates

Drug-related mortality rates (per 100,000 people) were calculated using restricted-use, individual-level multiple causes of death (MCOD) data from the Centers for Disease Control and Prevention (CDC), which provides information on the universe of death certificates.

The MCOD data provide the following information relevant to my analyses: the year and month of death, cause of death coded by the International Classification of Diseases Version 10 (ICD-10), and the age and county of residence at the time of death.[14] However, the MCOD data do not provide information on the deceased persons' health insurance coverage, income, or other socioeconomic status relevant to identifying their Medicaid eligibility. Since the expansion only applies to non-elderly adults, I limited the sample to U.S. residents who have deceased at ages 20 to 64 years. Adults aged 18 and 19 years were also excluded because the Children's Health Insurance Program provides coverage to eligible children up to the age of 19 years.

[14] I used the county of residence rather than the county of the occurrence of death as the deceased person's county.

I used the CDC's definition based on ICD-10 to identify drug overdose deaths. For opioid overdose deaths, the involved opioids were further identified using the ICD-10 "T-codes." Table 2 lists the ICD-10 codes for drug overdose deaths and the opioids involved (Ahmad, Rossen and Sutton 2021).[15] Overdose mortality rates due to specific drug categories (e.g., heroin and synthetic opioids) were defined similarly. For example, mortality rates due to heroin were calculated as the number of heroin deaths (identified by the ICD-10 code in Table 2) per 100,000 people.

5.3 Medicaid Expansion

States' ACA Medicaid expansion statutes were drawn from the Kaiser Family Foundation. Table 3 presents the number of expansion states by expansion quarter as of the end of the study period, 2018Q4. Most expansion states implemented an expansion on January 1, 2014.

5.4 Potential Mediators

Time-varying variables—such as insurance rates, opioid prescription rates, distributed controlled substance rates, and illicit drug seizure rates—could be potential mediators through which the expansion affects drug overdose mortality rates.

In this study, insurance rates refer to countywide health insurance rates for individuals between 18 and 64 years of age with an income at or below 138% of the FPL, that is, the population 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 are the statewide retail drug distribution rates in terms of grams per 100,000 people; these rates were obtained from Report 3 (Quarterly Distribution in Grams per 100K Population) from the Automation of Reports and Consolidated Orders System (ARCOS).[17] Finally, illicit drug seizure rates were de-

[15] When analyzing overdose deaths due to prescription opioids, the CDC looks at natural opioids (e.g., morphine and codeine), semisynthetic opioids (e.g., oxycodone, hydrocodone, hydromorphone, and oxymorphone), and methadone. Because information on overdose deaths does not distinguish between legally and illicitly manufactured fentanyl, the CDC separates synthetic opioids (other than methadone) from prescription opioids (CDC 2022).

[16] Opioid prescribing rates are confined to the initial or refill prescriptions dispensed at retail pharmacies; thus, they do not capture illicitly manufactured opioids (Shakya and Harris 2022).

[17] ARCOS is a data collection system through 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/

fined as the number of cases of drug seizure by law enforcement operations per 100,000 people; these rates were used to proxy the prevalence of illicitly manufactured and distributed opioids.[18] I calculated two illicit drug seizure rates for fentanyl and heroin. Note that drug seizures may vary in drug volume.

6 Results

6.1 Baseline Specification

Table 4 shows the results from estimating variations of the baseline specification (Equation 1 in Section 4) with drug overdose mortality rates as the dependent variable. To examine how robust the estimate is with various controls, I progressively included more controls in columns (1) to (3). Estimates from columns (1) to (3) are qualitatively similar and are all 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 expansion increased drug overdose mortality rates by 0.881. This represents a 15.4% increase compared to the average mortality rate of 5.714 in the expansion states in 2013, the last year before the expansion for most expansion states.

Goodman-Bacon (2021) indicated that estimates from traditional two-way fixed effects 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) remains similar in magnitude to that in column (4), but with reduced precision ($p = 0.057$). Therefore, the estimate in column (4) is robust to heterogeneity in the timing of treatment and is hereafter referred to as the "baseline estimate."

6.2 Event Study

Figure 2 shows estimates for the leads-and-lags regression (Equation 2 in Section 4), weighted by the county's population aged 20–64 years. The estimate for the last quarter

[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 analyze drugs seized by law enforcement agencies (NFLIS n.d.).

before the expansion quarter (the first lag) is anchored at zero. Therefore, the estimate for a lead or lag relative to the expansion quarter can be interpreted as an estimated mortality rate difference between the expansion and non-expansion states compared to that difference at the first lag, controlling for other covariates. None of the estimates before the expansion is statistically significant. Therefore, there is no pre-trend of mortality rates in the expansion versus non-expansion states, which is consistent with the parallel trends assumption of the DiD model. Moreover, the difference increased over time after the expansion, indicating that the effects of the expansion on drug overdose mortality rates have been rising over time.

6.3 Effects by Drug Category

To investigate which drug categories were responsible for the baseline effects, I separately estimated the effects of the expansion on mortality rates for different drug categories with the baseline specification. Figure 3 shows the results. Opioids accounted for more than half of the baseline effects of all drugs. Within this class of opioids, heroin and synthetic opioids (other than methadone) accounted for nearly all the effects of opioids. None of the estimates for the other drug categories was economically or statistically significant.

Prescription opioids mainly consist of natural/semi-synthetic opioids and methadone. However, as shown in Figure 3, none of these drug categories led to deaths associated with expansion. Moreover, heroin is manufactured and distributed illegally, and synthetic opioids (e.g., fentanyl and tramadol) are prescribed under the supervision of licensed medical professionals as well as the oversight of the DEA. Mortality rates from synthetic opioids are primarily driven by illicitly manufactured rather than legally prescribed drugs. As such, the effects of the expansion on opioid overdose deaths are likely to be attributed to illicitly manufactured opioids rather than to prescription opioids.

6.4 Mechanisms

In Sections 6.1 to 6.3, I found that the expansion significantly increased drug overdose mortality rates. In this section, I conducted mediation analyses to examine the potential mechanisms that explain these effects. Specifically, I separately controlled for potential mediators, in addition to the baseline specification, and observed how the estimated effect changed.

Table 5 shows the results. In the first column, the baseline estimate is reproduced for comparison. In column (6), in addition to the baseline specification, I controlled for insurance rates, which are widely accepted to have increased significantly because of the expansion (e.g., Miller and Wherry 2017). The estimate for insurance rates was negative and not statistically significant. Once the insurance rates were controlled for, the estimate of the expansion increased. This indicates that the expansion increased insurance rates, which in turn reduced mortality rates. This may occur if more drug addicts are insured and receive treatment for drug addiction, thereby reducing the mortality rate.

Additionally, in column (7), I controlled for distributed controlled substance rates. The estimate for the expansion remained unchanged, and the estimate for distributed controlled substance rates was statistically insignificant. This suggests that distributed controlled substance rates do not explain the baseline estimate.

Moreover, in column (8), I included opioid prescribing rates. The estimate for the expansion was essentially unchanged, and the estimate for opioid prescribing rates was statistically insignificant. This suggests that opioid prescribing rates do 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 attributed to legally manufactured and prescribed opioids, consistent with the findings in Section 6.3.

The findings in Section 6.3 suggest that illicitly manufactured opioids—synthetic opioids (e.g., fentanyl) and heroin in particular—might account for the effects of the expansion on drug overdose mortality. To examine this suggestion, in column (9), I controlled for illicit seizure rates for fentanyl and heroin as proxies for illicitly manufactured fentanyl and heroin, respectively. Once these illicit seizure rates were controlled for, the estimate of the expansion was significantly reduced and became statistically insignificant. That is, these illicit seizure rates explained most of the estimated effect. In addition, both illicit seizure rates significantly increased drug overdose mortality rates.

Furthermore, I investigated whether the relationships between these sets of potential mediators and the expansion were causal, a necessary condition for these variables to be channels by which the expansion affected the mortality rates. To this end, I conducted event studies using the baseline specification with each potential mediator separately as the dependent variable. Figure 4 shows the estimates from these leads-and-lags regressions. Panel (a) shows that the expansion discretely and significantly increased insurance rates. The DiD estimate with insurance rates as the dependent variable was 5.395 and statistically significant ($p = 0.000$). In panels (b) and (c), the expansion did not seem to influence distributed controlled substance and opioid prescribing rates. Panel (d) indicates that, in the pre-expansion period, the trend of illicit drug seizure rates (defined as the summation of illicit seizure rates for fentanyl and heroin) in the expansion states was similar to that in the non-expansion states after controlling for other covariates. However, after the expansion, the trend of illicit seizure rates started to rise dramatically in the expansion states compared to the non-expansion states. The DiD estimate with illicit seizure rate as the dependent variable was 7.982, which was statistically significant ($p = 0.015$). Together with the estimates in column (9) of Table 5, Panel (d) suggests that the expansion increased the amount of illicitly manufactured and distributed opioids, which in turn increased mortality rates. These results further strengthen the notion that illicitly manufactured opioids are at least partially responsible for the effects of the expansion on drug overdose mortality. A plausible explanation for this notion is that more stringent restrictions on prescription opioids following the expansion caused people to switch from legally to illicitly manufactured drugs, leading to more deaths.

Overall, the results in this section indicate that insurance rates and illicit drug seizure rates, whose effects on mortality rates were in opposite directions, were mechanisms by which the expansion impacted drug overdose mortality.

6.5 Heterogeneous Effects

To test the implications of the stylized model in Section 3, I examined the heterogeneous effects among the counties. In addition to the baseline specification, I included the interaction terms between the dummy for expansion and other variables, as described below.

Table 6 shows the results. In column (10), "Δinsurance rates" are defined as the increase in insurance rates (below 138% of the FPL) between 2013 and 2017 (i.e., before and after the expansion). The estimate on "Δinsurance rates" is negative and statistically significant. This indicates that the effects of the expansion on overdose mortality are less pronounced in expansion counties with a higher increase in insurance rates due to the expansion compared with the other expansion counties. Moreover, in column (11), "Δprescription rates" are defined as the increase in opioid prescription rates between 2013 and 2017. The estimate on "Δprescription rates" is negative and statistically significant. This indicates that the effects of the expansion are lower in expansion counties with a higher increase in prescription rates after the expansion compared with the other expansion counties. Furthermore, in column (12), the estimate on the interaction between the expansion and drug overdose mortality rates before 2014 (i.e., before the expansion) is positive and statistically significant. This indicates that the effects of the expansion on overdose mortality are more pronounced in expansion counties with previously high overdose rates (or more severe drug overdose problems) than those in other expansion states. Finally, I included all three interaction terms in column (13). The patterns in columns (10) to (12) are preserved in column (13). Hence, the results in this section are consistent with the implications of the stylized model in Section 3.

7 Conclusion

Deaths due to drug overdose have been rising dramatically in the U.S. over the past two decades. The conventional explanation places blame for this dramatic rise on the expansion of prescriptions and advertising of opioids in the 1990s. This “more prescriptions, more deaths” explanation has spurred federal and state governments to take measures to curtail opioid prescriptions and increase the cost of opioid production. Proponents of this view argue that policies restricting the supply of prescription opioids would reduce deaths due to overdose. However, a competing view, “more restrictions, more deaths,” holds that stringent restrictions on prescription opioids compel people to use illicitly manufactured drugs, which are more dangerous than legally prescribed versions, increasing the risk of overdose and death. Over the past decade, drug overdose mortality rates from heroin and synthetic drugs such as fentanyl have continued to rise despite reduced prescriptions. In addition, stringent restrictions on prescription opioids can result in undertreatment of pain, harm patients’ quality of life, and even drive some to commit suicide. Accordingly, loosening access to prescription opioids would disincentivize the use of illicitly manufactured drugs, curbing overdose deaths.

In this study, I investigated how the ACA’s Medicaid expansion affects drug overdose mortality rates. On the one hand, the expansion lowered the cost of opioid addiction treatment, helping to alleviate drug opioid dependence and overdose deaths. On the other hand, it reduced the cost of opioid prescriptions and thereby made opioids more accessible, possibly leading to increased addiction and deaths. Using a DiD framework, I estimated that the expansion increased drug overdose mortality rates by 0.881 per 100,000 people at the county and quarter level, over half of which were driven by opioids. This represents a 15.4% increase compared with the average drug overdose mortality rate in the expansion counties prior to the expansion—a sizable and statistically significant effect. An event study showed no significant trend in drug overdose mortality rates in the expansion counties relative to the non-expansion counties before the expansion, given other controls. However, this trend steadily increased after the expansion. Further analyses of the involved drugs suggested that the effects on opioid overdose deaths were almost all driven by illicitly manufactured opioids, such as heroin and synthetic opioids, rather than legally provided opioids. Moreover, potential mechanisms that connected the expansion to mortality rates were investigated. I found evidence that the expansion increased insurance rates (below 138% of the FPL), which reduced the mortality rates. I also found evidence suggesting that the expansion fueled the prevalence of illicitly manufactured drugs, thus raising mortality rates in the expansion counties compared with non-expansion counties.

To illustrate how the expansion can exacerbate overdose deaths from illicitly manufactured drugs in the presence of prescription opioid restrictions, in Section 3, I constructed a stylized model that has three implications for the heterogeneous effects across counties. To test the stylized model, I found that the effects of the expansion were less pronounced in expansion counties with higher increases in insurance or opioid prescription rates after the expansion compared to those in other expansion counties. In contrast, the effects were more pronounced in expansion counties with higher drug overdose mortality rates (more severe drug overdose problems) before the expansion. These results are consistent with the implications of the stylized model.

Taken together, this study's findings support the view of "more restrictions, more deaths" rather than the conventional view of "more prescriptions, more deaths." This study highlights the importance of policymakers assessing and weighing the costs and benefits of restricting legal access to opioids. Although greater access to prescription opioids may fuel opioid dependence and overdose, it may reduce pain, improve patients' quality of life, and curtail the prevalence of underground drug consumption. In addition, policymakers could consider a mixed strategy that targets the improper use of prescription opioids, simultaneously meeting the demand for prescription opioids and increasing the treatment for 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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Figures and Tables

Figure 1: Supply and Demand of Prescription Opioids Before and After the Expansion

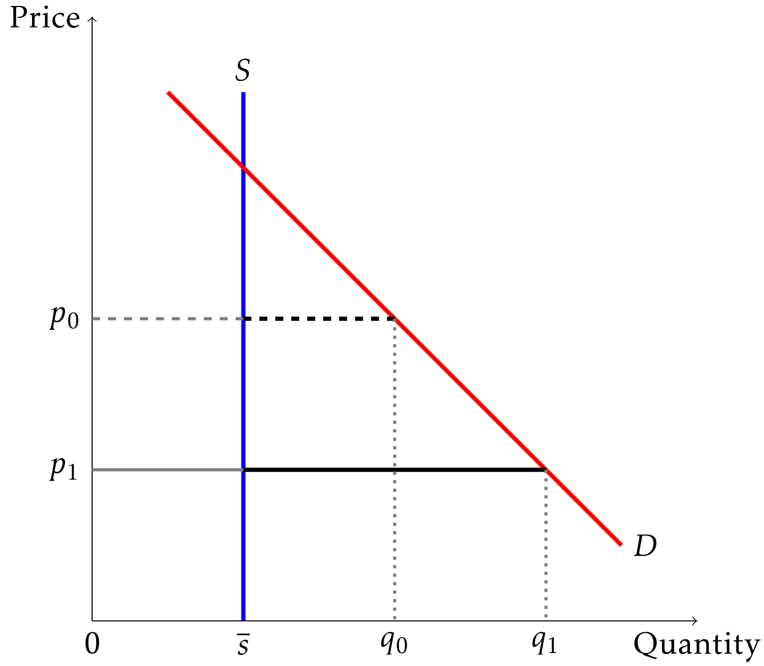
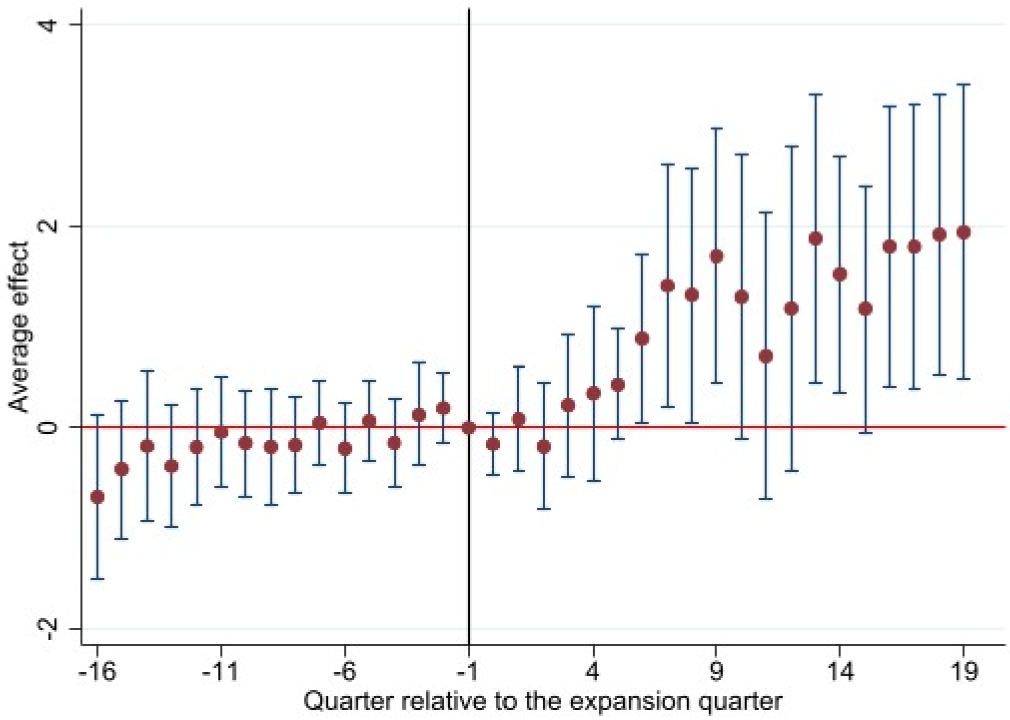
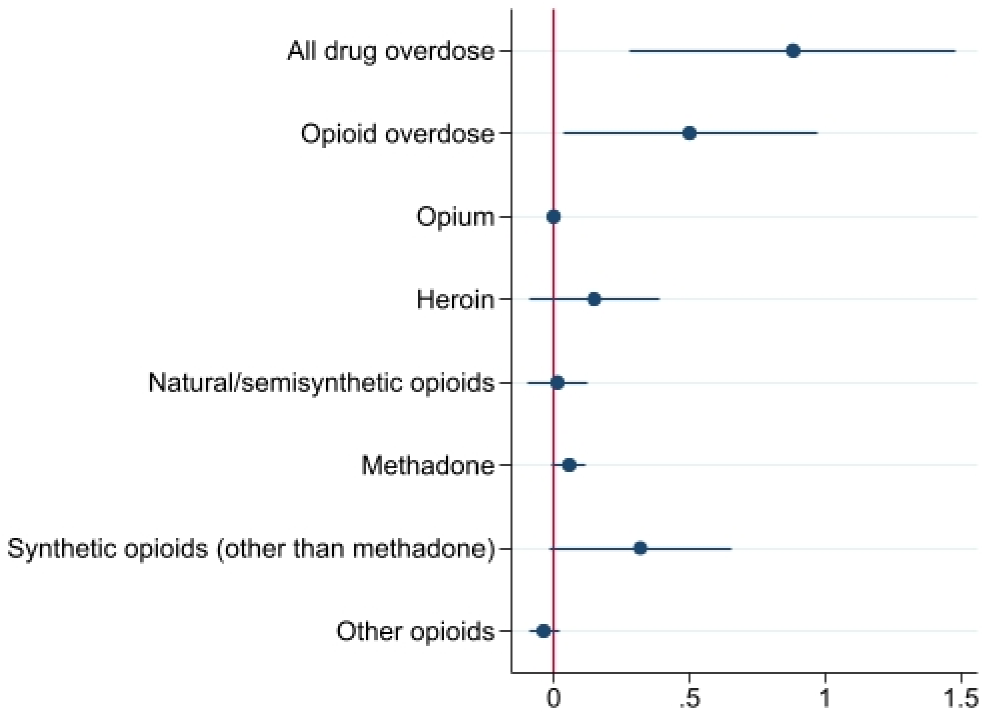


Figure 2: Lead and Lag Estimates for the Effect of the Affordable Care Act's Medicaid Expansion on Drug Overdose Mortality Rates



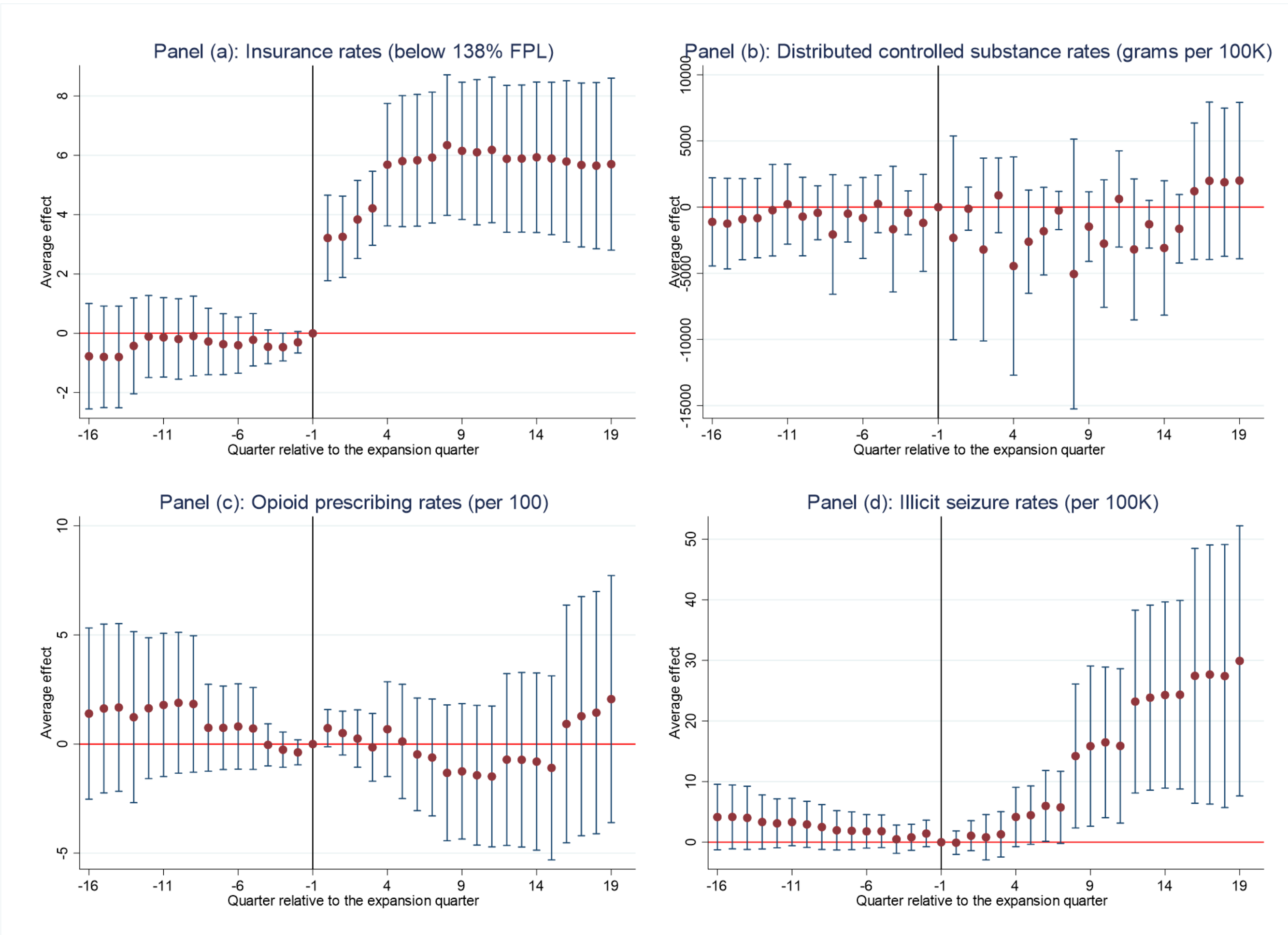
Notes: Dots show the lead-and-lag estimates. Vertical bars show 95% confidence intervals using standard errors clustered at the state level. The dependent variable is drug overdose mortality rates per 100,000 people aged 20–64 years. Covariates include control variables, county fixed effects, and quarter fixed effects. The regression was weighted by the county population aged 20–64 years. The estimate for the first lag (the last quarter before the expansion quarter) is anchored at zero, as indicated by the vertical line.

Figure 3: Effects of the Affordable Care Act's Medicaid Expansion on Mortality Rates Due to Different Drug Categories



Notes: Dots show the separately estimated effects of the expansion on mortality rates for different drug categories with the baseline specification. Horizontal bars show 95% confidence intervals using standard errors clustered at the state level. Covariates include control variables, county fixed effects, and quarter fixed effects. Each regression was weighted by the county population aged 20–64 years.

Figure 4: Lead and Lag Estimates With Different Potential Mediators as the Dependent Variable



Notes: Dots show the lead-and-lag estimates. Vertical bars show 95% confidence intervals using standard errors clustered at the state level. In each panel, the title indicates the dependent variable. Covariates include control variables, county fixed effects, and quarter fixed effects. Each regression was weighted by the county population aged 20–64 years. The estimates for the first lag (the last quarter before the expansion quarter) are anchored at zero, as indicated by the vertical lines.

Table 1: Variables, Units, and Data Sources

Variable(s)	Unit	Data Source
Mortality rates	county/quarter	National Vital Statistics System Multiple Causes of Death (MCOD) restricted-use data
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: ICD 10 Codes for Drug Overdose Deaths and the Involved Opioids

Cause of Death	Codes
Drug overdose	X40-X44, X60-64, X85, or Y10-Y14
Opioid overdose	
Opium	T40.0
Heroin	T40.1
Natural/Semisynthetic opioids	T40.2
Methadone	T40.3
Synthetic opioids (other than methadone)	T40.4
Other and unspecified opioids	T40.6

Table 3: Number of Expansion States by Expansion Quarter

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

Table 4: Effects of the Affordable Care Act's Medicaid Expansion on Drug Overdose Mortality Rates

	Drug overdose mortality rates				
	(1)	(2)	(3)	(4)	(5)
Expansion	1.168***	1.137***	0.859***	0.881***	0.823*
	(0.403)	(0.346)	(0.285)	(0.299)	(0.421)
Mean (2013)	5.653	5.653	5.653	5.714	5.700
Demographics	No	Yes	Yes	Yes	Yes
Other drug-related policies	No	No	Yes	Yes	Yes
Weighted by the population aged 20–64 years	No	No	No	Yes	Yes
Exclude later expansion states	No	No	No	No	Yes
Observations	113,044	112,888	112,888	112,888	98,568

Notes: The data are panel data with the unit of observation at the county/quarter level from 2010 to 2018. The dependent variable was the drug overdose mortality rate per 100,000 people aged 20–64 years. All columns include county and quarter fixed effects. The row "Mean (2013)" shows the average mortality rates in the expansion states in 2013. "Later expansion states" refer to expansion states that implemented the expansion after January 1, 2014. Standard errors are clustered at the state level and reported in parentheses. *$p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 5: Effects of the Expansion on Drug Overdose Mortality Rates With or Without Controlling for Potential Mediators

	Drug overdose mortality rates				
	baseline	(6)	(7)	(8)	(9)
Expansion	0.881***	1.112***	0.879***	0.863***	0.301
	(0.299)	(0.321)	(0.299)	(0.294)	(0.229)
Insurance rates (below 138% of the FPL)		-0.0427			
		(0.0398)			
Distributed controlled substance rates (grams per 100,000 people)			-0.00000268		
			(0.00000329)		
Opioid prescribing rates (per 100 people)				-0.0138	
				(0.00863)	
Illicit fentanyl seizure rates (per 100,000 people)					0.0537***
					(0.00680)
Illicit heroin seizure rates (per 100,000 people)					6.151***
					(1.585)
Observations	112,888	112,888	112,888	102,720	112,888

Notes: The data are panel data, with the unit of observation at the county/quarter level from 2010 to 2018. The dependent variable was the drug overdose mortality rate per 100,000 people aged 20–64 years. All columns include the control variables, county fixed effects, and quarter fixed effects. The regressions are weighted by the county's population aged 20–64 years. Standard errors are clustered at the state level and reported in parentheses. *$p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 6: Heterogeneous Effects

	Drug overdose mortality rates			
	(10)	(11)	(12)	(13)
Expansion × Δinsurance rates	-0.105**			-0.118***
	(0.0413)			(0.0324)
Expansion × Δprescribing rates		-0.0651***		-0.0373***
		(0.0182)		(0.0127)
Expansion × Mortality rates before 2014			0.285**	0.238*
			(0.126)	(0.121)
Expansion	2.848***	-0.437	-0.726	0.983
	(0.858)	(0.377)	(0.642)	(0.914)
Observations	112,888	98,716	112,788	98,676

Notes: The data are panel data, with the unit of observation at the county/quarter level from 2010 to 2018. The dependent variable was the drug overdose mortality rate per 100,000 people aged 20–64 years. All columns include the control variables, county fixed effects, and quarter fixed effects. The regressions are weighted by the county population aged 20–64 years. Standard errors are clustered at the state level and reported in parentheses. "Δinsurance rates" is the increase in insurance rates (below 138% of the FPL) between 2013 and 2017. "Δprescription rates" is the increase in opioid prescription rates between 2013 and 2017. "Mortality rates before 2014" are the average drug overdose mortality rates before 2014. *$p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.