Essays on Social Safety Net Programs and Drug Abuse

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Abstract

I study how public health policy and the social safety net shape drug use, access to treatment, and drug-related mortality in the United States. Across three chapters, I explore how Medicaid expansions and SNAP benefit generosity impact fatal drug overdoses and treatment for substance use disorder (SUD), highlighting both intended and unintended consequences.

In the first chapter, I examine the effects of the Affordable Care Act (ACA) Medicaid expansion on fatal drug overdoses and access to treatment. Using a difference-in-differences framework that accounts for staggered adoption and treatment effect heterogeneity, I find that overdose mortality increased following the expansion. At the same time, Medicaid expansion led to substantial increases in the use of medication-assisted treatment (MAT), including claims for Suboxone and Naltrexone. Treatment admissions rose most among new patients and those with extensive treatment histories, suggesting that improved access benefited both groups. The results highlight the complex relationship between expanding access to care and downstream health outcomes.

In the second chapter, I study how the ACA's 2014 provision allowing states to expand Medicaid coverage for certain controlled prescription drugs – specifically benzodiazepines – affected overdose mortality. Benzodiazepines are rarely fatal on their own but often co-ingested with opioids in overdose deaths. I find that states that expanded both Medicaid eligibility and drug coverage experienced significantly higher overdose death rates. The effects are most pronounced among men and individuals aged 30–64, and remain robust after accounting for other drug-related policies. These findings suggest that broader drug coverage, while improving access, may also introduce new risks.

In the third chapter, I examine how income support through the social safety net affects fatal drug overdose rates. Specifically, I study the relationship between changes in Supplemental Nutrition Assistance Program (SNAP) benefit levels and overdose mortality. SNAP is the largest federal nutrition assistance program in the U.S. and a central component of

economic support for low-income households. I exploit cross-state variation in how SNAP benefits are calculated and use a border-county design to compare overdose outcomes across adjacent counties facing different benefit levels. During periods of benefit increases, I find evidence of a decline in fatal overdose rates. However, this pattern does not reverse during periods of benefit decreases, where I do not observe a corresponding rise in overdose mortality. Notably, the subsequent fall in benefits is smaller than the preceding rise, lending further credence to the idea that benefit increases may help individuals reach a level of economic stability that buffers them against future reductions. This asymmetric pattern – where a period of decline follows a period of improvement – suggests a possible threshold effect in staying off drugs, whereby once a certain level of support is reached, individuals may be less vulnerable to relapse even if benefits later decrease. The overall effect is predominantly driven by males and remains robust to alternative definitions of the benefit increase period and broader definitions of local economic areas. These findings suggest that the generosity of income support programs can play a meaningful role in shaping drug-related mortality.

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Contents

Chapter 1

Heterogeneous Effects of ACA Medicaid Expansion on Drug Abuse

1.1 Introduction

The US is in the midst of a drug overdose epidemic. Drug overdose death rates have increased five-fold from 1980 to 2008 (Warner et al., 2011). In 2009, drug overdose deaths outnumbered deaths due to vehicular accidents for the first time. Furthermore, prescription drugs have been increasingly involved in these deaths (Paulozzi et al., 2011).

Given the scale of the drug overdose crisis, public health policies and social safety net programs play a crucial role in shaping access to treatment and prevention efforts. One such program is Medicaid, the second-largest expenditure among federal social programs. In 2022, total Medicaid spending reached approximately $805 billion (Theal and Judd, 2024). This places Medicaid ahead of other major programs like Medicare and SNAP in terms of federal spending, second only to Social Security. In this paper, I study how the ACA Medicaid expansion of 2010 affects outcomes related to drug abuse, while accounting for the heterogeneity in the program expansion. I will also focus on outcomes relating to treatments for substance use disorder (SUD).

In my difference-in-differences (DID) analysis, I utilize several sources of variation to assess the impact of the ACA Medicaid expansion. First, there is between-state variation, as I compare outcomes between states that adopted the Medicaid expansion and those that did not. Second, I incorporate time variation by examining the differences in outcomes before and after the Medicaid expansion in each state. A key feature of my analysis is the use of heterogeneous treatment timing, where different states implemented the Medicaid expansion at different times, introducing additional variation based on the timing of adoption. This

allows me to capture the varying effects of the ACA Medicaid expansion across states, accounting for the staggered rollout and differing implementation periods.

Using Gardner (2021) – which accounts for heterogeneous treatment effects as well as staggered treatment timings –, I find an increase in fatal drug overdoses by 30% following the ACA expansion, and that this effect is predominantly driven by increases for males and adults aged 20 to 49. Furthermore, I find evidence that the treatment effect plateaus three years after the expansion. My findings are robust to the inclusion of a host of state-level time-varying covariates, as well as the use of other DID estimators that account for the heterogeneity differently. I also find higher treatment admission rates and larger Medicaid claims for FDA-approved medications in expansion states. Lastly, while I find a general increase in treatment admissions across all groups, the largest increases are among individuals seeking treatment for the first time and individuals with a history of more than five prior treatment episodes. These findings highlight the complex nature of the ACA Medicaid expansion, which simultaneously increased access to treatment but also broadened access to prescription drugs, potentially contributing to the rise in fatal overdoses. The variation in effects across demographic groups and policies underscores the importance of considering both intended and unintended consequences of large-scale health policy changes.

My paper contributes to the existing body of literature in two aspects. Firstly, I add to the growing body of work that is studying the growing opioid epidemic and the sharp rise in fatal drug overdose. Saloner et al. (2018) found no evidence that the Medicaid expansion caused an increase in opioid deaths. Doleac and Mukherjee (2018) found that despite the ACA Medicaid expansion reducing out-of-pocket costs and improving access to Naloxone – a drug that counteracts an opioid overdose –, there was no observed decrease in opioid-related deaths. Conversely, Kravitz-Wirtz et al. (2020) found that the ACA Medicaid expansion resulted in a fall in heroin and synthetic opioid-related deaths, but an increase in methadone-related deaths. A common feature across most of the studies in this space is a difference-in-differences approach. However, as described in Section 1.3 below, due to the staggered roll-out of the

Medicaid expansions in different states, a generalized difference-in-differences approach will likely yield biased estimates. Instead, I adopt a two-stage difference-in-differences approach prescribed by Gardner (2021). Additionally, I account for heterogeneity in treatment effects by incorporating the Medicaid Federal Poverty Level (FPL) eligibility thresholds, as the magnitude of the ACA program expansion varies across states based on these thresholds, affecting the extent to which different states expanded Medicaid coverage.

Second, I contribute to the growing body of literature examining the impact of the ACA expansion on substance use disorder (SUD) treatments (Maclean and Saloner, 2019; Meinhofer and Witman, 2018; Sharp et al., 2018). My work is most closely related to Meinhofer and Witman (2018) who found an increase in SUD treatment admissions following the ACA expansion. I extend their findings in three important ways. First, they did not account for the heterogeneity in treatment timing due to the staggered rollout, nor did they consider the varying sizes of Medicaid expansions across states. Second, many studies in this area focus on a short time frame, typically from 2010 to 2015 (Maclean and Saloner, 2019; Sharp et al., 2018). While the ACA was announced in 2010, the Medicaid expansion did not take effect until 2014. By incorporating more pre- and post-expansion periods, I am able to better control for pre-existing trends and provide a more accurate analysis of the expansion's effects. Third, I differentiate between the types of individuals entering SUD treatment, examining both the extensive margin (new admissions) and the intensive margin (repeat admissions).

1.2 Data & Institutional Background

This section outlines the sources for my outcome and policy variables. Given the Difference-in-Differences research design, the primary measures of mortality and treatment-related outcomes are aggregated at the state level. The study covers the period from 2005 to 2019.

1.2.1 Medicaid

Medicaid is a joint federal and state program that provides health coverage to low-income individuals, including children, pregnant women, elderly adults, and people with disabilities. Established in 1965 under the Social Security Act, Medicaid is designed to assist those who meet specific income and eligibility requirements, with states having the flexibility to expand coverage and set eligibility criteria within federal guidelines. Medicaid plays a critical role in improving access to healthcare, particularly for vulnerable populations, by covering a wide range of services, including hospital care, prescription drugs, and mental health services.

The Affordable Care Act (ACA), passed in 2010, is a landmark piece of healthcare reform legislation aimed at expanding access to healthcare, reducing healthcare costs, and improving the quality of care in the United States. The ACA introduced several key provisions, including the creation of health insurance marketplaces, the expansion of Medicaid eligibility, and the implementation of protections for people with pre-existing conditions. One of the most significant components of the ACA was the expansion of Medicaid, which allowed states to extend coverage to low-income adults who were previously ineligible. Under the ACA, Medicaid expansion was designed to cover adults with incomes up to 138% of the Federal Poverty Line (FPL). Since Medicaid expansion under the ACA was not mandatory, it resulted in a staggered rollout across states, with some states adopting the expansion at different times. Additionally, states had flexibility in determining their eligibility thresholds, leading to variations in the size of the expansion.

To date, 39 states (including D.C.) have adopted the Medicaid eligibility expansion, with non-adoptees predominantly concentrated in the south, depicted in Figure 1. Notably, 25 states expanded Medicaid eligibility concurrently on the date the provision comes into force, in Jan 1st, 2014.

I use state-level information on the Medicaid eligibility threshold collected by the Kaiser Family Foundation. Information on the Medicaid FPL eligibility threshold is available from 2005 to 2019. I limit the analysis to 2019 to avoid potential distortions from the COVID-19

pandemic, which led to significant policy changes, temporary Medicaid expansions, and disruptions in healthcare access that could confound the effects of the ACA Medicaid expansion. Referencing Figure 2, there is notable heterogeneity in the scale of the Medicaid expansion. Although most expansion states extended coverage to 138% of the Federal Poverty Line (FPL), the size of the expansion varies due to differences in the eligibility thresholds prior to the expansion.

Referencing Table 1, the difference in the FPL between expansion states and non-expansion states are statistically significant in both pre and post-ACA. Crucially, this difference more than doubles after ACA.

1.2.2 Overdose Deaths

I employ restricted-use mortality data from the CDC, like other papers in the economics (Averett et al., 2019; Borgschulte and Vogler, 2020; Hollingsworth et al., 2017) and addiction literature (Cataife et al., 2021). Using the International Classification of Diseases (ICD-10), I construct a panel data set that contains yearly observations of the number of deaths due to drug overdose. This includes Unintentional Drug Overdose (ICD-10 Codes X40-X44), Drug Overdose (Suicide) (X60-X64), and Undetermined Drug overdose (Y10-Y14). Unlike the publicly available dataset, the restricted-use mortality data provides individual-level records, encompassing all recorded deaths in the United States.[1] By virtue of my research design, I aggregate these deaths due to drug overdose to the state level. The CDC began using a standardized data storage convention starting in 2005. To avoid potential measurement error due to inconsistencies in earlier data, I exclude observations from 2002 to 2004.

In total, I have a balanced panel of 735 observations from 49 states (including D.C.) from 2005 to 2019. Referencing Table 2, the mean rate of fatal drug overdoses per 100,000 individuals in non-expansion states is 14.52, while in expansion states, it is 17.63 (approximately

[1]Note that some overdose deaths may be misclassified or underreported due to incomplete toxicology reports or limitations in death certificate data. See Buchanich et al. (2018) or Ruhm (2018) for more details.

21% higher). The difference in mean overdose death rates is statistically significant at the 5% level. Table 3 in the Appendix presents death rates by gender. In expansion states, overdose rates are higher for both males and females, and the difference is statistically significant. However, male overdose rates are approximately 30% higher, compared to an 8% increase for females. Looking at death rates by age group in Table 4 in the Appendix, I note that the death rates for all age groups are higher in expansion states, with the difference being statistically significant for those aged 20 to 64, with the largest difference observed in death rates for young adults (aged 20-29) where death rates are 35% higher in expansion states.

1.2.3 Treatment Admissions

The Treatment Episode Data Set (TEDS-A) is a national data collection effort managed by the Substance Abuse and Mental Health Services Administration (SAMHSA). It compiles information on admissions to substance abuse treatment facilities across the United States. State laws require substance abuse treatment programs to report publicly funded admissions. Therefore, this data set does not capture admissions into privately-funded substance abuse treatment centers in the US. Given that the demographic of study are the Medicaid recipients, this exclusion of admissions to private treatment centers is possibly not a first order concern given the significant out-of-pocket costs are often prohibitive (SAMHSA, 2021).

Furthermore, since the ACA Medicaid expansion improves access to medicine-assisted treatments for substance use disorder, I will focus solely on admissions for these treatments, rather than other types of care – such as detoxification, behavioral therapy, counseling, and support groups – that individuals may receive at treatment centers. In 2019, only 13.7% of total admissions to treatment centers were for medication-assisted treatment for substance use disorders. Note that for my purposes, in the aggregate, the data set contains the total number of admissions for medicine-assisted treatments in a year for each state. This means that if an individual gets treated 3 different times during the year for substance abuse, the

data set will record that individual thrice. Just like in Powell et al. (2020), the substance abuse treatment admissions data can therefore be used as an alternative measure of the prevalence of the drug overdose problem.

Owing to irregular reporting by some states, I have an unbalanced panel of 47 states (Arkansas excluded), with a total of 589 observations. Referencing Table 5, on average, treatment admission rate (per 100,000) is significantly higher in expansion states, and this difference is statistically significant at the 5% level.

1.2.4 Medicines for SUD Treatment

Since this study focuses on Medicaid-funded treatments for substance use disorder (SUD), an alternative approach to measuring their prevalence is by analyzing total claims – measured in both unit counts and dollar value – for FDA-approved treatment drugs, as reported in the Medicaid State Drug Utilization dataset. I specifically examine three FDA-approved drugs: Methadone, Suboxone, and Naltrexone. I will look at both counts and value, measured in millions of units and millions of dollars respectively. I report the summary statistics in the Appendix, in Table 6. I find that in expansion states, the total units claimed are higher for two out of the three FDA-approved drugs, with this difference being statistically significant at the 5% level. Additionally, the total claim value – measured in millions of dollars – is higher for all three drugs in expansion states, with the difference being statistically significant at the 5% level.

1.3 Empirical Strategy

The existing economic literature studying the drug epidemic mostly relies on state-level comparisons using a difference-in-differences approach (Averett et al., 2019; Ghosh et al.,

2019; Kim, 2021; Meinhofer and Witman, 2018; Sacks et al., 2021). Using a treatment indicator for the ACA expansion, ACA_{st}, which takes the value 1 when a state expands its Medicaid program, and zero otherwise, the specification is as follows:

$$y_{st} = \beta_0 + \beta_1 \text{ACA}_{st} + \theta_s + \gamma_t + \varepsilon_{st}$$

where y_{st} is the outcome in state s at time t, and θ_s and γ_t are the state and time fixed effects respectively. State fixed effects control for all time-invariant characteristics, such as state-level policies (other than the treatment studied), geographical features, and cultural and demographic differences that do not vary over time. Including state fixed effects ensures that the treatment effect is not driven by these unobservable, state-specific characteristics that could otherwise bias the results. Time fixed effects control for common time trends and national macroeconomic conditions that change over time, ensuring that the estimated treatment effects are not influenced by global or national trends affecting all states similarly during the study period. The coefficient $\hat{\beta}_1$ captures the effect of the ACA expansion on outcomes. The key identifying assumption of a regular difference-in-differences approach is the parallel trends assumption, which posits that in the absence of the treatment, the treated and untreated states would have followed the same trend in their outcomes over time. Additionally, the assumption requires that there are no anticipatory effects of the treatment.

However, as shown in Goodman-Bacon (2021), the two-way fixed effects (TWFE) estimator will produce biased estimates if there are heterogeneous treatment effects and staggered treatment timing. To correct for this bias, I adopt a two-stage difference-in-differences (2SDID) estimator put forth by Gardner (2021). Intuitively, the 2SDID approach addresses the bias since the second stage regression measures the difference between treatment and control groups, after removing group and period effects, much like how a second stage regression equation in a standard two-stage least squares framework measures the effect of

the fitted value of the endogenous variable after removing the effect of the instrument. In the first stage, group and time fixed effects are identified using the sample of untreated and not-yet-treated observations.

$$y_{st} = \theta_s + \gamma_t + \varepsilon_{st}$$

where y_{st} is the outcome variable for state s at time t, and θ_s and γ_t are the state and year fixed affects respectively. Once the fixed effect parameters are estimated, in the second stage, the ATE is estimated by comparing the treated with the untreated/not-yet-treated outcomes after removing group and time fixed effects, with the following specification

$$\hat{\varepsilon}_{st} = \beta \cdot \text{ACA}_{st} + \Gamma \boldsymbol{X}_{st} + \nu_{st}$$

where $\boldsymbol{X}_{st}$ is a vector of time-varying state-level covariates. Crucially, under Gardner (2021), his estimation procedure reduces the reliance on the strict parallel trends assumption by accounting for staggered treatment and overlapping treatment-control periods, adjusting for pre-existing trends even if they diverge across units before treatment. In short, Gardner (2021) allows for differences in pre-treatment trends across units, as long as these differences are consistent over time.

That being said, I will also use Callaway and Sant'Anna (2021) to estimate the dynamic effects over time, rather than a single treatment effect as in Gardner (2021). While the two approaches differ slightly in their identifying assumptions – Callaway and Sant'Anna (2021) estimate treatment effects separately for each cohort, requiring the parallel trends assumption to hold within each treatment group – I will show that my estimates remain largely consistent across both methods of accounting for treatment effect heterogeneity.

Since I am also interested in accounting for the size of the ACA expansion, my preferred specification includes a variable that reflects the state's Medicaid FPL. Rather than including the Medicaid FPL variable directly, I use the change in Medicaid FPL, denoted as $\Delta\text{MedicaidFPL}_{st}$, which represents the difference between the Medicaid FPL of state s at

time t and a benchmark FPL prior to the ACA expansion. My results are robust to different choices of this benchmark, whether it is the earliest pre-period FPL available in my sample (2005) or the average FPL across all periods before 2014, when the ACA expansion occurred. A larger coefficient on the ΔMedicaidFPL interaction term thus indicates a state undergoing a more significant Medicaid expansion.

1.4 Results

In this section, I present the results from my main specification, using fatal drug overdoses as the primary outcome variable. Additionally, I examine treatment-related outcomes for substance use disorders.

1.4.1 Fatal Drug Overdose

Table 7 presents the results using a standard difference-in-differences approach in Column (1) and the Gardner (2021) 2SDID method in Column (2), with fatal overdose rates (per 100,000) as the outcome variable, following convention in the literature. The treatment effect is consistently positive and statistically significant. Referencing Column (2), I find that death rates increased by 5.03 per 100,000 following the ACA, or about 30% of the mean. Notably, the 2SDID estimates are almost twice as large as those from the generalized DID model. This suggests that relying solely on a generalized DID without accounting for staggered treatment timing and heterogeneous treatment effects may lead to an underestimate of the true effect. Although the interaction term is positive in both instances, it remains statistically insignificant even at the 10% level indicating that the size of the expansion does not appear to drive the increase in overdose rates.

A potential concern is that the larger magnitude may be influenced by the choice of the Gardner (2021) estimation procedure. To address this, I estimate the treatment effect

using the more widely adopted Callaway and Sant'Anna (2021) method and present the results in Figure 3 of the Appendix. The findings yield estimates of a similar magnitude (approximately 6.1 per 100,000). Additionally, I also find that the treatment effect stabilizes after two years of treatment. Repeating this analysis by gender, I find that increases in male overdose rates drive about 87% of the overall effect, as shown in Figure 4 and Table 8 of the Appendix. This aligns with previous findings by Wehby and Lyu (2018), who showed that uninsured rates among men declined more sharply than those among women following the ACA, suggesting that Medicaid expansion may have had a larger impact on men's healthcare access and, consequently, overdose rates. Repeating the analysis by age group, I find statistically significant effects when using death rates for adults aged 20 to 49, with the largest effect observed among young adults aged 20–29. In this group, overdose rates increased by 1.26 per 100,000 (approximately 46% of the mean), as shown in Table 9. Together, the increase in death rates for adults aged 20 to 49 accounts for about 81% of the overall rise. Notably, using the event study specification by Callaway and Sant'Anna (2021) I find positive and statistically significant treatment effects for all adults over age 20, as shown in Figure 5 in the Appendix.

1.4.2 Treatment Admissions

In this section, the outcome variable is medication-assisted treatment (MAT) admission rates (per 100,000). I present only the results based on the Gardner (2021) estimation, as that is the chosen empirical strategy. In Column (1) of Table 10, I observe a positive and statistically significant treatment effect, with the ACA expansion leading to a threefold increase in MAT admission rates. In Column (2), the interaction term with ΔMedicaidFPL is negative and statistically insignificant at the 10% level. This means that I do not find any evidence that states that underwent a larger Medicaid expansion saw differing changes to treatment admission rates. Using data from the University of Kentucky Center for Poverty

Research (UKCPR) on the number of Medicaid recipients, my results suggest that for every 11 to 15 new Medicaid enrollees, there is one additional MAT admission directly attributable to the expansion.

SAMHSA also collects data on the number of previous treatment episodes a client has had when admitted to a new treatment program. This enables me to categorize treatment admissions based on the number of prior episodes: no prior episodes, one, two, three, four, or more than five. This breakdown allows me to examine the dynamics of who enrolls in treatment following the ACA expansion – whether they are individuals seeking treatment for the first time or individuals with prior treatment experiences. I repeat my analysis with the 6 different outcome variables and report my findings in Table 11 of the Appendix. When calculating the treatment effect as a percentage of their respective means, I find that the largest effect occurs in admission rates for individuals with no prior treatment episodes (i.e., new users of treatment) and individuals with 5 or more previous treatment episodes. These findings suggest that the ACA expansion impacts both the extensive margin (new admissions) and the intensive margin (repeat admissions) for medication-assisted treatments for substance use disorder.

Next, I examine whether the ACA expansion led to substitution effects between treatment types, where individuals shifted from other treatment options (such as detoxification or rehabilitation) to MAT. The TEDS-A dataset identifies eight alternative treatment options for drug abuse. Using admission rates for each treatment type as the outcome variable, I repeat my analysis and report the findings in Table 12 of the Appendix. I find that admission rates more than doubled for four out of the eight alternative treatment options. While the increase is substantial, it remains significantly smaller than the threefold increase observed for MAT admissions. To further investigate this, I use the share of total treatment admissions as the outcome variable and repeat the analysis. Results are presented in Table 13 of the Appendix. I find strong evidence of substitution away from non-intensive ambulatory services and short-term rehabilitation/detoxification, and toward medication-assisted

treatment (MAT). Notably, both are non-acute treatment options aimed at helping individuals discontinue drug use – a role that MAT fulfills. I also observe substitution away from hospital-based detoxification services, which differ from the others in that they are classified as acute care and typically reserved for individuals experiencing severe medical complications during withdrawal.

These findings suggest two possible interpretations. First, individuals may be shifting from other detoxification services to MAT due to its increased affordability and accessibility under the ACA. Second, as MAT becomes more accessible, there is some evidence that individuals are engaging in treatment earlier, potentially avoiding more severe, acute-care interventions. This trend is further illustrated in the raw data (Figure 6 in the Appendix), which shows a sharp increase in MAT's share of total treatments beginning around 2014.

1.4.3 Medicines for SUD Treatment

This section analyzes outcomes for three FDA-approved drugs: Methadone, Suboxone, and Naltrexone, examining both total units claimed (in millions) and claim value (in millions of dollars). The results presented here are based solely on the Gardner (2021) estimation procedure. I find statistically significant and positive effects for two of the three drugs, whether measured in units claimed or total claim value, per Table 14. Unlike the case where overdose rates are the outcome variable and the treatment effect plateaus after two years, I find that the treatment effect here continues to increase over time, as shown in Figure 7 of the Appendix.

I find that the units prescribed and the value claimed for both Suboxone and Naltrexone more than doubled following the ACA expansion, while the units of Methadone prescribed fell by 60%. One possible reason for this is that Methadone is the cheapest of the three drugs, but it also carries a higher risk of misuse. With Medicaid coverage of MAT effectively equalizing copayments across the different drugs, individuals are likely choosing the alter-

natives, whereas, in the past, the higher costs of Naltrexone and Suboxone (which are 3-5 times more expensive for the uninsured) would have been prohibitive.

1.5 Conclusion

This paper examines the effects of the ACA Medicaid expansion on both fatal drug overdoses and access to substance use disorder (SUD) treatment. By leveraging variation in the timing and scale of Medicaid expansion across states, I document significant increases in both overdose mortality and treatment utilization following the expansion. Failing to account for staggered treatment timing and heterogeneous treatment effects leads to an underestimation of the true impact. Additionally, I find little evidence that the size of the Medicaid expansion influenced outcomes. The largest increases in treatment rates are observed among new drug users seeking treatment for the first time, as well as chronic users with more than five prior treatment episodes. This suggests that improved access to SUD treatment benefits both first-time and recurring patients. I also find evidence of a substitution effect toward MAT from other treatment types following the ACA expansion, along with changes in the types of drugs claimed under Medicaid. Specifically, I observe a decline in claims for Methadone and an increase in claims for Suboxone and Naltrexone. This shift is likely due to Medicaid equalizing copayments across these drugs, prompting individuals to opt for the alternatives with fewer side effects. While the Medicaid expansion successfully increased treatment admissions and claims for FDA-approved SUD medications, the concurrent rise in overdose deaths underscores the complexity of policy interventions in addressing the opioid crisis.

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1.7 Tables

Table 1: Summary Statistics for Medicaid FPL Policy Variable

Policy Variable (ppt) Medicaid FPL	(1) Expansion States	(2) Non-expansion States	(3) Difference
Pre-2014 Mean	99.5	66.5	33.0***
	(55.9)	(41.5)	(2.33)
Post-2014 Mean	131.3	49.2	82.1***
	(29.3)	(27.5)	(1.64)

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 2: Summary Statistics for Outcome Variables

Outcome Variable (per 100,000)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Drug Overdose Rates	17.63	14.52	3.11***
	(8.58)	(6.65)	(0.606)
N	465	270	735

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 3: Summary Statistics for Death Rates (By Gender)

Outcome Variable (per 100,000)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Male Overdose Death Rates	11.28	8.63	2.65***
	(6.01)	(4.69)	(0.45)
N	465	270	735
Female Overdose Death Rates	6.35	5.89	0.46***
	(2.79)	(2.20)	(0.20)
N	465	270	735

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 4: Summary Statistics for Death Rates (By Age Group)

Outcome Variable: Death Rates per 100,000	(1) Expansion States	(2) Non-expansion States	(3) Difference
Age <20	0.31	0.29	0.015
	(0.161)	(0.15)	(0.012)
Age 20-29	3.01	2.23	0.79***
	(1.60)	(1.06)	(0.11)
Age 30-39	4.15	3.15	1.00***
	(2.70)	(1.52)	(0.18)
Age 40-49	4.50	3.68	0.82***
	(2.01)	(1.34)	(0.14)
Age 50-64	4.82	4.36	0.46**
	(2.49)	(3.18)	(0.21)
Age 65+	0.84	0.81	0.025
	(0.46)	(0.67)	(0.042)
N	465	270	735

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 5: Summary Statistics for Outcome Variables

Outcome Variable (per 100,000)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Treatment Admission Rate	270.26	31.65	238.61***
	(619.50)	(52.66)	(43.69)
N	387	202	589

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 6: Summary Statistics for Drug Counts & Value

Outcome Variable (million units)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Methadone	26.70	37.29	10.59
	(83.20)	(118.64)	(8.48)
Suboxone	3.94	1.01	2.93***
	(6.52)	(1.57)	(0.474)
Naltrexone	1.53	0.49	1.04***
	(2.96)	(0.83)	(0.217)
Outcome Variable (million $)			
Methadone	0.235	0.122	0.113***
	(0.469)	(0.207)	(0.035)
Suboxone	10.19	2.62	7.57***
	(16.68)	(4.09)	(1.212)
Naltrexone	0.139	0.056	0.0829***
	(0.190)	(0.0676)	(0.0140)
N	385	195	580

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 7: Baseline Regression Estimates

Outcome: Death Rate (per 100,000)	(1) DID	(2) 2SDID
ACA	3.716**	5.034***
	(1.631)	(1.896)
ACA $\times \Delta$MedicaidFPL	0.001	0.028
	(0.020)	(0.030)
N	735	735
Mean Outcome	16.49	16.49

Note: Standard errors in parentheses are clustered by state. Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Regression Estimates (by Gender)

Outcome: Death Rate (per 100,000)	(2) Male	(3) Female
ACA	4.414***	0.620
	(1.437)	(0.505)
ACA $\times$ ΔMedicaidFPL	0.0118	0.0164**
	(0.0221)	(0.0083)
N	735	735
Mean Outcome	10.30	6.18

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses. Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

Table 9: Regression Estimates using Death Rates (By Age Group) as the Outcome Variable

Outcome: Death Rate (per 100,000)	(1) Under 20	(2) Age 20-29	(3) Age 30-39	(4) Age 40-49	(5) Age 50-64	(6) Age 65+
ACA	0.0344	1.2643***	1.7207***	1.1028***	0.8444	0.0676
	(0.0272)	(0.3055)	(0.4966)	(0.4058)	(0.8075)	(0.1533)
ACA $\times \Delta$MedicaidFPL	0.0005	0.0037	0.0097	0.0093	0.0046	0.0004
	(0.0003)	(0.0050)	(0.0088)	(0.0075)	(0.0087)	(0.0013)
N	735	735	735	735	735	735
Mean Outcome	0.30	2.72	3.78	4.20	4.65	0.83

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses. Estimation done using Gardner (2021). Here, ΔMedicaidFPL$_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

Table 10: Regression Estimates for Treatment Admission Rate

Outcome: Treatment Admission Rate (per 100,000)	(1)	(2)
ACA	470.66***	589.23***
	(159.00)	(153.11)
ACA $\times$ ΔMedicaidFPL		-3.787
		(2.45)
N	589	589
Mean Outcome	189.68	189.68

Note: Standard errors in parentheses are clustered by state. Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 11: Regression Estimates using Treatment Admission Rates as Outcome Variable (6 Categories)

Outcome Variable (Treatment Admission Rates)	(1) 0 Prior	(2) 1 Prior	(3) 2 Prior	(4) 3 Prior	(5) 4 Prior	(6) $\geq$ 5 Prior
ACA	155.143***	116.775***	82.056***	59.193***	35.317***	140.744***
	(53.640)	(35.318)	(23.240)	(15.572)	(7.996)	(34.353)
ACA $\times$ ΔMedicaidFPL	-0.677	-0.436	-0.438	-0.469*	-0.315***	-1.453***
	(0.860)	(0.577)	(0.381)	(0.241)	(0.120)	(0.465)
N	589	589	589	589	589	589
Mean Outcome	50.30	43.26	29.43	19.26	11.09	36.35

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses.Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

Table 12: Regression Estimates using Alternative Treatment Admission Rates as Outcome Variable

Outcome Variable (Treatment Admission Rates)	(1) Ambul Detox	(2) Ambul Intensive	(3) Ambul Non-Intensive	(4) Detox Hospital
ACA	0.160	144.715**	171.687**	3.809
	(1.995)	(65.480)	(76.016)	(8.527)
ACA × ΔMedicaidFPL	0.028	-1.608	0.234	-0.046
	(0.037)	(1.044)	(1.055)	(0.109)
N	674	674	674	674
Mean Outcome	3.56	88.24	201.12	6.82
Outcome Variable (Treatment Admission Rates)	(5) Detox Residential	(6) Rehab Short	(7) Rehab Long	(8) Rehab Hospital
ACA	154.174	76.925***	77.341**	4.231
	(97.999)	(25.786)	(34.026)	(2.606)
ACA × ΔMedicaidFPL	-1.310	-0.764*	-1.055*	-0.043
	(1.093)	(0.440)	(0.559)	(0.039)
N	674	674	674	674
Mean Outcome	90.95	78.34	44.88	2.25

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses.Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

Table 13: Regression Estimates using Alternative Treatment Shares as Outcome Variable

Outcome Variable (Treatment Shares)	(1) Ambul Detox	(2) Ambul Intensive	(3) Ambul Non-Intensive	(4) Detox Hospital	
ACA	-0.294	-2.033	-10.807***	-0.775**	
	(0.187)	(2.797)	(2.966)	(0.383)	
ACA × ΔMedicaidFPL	0.003	-0.022	0.132***	0.001	
	(0.002)	(0.047)	(0.042)	(0.003)	
N	674	674	674	674	
Mean Outcome	0.53	13.62	34.56	1.19	
Outcome Variable (Treatment Shares)	(5) Detox Residential	(6) Rehab Short	(7) Rehab Long	(8) Rehab Hospital	(9) MAT
ACA	-2.241	-4.807**	0.575	0.048	20.334***
	(2.569)	(2.035)	(1.729)	(0.085)	(4.114)
ACA × ΔMedicaidFPL	0.000	0.036	-0.021	0.001	-0.130**
	(0.036)	(0.036)	(0.027)	(0.004)	(0.064)
N	674	674	674	674	674
Mean Outcome	13.78	13.35	7.26	0.39	15.41

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses.Estimation done using Gardner (2021). Here, $\Delta\text{MedicaidFPL}_{st}$ is defined as the difference between Medicaid FPL of state s at time t and its average FPL pre-2014. My findings are robust to alternative specifications of the benchmark FPL.

Table 14: Regression Estimates using Medicines for SUD Treatment as the Outcome Variable

Outcome Variable (million units)	(1) Methadone	(2) Suboxone	(3) Naltrexone
ACA	-18.135**	5.036**	3.388***
	(9.119)	(1.982)	(0.879)
ACA × ΔMedicaidFPL	0.247	-0.011	-0.017*
	(0.254)	(0.027)	(0.010)
N	580	580	580
Mean Outcome	30.26	2.96	1.18
Outcome Variable (million $)	Methadone	Suboxone	Naltrexone
ACA	0.061	12.434***	0.183***
	(0.046)	(4.582)	(0.048)
ACA × ΔMedicaidFPL	0.001	-0.021	-0.001*
	(0.001)	(0.065)	(0.001)
N	580	580	580
Mean Outcome	0.197	7.64	0.111

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses. Estimation done using Gardner (2021).

1.8 Figures

Figure 1: Map of ACA Medicaid Expansion

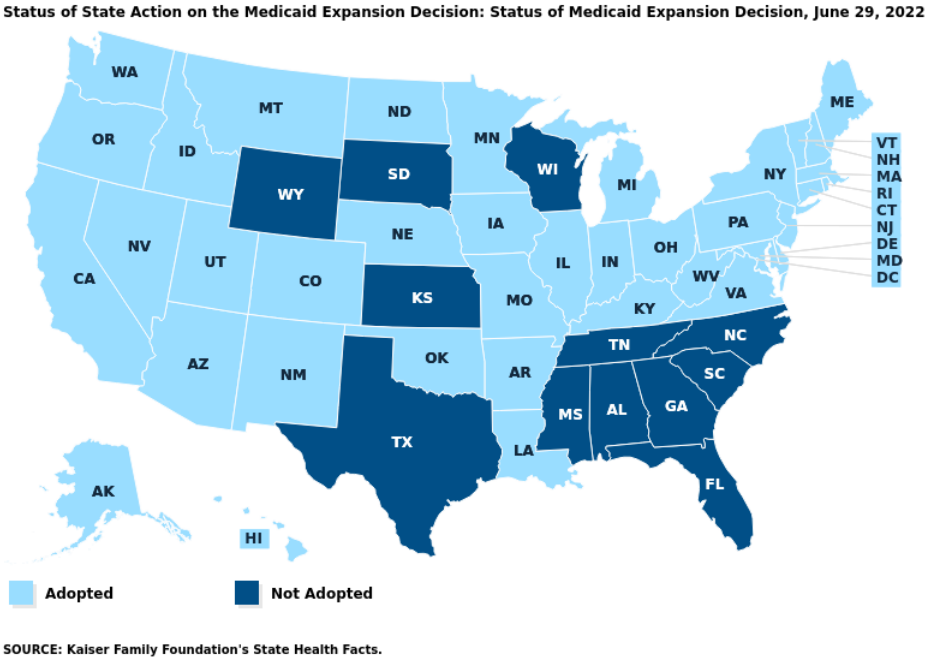


Figure 2: Plot of Medicaid FPL, By State

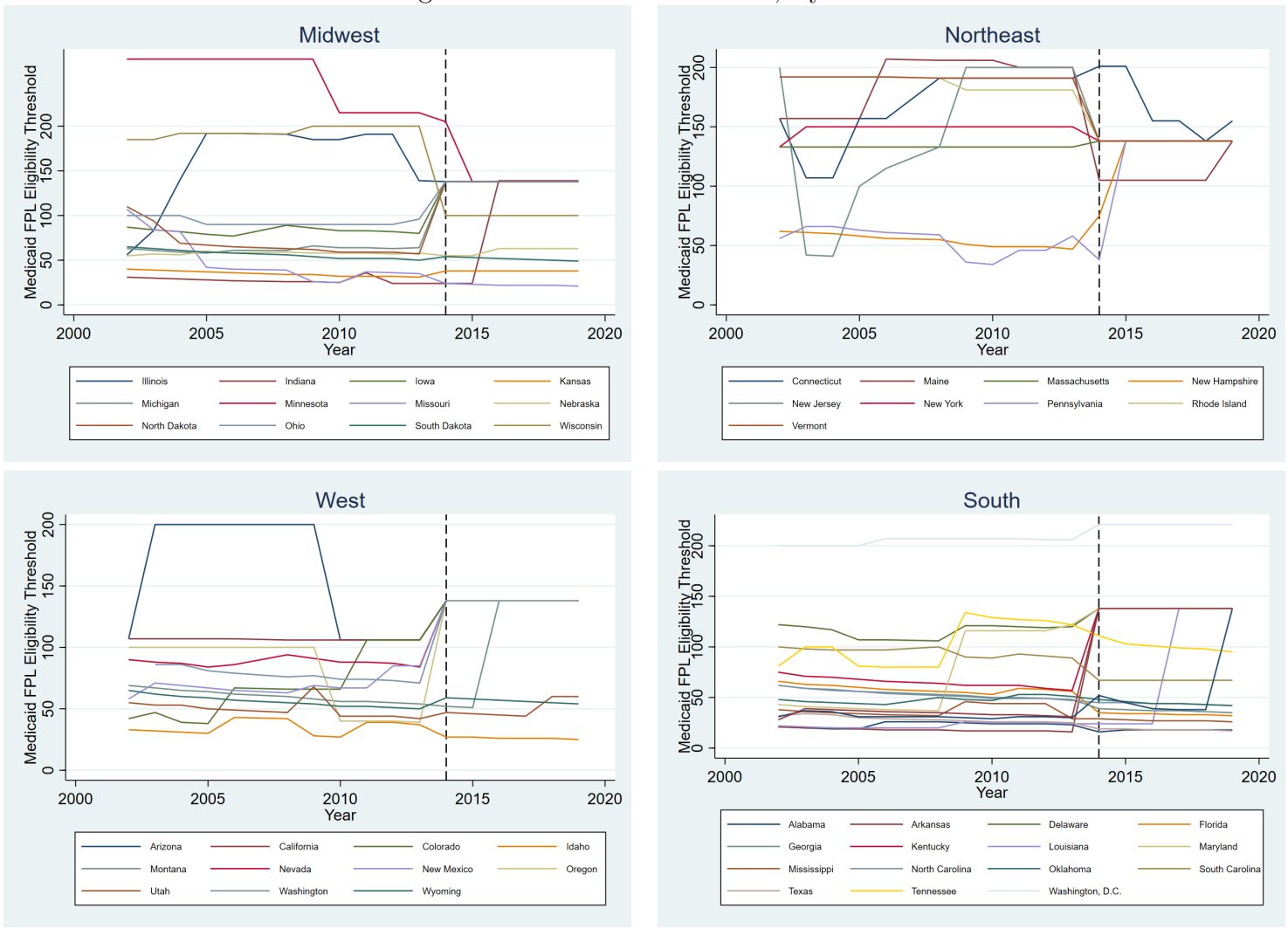


Figure 3: Event Study using Callaway and Sant'Anna (2021)

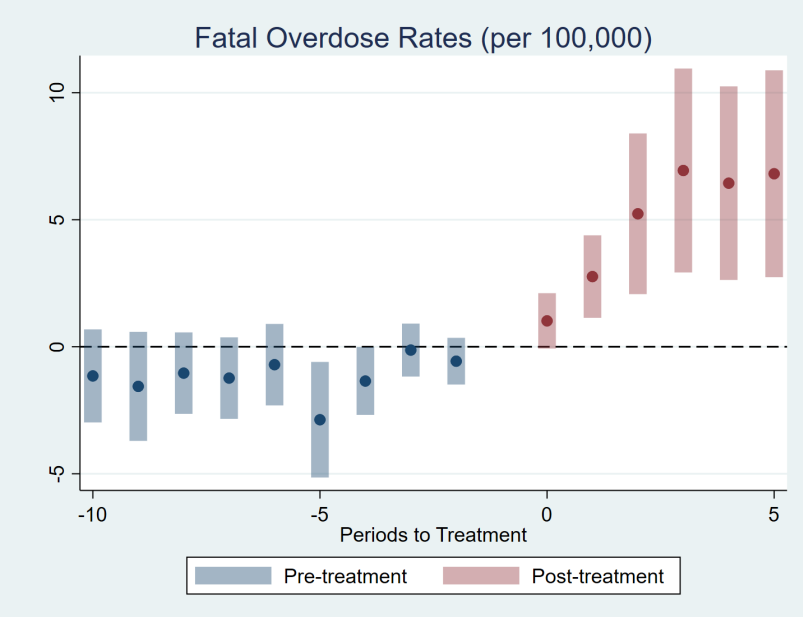


Figure 4: Event Study (by Gender) using Callaway and Sant'Anna (2021)

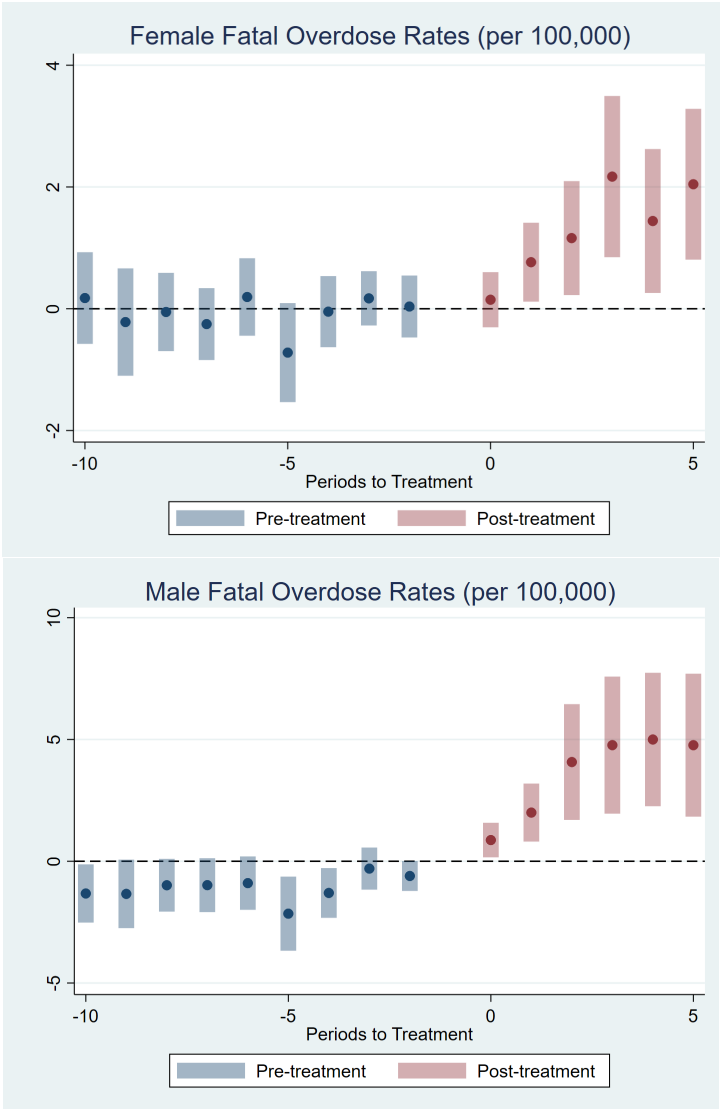


Figure 5: Event Study (by Age Groups) using Callaway and Sant'Anna (2021)

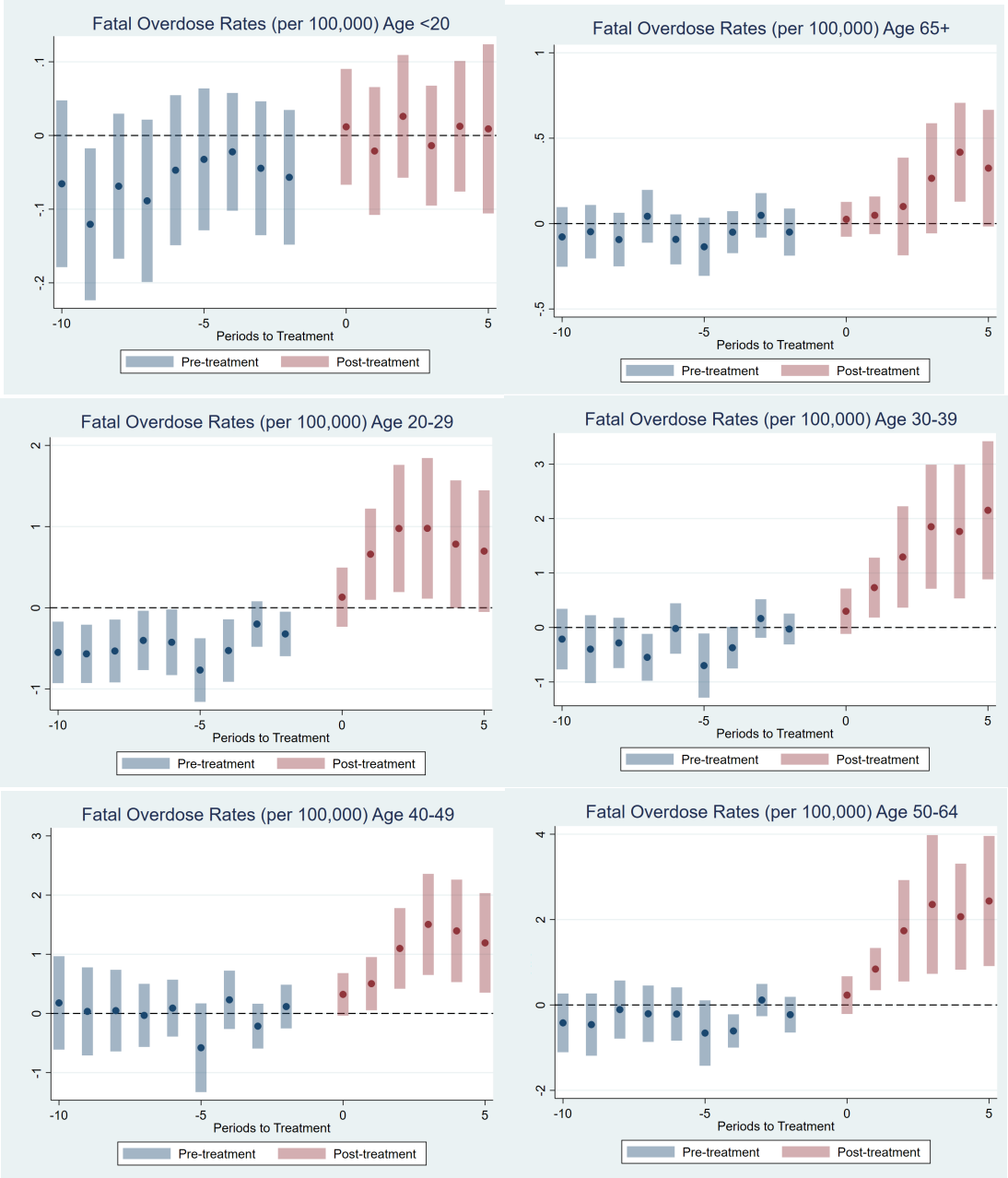


Figure 6: Plot of Shares of Treatment Types over Time

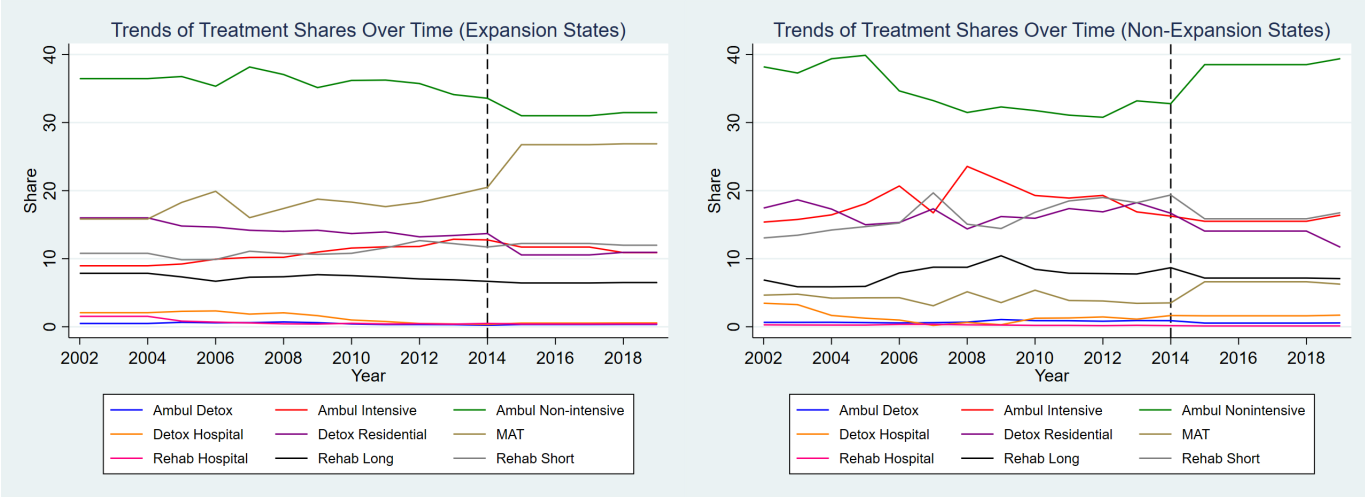
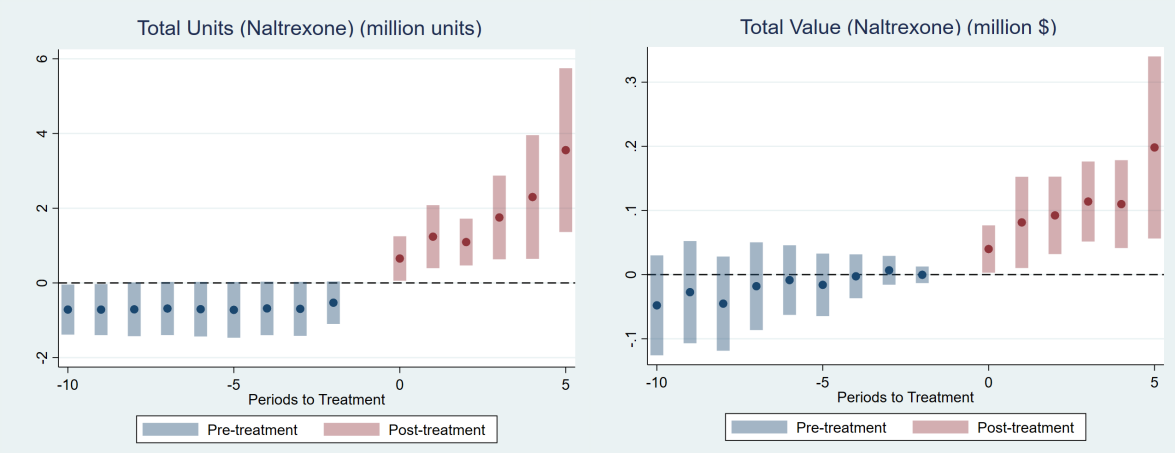


Figure 7: Event Study (Naltrexone) using Callaway and Sant'Anna (2021)



Chapter 2

Benzodiazepine Access and Drug Overdose: Evidence from the ACA Medicaid Expansion

2.1 Introduction

The United States is grappling with a persistent and worsening drug overdose epidemic. Between 1999 and 2021, drug overdose death rates increased fivefold (National Institute on Drug Abuse, 2022), and in 2009, overdose fatalities surpassed motor vehicle accident deaths for the first time. Prescription drugs have played an increasing role in this crisis, contributing to a significant share of overdose-related deaths (Paulozzi et al., 2011).

Medicaid, the nation's largest public health insurance program, plays a critical role in shaping access to prescription medications. In 2017, the federal government spent $581.9 billion on Medicaid, with $29.1 billion – roughly 5.1% of total spending – allocated to outpatient prescription drugs (CMS, 2018; MACPAC, 2019). Notably, gross drug spending within Medicaid has been rising steadily since 2014, reflecting the growing role of prescription drugs in the healthcare system. Understanding the implications of this expansion is crucial, particularly in the context of its potential influence on drug misuse and overdose trends.

Benzodiazepines offer a unique lens through which to study the effects of Medicaid expansion on drug overdose rates due to their widespread role in polysubstance abuse and their inclusion in Medicaid coverage following the Affordable Care Act (ACA). A 2014 provision of the ACA gave states the option to cover benzodiazepines, smoking cessation drugs, and barbiturates under Medicaid, significantly increasing access to these medications. Benzodi-

azepines are legally prescribed for anxiety disorders, insomnia, muscle spasms, and seizure disorders. They are also used for anesthesia and alcohol withdrawal management. However, despite their therapeutic benefits, benzodiazepines carry significant risks, particularly when misused. While benzodiazepines can be lethal in high doses, the vast majority of overdose deaths involving these drugs occur in combination with other substances. They are frequently co-used with opioids to enhance intoxication (Jones et al., 2012), with methadone to amplify its potency, or with cocaine to counteract its adverse effects (DEA, 2019). In 2018, benzodiazepines, opioids, and stimulants were the most commonly reported drugs in both single- and polysubstance overdose-related emergency department visits (Pickens et al., 2022). More recently, in the first half of 2020, 92.7% of benzodiazepine-related overdose deaths also involved opioids, with fentanyl accounting for 66.7% of these cases (Liu et al., 2021). Furthermore, generic benzodiazepines—such as Alprazolam, Diazepam, and Clonazepam—ranked among the top 15 drugs involved in overdose deaths from 2011 to 2016, with Alprazolam consistently appearing in the top five (Hedegaard et al., 2018). Given their frequent involvement in polysubstance use and overdose deaths, the expanded Medicaid coverage of benzodiazepines following the ACA presents a unique case for evaluating how increased access to prescription drugs may have unintentionally influenced overdose trends by facilitating polysubstance abuse.

In my difference-in-differences (DID) analysis, I leverage multiple sources of variation to assess the impact of the ACA drug coverage expansion. First, I exploit between-state variation by comparing outcomes between states that expanded Medicaid drug coverage and those that did not. Second, I incorporate time variation by examining the differences in outcomes before and after the expansion in each state. A key feature of my analysis is the use of heterogeneous treatment timing, where different states implemented the Medicaid drug coverage expansion at different times, introducing additional variation based on the timing of adoption. This allows me to capture the varying effects of the ACA drug coverage expansion across states, accounting for the staggered rollout and differing implementation

periods. Additionally, I control for the ACA Medicaid eligibility expansion, which raised the eligibility threshold to 138% of the federal poverty level (FPL) in expansion states.

Since information on state policy decisions is not readily available, I manually construct an indicator for Medicaid drug coverage expansion using data from multiple sources, primarily archived Preferred Drug Lists (PDLs) from state Medicaid websites. However, not all states maintain historical PDL archives. For states where this information was unavailable, I searched news releases, state legislative records, and minutes from Pharmaceutical and Therapeutics (P&T) Committee meetings. Through these efforts, I compiled data for 37 states. Appendix 2.9.1 provides the implementation dates for Medicaid drug coverage expansion in each state, along with links to the relevant source documents.

Using Gardner (2021) – which accounts for heterogeneous treatment effects as well as staggered treatment timings –, I find an increase in fatal drug overdoses by 44% in states that underwent an expansion to both access and drug coverage under Medicaid. Notably, a rise in male overdose deaths accounts for over two-thirds of the overall effect. When analyzed by age group, 83% of the increase is driven by overdoses among individuals aged 30 to 64. These findings are robust to the inclusion of other relevant policy measures affecting drug use, including Naloxone Access Laws and Prescription Drug Monitoring Programs.

My paper contributes to the existing literature in two key ways. First, it adds to the growing body of research examining the opioid epidemic and the sharp rise in fatal drug overdoses. While Saloner et al. (2018) found no evidence that Medicaid expansion increased opioid-related deaths, Doleac and Mukherjee (2018) reported that greater access to Naloxone – a drug that counteracts opioid overdoses – does not reduce opioid mortality. However, Medicaid expansion has been shown to improve access to prescription medications used to treat opioid use disorder (Saloner et al., 2018; Sharp et al., 2018). A common methodological approach in this literature is difference-in-differences (DID). However, as discussed in Section 2.4, the staggered implementation of the ACA Medicaid expansion across states makes a traditional DID approach prone to bias. To address this, I adopt a two-stage DID method

as outlined by Gardner (2021). Additionally, while most studies in this field focus on how expanding Medicaid eligibility influences overdose deaths, my research specifically examines the impact of expanding Medicaid drug coverage on fatal overdose.

Second, I contribute to the emerging literature on the effects of Medicare and Medicaid expansion on mortality. Most studies examining Medicaid eligibility expansion find that broader coverage reduces overall mortality (Borgschulte and Vogler, 2020a; Clayton, 2019). However, when focusing specifically on drug-related deaths, the evidence remains inconclusive. Wettstein (2019) found that the Affordable Care Act's (ACA) expansion of health insurance coverage for young adults reduced opioid-related mortality in this group, while Averett et al. (2019) found no evidence that Medicaid expansion increased opioid overdose deaths. In the medical literature, Maust et al. (2020) identified a link between Medicare benzodiazepine coverage and drug overdoses among the elderly.

To the best of my knowledge, Powell et al. (2020) is the only study to examine how increased drug access – through Medicare drug coverage – affects drug-related deaths. Using CDC mortality data and additional covariates from the American Community Survey, they employed a fixed effects model and found that expanded access to opioids led to a rise in opioid-related deaths. My study differs from Powell et al. (2020) in several ways. First, while they examined the effects of Medicare Part D, which broadly expanded access to a wide range of prescription drugs (including opioids), my analysis focuses on a more targeted Medicaid expansion covering a small set of drugs that rarely cause fatal overdoses on their own (Liu et al., 2021). Instead, these drugs – particularly benzodiazepines – contribute to overdose deaths primarily when consumed alongside illicit substances due to their complementary effects, as previously discussed. This provides a clear and plausible mechanism through which expanded prescription drug coverage may have influenced overdose mortality. Additionally, the populations under study differ significantly: Powell et al. (2020) focused on individuals aged 65 and older covered by Medicare, whereas the Medicaid expansion affects a broader demographic, including adults under 65.

2.2 Policy Background

2.2.1 Medicaid

Medicaid is a joint federal and state program that provides health coverage to low-income individuals, including children, pregnant women, elderly adults, and people with disabilities. Established in 1965 under the Social Security Act, Medicaid is designed to assist those who meet specific income and eligibility requirements, with states having the flexibility to expand coverage and set eligibility criteria within federal guidelines. Medicaid plays a critical role in improving access to healthcare, particularly for vulnerable populations, by covering a wide range of services, including hospital care, prescription drugs, and mental health services.

ACA Medicaid Expansion The Affordable Care Act (ACA), passed in 2010, is a landmark piece of healthcare reform legislation aimed at expanding access to healthcare, reducing healthcare costs, and improving the quality of care in the United States. The ACA introduced several key provisions, including the creation of health insurance marketplaces, the expansion of Medicaid eligibility, and the implementation of protections for people with pre-existing conditions. One of the most significant components of the ACA was the expansion of Medicaid, which allowed states to extend coverage to low-income adults who were previously ineligible. Under the ACA, Medicaid expansion was designed to cover adults with incomes up to 138% of the Federal Poverty Line (FPL). Since Medicaid expansion under the ACA was not mandatory, it resulted in a staggered rollout across states, with some states adopting the expansion at different times. Additionally, states had flexibility in determining their eligibility thresholds, leading to variations in the size of the expansion.

In addition to expanding access, a 2014 provision of the ACA expanded Medicaid's prescription drug coverage to include benzodiazepines, smoking cessation drugs, and barbiturates, significantly increasing access to these medications. While benzodiazepines can be

lethal in high doses, the vast majority of overdose deaths involving these drugs occur in combination with other substances. They are frequently co-used with opioids to enhance intoxication (Jones et al., 2012), with methadone to amplify its potency, or with cocaine to counteract its adverse effects (DEA, 2019). In 2018, benzodiazepines, opioids, and stimulants were the most commonly reported drugs in both single- and polysubstance overdose-related emergency department visits (Pickens et al., 2022).

While most states that expanded Medicaid eligibility under the ACA also expanded drug coverage, I separately control for Medicaid eligibility expansion in my empirical specification for two reasons. First, the timing of coverage varies, as some states chose to cover benzodiazepines before the ACA. Second, some states expanded Medicaid eligibility but did not cover benzodiazepines, while others covered benzodiazepines without expanding Medicaid eligibility under the ACA.

2.2.2 Other Relevant Policy Interactions

Policies do not operate in isolation. In this subsection, I document key policy interactions that I account for in my analysis. These are incorporated into my robustness checks in Section 2.6.

A Prescription Drug Monitoring Program (PDMP) is an online database that tracks the prescribing of controlled substances within a state. While PDMPs have existed for some time, recent amendments require physicians to consult the database before issuing new prescriptions. This measure aims to prevent over-prescription and deter patients from "doctor shopping" for controlled substances. While PDMPs have been effective in reducing prescription opioid misuse, they have also led to unintended consequences, including a shift toward illicit opioid use (Kim, 2021).

Naloxone is a prescription medication that reverses opioid overdoses. Naloxone Access Laws (NALs) remove criminal penalties for individuals who possess Naloxone without a pre-

scription. While NALs have been linked to increased opioid-related emergency department visits (Smart et al., 2021), their effectiveness varies across states (Cataife et al., 2021). Additionally, broader access to Naloxone may introduce moral hazard: since individuals carrying Naloxone are better equipped to respond to overdoses, some may engage in riskier drug use. Doleac and Mukherjee (2018) provide suggestive evidence that expanding Naloxone access is associated with increased fentanyl use.

2.3 Data

In this section, I discuss the sources for my outcome variable as well as state characteristics. All subsequent analyses are conducted at the state-level as the policy change occurs at the state level. The time period of study is 2005 to 2019. The CDC began using a standardized data storage convention starting in 2005. To avoid potential measurement error due to inconsistencies in earlier data, I exclude observations from 2002 to 2004. In addition, I limit the analysis to 2019 to avoid potential distortions from the COVID-19 pandemic, which led to significant policy changes, temporary Medicaid expansions, and disruptions in healthcare access that could confound the effects of the ACA Medicaid expansion. State-level characteristics include population, the participation rate of other social programs within each state, as well as the poverty and unemployment rate.

2.3.1 Overdose Deaths

I employ restricted-use mortality data from the CDC, like other papers in the economics (Averett et al., 2019; Borgschulte and Vogler, 2020b; Hollingsworth et al., 2017) and addiction literature (Cataife et al., 2021). Using the International Classification of Diseases (ICD-10), I construct a panel data set that contains yearly observations of the number of deaths due to drug overdose. This includes Unintentional Drug Overdose (ICD-10 Codes X40-X44), Drug Overdose (Suicide) (X60-X64), and Undetermined Drug overdose (Y10-Y14). Unlike the publicly available dataset, the restricted-use mortality data provides individual-level

records, encompassing all recorded deaths in the United States.[1] By virtue of my research design, I aggregate these deaths due to drug overdose to the state level. Given the availability of information on drugs covered under Medicaid, detailed below in Section 2.3.2, I restrict the sample to 39 states over 15 time periods.

My dataset consists of 585 observations from 39 states covering the period from 2005 to 2019. As shown in Table 3, the average fatal drug overdose rate per 100,000 individuals is 14.13 in non-expansion states and 16.87 in expansion states – approximately 20% higher. This difference is statistically significant at the 5% level. Table 4 presents summary statistics by gender. Death rates are higher in Medicaid expansion states for both males and females, with the difference statistically significant at the 5% level. However, the difference is notably larger for males, who exhibit a 24% higher death rate compared to a 12% increase among females. Table 5 reports summary statistics by age group. Here, higher death rates in expansion states are observed only among adults aged 20 – 64, with differences ranging from 15% to 26%.

2.3.2 Expanded Drug Coverage Indicator

Since information on state policy decisions is not readily available, I manually construct an indicator for Medicaid drug coverage expansion using data from multiple sources, primarily archived Preferred Drug Lists (PDLs) from state Medicaid websites. However, not all states maintain historical PDL archives. For states where this information was unavailable, I searched news releases, state legislative records, and minutes from Pharmaceutical and Therapeutics (P&T) Committee meetings. Through these efforts, I compiled data for 37 states. Appendix 2.9.1 provides the implementation dates for Medicaid drug coverage expansion in each state, along with links to the relevant source documents.

I include coverage maps for both the ACA expansion and benzodiazepine coverage in

[1]Note that some overdose deaths may be misclassified or underreported due to incomplete toxicology reports or limitations in death certificate data. See Buchanich et al. (2018) or Ruhm (2018) for more details. As such, my estimates should be viewed as likely underestimating the true effect.

Figure 1 of the Appendix. While the non-expansion states for the ACA eligibility expansion are predominantly in the South, most states that do not provide benzodiazepine coverage are located in the North. This stark geographical difference further supports the need for separate indicators for the ACA eligibility expansion and benzodiazepine coverage.

2.4 Empirical Strategy

The existing economic literature studying the drug epidemic mostly relies on state-level comparisons using a difference-in-differences approach (Averett et al., 2019; Ghosh et al., 2019; Kim, 2021; Meinhofer and Witman, 2018; Sacks et al., 2021). Following that same approach, I use a treatment indicator for the ACA expansion of drug coverage, Drug Coverage$_{st}$, which takes the value 1 when a state expands its Medicaid drug coverage, and zero otherwise. The DID specification is as follows:

$$y_{st} = \beta_0 + \beta_1 \text{Drug Coverage}_{st} + \theta_s + \gamma_t + \varepsilon_{st}$$

where y_{st} is the outcome in state s at time t, and θ_s and γ_t are the state and time fixed effects respectively. State fixed effects control for all time-invariant characteristics, such as state-level policies (other than the treatment studied), geographical features, and cultural and demographic differences that do not vary over time. Including state fixed effects ensures that the treatment effect is not driven by these unobservable, state-specific characteristics that could otherwise bias the results. Time fixed effects control for common time trends and national macroeconomic conditions that change over time, ensuring that the estimated treatment effects are not influenced by global or national trends affecting all states similarly during the study period. The coefficient $\hat{\beta}_1$ captures the effect of the ACA expansion of drug coverage on outcomes. The key identifying assumption of a regular difference-in-differences approach is the parallel trends assumption, which posits that in the absence of the treatment, the treated and untreated states would have followed the same trend in their outcomes over

time. Additionally, the assumption requires that there are no anticipatory effects of the treatment.

However, as shown in Goodman-Bacon (2021), the two-way fixed effects (TWFE) estimator will produce biased estimates if there are heterogeneous treatment effects and staggered treatment timing. To correct for this bias, I adopt a two-stage difference-in-differences (2SDID) estimator put forth by Gardner (2021). Intuitively, the 2SDID approach addresses the bias since the second stage regression measures the difference between treatment and control groups, after removing group and period effects, much like how a second stage regression equation in a standard two-stage least squares framework measures the effect of the fitted value of the endogenous variable after removing the effect of the instrument. In the first stage, group and time fixed effects are identified using the sample of untreated and not-yet-treated observations.

$$y_{st} = \theta_s + \gamma_t + \varepsilon_{st}$$

where y_{st} is the outcome variable for state s at time t, and θ_s and γ_t are the state and year fixed affects respectively. Once the fixed effect parameters are estimated, in the second stage, the ATE is estimated by comparing the treated with the untreated/not-yet-treated outcomes after removing group and time fixed effects, with the following specification

$$\hat{\varepsilon}_{st} = \beta \cdot \text{Drug Coverage}_{st} + \gamma \cdot \text{ACA}_{st} + \Gamma \boldsymbol{X}_{st} + \nu_{st}$$

where $\boldsymbol{X}_{st}$ is a vector of time-varying state-level covariates and ACA_{st} indicates if state s at time t expands Medicaid eligibility under ACA. My preferred specification also includes an interaction term between Drug Coverage and ACA as there are some states that expanded drug coverage but not eligibility under ACA and others that did not expand drug coverage but do expand eligibility under ACA.

Crucially, under Gardner (2021), his estimation procedure reduces the reliance on the strict parallel trends assumption by accounting for staggered treatment and overlapping

treatment-control periods, adjusting for pre-existing trends even if they diverge across units before treatment. In short, Gardner (2021) allows for differences in pre-treatment trends across units, as long as these differences do not change over time.

That being said, I will also use Callaway and Sant'Anna (2021) to estimate the dynamic effects over time, rather than a single treatment effect as in Gardner (2021). While the two approaches differ slightly in their identifying assumptions – Callaway and Sant'Anna (2021) estimate treatment effects separately for each cohort, requiring the parallel trends assumption to hold within each treatment group – I will show that my estimates remain largely consistent across both methods of accounting for treatment effect heterogeneity.

2.5 Results

In this section, I present results from my main specification using both the standard Difference-in-Differences (DID) approach and the two-stage DID (2SDID) method proposed by Gardner (2021). Using either approach, I find a statistically significant and substantial increase in overdose deaths in states that expanded both Medicaid drug coverage and eligibility. This effect is primarily driven by males and individuals aged 30 to 64. My findings remain robust to the inclusion of other relevant policy instruments.

I report results from my main specification in Table 6 with total overdose deaths as the outcome variable. Columns (1) and (2) correspond to the generalized difference-in-differences (DID) approach, while columns (3) and (4) adopts the 2SDID estimation procedure prescribed by Gardner (2021).

Referencing Columns (1) and (3), when including only the two indicators for drug coverage and ACA eligibility, I find that an increase in death rates is only observed in states that expanded Medicaid eligibility under ACA, and that this increase is statistically significant at the 5% level. After adding in the interaction term, in Columns (2) and (4), I find that this increase is only observed in states that expanded *both* drug coverage and Medicaid eligibility. I do not find any changes to death rates in states that only expanded one or the

other. Furthermore, comparing the magnitude of the coefficients on the interaction term, I find that after correcting for the bias from the staggered rollout and the variation in the scale of the Medicaid expansion, the coefficient in Column (4) is about 35% larger than that in Column (2). Overall, I find that death rates increased by 7.15 per 100,000 (approximately 44% of the mean) in states that expanded both drug coverage and Medicaid eligibility.

Next, I apply the method of Callaway and Sant'Anna (2021) to estimate the treatment effect over time and present the event study results in Figure 2 in the Appendix. The estimates are of a similar magnitude, approximately 6.5 per 100,000, and indicate that the treatment effect stabilizes two years after the expansion of drug coverage.

When analyzing the effect by gender, with the results presented in Table 7, I find an increase in death rates for both males and females in states that expanded both drug coverage and Medicaid eligibility. As shown in Figure 3 & 5 in the Appendix, males account for approximately 67% of the total estimated effect, suggesting that the policy intervention or external factors influencing overdose mortality disproportionately impact men. This finding aligns with existing literature indicating higher rates of substance use and overdose mortality among males compared to females (Keyes et al., 2008; Seedat et al., 2009).

Turning to the analysis by age group, I find a positive and statistically significant treatment effect across all age cohorts, as reported in Table 8 in the Appendix. Notably, the effect is most pronounced for individuals aged 50 to 64, where the estimated increase in overdose deaths is approximately 50% of the mean pre-treatment death rate for this group. This suggests that middle-aged adults, particularly those approaching retirement age, are especially vulnerable to the factors driving the rise in overdose deaths.

To further illustrate these patterns, event study plots disaggregated by age group are presented in Figure 4 in the Appendix. Additionally, when considering the overall distribution of effects across demographic subgroups, individuals aged 30 to 64 collectively account for 82% of the total estimated effect, as shown in Figure 5. This breakdown highlights that the increase in overdose deaths is heavily concentrated among working-age and near-

retirement individuals, raising important questions about the underlying economic, social, and health-related factors contributing to this trend.

2.6 Robustness Checks

In this section, I present and discuss the results of my main specification, after controlling for the various related policies previously described in Section 2.2.2. My findings are robust to the inclusion of other relevant policy parameters that also affect drug abuse.

2.6.1 Patient Drug Monitoring Programs

All U.S. states have implemented some form of a Prescription Drug Monitoring Program (PDMP) aimed at curbing prescription drug misuse. Using data from the Prescription Drug Monitoring Program Training and Technical Assistance Center (PDMP TTAC)[2], I construct indicators for both the presence of a PDMP and the implementation of "Prescriber Must-Access" rules, as detailed in Appendix 2.9.2. The latter refers to policies that require prescribers – typically physicians – to consult the PDMP database before issuing a new prescription for controlled substances. This mandatory database check enables prescribers to identify whether a patient is already receiving similar medications from other providers, a practice commonly referred to as “doctor hopping.” By flagging such behavior, Must-Access rules are designed to reduce overprescribing and improve clinical decision-making.

PDMPs play a critical role in mitigating overdose risk, especially as access to controlled substances expands through broader drug coverage. As prescription drugs become more widely available, PDMPs provide an essential layer of oversight by monitoring and regulating prescribing practices. Consequently, the impact of drug coverage expansion may vary across states depending on the strength and enforcement of their PDMP policies. To account for this potential heterogeneity, I augment my baseline specification with the two PDMP indicators and present the results in Table 9.

Two key findings emerge from the analysis. First, the coefficient on the interaction term

[2]https://www.pdmpassist.org/State

becomes slightly larger after controlling for PDMP policies, suggesting that the estimated effect of drug coverage expansion may be understated when PDMP oversight is omitted. Second, the coefficient on the PDMP Prescriber indicator is positive and statistically significant in both the full sample and in gender-specific subsamples. This result is consistent with Kim (2021), who finds that Must-Access rules – by tightening the supply of prescription drugs – can lead some individuals to substitute toward more dangerous illicit alternatives, such as heroin, thereby increasing overdose risk.

2.6.2 Naloxone Access Laws

Naloxone Access Laws (NALs) have become a central component of state-level efforts to combat the opioid overdose crisis. As of 2020, all U.S. states have enacted some form of NAL, aimed at expanding the availability of naloxone – a life-saving medication that reverses opioid overdoses. Using data from the Legislative Analysis and Public Policy Association (LAPPA)[3], I construct an indicator for NAL adoption and incorporate it into my empirical specification. The implementation dates for each state are compiled and reported in Appendix 2.9.2.

Controlling for Naloxone Access Laws is important in the context of expanded drug coverage, as greater access to prescription opioids or other controlled substances may increase the risk of overdose. NALs, by improving the availability of naloxone to both medical professionals and the general public, can mitigate this risk by reducing the likelihood that an overdose results in death. As such, the presence and timing of NALs may confound the relationship between drug coverage expansion and overdose mortality. I include an indicator for NALs in the baseline specification and present my results in Table in 10.

Two findings are worth noting. First, referencing Column (1), controlling for NAL reduces the magnitude of the interaction term by approximately 5%, bringing it to 6.76 per 100,000. Second, and more notably, while the coefficient on NAL is negative across all samples, it is

[3]https://legislativeanalysis.org/naloxone-summary-of-state-laws/

only statistically significant in the female subsample, suggesting a potentially gender-specific protective effect of naloxone access on overdose mortality. This is in contrast to Doleac and Mukherjee (2018) who found that broadening access to naloxone increased overdose rates as it encourages riskier behavior.

Finally, I include controls for both PDMP and NAL policies and present the results in Table 11. All previous findings regarding the individual effects of PDMP and NAL remain consistent. Importantly, even after accounting for both types of policy interventions, the interaction term capturing the joint expansion of drug coverage and Medicaid eligibility remains positive and statistically significant. This suggests that the observed increase in overdose death rates in expansion states is not merely driven by variation in PDMP enforcement or naloxone access, reinforcing the robustness of the main result.

2.7 Conclusion

Prescription drugs play a critical role in the ongoing drug overdose epidemic in the United States. Among these, benzodiazepines – a class of drugs expanded under Medicaid drug coverage – have significant complementary uses with other illicit substances, such as opioids and stimulants (DEA, 2019; Jones et al., 2012). The combination of benzodiazepines with these other drugs can be both highly dangerous (Bannon et al., 2021) and potentially fatal (Afzal and Kiyatkin, 2019). What sets this study apart from previous research on prescription drug abuse is its focus on benzodiazepines, which, when abused alone, are rarely fatal. In fact, as of the first half of 2020, 92.7% of benzodiazepine-related deaths also involved opioids, with illicitly manufactured fentanyls playing a prominent role (Liu et al., 2021).

In this paper, I find that states that expanded both Medicaid eligibility and drug coverage experienced significantly higher overdose death rates – approximately 7.15 per 100,000 individuals. The heterogeneity analysis indicates that this increase is primarily driven by males and individuals aged 30 to 64. Notably, the treatment effect appears to stabilize two

years after the expansion, suggesting a delayed, yet persistent, response to the policy change. Importantly, these findings are robust even after controlling for other relevant policy instruments, such as Naloxone Access Laws and Prescription Drug Monitoring Programs, that also influence drug-related outcomes. This research highlights the complex relationship between prescription drug access and overdose deaths, particularly the unintended consequences of expanding access to drugs that have the potential to be used in combination with illicit substances. Future research could further explore how specific substances interact within this framework, potentially leading to more targeted policy interventions.

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2.9 Appendix

2.9.1 Construction of Policy Indicator

State	Date of Drug Coverage Expansion	Source	Date of Eligibility Expansion	Source
Alabama	1st Jan 2014	News Release		KFF
Arizona	2014	MDRP Data	1st Jan 2014	KFF
Arkansas	1st April 2014	State Legislature	1st Jan 2014	KFF
California	2014	MDRP Data	1st Jan 2014	KFF
Connecticut	2014	PDL	1st Jan 2014	KFF
Delaware	Apr 2014	State Legislature	1st Jan 2014	KFF
Florida	2014	PDL		KFF
Georgia	1st Jan 2014	PDL		KFF
Idaho	1st Jan 2019	PDL Update	1st Jan 2020	KFF
Illinois	1st Jan 2018	PDL	1st Jan 2014	KFF
Iowa	15th Jan 2005	PDL	1st Jan 2014	KFF
Kansas		PDL		KFF
Kentucky	2014	MDRP Data	1st Jan 2014	KFF
Louisiana	2014	MDRP Data	1st Jul 2016	KFF
Maine	2010	PDL	2nd Jul 2018	KFF
Maryland	2010	PDL	1st Jan 2014	KFF
Minnesota		PDL	1st Jan 2014	KFF
Mississippi	1st Oct 2013	PDL		KFF
Missouri		PDL	1st Jul 2021	KFF
Nebraska	1st Jan 2014	P&T Minutes	1st Oct 2020	KFF

Nevada	1st Jul 2010	PDL	1st Jan 2014	KFF
New York	20th Mar 2014	News Release	1st Jan 2014	KFF
North Dakota		PDL archive	1st Jan 2014	KFF
Ohio	1st Nov 2013	PDL	1st Jan 2014	KFF
Oregon	13th April 2015	PDL	1st Jan 2014	KFF
Rhode Island		PDL	1st Jan 2014	KFF
South Carolina		PDL		KFF
Utah	1st June 2017	PDL	1st Jan 2020	KFF
Virginia	1st Jan 2015	PDL	1st Jan 2019	KFF
Washington	1st Jul 2018	PDL	1st Jan 2014	KFF
West Virginia	2nd Jan 2004	PDL	1st Jan 2014	KFF
Wisconsin	2011	PDL		KFF
Wyoming	1st Jan 2020	PDL		KFF

2.9.2 Construction of NAL & PDMP Indicators

State	Date of Naloxone Access Law	Source	Operational Date for PDMP	Prescriber - Mandatory PDMP Use	Source
Alabama	5th June 2015	LAPPA	2006	9th March 2017	PDMP TTAC
Alaska	15th March 2016	LAPPA	2011	17th July 2017	PDMP TTAC
Arizona	6th August 2016	LAPPA	2008	20th July 2011	PDMP TTAC
Arkansas	22nd July 2015	LAPPA	2013	22nd July 2015	PDMP TTAC
California	11th October 2007	LAPPA	1939	2nd October 2018	PDMP TTAC
Connecticut	1st October 2003	LAPPA	2008	1st October 2015	PDMP TTAC
Delaware	4th August 2014	LAPPA	2012	15th July 2010	PDMP TTAC
Florida	10th June 2015	LAPPA	2011	1st July 2018	PDMP TTAC
Georgia	24th April 2014	LAPPA	2013	13th July 2014	PDMP TTAC
Idaho	1st July 2015	LAPPA	1967	1st July 2020	PDMP TTAC
Illinois	1st January 2010	LAPPA	1968	1st January 2018	PDMP TTAC
Iowa	27th May 2016	LAPPA	2009	14th May 2018	PDMP TTAC
Kansas	1st July 2017	LAPPA	2011	None	PDMP TTAC
Kentucky	25th June 2013	LAPPA	1999	1st July 2012	PDMP TTAC
Louisiana	23rd June 2015	LAPPA	2008	1st August 2014	PDMP TTAC
Maine	29th April 2014	LAPPA	2004	1st January 2017	PDMP TTAC
Maryland	1st October 2013	LAPPA	2013	1st July 2018	PDMP TTAC
Minnesota	10th May 2014	LAPPA	2010	1st August 2013	PDMP TTAC
Mississippi	1st July 2015	LAPPA	2005	2018	PDMP TTAC
Missouri	28th August 2016	LAPPA	2017	31st December 2021	PDMP TTAC
Nebraska	28th May 2015	LAPPA	2011	5th July 2022	PDMP TTAC
Nevada	1st October 2015	LAPPA	1997	1st October 2017	PDMP TTAC
New York	1st April 2006	LAPPA	1973	27th August 2013	PDMP TTAC
North Dakota	1st August 2015	LAPPA	2007	1st October 2014	PDMP TTAC
Ohio	11th March 2014	LAPPA	2006	20th May 2011	PDMP TTAC
Oregon	6th June 2013	LAPPA	2011	1st October 2021	PDMP TTAC
Rhode Island	27th January 2016	LAPPA	1979	31st January 2013	PDMP TTAC
South Carolina	3rd June 2015	LAPPA	2008	19th May 2017	PDMP TTAC
Utah	13th May 2014	LAPPA	1996	10th May 2016	PDMP TTAC
Virginia	13th March 2013	LAPPA	2003	2018	PDMP TTAC

Washington	24th July 2015	LAPPA	2011	1st July 2013	PDMP TTAC
West Virginia	27th May 2015	LAPPA	1995	8th June 2012	PDMP TTAC
Wisconsin	9th April 2014	LAPPA	2013	1st April 2017	PDMP TTAC
Wyoming	1st July 2017	LAPPA	2004	2021	PDMP TTAC

2.10 Tables

Table 3: Summary Statistics for Outcome Variables

Outcome Variable (per 100,000)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Drug Overdose Rates	16.87	14.13	2.75***
	(8.11)	(6.19)	(0.794)
N	465	120	585

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Expansion states are states that have expanded drug coverage of benzodiazepines under ACA. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 4: Summary Statistics for Death Rates (By Gender)

Outcome Variable (per 100,000)	(1) Expansion States	(2) Non-expansion States	(3) Difference
Male Overdose Death Rates	10.65	8.50	2.06***
	(5.69)	(4.40)	(0.558)
N	465	120	585
Female Overdose Death Rates	6.31	5.62	0.688***
	(2.62)	(2.07)	(0.258)
N	465	120	585

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Expansion states are states that have expanded drug coverage of benzodiazepines under ACA. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 5: Summary Statistics for Death Rates (By Age Group)

Outcome: Death Rate per 100,000	(1) Expansion States	(2) Non-expansion States	(3) Difference
Under 18	0.31	0.29	0.0186
	(0.155)	(0.143)	(0.0156)
Age 20-29	2.84	2.30	0.541***
	(1.50)	(1.15)	(0.147)
Age 30-39	3.97	3.14	0.828***
	(2.52)	(1.68)	(0.243)
Age 40-49	4.33	3.65	0.676***
	(1.89)	(1.57)	(0.187)
Age 50-64	4.62	4.00	0.615**
	(2.34)	(1.97)	(0.232)
Age 65+	0.82	0.75	0.0688
	(0.438)	(0.434)	(0.0448)
N	465	120	585

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Expansion states are states that have expanded drug coverage of benzodiazepines under ACA. Mean Coefficients in (1) & (2); Absolute difference in (3). SD in parentheses for (1) & (2), SE in parentheses for (3).

Table 6: Baseline Regression Estimates

Outcome: Death Rate (per 100,000)	(1) DID	(2) DID	(3) 2SDID	(4) 2SDID
Drug Coverage	-0.899	-2.27	1.70	0.382
	(1.28)	(1.49)	(1.75)	(1.50)
ACA	3.94**	-0.0877	4.64***	0.177
	(1.59)	(1.82)	(1.55)	(1.96)
ACA × Drug Coverage		5.29**		7.15**
		(2.53)		(2.79)
N	585	585	585	585
Mean Outcome	16.30	16.30	16.30	16.30

Note: Standard errors in parentheses are clustered by state. Columns (1) & (2) correspond to estimates using the difference-in-differences/TWFE approach (with time and state fixed effects), while columns (3) & (4) correspond to estimates obtained using the Gardner (2021) two-stage difference-in-difference procedure. Drug Coverage indicates if state s at time t has expanded drug coverage. ACA indicates if state s at time t has expanded Medicaid eligibility.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Regression Estimates using Gendered Drug Overdose Death Rates as Outcome Variable

Outcome: Death Rate (per 100,000)	Males (1)	Males (2)	Females (3)	Females (4)
Drug Coverage	1.39	0.515	0.310	-0.133
	(1.24)	(1.06)	(0.561)	(0.498)
ACA	3.816***	0.861	0.820*	-0.684
	(1.12)	(1.47)	(0.462)	(0.536)
ACA × Drug Coverage		4.74**		2.41***
		(2.018)		(0.837)
N	585	585	585	585
Mean Outcome	10.14	10.14	6.17	6.17

Note: Standard errors in parentheses are clustered by state. All Columns correspond to estimates obtained using the Gardner (2021) two-stage difference-in-difference procedure. Drug Coverage indicates if state s at time t has expanded drug coverage. ACA indicates if state s at time t has expanded Medicaid eligibility.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Regression Estimates using Death Rates (By Age Group) as the Outcome Variable

Outcome Variable: (Death Rates Per 100,000)	(1) Age <20	(2) Age 20-29	(3) Age 30-39	(4) Age 40-49	(5) Age 50-59	(6) Age 65+
Drug Coverage	-0.0282	0.113	0.362	0.144	-0.152	-0.0572
	(0.0228)	(0.286)	(0.427)	(0.409)	(0.393)	(0.0666)
ACA	-0.0112	0.2718	0.1467	-0.0787	-0.1329	-0.0187
	(0.0226)	(0.326)	(0.521)	(0.480)	(0.590)	(0.102)
ACA × Drug Coverage	0.0896**	0.904*	1.85**	1.78***	2.25***	0.277*
	(0.0363)	(0.481)	(0.829)	(0.673)	(0.804)	(0.143)
N	585	585	585	585	585	585
Mean Outcome	0.302	2.73	3.80	4.19	4.49	0.805

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors clustered by state in parentheses. Estimation done using Gardner (2021).

Table 9: Regression Estimates when controlling for PDMPs

Outcome: Death Rate per 100,000	(1) Total	(2) Male	(3) Female
Drug Coverage	0.266	0.451	-0.185
	(1.48)	(1.08)	(0.464)
ACA	-1.41	-0.249	-1.16*
	(2.29)	(1.67)	(0.693)
Drug Coverage × ACA	7.48**	4.96**	2.53***
	(2.96)	(2.10)	(0.940)
PDMP	-1.17**	-0.862**	-0.307
	(0.551)	(0.377)	(0.206)
PDMP Prescriber	3.81**	2.72**	1.09**
	(1.54)	(1.08)	(0.510)
N	585	585	585
Mean Outcome	16.30	10.14	6.17

Note: Standard errors in parentheses are clustered by state. All Columns correspond to estimates obtained using the Gardner (2021) two-stage difference-in-difference procedure. Drug Coverage indicates if state s at time t has expanded drug coverage. ACA indicates if state s at time t has expanded Medicaid eligibility.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 10: Regression Estimates when controlling for NAL

Outcome: Death Rate per 100,000	(1) Total	(2) Male	(3) Female
Drug Coverage	0.825	0.672	0.153
	(1.49)	(1.07)	(0.487)
ACA	0.913	1.12	-0.209
	(2.29)	(1.74)	(0.601)
Drug Coverage × ACA	6.76**	4.60**	2.16**
	(2.88)	(2.10)	(0.856)
NAL	-1.06	-0.375	-0.682**
	(1.07)	(0.797)	(0.334)
N	585	585	585
Mean Outcome	16.30	10.14	6.17

Note: Standard errors in parentheses are clustered by state. All Columns correspond to estimates obtained using the Gardner (2021) two-stage difference-in-difference procedure. Drug Coverage indicates if state s at time t has expanded drug coverage. ACA indicates if state s at time t has expanded Medicaid eligibility.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 11: Regression Estimates when controlling for both PDMP & NAL

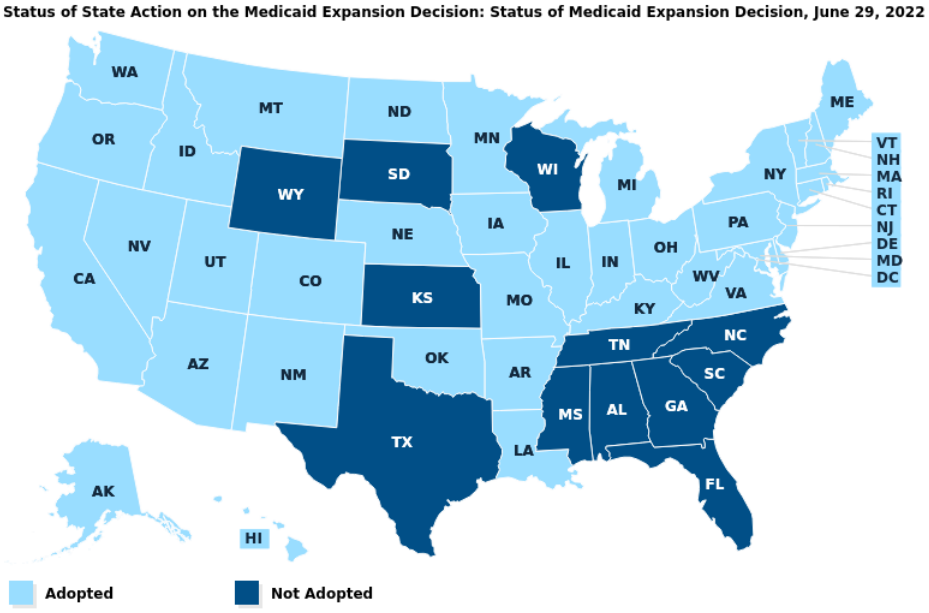
Outcome: Death Rate per 100,000	(1) Total	(2) Male	(3) Female
Drug Coverage	0.545	0.565	-0.0197
	(1.45)	(1.08)	(0.444)
ACA	-0.911	-0.0462	-0.865
	(2.37)	(1.76)	(0.677)
Drug Coverage × ACA	7.34**	4.90**	2.44***
	(2.94)	(2.11)	(0.910)
PDMP	-0.604	-0.633*	0.0285
	(0.564)	(0.370)	(0.215)
PDMP Prescriber	4.368***	2.948***	1.42***
	(1.554)	(1.094)	(0.502)
NAL	-1.91	-0.776	-1.13***
	(1.19)	(0.857)	(0.380)
N	585	585	585
Mean Outcome	16.30	10.14	6.17

Note: Standard errors in parentheses are clustered by state. All Columns correspond to estimates obtained using the Gardner (2021) two-stage difference-in-difference procedure. Drug Coverage indicates if state s at time t has expanded drug coverage. ACA indicates if state s at time t has expanded Medicaid eligibility.

* $p < .1$, ** $p < .05$, *** $p < .01$

2.11 Figures

Figure 1: Map of ACA Medicaid Expansion (Top) & Benzodiazepine Coverage (Bottom)



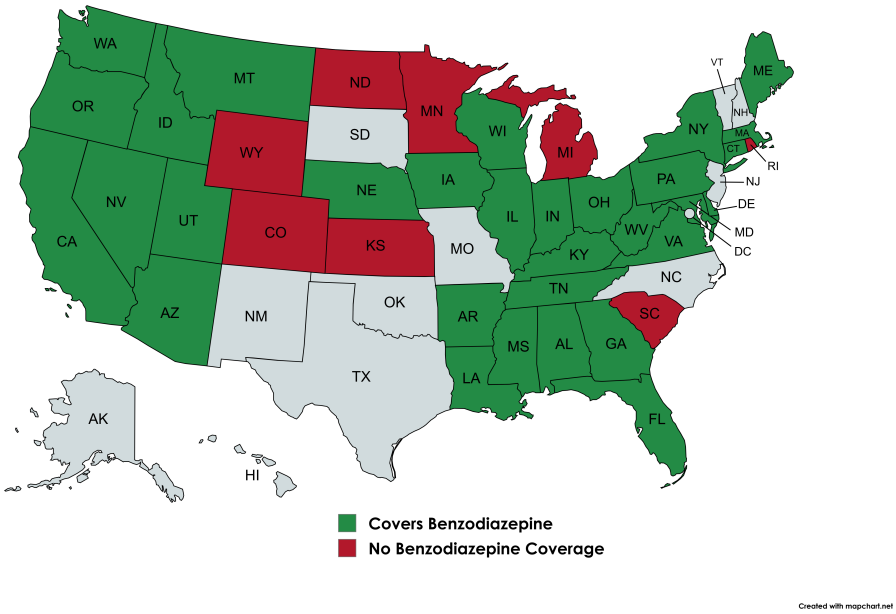


Figure 2: Event Study using Callaway and Sant'Anna (2021)

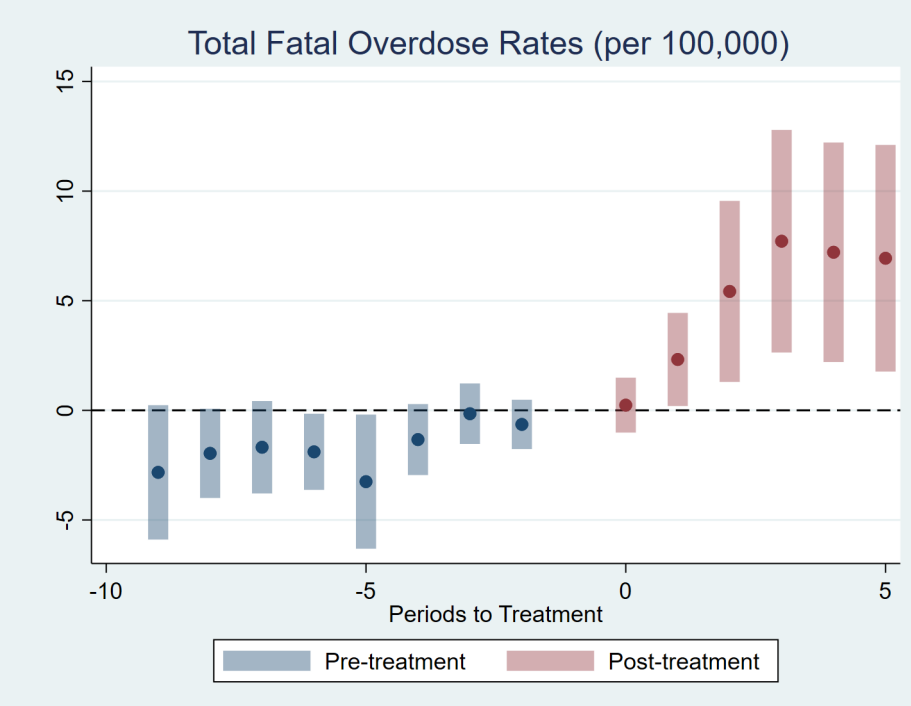


Figure 3: Event Study (by Gender) using Callaway and Sant'Anna (2021)

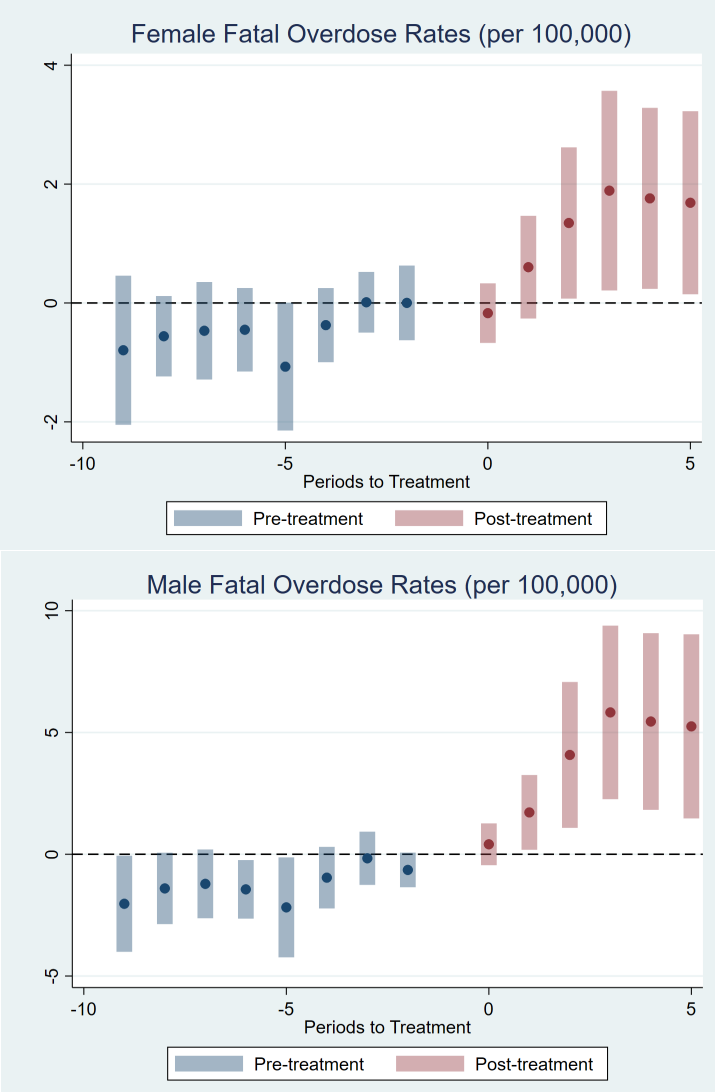


Figure 4: Event Study (by Age Group) using Callaway and Sant'Anna (2021)

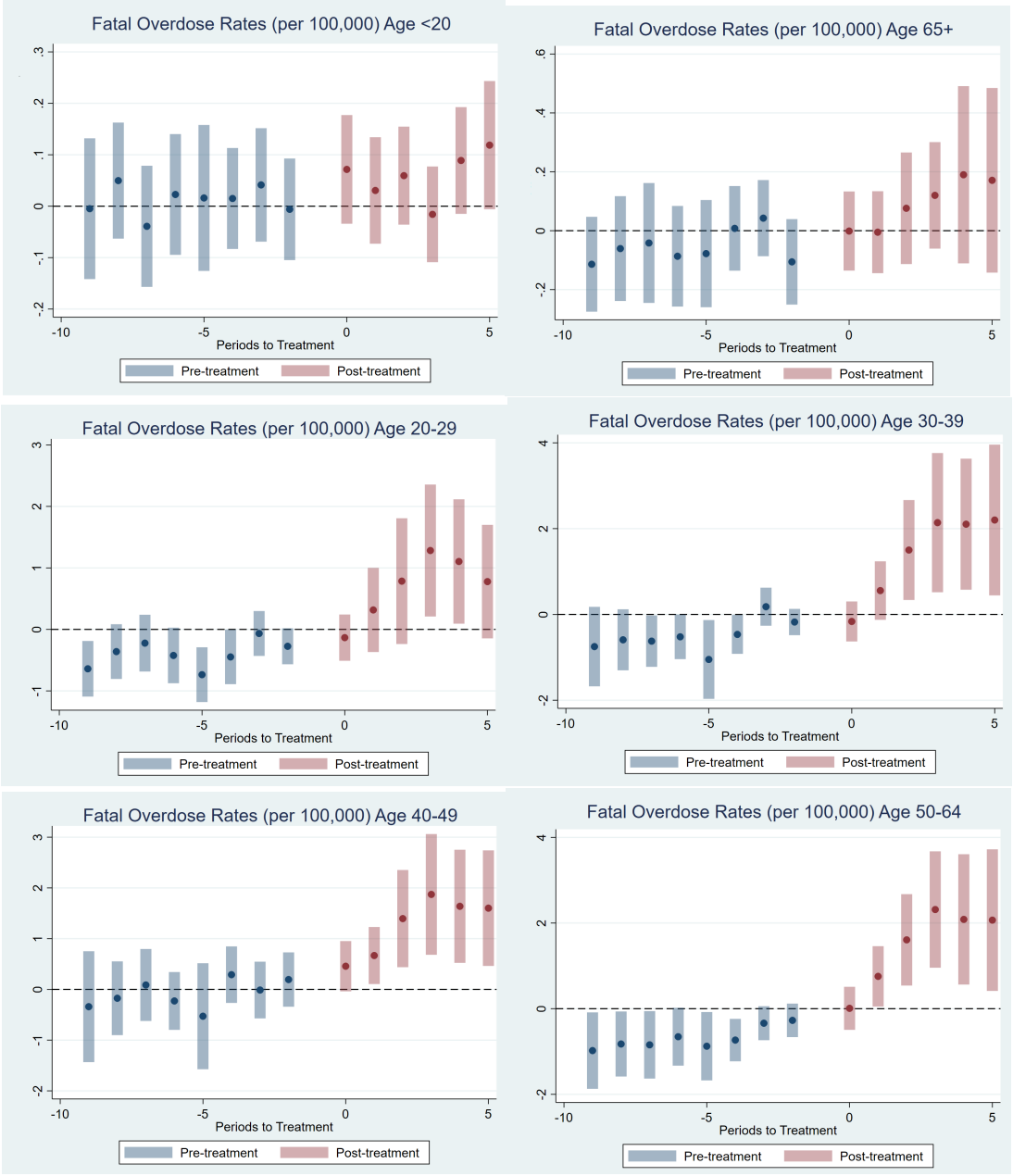
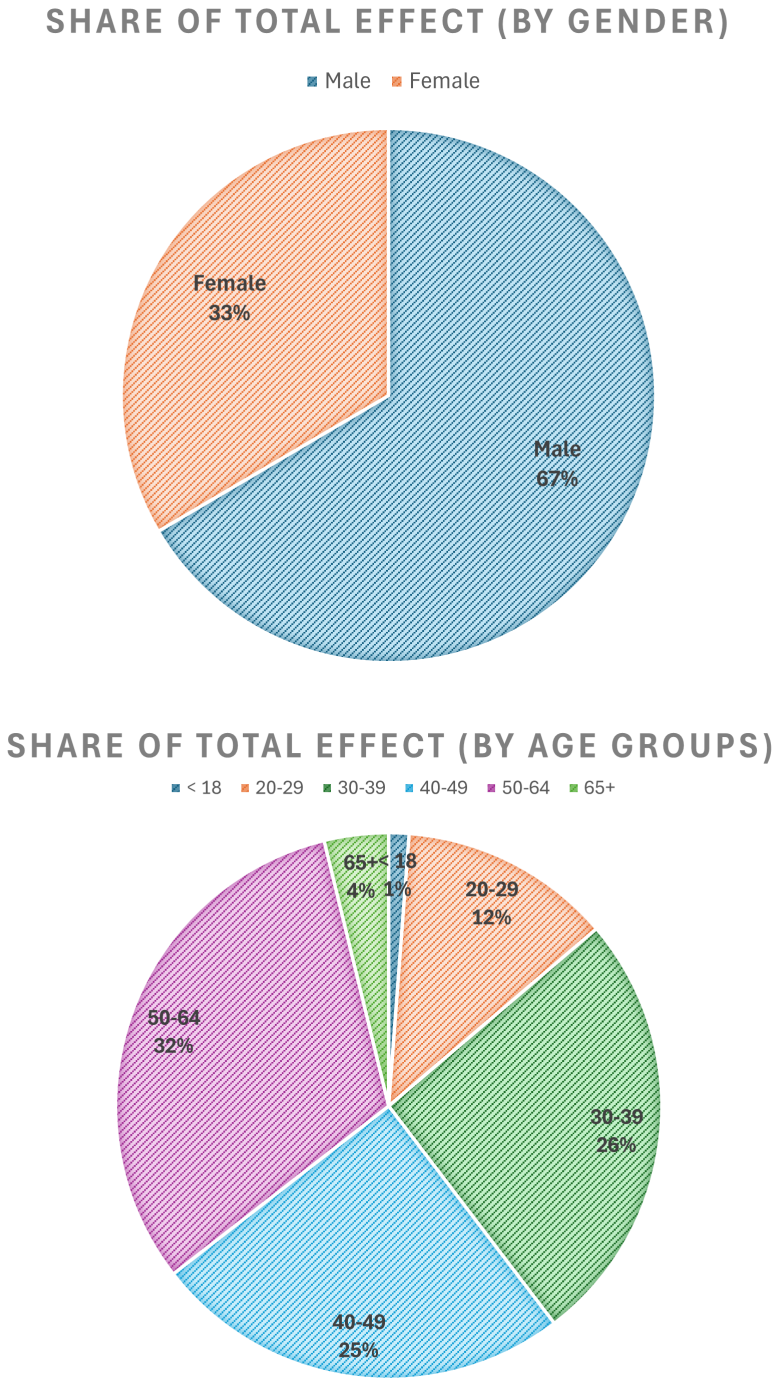


Figure 5: Share of Total Effect, By Gender (Top) and Age Group (Bottom)



Chapter 3

The Effect of Changes in SNAP Benefit on Drug Abuse Rates

3.1 Introduction

Means-tested cash-like assistance programs in the United States – most notably the Supplemental Nutrition Assistance Program (SNAP) – play a central role in supporting low-income households during periods of economic hardship. While research has established a relationship between drug use and adverse economic conditions (Ayllón and Ferreira-Batista, 2018; Carpenter et al., 2017; Hollingsworth et al., 2017), much of the literature on the drug overdose epidemic focuses primarily on health assistance through Medicare and Medicaid (Borgschulte and Vogler, 2020; Ghosh et al., 2019; Maclean and Saloner, 2019; Meinhofer and Witman, 2018; Powell et al., 2020). In this paper, I examine the role of SNAP – the largest cash-like assistance program in the U.S. – in shaping drug overdoses outcomes.

The U.S. is in the middle of the third wave of the opioid epidemic (CDC, 2021). Unlike the previous two waves involving prescription opioids and heroin, synthetic opioids are more potent and have the potential to result in a fatal overdose (DEA, 2020). Drug overdose death rates have increased five-fold from 1999 to 2021 (National Institute on Drug Abuse, 2022). To date, more than 1 million people have died since 1999 from a drug overdose (CDC, 2023). Importantly, fatal drug overdoses are one of the three types of *deaths of despair* – a class of behaviour-related medical conditions that, it has been suggested, increase in groups of people who experience despair due to poor social and economic prospects (Case and Deaton, 2020; Pierce and Schott, 2020; Sullivan and von Wachter, 2009). Social safety net programs are specifically designed to ameliorate poor economic conditions as eligibility is set relative to the federal poverty line, and so might reduce the extent of substance use and

overdose. Approximately 30% of the US population participated in at least one social safety net program in 2019 (Macartney and Ghertner, 2023).

I exploit two sources of variation to identify the effect of changes in average SNAP benefits on outcomes related to drug abuse. Firstly, I exploit between-state variation in the generosity and eligibility criteria of SNAP. Secondly, I leverage spatial variation by comparing outcomes in contiguous counties that lie across a common state border. Since these are contiguous counties, they are more likely to share similar populations, as well as local labor market conditions.

I measure SNAP generosity using average monthly benefits, following Leung and Seo (2023). Specifically, I divide total SNAP benefits by the number of recipients in each state, ensuring that my measure is not influenced by population size changes. Additionally, since my empirical specification includes county fixed effects, I account for heterogeneity in the efficiency and leniency of local administrators in approving new SNAP applications.

Using my contiguous county research design, I find evidence of asymmetric responses relating to drug abuse. When focusing on periods of benefit increases, I find that a $10 increase in average SNAP benefits reduced fatal overdose rates by 0.511 per 100,000 (or about 3.6% of the mean). However, when during periods of benefit decreases, I do not observe a commensurate increase in fatal overdose rates. Instead, I find a smaller reduction in fatal overdose rates by 0.118 per 100,000 (or about 0.6% of the mean), though statistically insignificant. The reduction in fatal overdose rates during periods of benefit increases is predominantly driven by males, and individuals aged 20-29.

My work contributes to the existing literature in two key ways. First, I provide new evidence on the role of other social safety net programs in relation to fatal drug overdoses. Existing research predominantly focuses on Medicare and Medicaid (Borgschulte and Vogler, 2020; Meinhofer and Witman, 2018; Powell et al., 2020). However, more recent studies on *deaths of despair* suggest that other social programs can help alleviate economic hardship, which may, in turn, influence drug overdose outcomes. For example, Dow et al. (2020)

found that increases in the minimum wage and EITC reduced suicides by approximately 3%. Additionally, unlike much of the literature on the drug overdose epidemic, which relies on state-level comparisons (Carpenter et al., 2017; Ghosh et al., 2019; Kim, 2021; Powell et al., 2020), I adopt a cross-border research design following Dube et al. (2010). This approach allows me to isolate the effects of social program expansions while controlling for shared geographical factors such as local labor market conditions and illicit drug markets.

Second, my work contributes to the broader literature on asymmetric consumer responses. Prior research has shown that consumers react asymmetrically in their demand for necessities like childcare (Iizuka and Shigeoka, 2023) and public transport (Yaman and Offiaeli, 2022), as well as for addictive goods like coffee (Bonnet and Villas-Boas, 2016). Given that drugs are inherently addictive, consumers may be more resistant to reducing drug use during economic downturns but may more readily increase consumption following economic improvements or expanded safety net benefits. Existing research on the relationship between economic conditions and drug use has primarily examined exogenous negative employment shocks (Ayllón and Ferreira-Batista, 2018), or broader economic downturns (Carpenter et al., 2017; Hollingsworth et al., 2017).

3.2 Data & Institutional Background

In this section, I discuss the sources for my outcome and policy variables. By virtue of the contiguous county research design, fatal overdose rates are recorded at the county-level. The time period of study is 2005 to 2019. I limit the analysis to 2019 to avoid potential distortions from the COVID-19 pandemic.

3.2.1 Overdose Deaths

I obtain county-level data on fatal drug overdoses from the CDC, as used in other studies in the economics (Averett et al., 2019; Borgschulte and Vogler, 2020; Hollingsworth et al., 2017) and addiction literature (Cataife et al., 2021). Using the International Classification of Diseases (ICD-10), I construct a panel data set with yearly observations of drug overdose deaths, including Unintentional Drug Overdose (ICD-10 Codes X40-X44), Drug Overdose (Suicide) (X60-X64), and Undetermined Drug Overdose (Y10-Y14). Unlike the publicly available dataset, the restricted-use mortality data provides individual-level records, covering all recorded deaths in the United States.

In total, I have a balanced panel of 23,632 observations from over 800 unique counties across 47 states, spanning from 2005 to 2019. The CDC began using a standardized data storage convention starting in 2005. To avoid potential measurement error due to inconsistencies in earlier data, I exclude observations from 2002 to 2004. Furthermore, I limit my analysis to 2019 to avoid potential distortions from the COVID-19 pandemic. Figure 2 in the Appendix maps the counties included in my sample. Summary statistics are presented in Table 1. Panel I reports statistics for the full sample, while Panel II presents statistics after removing duplicate counties. Notably, fatal overdose rates for males account for 60% of the total and are statistically significantly higher than those for females at the 5% level. Turning our attention to Panel II, I find a reduction in mean overdose rates in all categories. However, fatal overdose rates for males continue to account for over 60% of the total, and remain statistically significantly higher than those for females at the 5% level.

3.2.2 Supplemental Nutrition Assistance Program (SNAP)

I measure SNAP generosity using nominal average monthly benefits, following Leung and Seo (2023). Specifically, I divide total SNAP benefits by the number of recipients in each state, ensuring that my measure is not influenced by population size changes. Additionally,

since my empirical specification includes county fixed effects, this accounts for heterogeneity in the efficiency and leniency of local administrators in approving new SNAP applications, as well as average differences in income across counties.

SNAP provides food assistance to low-income households, supplementing their income to help them afford nutritious meals. Unlike Medicaid, whose eligibility requirements vary by state, SNAP is available nationwide to all households that meet the federal income criteria.

There are two key sources of between-state variation in SNAP benefits. First, while eligibility rules (set at 130% of the federal poverty level) and benefit levels are federally determined, states have flexibility in how benefits are calculated. The most significant variation comes from the Standard Utility Allowance (SUA), which accounts for household utility costs. Each state sets its own SUA standards based on average in-state utility expenses. SUA calculations generally fall into two categories: (1) using recent state-specific utility data or (2) indexing the SUA to an inflation measure, such as the Consumer Price Index (CPI) for utility costs. This leads to substantial differences in total utility allowances, ranging from $650 in Idaho to $1,400 in Maine.

Additionally, states differ in how they assess asset limits when determining SNAP eligibility. While 24 states waive asset limits entirely, 11 states apply waivers with additional conditions.

A second source of variation comes from legislative changes. The 2008 Farm Bill and the American Recovery and Reinvestment Act (ARRA) of 2009 increased maximum SNAP benefits by 8.5% and 13.6%, respectively. Because these increases were applied as a percentage of pre-existing benefit levels, states with higher baseline SNAP benefits saw larger absolute increases. Furthermore, the benefit increases introduced by ARRA 2009 expired in 2013, leading to varying reductions across states.

The data on average SNAP benefit comes from the Kaiser Family Foundation and covers the period from 2005 to 2019. As shown in Figure 3, the effects of the 2008 Farm Bill and ARRA 2009 are evident in the raw data, with average SNAP monthly benefits rising from

2007 to 2008 and increasing sharply from 2008 to 2009. Following the expiration of ARRA in 2013, benefits declined from 2013 to 2014. However, average benefits did not return to pre-ARRA levels. Instead, across nearly all states, they stabilized at a level higher than pre-ARRA. To illustrate the variation in benefits between states, Figure 4 presents trends in average SNAP benefits over time, disaggregated by geographic region.

3.3 Empirical Strategy

The existing economic literature studying the drug epidemic mostly relies on state-level comparisons using a difference-in-differences approach (Averett et al., 2019; Ghosh et al., 2019; Kim, 2021; Meinhofer and Witman, 2018; Sacks et al., 2021) or propensity score matching to match and compare outcomes in similar counties in expansion and non-expansion states (Borgschulte and Vogler, 2020). However, given that drug abuse increases when economic conditions worsen (Ayllón and Ferreira-Batista, 2018; Carpenter et al., 2017; Hollingsworth et al., 2017), failure to control fully for local economic conditions might bias any estimates. To address this, I adopt a cross-border research design that exploits variation between contiguous counties that lie on opposite sides of a common state boundary, and that otherwise have similar demographics and local labor market conditions. Following Dube et al. (2010), my empirical specification is as follows:

$$y_{ipt} = \beta_0 + \beta_1 \text{Policy}_{it} + \boldsymbol{X}_{it} + \pi_i + \tau_{pt} + \varepsilon_{ipt}$$

where y_{ipt} denotes the outcome variable for county i in pair p at time t, Policy_{it} is the policy measure of county i at time t, specifically the average SNAP benefit amount. $\boldsymbol{X}_{it}$ is a vector of county-specific characteristics that vary with time (e.g. population, poverty rate and unemployment rate), π_i denotes county fixed effects and τ_{pt} is a pair-specific fixed effect that is allowed to vary with time. By employing a vector of pair-by-time fixed effects, I identify

the effect of the policy on the chosen outcome variable by only using the variation within each border county-pair. The pair-by-time fixed effects control for local shocks or trends as well as common time-varying unobservables such as labor market interdependence. As a result of my research design, certain counties appear in multiple pairs, leading to repeated observations – this is further detailed in the Appendix Section 3.8.1. The county fixed effects control for county-specific time-invariant unobservables such as the long run economic condition of the county, historical policies and political leanings that might influence its stance on drugs. The identifying assumption is that differences in the policy threshold or program generosity is uncorrelated with unobservable characteristics in either county within each county-pair. This is plausible given that the policy variable is defined at the state level, and eligibility thresholds and program generosity are typically set by the state or federal government. Standard errors are clustered at the pair and county level.

There are two main limitations specific to the cross-border research design. The first is spatial spillovers. Spatial spillovers refer to the unintended effects that an intervention or treatment in one geographic area may have on neighboring regions. In a cross-border research design, this becomes a concern because policy changes in one county or region can influence outcomes in nearby areas, confounding any obtained estimates. Because such spillover effects empirically decay with distance (Anderson, 2011), I can empirically test for this using a method prescribed by Dube et al. (2010) which suggests the use of an average of all interior counties as a control county within each county-pair.

The second limitation is heterogeneous treatment effects. The cross-border sample includes only county-pairs that are on the edges of their respective states. If these border counties are systematically different from their interior counterparts within each state, then the research design only identifies the treatment effect for such counties. Given that there are more rural counties along state borders than urban counties, and that SNAP was found to be more valuable in rural counties (Vogel et al., 2021), my estimates can be viewed as an overestimate of the true effect. Crucially however, systematic differences between interior

and border counties affect the interpretation of any resulting estimates but do not invalidate the exogeneity of the policy variable.

3.4 Results

Column (1) of Table 2 reports the result from my main specification outlined earlier. While the point estimates are negative, and in line with the broader evidence in the existing literature (Ayllón and Ferreira-Batista, 2018; Carpenter et al., 2017; Hollingsworth et al., 2017), they are statistically insignificant at the 10% level. One possible reason for this is that over the entire sample period, average SNAP benefits increased and subsequently decreased as a result of ARRA 2009, previously detailed in Section 3.2.2. Therefore, asymmetric responses to changes in income might confound my obtained estimates. To separately identify the effects of the increase and decrease in benefits, I estimate two subsamples: one from 2005 to 2013 to capture the increase in average SNAP benefits, and another from 2010 to 2019 to capture the decline in average SNAP benefits. I report my findings in Columns (2) and (3) respectively. Here, I find evidence of asymmetric response relating to drug abuse. In Column (2), I observe a statistically significant fall in fatal overdose rates by 0.511 per 100,000 (or about 3.66% of the mean) for every $10 increase in average SNAP benefits. However, in Column (3), I do not find any evidence of a commensurate increase in fatal overdose rates when average SNAP benefits subsequently fell. In fact, the point estimates remain negative, albeit statistically insignificant. One possible explanation for this asymmetric response is that individuals often turn to drugs as a coping mechanism for stress (Rigg and Ibañez, 2010; Evans and Cahill, 2016). Thus, an increase in average SNAP benefits can alleviate some of the financial strain, reducing the reliance on drugs as a way to manage stress. The lack of a significant increase in fatal overdose rates following a reduction in SNAP benefits may be due to the relatively small decrease in benefits.

When repeating this analysis by gender, I find evidence of this asymmetric response

among males, as show in Panel I of Table 3 of the Appendix. While the coefficient is negative when using both the full sample and the subsample corresponding to the period of benefit increases, the point estimate is only statistically significant in the latter case. In Column (2), I observe a fall in fatal overdose rates for males by 0.321 per 100,000 (or about 3.8% of the mean) for every $10 increase in average SNAP benefits. Turning our attention to Panel II, I do not find any statistically significant effects when using female fatal overdose rates as the outcome variable.

Turning to age groups in Table 4 of the Appendix, I only find evidence of asymmetric responses among individuals aged 20-29 where a $10 increase in average SNAP benefits resulted in a fall in overdose rates by 0.21 per 100,000 (or approximately 10% of the mean). In all other age groups, the point estimates are statistically insignificant at the 10% level.

3.4.1 Alternative Definitions of the SNAP Benefit Increase Period

In the previous analysis, I defined the period of SNAP benefit increases as spanning from 2005 to 2013. In this section, I examine alternative definitions of this period and report the corresponding findings in Table 5. My baseline results are presented in Column (3). I find that, regardless of whether I shorten or lengthen the definition of the SNAP benefit increase period, the point estimates remain consistently statistically significant and negative. This suggests that the results are not sensitive to the specific time frame chosen for the benefit increases. Based on these findings, I conclude that my results are robust to variations in the definition of the SNAP benefit increase period.

3.4.2 Accounting for Inflation

In all previous analyses, I use nominal average SNAP benefit dollars as the policy variable. To assess the robustness of my results to inflation adjustments, I now re-estimate the models

using SNAP benefits deflated to 2005 dollars. I implement two approaches: first, by adjusting for inflation using the Consumer Price Index (CPI), and second, by benchmarking against the USDA Thrifty Food Plan (TFP) costs, which more directly capture the purchasing power of SNAP benefits in relation to food affordability.

I report the results in Table 6. Column (1) presents the baseline estimates using nominal SNAP benefit dollars. Columns (2) to (5) use real SNAP benefit dollars as the policy variable, deflated to 2005 dollars using different indices. Column (2) uses the CPI, while Columns (3) to (5) use TFP costs for (3) a two-person married couple, (4) a two-person married couple with two children under age five, and (5) a two-person married couple with two children over age five, respectively.

Two findings are worth highlighting. First, the point estimates remain statistically significant across all specifications for the period of SNAP benefit increase (2005–2013). Second, real SNAP benefits yield larger effects: the estimated reduction in overdose deaths increases by about 20%, to approximately 0.6 deaths per 100,000 for a $10 increase in real benefits. Importantly, the estimates are robust to the choice of deflator, with effect sizes remaining consistent across Columns (2) to (5).

3.4.3 Frequency Weights

As discussed in Section 3.8.1 of the Appendix, my research design includes repeated observations for some counties in the sample. To account for the potential bias introduced by these duplicate observations and ensure that each county is appropriately weighted in the analysis, I apply frequency weights. This adjustment ensures that the results accurately reflect the true distribution of observations across counties (Wooldridge, 2010). I report my findings in Table 7 of the Appendix. After this adjustment, the coefficient is statistically insignificant, even in the subsample corresponding to the sample period of benefit increases. One possible explanation for this is that my full sample of 23,026 observations only contain 12,000 unique

observations, as reported in the summary statistics in Table 1. This means that the effective sample size is smaller than the raw number of observations. This adjustment increases the estimated variance, resulting in statistically insignificant point estimates at the 10% level.

3.4.4 Population Differences

Counties within each pair may differ significantly in size. This disparity could lead to situations where policy changes in the larger county disproportionately affect outcomes in the neighboring, smaller county. The greater population and economic scale of the larger county may exert more influence on regional dynamics, potentially distorting the comparison between counties and introducing bias into the results.

To address this issue, I calculate the population difference between counties within each pair and scale it by the population of the smaller county. This provides a numerical value that represents the population disparity. For example, a value of 200% indicates that the larger county's population is three times that of its neighboring county. I then systematically exclude county-pairs where this population difference exceeds a certain threshold, ranging from 2000% down to 100%. In this section, I report my findings using only the subsample of observations from the period in which average SNAP benefits increased – 2005 to 2013 –, and present the results in Table 8. I first note that throughout the entire analysis, the point estimates remain negative. However, when I restrict the sample to counties with a population difference of less than 300%, these estimates become statistically insignificant. At first glance, this suggests that policy changes in larger counties may disproportionately affect outcomes in their neighboring counties. However, I also note that as the sample size decreases, statistical power is reduced due to a loss in degrees of freedom.

3.4.5 Commuting Zones as Local Economic Areas

The pair-by-time fixed effects employed in the empirical strategy allows the local economic conditions that are shared between the county-pair to vary over time. In this section, I expand this definition of local economic condition and consider commuting zones as local economic areas. Generally, a commuting zone encompasses a larger geographical area than just two contiguous counties and is defined by commuting patterns rather than state boundaries. I report three types of results in this section: first, the baseline results with pair-by-time fixed effects; second, a specification that replaces pair-by-time fixed effects with commuting zone-by-time fixed effects; and third, a specification that includes both. My findings are presented in Table 9. I note that the point estimates remain statistically significant and negative in all specifications. Notably, when I include both pair-by-time and commuting zone-by-time fixed effects, the magnitude of the coefficient increases by 50%. Now, a $10 increase in average SNAP benefits reduces fatal overdose rates by 0.776 per 100,000 (or about 5.5% of the mean). One possible reason for this is that commuting zones better capture local economic conditions and regional dynamics.

3.4.6 Spatial Spillovers

Spatial spillovers refer to the unintended effects that an intervention or treatment in one geographic area may have on neighboring regions. In a cross-border research design, this becomes a concern because policy changes in one county or region can influence outcomes in nearby areas, confounding any obtained estimates. If spillovers are not properly accounted for, it may lead to biased estimates, as the treatment effect observed in one region could be distorted by the impacts of neighboring regions. Dube et al. (2010) proposed a method to test for spatial spillovers, suggesting the use of an average of all interior counties as a control county. To implement this, I replace one county in each county pair with its

corresponding "interior average county" and re-estimate the model. I then compute the statistical significance of the absolute difference between the coefficient estimates obtained from this modified model and the baseline coefficient estimates presented in Table 2. A statistically significant difference would indicate the presence of spatial spillovers. In my analysis, I fail to reject the null hypothesis even at the 10% significance level, suggesting no evidence of spatial spillovers, regardless of the sample of choice – be it the full sample or the subsample corresponding to the period where SNAP benefits increased.

3.5 Conclusion

This paper examines the relationship between changes in SNAP benefits and fatal overdose rates. I find that increases in SNAP benefits lead to a statistically significant reduction in overdose deaths, with no corresponding rise in overdose rates following subsequent decreases in benefits – suggesting an asymmetric response. The effect is particularly pronounced among males, pointing towards potential gendered dimensions in how financial support influences substance use outcomes. These findings are robust to alternative definitions of the SNAP benefit increase period, the use of real versus nominal SNAP benefit dollars, and broader definitions of local economic areas, lending additional credibility to the results. Taken together, these results highlight the importance of social safety net programs not only as tools for poverty alleviation, but also as potential interventions in mitigating public health crises such as the drug overdose epidemic.

That said, one limitation of using average SNAP benefits as the policy variable is that changes in benefit levels over time may reflect shifts in the composition of recipients. For instance, even holding income constant, a married household with children typically receives more SNAP benefits per person than a married household without children. As a result, average benefit levels may increase due to changes in household structure rather than policy changes. To address this concern, future work can consider using simulated instru-

ments (Hoynes and Schanzenbach, 2009), constructed by applying the national SNAP benefit schedule to a fixed or historical distribution of household characteristics – such as from the American Community Survey (ACS) – at the state level. This generates a predicted average benefit that varies over time solely due to policy changes, and not due to demographic shifts. Such an approach offers a plausibly exogenous source of variation for estimating causal effects. In future work, I plan to construct and implement such an instrument to assess the sensitivity of my findings.

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3.8 Appendix

3.8.1 Repeated Observations in a Contiguous County Research Design

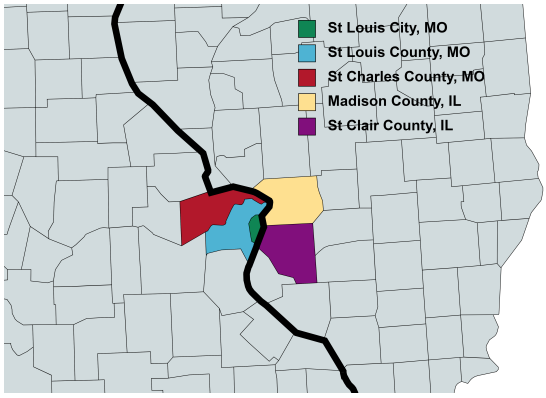

Figure 1: Snapshot of Counties along the IL-MO State Border

In my contiguous county research design, repeated observations arise because certain counties serve as comparison units in multiple county pairs. As seen in the map above, there are five counties but four county pairs:

1. St. Charles County (MO) – Madison County (IL) (Red-Yellow)
2. St. Louis County (MO) – Madison County (IL) (Blue-Yellow)
3. St. Louis City (MO) – Madison County (IL) (Green-Yellow)
4. St. Louis City (MO) – St. Clair County (IL) (Green-Purple)

This structure results in some counties appearing multiple times—for example, Madison County (IL) appears in three separate pairs, while St. Louis City (MO) appears in two pairs. These repeated observations allow for a richer set of comparisons, improving the precision of estimates by leveraging multiple contiguous relationships. Without repeated observations, I would be limited to at most two county pairs, significantly reducing the sample size and limiting the ability to detect policy effects. The inclusion of repeated observations enhances statistical power while maintaining valid comparisons based on geographic proximity.

3.9 Tables

Table 1: Summary Statistics for Outcome Variables

Outcome Variable (per 100,000)	(1) Total	(2) Males	(3) Females
Panel I (Full Sample)			
Overdose Death Rates	16.43	9.95	6.48
	(12.85)	(8.96)	(5.90)
N	23632	23632	23632
Panel II (Excluding Repeated Counties)			
Overdose Death Rates	15.28	9.51	6.29
	(12.47)	(8.71)	(5.89)
N	12000	12000	12000

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$. Standard Deviations in parentheses.

Table 2: Baseline Regression Estimates

Outcome: Death Rate (per 100,000)	(1)	(2)	(3)
Average SNAP Benefit ($)	-0.0221	-0.0511*	-0.0119
	(0.0189)	(0.0266)	(0.0213)
N	23504	14096	15680
Mean Outcome	16.43	13.97	18.30

Note: Standard errors in parentheses are clustered by pair and county. Column (1) corresponds to the full sample period 2005 to 2019. Column (2) corresponds to the sample period where SNAP benefits increased (2005 to 2013). Column (3) corresponds to the sample period where SNAP benefits fell (2010 to 2019).

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Regression Estimates for Gender

Outcome: Death Rate (per 100,000)	(1)	(2)
Panel I (Males)		
Average SNAP Benefit ($)	-0.0120	-0.0321*
	(0.0140)	(0.0191)
N	23504	14096
Mean Outcome	9.94	8.30
Panel II (Females)		
Average SNAP Benefit ($)	-0.0100	-0.0190
	(0.00924)	(0.0169)
N	23504	14096
Mean Outcome	6.49	5.67

Note: Standard errors in parentheses are clustered by pair and county. Column (1) corresponds to the full sample period 2005 to 2019. Column (2) corresponds to the sample period where SNAP benefits increased (2005 to 2013).

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Regression Estimates using Death Rates (By Age Group) as the Outcome Variable

Outcome Variable: (Death Rates Per 100,000)	Under 18	Age 20-29	Age 30-39	Age 40-49	Age 50-64	Age 65+
Panel I (Full Sample)						
Average SNAP Benefit ($)	-0.00431	0.00201	-0.0131	0.00383	-0.0116	0.00110
	(0.00289)	(0.00515)	(0.00839)	(0.00672)	(0.00787)	(0.00317)
N	23504	23504	23504	23504	23504	23504
Mean Outcome	0.307	2.618	3.867	4.437	3.979	0.726
Panel II (Sample Period 2005 to 2013)						
Average SNAP Benefit ($)	-0.00557	-0.0210**	0.00282	-0.00516	-0.0.0156	-0.00660
	(0.00382)	(0.00900)	(0.0114)	(0.0124)	(0.0136)	(0.00507)
N	14096	14096	14096	14096	14096	14096
Mean Outcome	0.342	2.296	3.090	4.174	3.513	0.555

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Note: Standard errors in parentheses are clustered by pair and county.

Table 5: Regression Estimates (Varying Definition of Subsample)

Outcome Variable: Death Rates Per 100,000	(1)	(2)	(3)	(4)	(5)	(6)
Sample Period:	2005 - 2015	2005 - 2014	2005 - 2013	2005 - 2012	2005 - 2011	2005 - 2010
Average SNAP Benefit ($)	-0.06822***	-0.06005**	-0.05110*	-0.06290**	-0.07825**	-0.09625***
	(0.02312)	(0.02515)	(0.02659)	(0.02923)	(0.03293)	(0.03668)
N	17232	15664	14096	12528	10960	9392
Mean Outcome	14.59	14.23	13.97	13.70	13.42	13.05

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Note: Standard errors in parentheses are clustered by pair and county.

Table 6: Regression Estimates (Nominal vs Real SNAP $)

Outcome Variable: Death Rates Per 100,000	(1)	(2)	(3)	(4)	(5)
Average SNAP Benefit ($)	-0.05110*	-0.06175*	-0.06039*	-0.06134*	-0.06093*
	(0.02659)	(0.03219)	(0.03299)	(0.03330)	(0.03340)
N	14096	14096	14096	14096	14096
Mean Outcome	13.96	13.96	13.96	13.96	13.96

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Note: Standard errors in parentheses are clustered by pair and county. Baseline estimates using nominal average SNAP benefits are presented in Column (1). Columns (2) to (5) represent real average SNAP benefits. Column (2) deflates SNAP dollars to 2005 values using CPI, while Columns (3) to (5) deflates SNAP dollars to 2005 values using USDA's Thrifty Food Plan Costs for a 2-person married couple, 2-person married couple with 2 young kids (under the age of 5), and 2-person married couple with 2 older kids (over the age of 5).

Table 7: Regression Estimates (Frequency Weights)

Outcome: Death Rate (per 100,000)	Unweighted	Freq. Weights
Panel I (Full Sample)		
Average SNAP Benefit ($)	-0.0221	0.00026
	(0.0189)	(0.0201)
N	23504	13982326
Mean Outcome	16.43	16.41
Panel II (Sample Period 2005 to 2013)		
Average SNAP Benefit ($)	-0.0511*	-0.04452
	(0.0266)	(0.03315)
N	14096	8388106
Mean Outcome	13.97	13.96

Note: Standard errors in parentheses are clustered by pair and county. Column (1) corresponds to the full sample period 2005 to 2019. Column (2) corresponds to the sample period where SNAP benefits increased (2005 to 2013). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Regression Estimates (Population Difference)

Outcome Variable: (Death Rates Per 100,000)	2000%	1000%	900%	800%	700%	600%
Average SNAP Benefit ($)	-0.05114*	-0.05107*	-0.04935*	-0.05056*	-0.04982*	-0.04756*
	(0.02664)	(0.02754)	(0.02759)	(0.02771)	(0.02815)	(0.02841)
N	14060	13736	13700	13574	13412	13358
Mean Outcome	13.98	13.98	13.98	13.96	13.97	13.98
	550%	500%	400%	300%	200%	100%
Average SNAP Benefit ($)	-0.05832**	-0.05905**	-0.05269*	-0.03095	-0.01856	-0.01997
	(0.02862)	(0.02867)	(0.02952)	(0.02960)	(0.03216)	(0.03955)
N	12980	12728	12314	11504	10622	7364
Mean Outcome	13.94	13.89	13.93	13.76	13.81	13.56

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Note: Standard errors in parentheses are clustered by pair and county.

Table 9: Regression Estimates (Commuting Zones)

Outcome: Death Rate (per 100,000)	(1)	(2)	(3)
Average SNAP Benefit ($)	-0.05110*	-0.04371*	-0.07755**
	(0.02659)	(0.02378)	(0.03282)
Fixed Effects			
Pair-by-Time	✓		✓
Commuting Zone-by-Time		✓	✓
N	14096	13690	13286
Mean Outcome	13.97	14.00	14.057

Note: Standard errors in parentheses are clustered by pair and county. In all columns, the sample period is 2005 to 2013.

* $p < .1$, ** $p < .05$, *** $p < .01$

3.10 Figures

Figure 2: Map of Counties in Sample

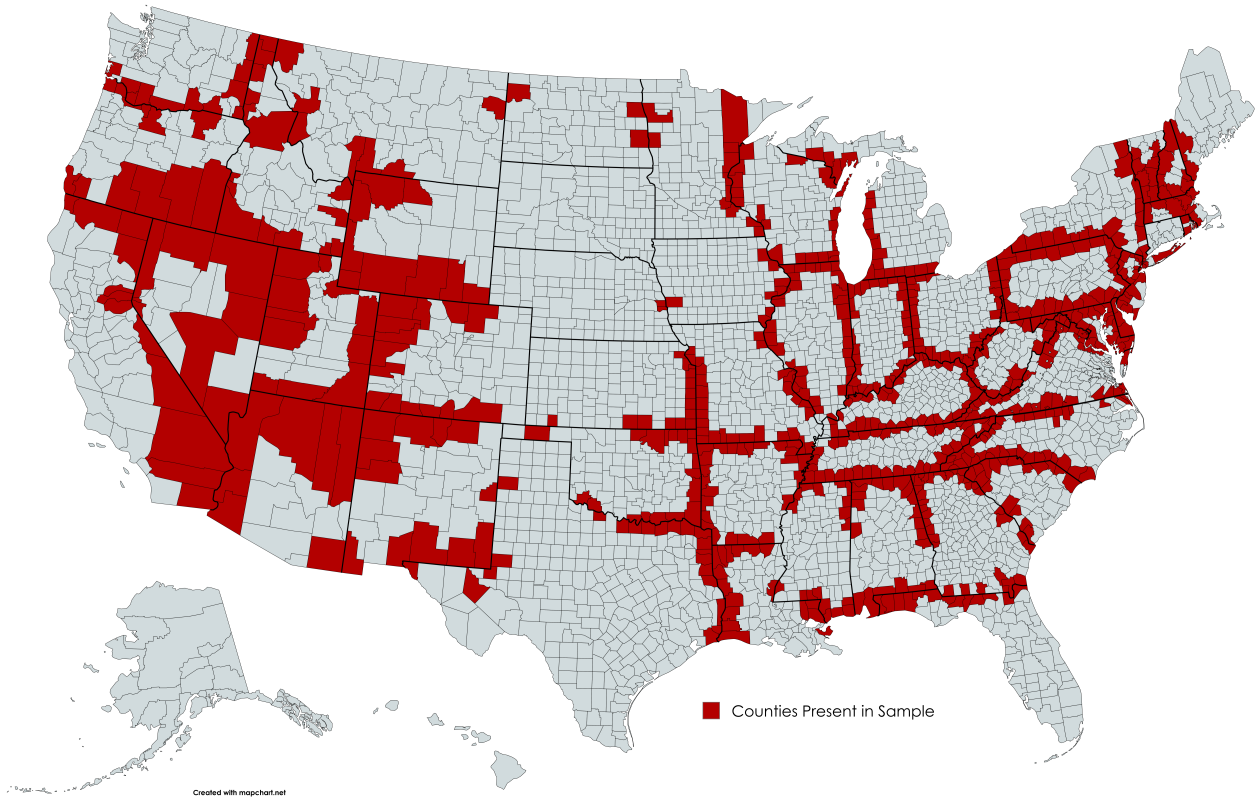

Figure 3: Dot Plot for Average Monthly SNAP Benefit ($)

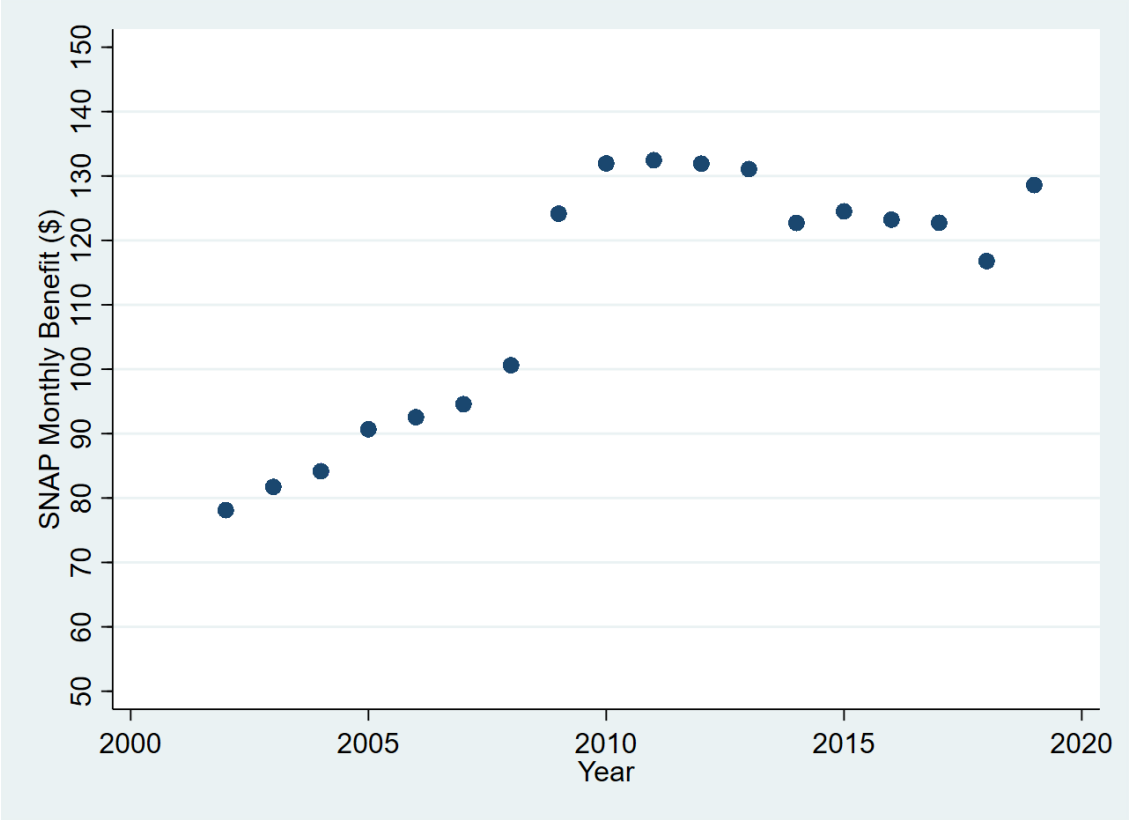

Figure 4: Plot of Average Monthly SNAP Benefit ($), By State