The Early Opioid Epidemic and Medicaid: Is Prescription Access to Blame?

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Abstract

Is the opioid epidemic attributable to prescription painkillers becoming more accessible? I find that, for an average county, Medicaid expansions under the Affordable Care Act caused approximately 175,000 more opioid units to be prescribed per year, and 4 additional opioid-related deaths per year. Medicaid expansions explain nearly a third of the overall death toll from 2012-2016. These results are driven largely by deaths of white men aged 18-65, and vary by local access to marijuana (an opioid substitute). Results are robust to treatment heterogeneity concerns. After estimating the interactive impact of Medicaid expansions and marijuana legalization on opioid-related deaths, I conclude that opioid mortality can be reduced without restricting opioid access.

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1 Introduction

The United States is in the midst of a drug overdose epidemic driven by a rise in opioid overdose. In 2020, over 68,000 people died from opioid overdose (Hedegaard et al., 2021).[1] This death toll - more than 100 people per day - makes opioid misuse as deadly as car accidents and gun shots (Paulozzi, 2012). Because of its magnitude, the opioid epidemic has drawn concern from the public and policy-makers alike, prompting many to ask what policies might be able to combat this epidemic.

To what extent has the epidemic been driven by changes in the accessibility of opioid prescriptions? I exploit the staggered state-level expansion of the Medicaid program (as allowed under the Affordable Care Act) as a natural experiment to ascertain whether increased access to medical services, including prescription drugs, increased opioid-related deaths. The impact of Medicaid expansion on opioid-related deaths is theoretically ambiguous: on the one hand, expansions increased access to opioid abuse treatments (Wen et al., 2017), but on the other hand, expansion increased access to opioid prescriptions. Using a two-way fixed-effects estimation strategy, I produce difference-in-difference estimates of the impact of Medicaid expansion on opioid-related deaths. I find that states that expanded Medicaid under the Affordable Care Act saw substantial increases both in opioid prescriptions and in opioid-related deaths. These results vary strongly by demography, being driven largely by deaths of white men aged 18-65. A back of the envelope calculation suggests that for an average county, Medicaid expansion caused approximately 2,800 more people to be insured per year, 175,000 more opioid units to be prescribed per year, and 4 additional opioid related deaths per year. Overall, these opioid accessibility shocks explain about 12,000 opioid deaths per year, or nearly a third of the overall death toll from 2012-2016. Main results using a two-way

[1] Author's calculations from restrcited CDC Mortality Files suggest that in 2016, approximately 30,000 deaths were attributable to opioid misuse.

fixed effects estimator are robust to concerns of treatment heterogeneity (Goodman-Bacon et al., 2019; Sun and Abraham, 2020; de Chaisemartin et al., 2018) (Appendix B).

Because my estimation strategy controls for location-specific and time-specific factors, it is able to isolate the causal impact of Medicaid expansion on opioid mortality. I find no evidence of pre-existing differences in opioid-related mortality, opioid prescription rates or health insurance coverage prior to Medicaid expansion. I find larger but less precisely estimated results when I restrict the sample to counties on the geographic border between expander and non-expander states. I also find larger but less precisely estimated results when I restrict the sample to states which are matched based on their pre-existing propensities to expand Medicaid. I show that my main estimates are robust to the inclusion of six separate state-level controlled substance prescribing policies including prescription drug monitoring programs (PDMP), and robust to the inclusion of county-year unemployment measures. I show in a triple difference specification that the effects of Medicaid expansion are larger in counties with larger pre-existing uninsured populations. I also show how the effects of opioid-access on opioid deaths vary by demography, as well as how the effects vary by pre-existing access to marijuana, an opioid substitute. Furthermore, I show the main results are robust to excluding early adopter states and to correcting opioid death counts in the manner of Ruhm (2018). Lastly, I show how Medicaid expansion interacts with the state-level decision to legalize recreational marijuana, and find that marijuana legalization was able to mitigate the increases in opioid-related deaths attributable to Medicaid expansion.

Utilizing variation in both Medicaid expansion and marijuana legalization allows me to study the full interaction of the impacts of opioid access and opioid-substitute access on opioid mortality. Given that I find evidence that increases in opioid-accessibility under Medcaid expansions are responsible for approximately a third of the death-toll from 2012-2016, it is natural to ask whether policy ought to aim at restricting access to opioids. On the one hand, my results suggest that restricting access to opioid prescriptions (e.g. encouraging doctors to write fewer prescriptions) could save lives. But, on the other hand, these restrictions might harm those who rely upon, but do not misuse, opioid prescriptions to manage chronic pain. Furthermore, there is evidence that an existing attempt at restricting access to deter misuse (namely, the reformulation of a popular opioid to be abuse-deterrent) caused substitution to heroin, rather than a reduction of deaths (Alpert et al. (2017)). Accordingly, using variation in marijuana legalization allows me to test whether a policy can effectively mitigate opioid-related mortality by expanding access to an opioid-substitute instead of restricting access to opioids.

This paper makes two main contributions to the literatures on the opioid-epidemic, the effects of Medicaid expansion, and the determinants of substance misuse: (1) This paper provides the first evidence that Medicaid expansions and the increases in medical access they occasioned have directly contributed to the opioid crisis. (2) This paper provides the first evidence concerning how Medicaid expansions interacted with the legalization of marijuana, an opioid substitute, to influence opioid mortality. Accordingly, I provide the first policy analysis that compares the relative effectiveness of drug access policies and drug-substitute access policies a policy-maker could use to combat the opioid epidemic.

2 Background

2.1 Is Opioid Prescription Accessibility to Blame?

Popular discussions of the opioid epidemic in the media are replete with anecdotes that feature patients who are prescribed powerful opioids to manage pain and later find themselves suffering from addiction. These anecdotes have motivated some to question whether doctors are overprescribing opioids at least in part because they are motivated by remunerations from pharmaceutical companies. Indeed, there is a large literature on the extent to which physicians, in general, respond to financial incentives (Gruber and Owings (1994), Grant (2009), Clemens and Gottlieb (2014), Hillman et al. (1998), Schnell (2017), Yeh et al. (2016), Wood et al. (2017), Fleischman et al. (2016)). In the case of opioids in particular, there is a small literature documenting various correlations between physician reimbursements and prescription volumes (Hadland et al. (2017), Fleischman et al. (2016), Wood et al. (2017), Yeh et al. (2016)).

These pieces of evidence together paint a picture according to which physician behavior increased the accessibility of opioids and thereby contributed to opioid misuse. Utilizing variation in the implementation of Medicare Part D, Powell et al. (2015) find that increased opioid access for the Medicare-eligible population had a spillover effect on the Medicare-ineligible population, increasing the opioid-death rates for the non-elderly population who did not gain direct access to opioids. Goodman-Bacon and Sandoe (2017) uses public CDC Wonder data to show that while difference in difference estimates suggest that Medicaid expansion increased opioid mortality rates, the mortality effects are not strongly related to state-level uninsurance rates prior to expansion. My study takes advantage of more detailed, restricted mortality data and a richer set of within state variation to offer new evidence of this effect. Likewise, Averett et al. (2019) use restricted CDC data at the state-year level and find "no evidence that Medicaid expansion is related to opioid deaths". My analysis features key county and individual-level variation, which follows the suggestion of Averett et al. (2019) who note that while their analysis is at the state-year level, "a more robust analysis would use individual-level data ... Such an analysis would allow controlling for individual characteristics such as gender, race and age, which might also be predictors of opioid use." Appendix H offers a thorough comparison of my results to Averett et al (2019). While I disagree with Averett et al.'s conclusions, I do concede that if we favored one of Averett et al (2019)'s specifications (using the log of the opioid rate, a large set of control variables, and a subset of the full expansion variation), the evidence for an effect would become weaker

(Appendix H).

2.2 Marijuana as an Opioid Substitute

Because marijuana is a painkiller it can be used to manage chronic pain and hence is a candidate substitute for opioids. Because marijuana is much less likely to cause overdoses than opioids, it is also a safer alternative to powerful, synthetic opioids (Abuhasira et al., 2018). There is a body of evidence showing that access to an opioid substitute such as marijuana lowers opioid mortality (Cerdá et al., 2012; Bachhuber et al., 2014; Monte et al., 2015; Wen and Hockenberry, 2018; Bradford et al., 2018; Hill and Saxon, 2018). However, Mathur and Ruhm (2022), using microdata from 1999-2019 find results which contrast with Bachhuber et al. (2014). (See Appendix G for a full discussion.) Still there does not yet exist in the literature an estimate of the mortality effects of marijuana legalization relative to the mortality effects of Medicaid expansion. Understanding the interaction between marijuana access and Medicaid expansion can help address whether policy attempts to reduce opioid deaths would be better served by addressing opioid access or opioid-substitute access.

3 Data

Data measuring health insurance coverage are taken from Census Small Area Health Insurance Estimates (SAHIE), which measure the number of uninsured at the county-year level. Because the ACA Medicaid expansions expanded coverage for childless adults aged 18-65 with income at or below 138 percent of the Federal Poverty Line (FPL), my variable of interest is the uninsured rate for adults between the ages of 18 to 65 whose reported income falls at or below 138 percent of the FPL. Table 1, Panel A contains the summary statistics of these insurance variables. Expander states have a 9 percentage point lower rate of uninsured individuals (Table 1, Panel A, Row 2).

The Centers for Medicare and Medicaid Services (CMS) claims data give counts of prescription drug products at the state-quarter level for the universe of claims billed to Medicaid. I use these data to measure the extent to which Medicaid expansion affected the number of units of opioids prescribed, the number of opioid prescriptions, as well as the amount reimbursed for opioids.[2] Table 1, Panel B contains summary statistics for variables measuring opioid drug claims filed through Medicaid. We can see that across several measures, expander states provided more opioids on average than non-expander states.

Data measuring opioid-related deaths are taken from restricted CDC Mortality files. These individual level data include demographic information about the deceased (race, ethnicity, gender, educational attainment) as well as ICD-10 codes which classify the imputed "underlying cause of death". I count a death as "opioid-related" if one of its cause of death codes involves an opioid misuse.[3] My key outcome of interest is the rate of opioid-related deaths at the county-quarter level, which I obtain by collapsing the individual level data. While an average county saw less than two opioid-related deaths per quarter, some counties experienced more than 100 such deaths in a month. As the summary statistics contained in Panel C of Table 1 show, expander states average approximately 2 additional deaths per quarter.

4 Medicaid Expansion and Opioid Deaths

I utilize the staggered state-level Medicaid expansion licensed by the Affordable Care Act to provide evidence that increased access to prescription drugs increased opioid-related deaths (Sections 4.1- 4.2). Appendix B shows that these estimates are not biased by treatment-cohort heterogeneity. I address key identification concerns (Section 4.3), and show that the

[2] I classify prescription drug products as opioids following Jena et al. (2014). For details, see Data Appendix.

[3] This includes the following ICD-10 codes: T40.1 (Heroin), T40.2 (Other opioids), T40.4 (Other synthetic narcotics)

main estimates are robust to introducing other state-level policy variation, robust to controls for local economic conditions, robust to excluding early adopters, and robust to correcting opioid death counts in the manner of Ruhm (2018) (Section 4.4). I find that, for an average county, Medicaid expansion caused approximately 2,800 more people to be insured per year, 175,000 more opioid units to be prescribed per year, and 4 additional opioid-overdose deaths per year. Overall, the increases in opioid-access occasioned by Medicaid expansion explain about 12,000 opioid-related deaths per year, or nearly a third of the overall death toll from 2012-2016.

4.1 Estimation

I use a two-way fixed effects model to estimate the impact of Medicaid expansion on opioid-related deaths, health insurance coverage, and opioid prescriptions. My main specification is a difference-in-difference equation:

$$y_{cst} = \alpha_c + \alpha_t + \beta Expansion_{st} + \epsilon_{cst}, \tag{1}$$

where y_{cst} is the either the rate of opioid-related deaths, the rate of uninsured persons, or the rate of opioid prescriptions in county c, state s and time period t. In all specifications, standard errors are clustered at the state-level. I am primarily interested in the coefficient on $Expansion_{st}$, which indicates that the county-quarter observation is from an expander state after it expanded Medicaid.

To understand the dynamics of the effects, I also use a dynamic two-way fixed effects model in which I include a set of variables which indicate time periods away from Medicaid expansion:.

$$y_{cst} = \alpha_c + \alpha_t + \sum_{\tau=-n}^{n+} \beta_\tau 1(t = T_s + \tau) + \epsilon_{cst}, \tag{2}$$

where y_{cst} is defined as in Equation 1. I am primarily interested in the coefficients on the indicator variables, $1(t = T_s + \tau)$, each of which indicates how many time periods t in state s a given observation is removed from the first time period in which state s expanded Medicaid, T_s. In all specifications, standard errors are clustered at the state-level. Main results using a two-way fixed effects estimator are robust to concerns of treatment heterogeneity (Goodman-Bacon et al., 2019; Sun and Abraham, 2020; de Chaisemartin et al., 2018) (Appendix B).

4.2 Main Results: Opioid Death Increases

Ex ante, we expect an expansion in access to health insurance to result in increased access to medical services including opioid misuse treatments (Wen et al., 2017), which we expect to improve health outcomes and decrease mortality. If, however, physicians were prescribing opioids for pain treatment in ways that, on average, harm patients we might find that Medicaid expansion results in increased opioid mortality. To test this, I estimate equation 1 and equation 2, where the dependent variable is the rate of opioid-related deaths.

Figure 1a shows the geographic distribution of states that expanded Medicaid beginning in 2014, while Figure 1b shows the timing of the expansions. Most of the expansions occurred in January of 2014. Results are robust to excluding early adopters (Section 4.4). As Figures 2a and 2b show, before expansion expander and non-expander states were not trending differentially, but after expansion states that expanded began to experience an increase in opioid-related mortality relative to states which did not expand. The coefficients estimates show that, in the first quarters after expansion, counties in expander states experienced an increase in opioid-related deaths of approximately .15 opioid-related deaths per 100,000 residents; within a year after expansion the coefficent estimates are significant at the 5% level and show an increase of nearly .5 opioid-related deaths per 100,000 residents (Figure 2b).[4]

[4]My main results use rates of opioid-related deaths per 100,000 people. The results are robust to using rates of opioid-related deaths per all deaths, the rate of opioid deaths per all overdose deaths, as well counts of opioid related deaths as the dependent variable (See Appendix Figure A5).

Appendix Figure A1 shows that these increases are driven by increases in synthetic opioids, rather than heroin.[5] Appendix Figure A3 shows that these estimates are robust to using up to 24 quarters of pre-expansion and of post-expansion data. Appendix B shows that these estimates are not biased by treatment-cohort heterogeneity.

The static difference-in-difference estimators summarize the overall impact of expansion on opioid-related deaths; the estimate in Column (3) of Table 2 indicates that Medicaid expansions are responsible for approximately .4 more opioid-deaths per 100,000 residents per county-quarter, an increase of approximately 28% or 4 more opioid-related deaths per county-year. Column (2) of Table A2 shows that these effects are four times as large in counties that had above the median rate uninsured adults who would have become newly eligible for Medicaid under the ACA expansion. Thus, I conclude that Medicaid expansion is responsible for a statistically significant and economically meaningful increase in opioid-related deaths.

It is well known that the Medicaid expansion under the ACA increased health insurance coverage (See Kaestner et al. (2017)). Nevertheless, in order to produce first stage estimates to aid the interpretation of my main reduced form estimates, I estimate Equations 1 and 2 where the dependent variable is the rate of uninsured adults aged 18-65 whose income falls at or below 138 percent of the FPL.[6] In my preferred specification I find that, for an average county, Medicaid expansion led approximately 2,800 more individuals to be insured per year.[7]

Figure 3 shows estimates of Equation 2 where some measure of opioid-drug reimbursement

[5]Appendix Figure A2b shows results separately for all other T40 ICD-10 codes including opium, methadone and "Other unspecified narcotics". Appendix Figure A2c shows the main results are robust to their inclusion. The breakdowns in Appendix Figure A1 which show lagged effects of deaths attributable to Heroin, are consistent with an account in which persons switched over from prescription opioids to illegal opioids, but does not prove that such switching occurred.

[6]Appendix Figure A4b shows the dynamic difference-in-difference estimates for health insurance coverage.

[7]I find that Medicaid expansion increased the share insured by 6.8% for those aged 18-65 with incomes at or below 138% of the FPL (Table 2). This estimate is consistent with existing estimates in the literature. Miller (2017) finds an increase of 8.2% for those aged 19 to 64 with incomes below 138% of the FPL. Black et al (2019) studies a different demographic and finds a much smaller increase: expansion increased insurance coverage by only 1.1% for those aged 50-64, and by 4% for low-educated populations of that age.

by Medicaid is the dependent variable. Panel (a) of Figure 3, shows the dynamic estimates of the rate of opioid drug units reimbursed by Medicaid[8]; Panel (b) shows the dynamic estimates of the dollar amounts reimbursed by Medicaid for those drug units. While the difference-in-difference estimates hover around zero prior to expansion, after expansion there is a large and rather pronounced relative increase in the expander states both in opioid units prescribed and in opioid units reimbursed. In my preferred specification, I estimate that Medicaid expansion led 2,800,000 more opioid units to be prescribed per state-quarter or 175,000 more opioid units to be prescribed per county-year.[9] Appendix F shows that comparable estimates can be obtained using an alternative data source on opioid drug distribution.

In summary, Table 2 compares the difference-in-difference estimates for insurance coverage, opioid prescriptions and opioid-related deaths. The estimates listed across Columns (1)-(3) of Table 2, suggest that the ACA Medicaid expansions through 2016 were responsible for increasing insurance coverage by nearly 7 percentage points, increasing the opioid prescription rate by .01 prescriptions per person and increasing the opioid-death rate by .4 per 100,000 persons. These estimates imply that, for an average county, Medicaid expansion caused approximately 2,800 more people to be insured per year, 175,000 more opioid units to be prescribed per year, and 4 additional opioid-overdose deaths per year. Consistent with Black et al. (2019) I do not find evidence of Medicaid expansion increasing overall mortality.[10] Overall, the increases in opioid-access occasioned by Medicaid expansion explain about

[8]CMS reports drug unit counts, where the "drug unit" is based on the unit type, and varies across national drug codes (NDC). The Drug Appendix below details which drugs I classify as opioids. Opioid drug units vary in strength by NCD code, and are not standardized to their unit morphine equivalents.)

[9]Calculated as the opioid unit increase (2.8 million) multiplied by four quartesr, and divided by 63 counties. The average state in the US has 63 counties.

[10]My estimates are consistent with Black et al. (2019)'s cautions about the use of the ACA Medicaid expansions as a natural experiment. Studying the general adult population Black et al. (2019) do not find a statistically significant effect of Medicaid expansion on overall mortality. Black et al. (2019) point out that overall mortality is an infrequently occurring outcome which health insurance would likely impact only in the long run. Black et al. (2019) also show that "the nationwide natural experiment provided by the ACA is severely underpowered to detect effects on mortality of plausible size in county-level death certificate data", and recommend focusing analysis on subpopulations which experienced larger insurance gains and were more sensitive to the mortality effects of healthcare utilization. In accordance with these recommendations, my

12,000 opioid-related deaths per year, or nearly a third of the overall death toll from 2012-2016. Back of the envelope calculations suggest that, on average, a newly insured Medicaid enrollee was prescribed 63 opioid units and that one opioid-related death occurred for every 44,000 newly prescribed opioid-units.

4.3 Identification: Threats to Internal Validity

An identification concern would arise if, for example, states that expanded insurance access for the poor were, during this same time period, also implementing policies that in some way encouraged substance abuse. Ex ante it would be reasonable to expect that states which expanded Medicaid were also actively pursuing various means of *improving* health outcomes and *decreasing* substance abuse, which would suggest taking my estimates as a lower bound of the true effect. Nevertheless, to address threats to internal validity, I provide evidence of internal validity using three samples: all counties ("main sample"), counties on the geographic border ("border sample"), counties on the margin of expansion ("propensity sample").

4.3.1 Main Sample: Common Trends

In the main sample, there are no pre-period differences either in calendar-year trends or in dynamic difference-in-difference estimates across expander and non-expander states, with respect to opioid-related death measures, opioid prescription measures, and health insurance measures. Figure 2a shows that, prior to all Medicaid expansions, expander states

study is focused on a subpopulation where the first stage is 6 times as large as that in Black et al. (2019) and concerns a specific cause of mortality which is known to be very dangerous in the short run and whose occurrence was rising rapidly during my sample period. Finally, Black et al. (2019) perform several power calculations (on age groups 55-64) concerning the minimally detectable effect size and find that to get power of 80% you would need an overall mortality effect of 1.8%, but found an overall first stage of 1.1%, implying that to have 80% power you would need an unreasonably large treatment on the treated effect of 1.8/1.1 = 163%. I find a mortality effect of 28%, the size of which I attribute to the highly lethal nature of synthetic opioids. I conclude that, for the specific subpopulation and cause of death I am studying, the study design has sufficient power.

track non-expander states in rates of opioid-related deaths, while Figure 2b shows that the dynamic difference-in-difference estimates in the pre-period hover around zero, confirming that expander and non-expander states were on common trends prior to expansion.[11] Secondly, prior to expansion, expander and non-expanders states also share common trends in health insurance measures and opioid prescription rates (Appendix Figures A4a and Appendix A4b). Figure A4a shows that expander and non-expander states shared common trends in uninsured individuals aged 18-64 with income at or below 138% of the Federal Poverty line residing in their counties; Figure A4b shows that the difference-in-difference estimates of health insurance rates before expansion also hover around zero, confirming that the groups share a common trend. Lastly, as Figure 3 shows, the difference-in-difference estimates of opioid prescriptions before expansion hover around zero both for opioid units reimbursed by Medicaid and amounts reimbursed.

4.3.2 Geographic Boundaries (Border Sample)

As a further identification check, I restrict my analysis to counties on the geographic border between states which expanded and states which did not (Appendix C). This approach addresses the concern that expander and non-expander states differ in unobservable ways that are correlated by local geography. Taking advantage of state borders allows us to compare counties in expander states to neighboring counties in non-expander states in such a way that counties in both treatment and control groups would share these unobservable, geographically clustered characteristics.

Figures C1a and C1b shows trends and dynamic difference-in-difference estimates of opioid-deaths for this geographically restricted border sample. Table C1 reports the results obtained when I apply my main estimation strategy to the border sample. I find results in

[11] The dynamic difference-in-difference estimates in the pre-period also hover around zero when the dependent variable is opioid mortality measured as a rate per all deaths, as a rate per all overdose deaths as well as counts (See Appendix Figures A5).

line with my main results (Table C1). Estimates using the border sample are approximately twice as large as estimates from the sample which includes all counties, but the border sample includes less than $1/6^{\text{th}}$ of all the counties and the standard errors are more than twice as large, resulting in greater imprecision.[12]

4.3.3 Determinants of Expansion (Propensity Sample)

To further explore the internal validity of my identification strategy, I consider a rich set of state level covariates including variables which measure the political environment of the state, the expenditure portfolio of the state, as well as demographic and economic conditions. I use these measures to conclude that expansion was driven by the pre-period political environment in the state legislature, and to form a subsample of states chosen based on the propensity of each state to expand.

First, I estimate an equation of the form:

$$Pr(Expansion_s) = \sum \beta_{s,pol} Z1_{s,pol} + \sum \beta_{s,exp} Z2_{s,exp} + \sum \beta_{s,demog} Z3_{s,demog} + \sum \beta_{s,econ} Z4_{s,econ} + \epsilon_s \quad (3)$$

where $Z1$ is a vector of variables measuring the state's political environment, $Z2$ is a vector of variables measuring state level expenditures, $Z3$ is a vector of variables measuring demography, and $Z4$ is a vector of variables measures economic conditions. (All measures are taken in the year prior to expansion. Non-expanders are states which failed to expand by the end of 2016.) Column 1 of Table 3 reports the results of estimating Equation 3. The strongest predictor of expansion is the percent of the state's lower chamber which is Republican. This along with the coefficient on education expenditures, welfare expenditures, and percent white, suggests that the state level decision to expand was strongly associated with

[12]Calculated by comparing column (2) in the top and bottom panels of Table C1, each scaled by their different means (.442/1.744)/(.165/1.553)=2.4.

the state-level political environment in the time-period prior to expansion.[13] I conclude from these results that the choice to expand was driven by the pre-period political composition of the state legislature. I do *not* use pre-period political composition as an instrument for the decision to expand, and Democratic legislators might have expanded Medicaid partly to alleviate substance abuse disorder in their constituencies. However, given that the pre-period political composition along with other covariates predicts expansion, the covariate predicted propensity to expand can be used to construct a sample of states on the margin of expansion.

In Appendix D, I run my main estimation strategy on a subsample of states chosen based on the propensity of each state to expand. Figure D1 shows the states included in the propensity restricted sample, while Table D1 shows that expander and non-expander states in this sample differed by only 1 percentage point in their calculated propensity to expand. Figure D2a shows trends in opioid deaths across expander and non-expander states in this propensity restricted sample. Table D2 reports the results obtained when I apply my main estimation strategy to this propensity restricted sample. I find results in line with my main results (Column (3) of Table 2). Estimates using the propensity restricted sample are nearly twice as large as estimates from the sample which includes all counties, but the propensity sample includes only $1/6^{\text{th}}$ of all the counties and the standard errors are also about 6 times as large, resulting in much greater imprecision.

4.3.4 Placebo Tests

To further corroborate the internal validity of my identification strategy, I perform a battery of placebo tests which show:

1. Age 65 and over mortality does not respond to expansion (Figure A6).
2. Opioid mortality estimates are substantially larger when focused on likely Medicaid beneficiaries (Figure A7).

[13] Column 2 of Table 3 shows that these and other associated factors were also predicative of the timing of the state-level decision to expand.

3. Deaths from poisoning by nonopioid analgesics do not respond to expansion (Figure A8).
4. Deaths from five separate non-prescription drug mortality categories (cocaine, cannabis, LSD, hallucinogens, alcohol) do not respond to expansion (Figure A8).
5. Deaths from four non-prescription, non-drug poisonings (metals, foodlike substances, seafood, animal venom) do not respond to expansion (Figure A8).
6. All-cause mortality does not respond to expansion.

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If Medicaid expansions increased opioid mortality we would expect the effects to be driven by deaths of 18-65 year olds rather than those 65 and older[14], which is exactly what Figure A6 shows. Furthermore, if expansion increased opioid mortality we would expect the effects to be driven by likely Medicaid beneficiary populations. To test this, I compute the opioid-mortality rate per 100,000 persons in the "Medicaid population", where I define the "Medicaid population" as the number of persons who are uninsured and at or below 138% of the federal poverty line. I then weight these estimates by the county-level Medicaid population in 2013 (the last pre-period). Figure A7 compares my main estimates (which use deaths per 100,000 total population) in panel (a) with estimates using the Medicaid population in panel (b). The estimates using the Medicaid population are very tightly concentrated around zero in the pre-period and jump upwards very rapidly in the post period. Overall, they are approximately 10 times larger than the main estimates (Figure 2b).

Figure A8 reports results from a series of placebo tests, which are designed to test whether non-opioid mortality types also responded to expansion. Panels (a) and (b) of Figure A8 show that six non-prescription, non-opioid drug mortality types did not respond to expansion. Panel (a) reports estimates where the dependent variable is, respectively, the rate of deaths

[14] 18-65 year olds are the age group who received the vast majority of the ACA Medicaid coverage expansions.

attributed to "poisoning by nonopioid analgesics, antipyretics and antiheumatics", cocaine, and the "toxic effects of alcohol". Panel (b) reports estimates where the dependent variable is, respectively, the rate of deaths attributed to cannabis, LSD, and "other and unspecified hallucinogens". I find no evidence that Medicaid expansion impacted these categories of non-opioid, non-prescription drug mortality. Panel (c) of Figure A8 shows that four non-prescription, non-opioid, non-drug poisonings mortality types did not respond to expansion. Panel (c) reports estimates where the dependent variable is, respectively, the rate of deaths attributed to the "toxic effect of metals" such as lead and mercury, the "toxic effects of seafood" such as ciguatera, scombroid, and shellfish, the "toxic effect of substances eaten as food" such as mushrooms and berries, and finally the "toxic effect of contact with venomous animals" such as snakes and spiders. I find no evidence that Medicaid expansion impacted any of these poisoning mortality types.

Lastly, I tested whether all-cause mortality responded to Medicaid expansion. Consistent with Black et al. (2019), I find that estimates using all-cause mortality as the outcome variable are noisy and *slightly* negative. The absence of a strong all-cause mortality effect on the general adult population is studied at length in Black et al. (2019).

4.4 Robustness

Following Doleac and Mukherjee (2018), I address the concern that other state laws aimed at preventing opioid-related deaths might be confounding my estimates of the impact of Medicaid expansion on opioid-related deaths. Table 4 shows that the baseline estimates in Table 2 are robust to the inclusion of six separate state-level controlled substance prescribing policies enacted from 2012-2016. Because these policies were designed in part to reduce opioid misuse and opioid-related deaths, they are candidate policy confounders. For example, prescription drug monitoring programs (PDMP), added by 19 states over this period, collect data on controlled-substance dispensing to flag potentially excessive prescribing behavior.[15] In columns (2)-(5) of Table 4, I include various combinations of these potential confounders as regressors. The main results are robust to the inclusion of these policies both individually and collectively.[16]

The main results are not driven by zero counts of opioid deaths (Figure A9) and they are not driven by states such as West Virginia and Kentucky, which were most severely impacted by the epidemic (Figure A10). First, Figure A9 shows results from specifications that address the presence of county-quarter observations with zero opioid-related deaths.[17] Panel (a) shows results at the state-year level, a level of aggregation at which there are no longer any zeroes. Panel (b) compares the main results from Figure 2b with estimates from a sample that drops all counties with zero deaths from 2012-2016 ("dropping all zeroes"). Panel (c) further compares the main results and the "dropping all zeroes" results with estimates from a tobit specification where the lower truncation limit is set to zero. Panel (d) compares the main estimates with estimates from a linear Cragg Hurdle specification, in which the first stage selection estimation of the zero-bound is computed using the log of the county-year population. The results remain robust across these specifications. Second, Figure A10 shows results from specifications that omit states based on how severaly impacted they were by the opioid epidemic. I form state opioid-ranks by dividing the total 2012-2016 opioid deaths by population for each state.[18] I then rank states by this aggregate death rate. I drop the top 2 highest ranking, the top 5, the top 10 and then WV, OH, and KY, respectively in separate regressions. I find that dropping these high ranking states attenuates the post-period estimates towards zero, but that the overall results still look convincing.

[15] Buchmueller (2017) finds that "must access" PDMPs significantly reduce opioid misuse in Medicare Part D. See Meara et al. (2016) for a full description of each policy considered by Doleac and Mukherjee (2018)

[16] An extended version of the state-year policy variation in Meara et al. (2016) was generously shared by Jennifer Doleac and Anita Mukherjee, who utilize these data in similar robustness checks in Doleac and Mukherjee (2018).

[17] Approximately 11% of counties show zero opioid-related deaths from 2012-2016.

[18] The top 5 highest opioid ranked states in ascending order are: RI (5), KY, OH, NH, WV (1).

Furthermore, the baseline estimates in Table 2 are robust to the inclusion of county-year unemployment measures. Table A1 reports estimates from specifications which allow both linear and quadratic contributions from both contemporaneous unemployment and its lags. Columns (2)-(3) of Table A1 shows that these estimates are robust to the inclusion of the contemporaneous county-year unemployment rate and its square. Columns (3)-(6) show robustness to the inclusion of unemployment, its square, as well as two lags and their squares. This suggests that the opioid-death impacts I estimate are not driven by changes in local economic conditions.

Moreover, Appendix Table A2 reports estimates from a triple difference specification, which considers whether the effects of Medicaid expansion are larger in counties with larger uninsured populations. Because the ACA Medicaid expansion opened access to childless adults 18-65 with income at or below 138% of the poverty line, I divide counties into "high" and "low" uninsurance categories based on whether they are above or below the median rate of uninsured individudals aged 18-65 with income at or below 138% FPL. Column (2) of Appendix Table A2 shows that the impact of expansion in high uninsurance counties is 3.5 times larger than in low uninsurance counties.[19]

Furthermore, I show robustness to samples which do not include states that in some way adopted Medicaid prior to 2014 (Figure A11). Following Denham and Veazie (2019), I distinguish four "prior expander" states which had relatively generous eligibility levels pre-2014 (NY, VT, MA, DE).[20] Following Sommers et al. (2013), I distinguish six "early expander" states which engaged in ACA expansion efforts prior to 2014 (CA, CT, D.C., MN, NJ, WA.). I run my main estimates separately on a sample that drops prior expanders, and again on a sample that drops early expanders. Overall, the estimates are very similar

[19]Calculated as the impact in high uninsurance counties (.171+.062+.501.=.734) divided by the impact in low uninsurance counties (.171).

[20]Denham and Veazie (2019) shows that these prior expander states still experienced substantial gains in insurance coverage following the 2014 expansions.

across these samples. If anything, the opioid mortality increases are *slightly* larger when we drop the early expansion states.

Lastly, Ruhm (2018) argues that restricted CDC data understate opioid-involved deaths since “specific drugs leading to death are frequently not identified on death certificates”. To ensure that these under counts do not vary systematically with Medicaid expansion, I apply Ruhm (2018)’s code to the data from 2012-2016 and rerun my specifications with Ruhm-corrected opioid mortality rates as the dependent variable (Figure A12). Because Ruhm-corrected opioid deaths are at least 20% larger than the raw rates, Figure A12 compares estimates using raw data with estimates using Ruhm-corrected data as percentages of their respective pre-period means. Overall, the Ruhm-corrected estimates are qualitatively similar to the raw-data estimates, the main difference being that the Ruhm-corrected estimates suggest that opioid-related deaths increase more slowly even a year or more after expansion, and that pre-period estimates exhibit less variation than those using raw-data. The broad result that Ruhm-corrected estimates exhibit less dynamic variation is likely due to the fact that the Ruhm corrections are achieved by using individual level and county-level controls (including median income, education and poverty) to predict deaths. These covariates increase the measures of opioid-related deaths, but also smooth out the time-series leading to less dynamic variation. However, the corrective techniques do *not* change opioid related mortality in ways that systematically differ by expansion status or timing.

5 Heterogeneity

In this section, I explore how the effects of opioid-access on opioid deaths vary by demography (section 5.1), as well as how the effects vary by access to marijuana, an opioid substitute (section 5.2). Lastly, I show how Medicaid expansion interacts with the state-level decision to legalize recreational marijuana, and find that marijuana legaization was able to mitigate the increases in opioid-related deaths attributable to Medicaid expansion (section 5.3).

5.1 Heterogeneity By Demography

The results of Sections 4.1-4.4 suggest that approximately a third of the death-toll from 2012-2016 is attributable to increases in opioid access ocassioned by Medicaid expansion. Existing descriptive evidence suggests that the recent rising trend in so-called "deaths of despair" fell largely on white non-Hispanic whites and on men (Case and Deaton (2015)). Accordingly, we might expect the effects of expansion on opioid-related deaths to be larger for white men than for other subgroups.

Figure 4 contrasts dynamic difference-in-difference estimates of Equation 2 restricting to white men with estimates restricting to white women. These estimates suggest that the increases associated with Medicaid expansion are more pronounced for white men than for white women. Figure 5 contrasts demographic specific difference-in-difference estimates of Equation 1 and confirms that the effects are relatively large for men, and white men in particular. Appendix Figure A13 shows further estimate comparisons separately by race, gender and education cells. Overall, I find that whites, white men and those without college degrees are particularly strongly affected.[21]

[21] The difference-in-difference estimates for blacks also suggest very large effects, but the dynamics estimates for blacks exhibit considerably more variance than other subgroups and lack a convincing pattern of solid pre-trends hovering about zero (See Appendix Figure A13c). Accordingly, while I cannot rule out large effects for blacks and black men in particular, these data and this estimation strategy do not provide strong evidence in favor of a causal interpretation for this particular subgroup.

5.2 Heterogeneity by Marijuana Access

Because there is evidence that marijuana is a less dangerous substitute for opioids,[22] we expect effects to vary by access to marijuana.[23] Accordingly, I consider the effects of Medicaid expansion on opioid-related deaths in light of marijuana access in two ways: first, I explore whether states with medical marijuana laws (MMLs) prior to Medicaid expansions experienced differential effects of expansion on opioid-related deaths, and second, I explore how recreational marijuana laws interacted with Medicaid expansion in determining opioid-related deaths. I find, first, that counties with pre-existing MMLs experienced smaller increases in opioid-related deaths when they expanded Medicaid and, second, that recreational marijuana laws substantially lowered opioid-related mortality even in the presence of Medicaid expansion.[24]

5.2.1 Marijuana Legalization Prior to Medicaid Expansion

Following Anderson and Rees (2023) and Anderson et al. (2013), I consider state-year variation in medical marijuana laws (MMLs), distinguishing between MMLs that allow "group growing" (GG) or "collectve cultivation" and those that allow "home growing" (HG). Following Smith (2017) I consider county-year variation in the presence of marijuana dispensaries

[22]While no RCT's have been conducted to compare the effectiveness of opiate and cannabis-based products on chronic pain (Carlini et al 2018), the chemical properties of marijuana can stimulate opioid receptors (Chihuri and li 2019), and there is survey evidence that US adults substitute marijuana for prescription opioids (Ishida et al 2019).

[23]There are several studies that find effects of marijuana legalization on opioid mortality that point in this direction. See Powell et al. (2018), Bachhuber et al. (2014), Wen and Hockenberry (2018), and Hill and Saxon (2018) for an overview. It is important to note that restricted data might be necessary to find these effects. Shovner et al (2019) use public CDC data to extend Bachhuber et al (2014) through 2017, do not find convincing evidence of an effect and suggest that if cannabis laws have an effects on opioid mortality "they cannot be rigorously discerned with aggregate data." Likewise, Chihuri et al (2019) perform a meta-analysis of the impact of marijuana legalization on opioid deaths and find that evidence about the effect of marijuana legalization on opioid overdose mortality is "inconsistent and inconclusive".

[24]There already exist studies of the impact of marijuana legalization on opioid-misuse and opioid mortality (Mathur and Ruhm, 2022; Powell et al., 2018; Bachhuber et al., 2014; Wen and Hockenberry, 2018; Hill and Saxon, 2018). My study complements these studies by interacting the the impact of marijuana legalization with the impact of Medicaid expansion.

(“dispenary penetration”). Taken together, these sources of variation constitute three different types of MMLs enacted prior to Medicaid expansion. Table 5 considers how these different MML types interact with Medicaid expansion in the determination of opioid mortality.

Column (1) of Table 5 repeats the main estimates using the entire sample. Columns (2)-(3) of Table 5 report estimates of Equation 1 restricting the sample to states with any MML and those without an MML, respectively. Together, Columns (2)-(3) show that the impacts of Medicaid expansion on opioid-related deaths are driven by states without MMLs. Columns (4)-(5) of Table 5 report interactive estimates by MML type, distinguishing between MMLs that allowed group growing or collective cultivation (“GG MML Interaction”) and those that allowed home growing (“HG MML Interaction”). Both MML types give a similar qualitative story: MMLs counterbalanced or even slightly outweighed the opioid increases from expansion. Consistent with Anderson and Rees (2023), the interactive impacts from GG MMLs are larger than those from HG MMLs. Lastly, Column (6) reports interactive estimates using the pre-expansion dispensary penetration measure in Smith (2017). In particular, I use county-year level variation in the presence of a dispensary to construct a state-year “dispensary penetration measure”, calculated as the share of counties in a state that have a dispensary. The results qualitatively mirror those obtained in the other MML interactions, showing that states with higher dispensary penetration experienced smaller impacts of expansion on opioid-related deaths following Medicaid expansion. In particular, the results suggest that a non-expander state without any dispensaries could expand Medicaid and also counteract the opioid mortality increases attributable to Medicaid expansion by first allowing dispensaries in as few as 1/3 of its counties.[25] Overall, these results suggest that pre-existing access to marijuana, an opioid substitute, mitigated the opioid-related deaths

[25]Calculated as the conditional effect of full dispensary penetration scaled by 1/3 (-1.982*.33=-.654), which is slightly larger than the conditional effect of expansion (.586)

associated with increased access to opioid prescriptions.

5.2.2 Recreational Marijuana After Expansion

The literature finds mixed evidence concerning the impact of marijuana legalization on opioid mortality (Mathur and Ruhm, 2022; Chihuri and Li, 2019). One explanation for the mixed nature of these findings is that they do not account for the impact of Medicaid expansion on opioid prescription access. Figure 6 shows how the timing of marijuana legalization interacted with the timing of Medicaid expansion. We see that legalization occurred steadily, starting toward the end of 2012;[26] only expander states legalized marijuana.[27]

As Figure 7 shows, opioid-related mortality as a share of overdoses decreased in states that legalized marijuana by slightly over 1 death per county-quarter or 4 deaths per county-year. These estimates suggest that marijuana legalization is associated with a decrease of approximately 4 opioid-related deaths per county-year. My estimates are consistent with existing estimates in the literature.[28] A back of the envelope calculation suggests that the effect of national-level legalization of recreational marijuana would save approximately 12,000

[26]I measure legalization of marijuana based on the date legalization was enacted rather than the date dispensaries officially became operational.

[27]In Appendix E, I examine whether these policy shocks are connected with each other and concluded that they constitute independent shocks to opioid use.

[28]I compare my results to Mathur and Ruhm (2022) in Appendix G. Wen and Hockenberry (2018) finds that the legalization of recreational marijuana is associated with a 6% reduction in the opioid-prescription rate for Medicaid-covered individuals (which amounts to a 3,000 fewer total prescriptions per state-quarter) The average number of opioid-prescriptions per 1,000 enrollees during 2010-2016 was 162.04. Approximately 65 million individuals were enrolled in Medicaid per year over this period, which suggests that for an average state approximately 320,000 individuals were enrolled in a quarter. Powell et al. (2018) finds that certain marijuana legalization policies were responsible for lowering pain reliever treatment admissions by 18.5% and (together with the presence of active dispensaries) were responsible for reducing opioid-related deaths by 18%. These estimates are similar in magnitude to those in Bachhuber et al. (2014), which estimates that legalization is responsible for a 25% reduction in opioid-related deaths.Livingston et al. (2017) studies time trends in Colorado and finds that legalization is associated with .7 fewer deaths per month, a reduction of 6.5% My preferred estimates suggest that marijuana legalization was responsible for .3 fewer opioid-related deaths per county-month from 2012 to 2016, which is a reduction of 37% off the mean. My estimates are higher than other estimates since I am studying outcomes for a slightly later time period, chosen to facilitate the comparison to Medicaid expansion.Bachhuber et al. (2014) uses a sample from 1999 to 2010 while Powell et al. (2018) adds 2011-2013 to the sample. My sample is from 2012 to 2016.

opioid-related deaths per year.[29]

5.3 Interaction between Medicaid Expansion and Marijuana Legalization

In Section 4.2, we saw that Medicaid expansions increased opioid deaths. In Section 5.2 we saw that increased marijuana access mitigated the increases associated with expansion and that marijuana access alone decreases opioid deaths. Appendix E (especially Table E3) shows evidence that Medicaid expansion and marijuana legalization constitute independent policy shocks. Table 6 reports the results from estimating a specification of Equation 1 in which both Medicaid expansion and marijuana legalization as well as their interaction are included as regressors. The coefficient estimates suggest that a state which *both* expanded Medicaid *and* legalized recreational marijuana would expect to see a modest net *reduction* in opioid-related deaths, namely .552 fewer deaths per population, or about 2 fewer deaths per county per year.[30] These estimates using the full interaction between Medicaid expansion and marijuana legalization suggest that policy makers can increase access to opioid prescriptions, while at the same time not increasing opioid deaths.

[29]The full welfare implications of recreational marijuana legalization, however, remain less clear. Although it is clear that marijuana is safer than opioids in that it is far less addictive and carries a much lower risk of overdose (Hall and Pacula (2003), Powell et al. (2018)), there could still be substantial social welfare costs to the legalization of recreational marijuana if it acts as a complement to encourage other risky behavior. Nevertheless, the evidence on the legalization of medical marijuana is encouraging in that it suggests that increased access to marijuana is *not* associated with other increases in risky behavior. For example, there is evidence that medical marijuana legalization does not increase either alcohol use or marijuana use among minors (Wen et al. (2015), Lynne-Landsman et al. (2013)). Furthermore, among adults, medical marijuana legalization is associated with increased marijuana use, and this increased use is at least in part due to adults substituting marijuana for alcohol, a substitution which may be responsible for the fact that marijuana legalization is associated with a 9 percent decrease in traffic fatalities (Anderson et al. (2013)). Though these results suggest that increased marijuana access is not necessarily associated with increases in alcohol consumption and traffic fatalities, more work is needed to understand how these outcomes respond to recreational marijuana legalization, as opposed to medical marijuana legalization.

[30]Calculated by summing the estimates reported in Table 6.

6 Conclusion: Policy Can Address Epidemic Without Restricting Access

In order for policy to be effective at reducing opioid-related deaths, it must be aimed at the underlying cause of the epidemic. To isolate factors pertaining to opioid prescription accessibility, I exploit the staggered state-level expansion of the Medicaid program (as allowed under the Affordable Care Act) as a natural experiment to ascertain whether increased access to medical services, including prescription drugs, increased opioid-related mortality. I find that states that expanded Medicaid under the Affordable Care Act saw substantial increases both in opioid prescriptions and in opioid-related deaths. These results vary strongly by demography, being driven largely by deaths of white men aged 18-65. A back of the envelope calculation suggests that for an average county, from 2012-2016 Medicaid expansion caused approximately 2,800 more people to be insured per year, 175,000 more opioid units to be prescribed per year, and 4 additional opioid related deaths per year. Overall, these opioid accessibility shocks explain about 12,000 opioid deaths per year, or nearly a third of the overall death toll from 2012-2016. This increase in mortality is only part of the welfare impact of Medicaid expansion and does not constitute a full cost-benefit analysis of Medicaid expansion.[31]

Main results using a two-way fixed effects estimator are robust to concerns of treatment heterogeneity (Goodman-Bacon et al., 2019; Sun and Abraham, 2020; de Chaisemartin et al., 2018) (Appendix B). I find larger but less precisely estimated results when I restrict the sample to counties on the geographic border between expander and non-expander states. I also find larger but less precisely estimated results when I restrict the sample to states which are matched based on their pre-existing propensities to expand Medicaid. Furthermore, I

[31]Furthermore, since the series of restricted microdata ends in 2016, I have at most eight quarters of post-period data, therefore I cannot test whether these mortality effects persist past two years.

show that my main estimates are robust to the inclusion of six separate state-level controlled substance prescribing policies including prescription drug monitoring programs (PDMP), and robust to the inclusion of county-year unemployment measures. I show in a triple difference specification that the effects of Medicaid expansion are larger in counties with larger pre-existing uninsured populations. I also show how the effects of opioid-access on opioid deaths vary by demography, as well as how the effects vary by pre-existing access to marijuana, an opioid substitute. Furthermore, I show the main results are robust to excluding early adopter states and to correcting opioid death counts in the manner of Ruhm (2018). Lastly, I show how Medicaid expansion interacts with the state-level decision to legalize recreational marijuana, and find that marijuana legalization was able to mitigate the increases in opioid-related deaths attributable to Medicaid expansion.

Given that I find evidence that the increases in opioid access under Mediciad expansions are responsible for approximately a third of the death-toll, it is natural to ask whether policy ought to aim at restricting access to opioids. On the one hand, these results suggest that restricting access to opioid prescriptions (e.g. encouraging doctors to write fewer prescriptions) could save lives. But, on the other hand, these restrictions might harm those who rely upon, but do not misuse, opioid prescriptions to manage chronic pain. Furthermore, there is evidence that an existing attempt at restricting access to deter misuse (implemented by reformulating a popular opioid to be abuse-deterrent) caused substitution to heroin, rather than a reduction of deaths (Alpert et al. (2017)). Instead of restricting access to opioids, another policy option would be to increase access to an opioid-substitute. Accordingly, the results of studying the full interaction of Medicaid expansion and marijuana legalization suggest that a policy-maker could increase access to prescription opioids *without* increasing opioid-related mortality so long as the policy maker also increases access to comparatively safe substitutes such as marijuana.

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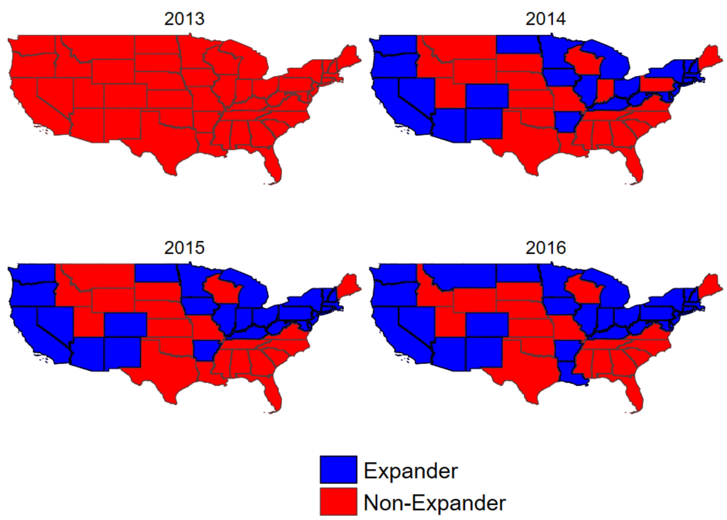
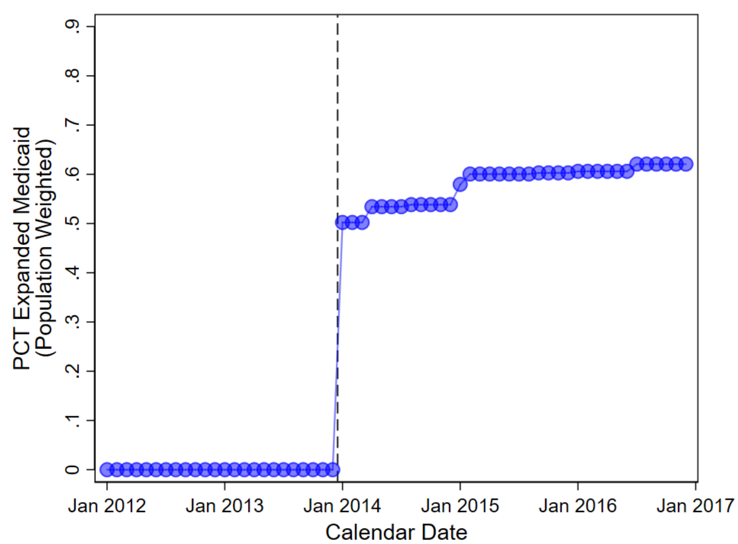
Figures and Tables

Figure 1: Medicaid Expansion under the ACA (2010-2016)



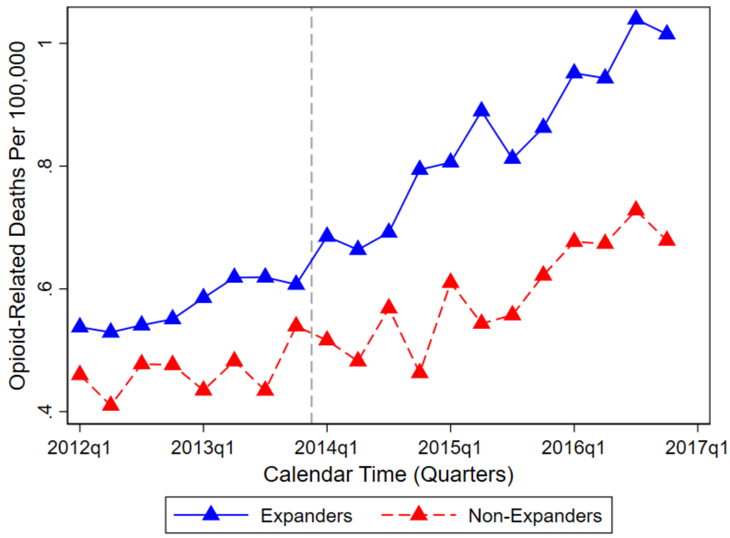
(a) Map



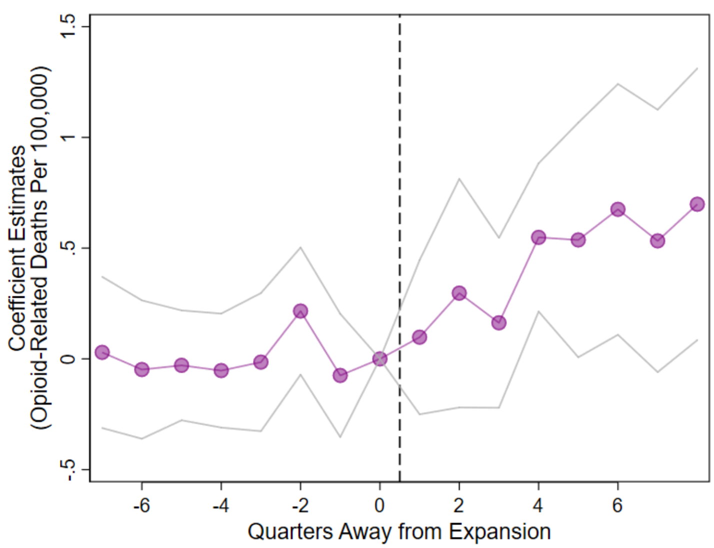
(b) Timing

Note: Panel (a) shows which states expanded Medicaid (under the Affordable Care Act) by the end of a given calendar year. Panel (b) shows the share of the population exposed to Medicaid expansion under the Affordable Care Act. Observations are at the state-month level. Expansion began in January of 2014. 2010 state populations are used as population weights

Figure 2: Medicaid Expansion and Opioid-Related Deaths



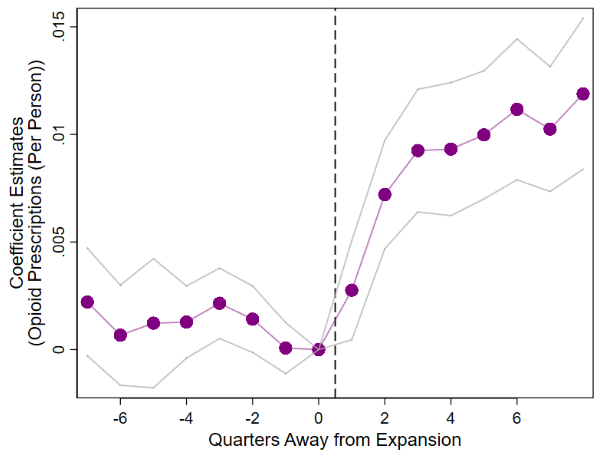
(a) Trends



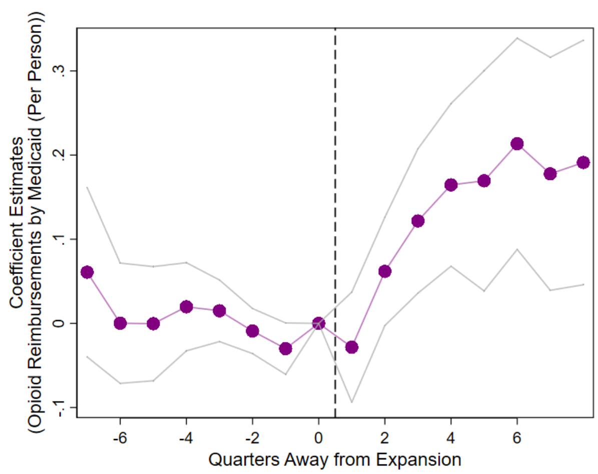
(b) DD Estimates

Note: Panel (a) shows trends in opioid-related deaths separately for counties in states that expanded Medicaid and those that did not. Expansions began in January of 2014. Panel (b) shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with the rate of opioid-related deaths as the dependent variable. Main results using a two-way fixed effects estimator are robust to concerns of treatment heterogeneity (Goodman-Bacon et al., 2019; Sun and Abraham, 2020; de Chaisemartin et al., 2018) (Appendix B). Standard errors are clustered at the state level. In both panels, observations are at the county-quarter level. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure 3: Impact of Medicaid Expansion on Opioid Units Prescribed (CMS State Drug Utilization)



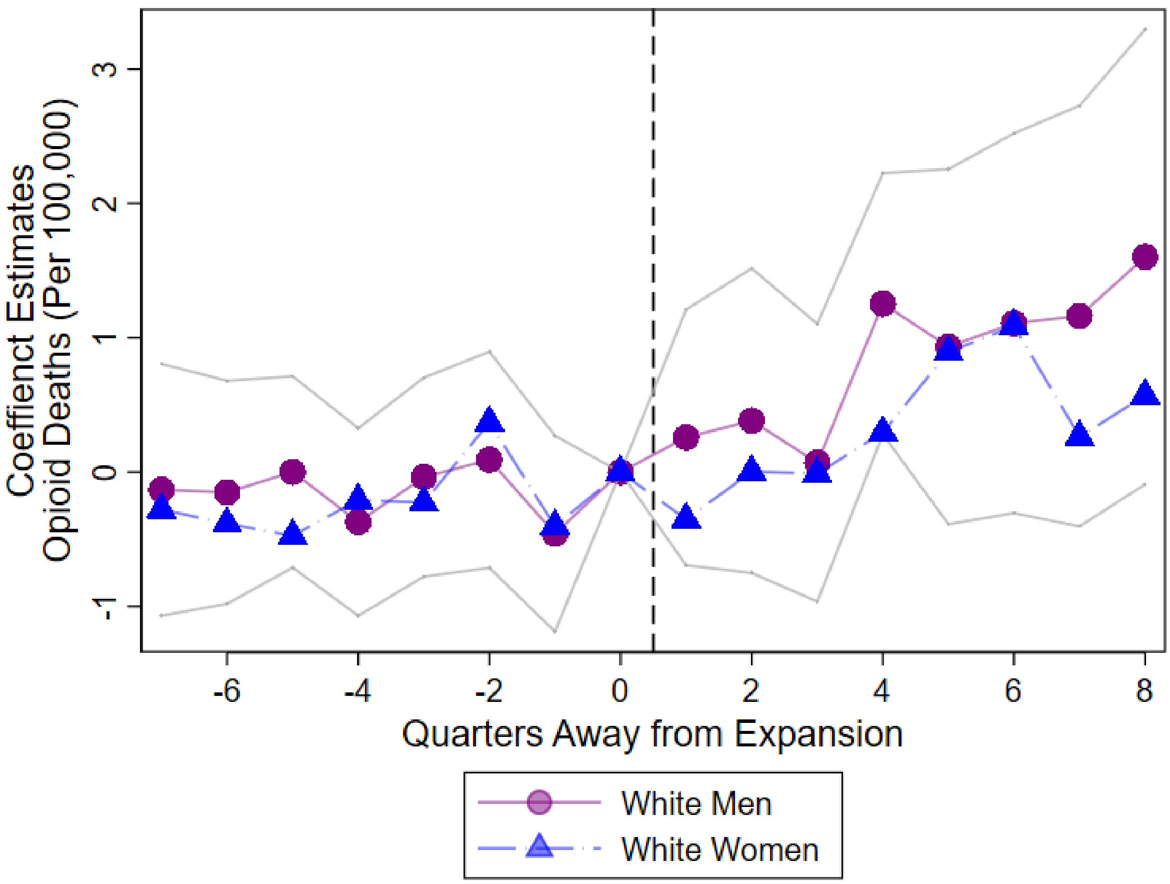
(a) Opioid Drug Units Reimbursed by Medicaid



(b) Amount Reimbursed for Opioids by Medicaid

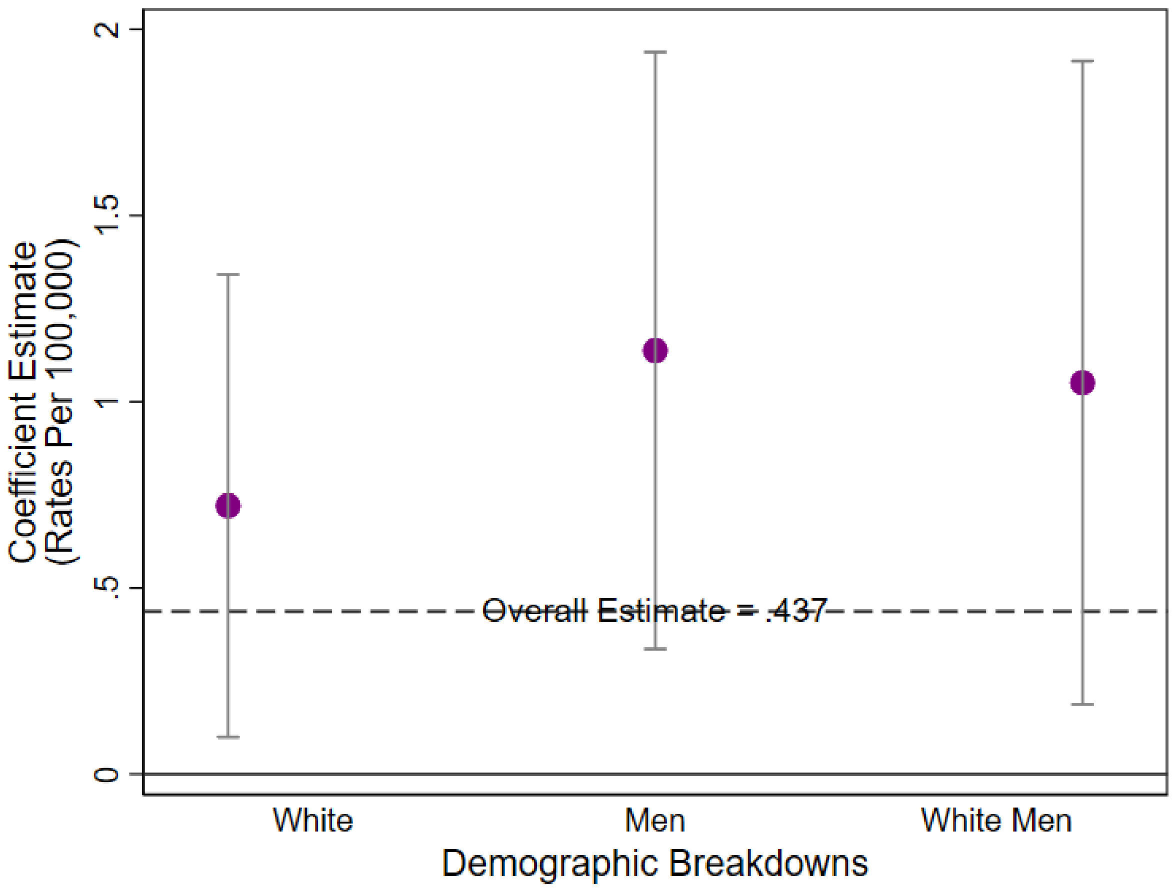
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with opioid drug units prescribed per person and amounts reimbursed per person as the dependent variable, respectively. Standard errors are clustered at the state level. Observations are at state-quarter level. Both specifications include state fixed effects, as well as calendar quarter and year fixed effects. The sample contains the universe of claims filed through Medicaid. The sample includes all states and all demographics. The source is CMS state drug utilization data (2010-2017).

Figure 4: Medicaid Expansion and Opioid-Related Deaths: Heterogeneity Dynamics



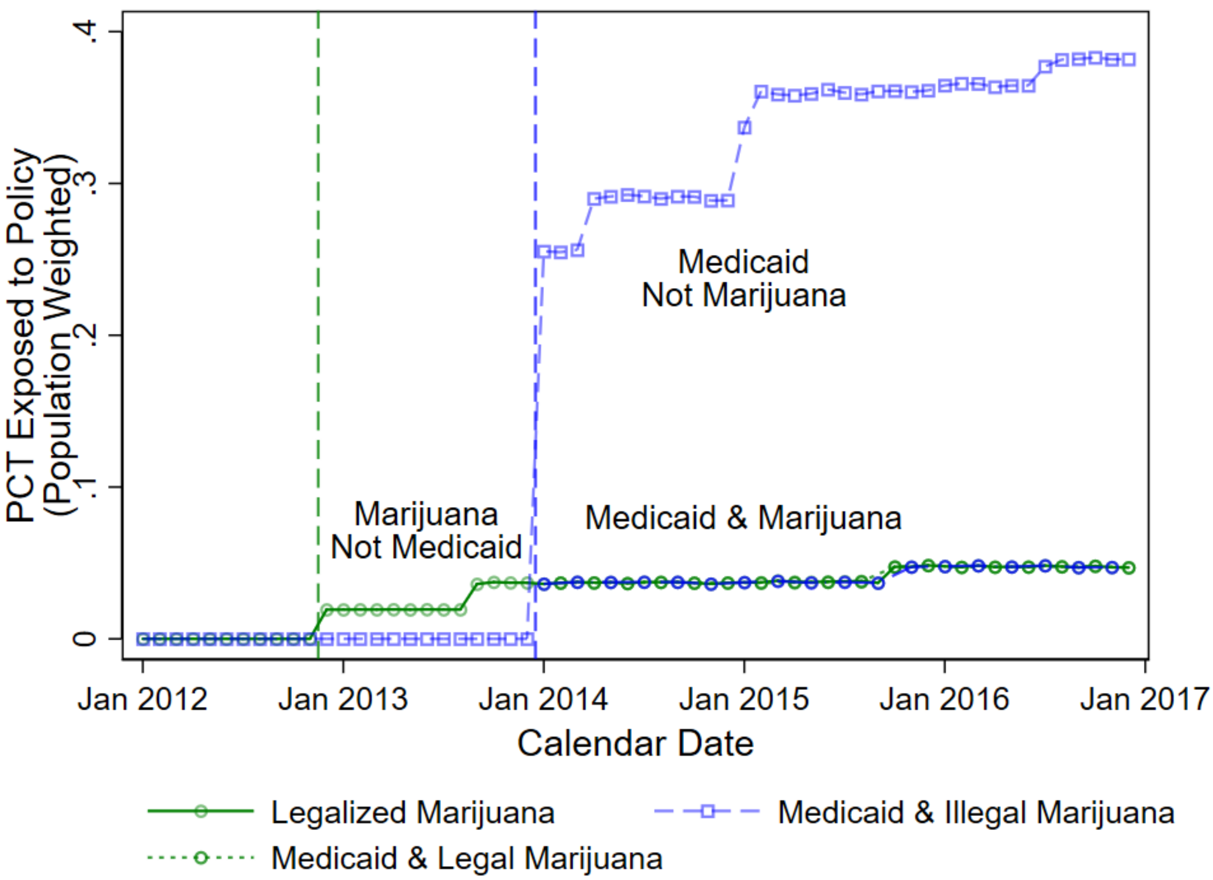
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with demography specific rates of opioid-related deaths as the dependent variable. Estimates for two rates are shown: the rate of white male opioid-related deaths per 100,000 white men and the rate of white female opioid-related deaths per 100,000 white women. Standard errors are clustered at the state level. Observations are at the county-quarter level. Both specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states but restricts to white men and white women, respectively. For visual ease, confidence intervals are shown for the estimates for white men only. More complete demographic estimate breakdowns are given in Figure 5 and Appendix Figure A13. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure 5: Medicaid Expansion and Opioid-Related Deaths: Heterogeneity



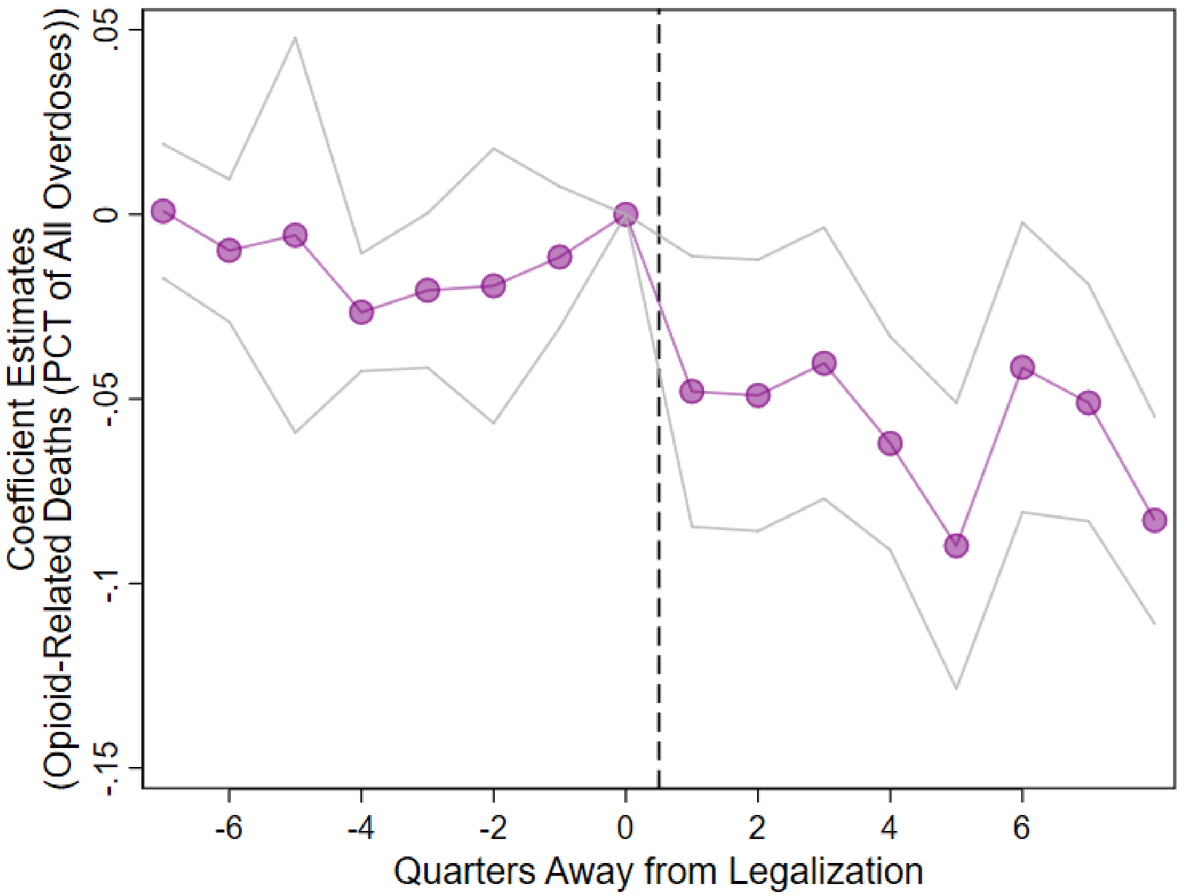
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 1 with demography specific rates of opioid-related deaths as the dependent variable. For example, the rate of white male opioid-related deaths is calculated per 100,000 white men. Standard errors are clustered at the state level. Observations are at the county-quarter level. Each specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states but restricts various demographics in each regression, respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure 6: Medicaid Expansion Interacting with Marijuana Legalization (2010-2016)



Note: Figure shows the share of the population exposed to Medicaid expansion, legalized recreational marijuana and the interaction of these policies. In particular, the figure separately shows the share of the population living in states with legalized recreational marijuana, as well as the share of the population living in states with both expansions of Medicaid and legalized recreational marijuana, as distinct from the share of the population living in states with Medicaid expansion, but not legalized recreational marijuana. Every state which legalized recreational marijuana at some time expanded Medicaid, but many states which expanded Medicaid never legalized marijuana. I measure exposure to legal marijuana based on the date legalization was enacted rather than the date dispensaries officially became operational. Medicaid expansion began in January of 2014, while marijuana legalization began in December of 2012. Observations are at the state-month level. 2010 state populations are used as population weights

Figure 7: Impact of Marijuana Legalization on Opioid-Related Deaths



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2. Standard errors are clustered at the state level. Observations are at the county-quarter level. The dependent variable is the rate of opioid-related deaths as a share of total overdose deaths. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states. The source is CDC Individual-Level Mortality Files (2012-2016)

Table 1: Summary Statistics

	Non-Expander	Expander	Total
Panel A: Health Insurance			
PCT Uninsured and $\leq$ 138% FPL	38.70	29.55	34.31
	(9.70)	(11.61)	(11.60)
NMB Uninsured and $\leq$ 138% FPL	8147	9402	8748
	(63622)	(62569)	(63121)
Observations	11629	10705	22334
Panel B: Opioid Prescriptions			
Opioid Units (Per Person)	1.31	1.74	1.58
	(0.62)	(0.83)	(0.78)
Opioid Prescriptions (Per Person)	0.02	0.03	0.03
	(0.01)	(0.01)	(0.01)
Observations	570	960	1530
Panel C: Opioid Deaths			
Opioid-Related Deaths (Per 100,000)	1.61	2.18	1.92
	(3.61)	(4.06)	(3.88)
Opioid-Related Deaths (PCT of All Deaths)	0.01	0.01	0.01
	(0.01)	(0.02)	(0.02)
Opioid-Related Deaths (PCT of All Overdoses)	0.24	0.31	0.28
	(0.36)	(0.37)	(0.37)
Opioid Related Deaths (Raw Counts)	1.51	3.50	2.61
	(4.88)	(10.06)	(8.21)
PCT Counties with Zero Opioid Deaths	0.15	0.09	0.12
	(0.35)	(0.29)	(0.32)
Observations	17100	21000	38100

Note: Means reported with standard errors in parentheses. Distributions are reported separately based upon the state-level decision to expand Medicaid. Panel A: Source is SAHIE (2010-2016). Observations are at the county-year level and the sample is restricted to uninsured adults 18-65 with an income at or below 138 percent of the poverty line, the group which became newly eligible for Medicaid under the expansion. Panel B: Source is CMS State-Drug Utilization Data (2010-2017). Observations are at the state-quarter level and include Washington D.C. Data contain the universe of claims filed to Medicaid. Panel C: Source is CDC Individual-Level Mortality Files (2010-2016). Observations are at the county-quarter level and rounded to nearest hundred to avoid disclosing small cells.

Table 2: Impact of Medicaid Expansion on Insurance, Opioid Prescription and Opioid Deaths

	(1) PCT Uninsured	(2) Prescription Rate	(3) Death Rate
Medicaid Expansion	-0.068***	0.009***	0.437**
	(.011)	(0.002)	(0.165)
Observations	22334	1530	38100
Mean (Pre-Period)	.401	.024	1.553
Percent Effect CI	[12%,23%]	[21%,49%]	[7%,48%]

Note: Each column reports estimates from a separate regression with standard errors clustered at the state-level and reported in parentheses. The reported coefficient of interest is an indicator for whether the observation is in a state which expanded Medicaid. The reported percent effect confidence interval (CI) is the 95% confidence interval of the estimate divided by the mean of the outcome variable over years prior to Medicaid expansion. In column (1), the share of uninsured individuals aged 18-65 with income at or below 138% of the Federal Poverty Line is the dependent variable in an an OLS estimation of Equation 1. Observations are at the county-year level. The source is SAHIE estimates of the uninsured (2010-2016). In column (2), the rate of opioid prescriptions filed through Medicaid per person is the dependent variable in an OLS estimation of Equation 1. Observations are at the state-quarter level. The source is CMS state-drug utilization (2010-2017). In column (3), the rate of opioid-related deaths per 100,000 people is the dependent variable in an OLS estimation of Equation 1. Observations are at the county-quarter level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Covariate Predications of State Level Medicaid Expansion

	Expanded	Expanded Late
Political Environment		
PCT R (Lower Chamber)	-0.020**	-0.031**
PCT R (Upper Chamber)	0.005	0.047***
R Governor	0.000	-0.002**
Expenditures		
PCT Education Expenditure	0.007***	0.010*
PCT Welfare Expenditure	-0.009***	-0.008
PCT Hospital Expenditure	-0.007	-0.014***
PCT Health Expenditure	0.010	-0.002
PCT Police Expenditure	-0.025	0.007
PCT Unemp Insurance Expenditure	0.007**	0.006
Demography		
Log Population	0.090	-0.202***
PCT Male	0.010	0.111***
PCT White	0.016*	0.017
PCT Black	-0.005	-0.011***
PCT Hispanic	0.001	-0.005
Economic Covariates		
PCT Unemployed	-0.032	-0.064*
PCT Rural	-0.002	-0.019*
PCT Uninsured	-0.059	-0.266**
Per Capita GDP	-0.117	-1.430***
Per Capita Personal Income	0.006	0.234***
Per Capita Medicaid Beneficiaries	0.036	-0.036**
Poverty Rate	0.041	0.206***
Observations	49	31

Note: Each column reports a separate OLS regression with standard errors clustered at the state level reported in parentheses. Observations are at the state level; Nebraska is omitted because its state government is unicameral and non-partisan. In the left column, the dependent variable is an indicator for whether or not the state ever expanded Medicaid. In the right column, the dependent variable is an indicator for whether the state expanded Medicaid sometime after January 2014. (Approximately 70% of states who expanded did so in January 2014.) Also included but not reported are demographic variables that measure the percent of the population aged 0-9, aged 10-19, . . ., aged 80+. Significance levels indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impact of Medicaid Expansion on Opioid-Related Deaths: Confounders

	(1)	(2)	(3)	(4)	(5)
Medicaid Expansion	0.437**	0.440***	0.439***	0.431**	0.437**
	(0.165)	(0.163)	(0.159)	(0.170)	(0.165)
Observations	38100	38100	38100	38100	38100
Mean (Pre-Period)	1.553	1.553	1.553	1.553	1.553
Policy Confounders					
PDMP		X	X		X
Doctor Shopping, Pain clinic regs			X		X
Physician exam, Pharm verification, Require ID				X	X

Note: Each column reports a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. The dependent variable is the rate of opioid-related deaths per 100,000. Standard errors are clustered at the state level. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Sample includes deaths of all US Residents. All specifications include county fixed effects as well as calendar quarter and year fixed effects. Column (1) repeats the main estimates found in Column (3) of Table 2. Following Doleac and Mukherjee (2018), in columns (2)-(5) various combinations of state level health care policies aimed at reducing opioid misuse are included as regressors. Observations are at the county-quarter level. For disclosure reasons, the number of observations is rounded to the nearest hundred. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Interactive Impact of Medicaid Expansion and Pre-Existing MMLs on Opioid-Related Deaths

	(1)	(2)	(3)	(4)	(5)	(6)
		MML (Binary)		MML Type Interactions		
	Main Est	MML	No MML	GG MML Interaction	HG MML Interaction	Dispensary Interaction
Medicaid Expansion	0.437**	-0.108	0.613***	0.641***	0.658***	0.586***
	(0.165)	(0.456)	(0.210)	(0.190)	(0.204)	(0.188)
Medicaid × GG MML				-0.723***		
				(0.247)		
Medicaid × HG MML					-0.649**	
					(0.246)	
Medicaid × Dispensary Penetration						-1.982***
						(0.557)
Observations	38100	8000	30100	38100	38100	38100
Mean	1.925	1.979	1.910	1.925	1.925	1.925

Note: Each column reports estimates from a separate regression with standard errors clustered at the state-level and reported in parentheses. The outcome variable is the rate of opioid-related deaths per 100,000. Standard errors are clustered at the state level. All specifications include county fixed effects as well as calendar quarter and year fixed effects. In each column, the reported coefficient of interest in the top row is an indicator for whether the county is in a state which expanded Medicaid. Column (1) repeats the main estimate of the impact of Medicaid expansion on opioid related deaths. (It repeats Column (3) of Table 2.) Columns (2)-(3) report estimates from the same specification in Column (1), but using two different subsamples. Column (2) restricts to counties in states with any medical marijuana laws (MML) prior to Medicaid expansion, while Column (3) restricts to those in states without legal marijuana prior to Medicaid expansion. Columns (4)-(6) report interactive estimates, which interact Medicaid expansion with three different types of pre-expansion MMLs separately. MML types follow Section 2.2 Anderson and Rees (2023). Column (4) reports estimates from Equation 1, but featuring the full interaction between an indicator variable for Medicaid expansion and an indicator for MMLs that allowed for "group growing" (GG) or "collective cultivation". Column (5) reports estimates from Equation 1, but featuring the full interaction between an indicator variable for Medicaid expansion and an indicator for MMLs that allowed for "home growing" (HG). Finally column (6) reports estimates from Equation 1, but featuring the interaction of an indicator variable for Medicaid expansion with marijuana dispensary penetration into a state. Dispensary penetration is defined as the percent of counties in the state with a marijuana dispensary. Information about dispensary penetration is taken from Table 1 of Smith (2017). Observations are at the county-quarter level. For disclosure reasons, the number of observations is rounded to the nearest hundred. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Interactive Impact of Medicaid Expansion and Marijuana Legalization on Opioid-Related Deaths

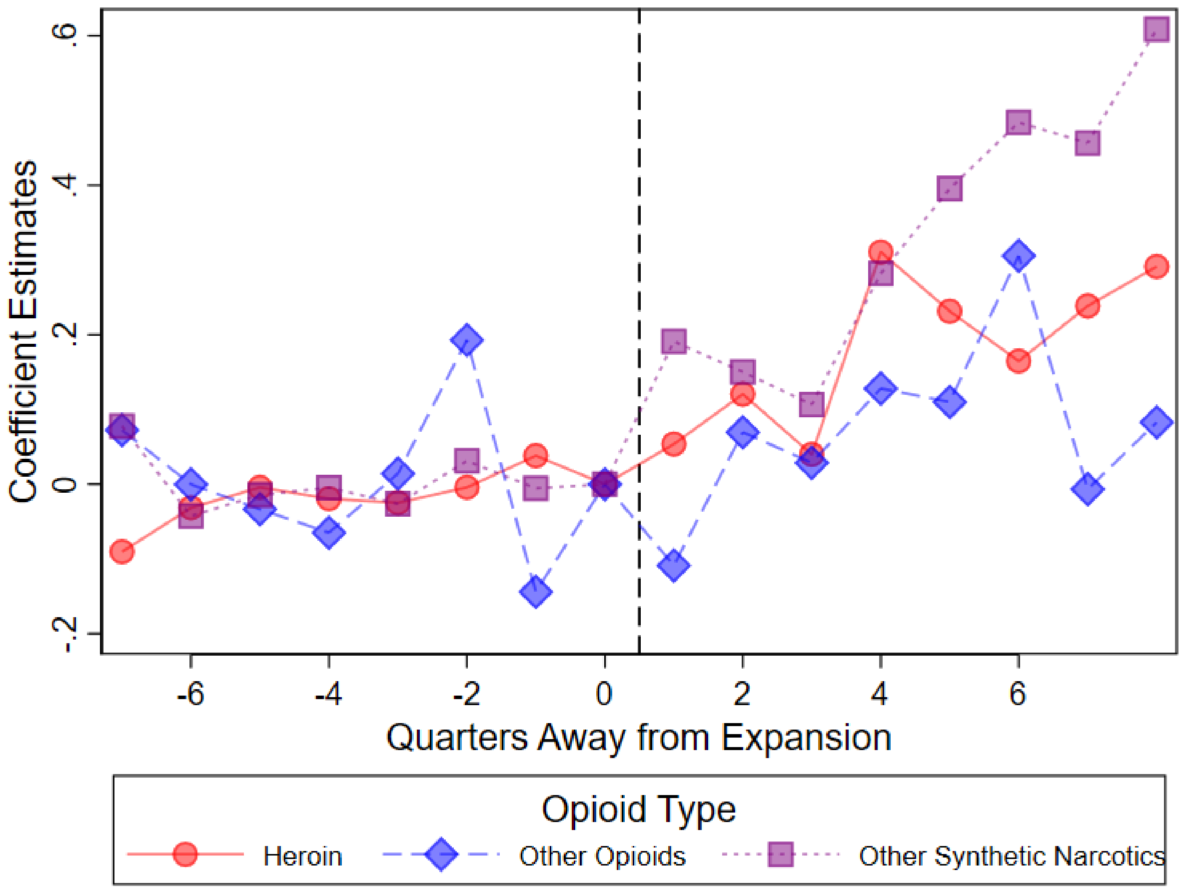
	(1)
Marijuana Legalization	-0.600***
	(0.106)
Medicaid Expansion	0.529***
	(0.169)
Medicaid and Marijuana	-0.481***
	(0.145)
Observations	38100
Mean (Overall Sample)	1.925

Note: Each column reports estimates from a separate regression with standard errors clustered at the state-level and reported in parentheses. The outcome variable is the rate of opioid-related deaths per 100,000 people. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid, legalized recreational marijuana or did both. All specifications include county fixed effects as well as calendar quarter and year fixed effects. Observations are at the county-quarter level. For disclosure reasons, the number of observations is rounded to the nearest hundred. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix A: Appendix Figures and Tables

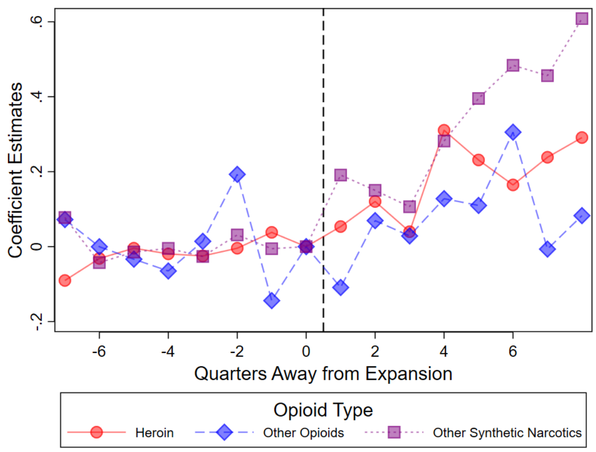
Appendix Figures

Figure A1: Impact of Medicaid Expansion on Opioid-Related Deaths: Opioid-Type Breakdowns

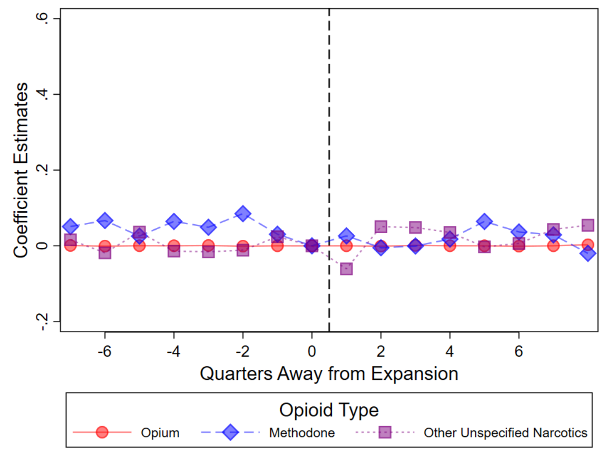


Note: Figure shows the coefficient estimates from an OLS regression of Equation 2 where the dependent variable is the rate of opioid related-deaths attributable to one of three specific opioid types. In the CDC Mortality Files, three types of opioid-related deaths are listed with separate ICD-10 codes: T40.1 “Heroin”, T40.2 “Other Opioids”, T40.4 “Other Synthetic Narcotics”. The dependent variable counts a death in which any of the 20 causes of death include the given ICD-10 code, and shows estimates for each rate separately. A death in which, e.g., T40.1 was listed as one cause of death and T40.2 was listed as another cause of death is counted in both the “Heroin” and “Other Opioids” categories. Standard errors are clustered at the state level. Observations are at the county-quarter level. In Figure 2b, comparable estimates are reported for all opioid-related deaths pooled together. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016).

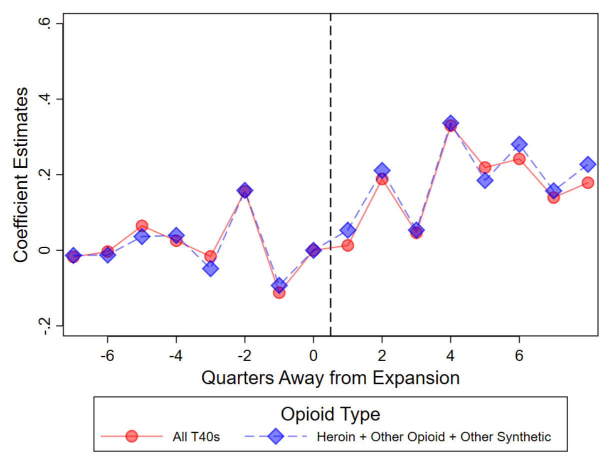
Figure A2: Impact of Medicaid Expansion on Opioid-Related Deaths: Further Opioid-Type Breakdowns



(a) Figure A1 repeated



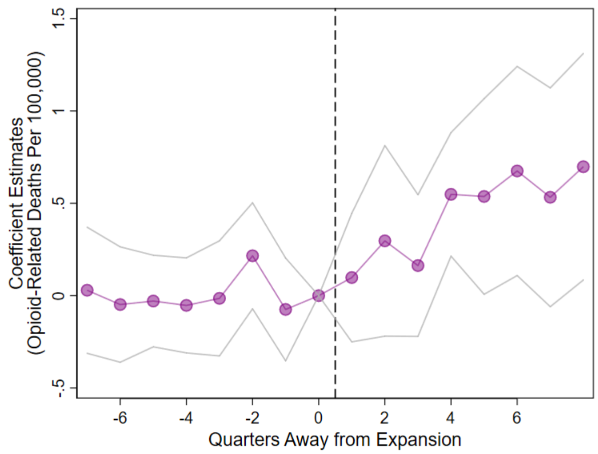
(b) Opioid Types not Included



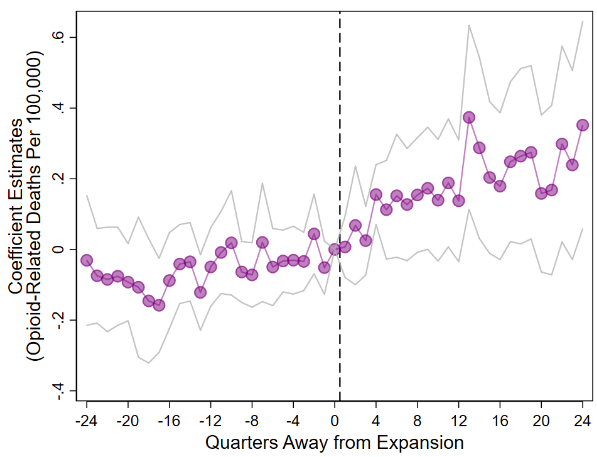
(c) Pooling All Opioid Types

Note: Figure shows the coefficient estimates from an OLS regression of Equation 2 where the dependent variable is the rate of opioid related-deaths attributable to various, specific types of opioid related deaths. In the main estimates in this paper, three types of opioid-related deaths are counted as "opioid deaths": ICD-10 codes T40.1 "Heroin", T40.2 "Other Opioids" and T40.4 "Other Synthetic Narcotics". Panel a shows estimates where the dependent variable counts a death in which any of the 20 causes of death include the given ICD-10 code, and shows estimates for each rate separately. (A death in which, e.g., T40.1 was listed as one cause of death and T40.2 was listed as another cause of death is counted in both the "Heroin" and "Other Opioids" categories.) Panel b shows similar estimates for other T40 ICD-10 codes: T40.0 "Opium", T40.3 "Methadone", T40.6 "Other unspecified narcotics". Panel c shows estimates pooling all three types broken down in panel a ("Heroin+Other Opioid+Other Synthetic"), and again pooling all 6 T40 codes ("All T40s"). Standard errors are clustered at the state level. Observations are at the county-quarter level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016).

Figure A3: Impact of Medicaid Expansion on Opioid-Related Deaths: Further Time Periods



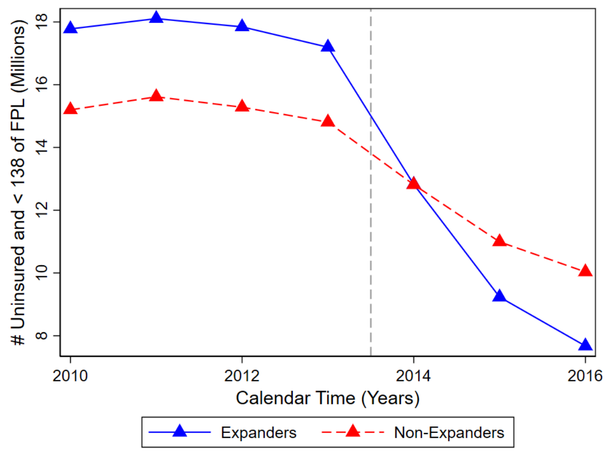
(a) 2012-2016 (Figure 2b)



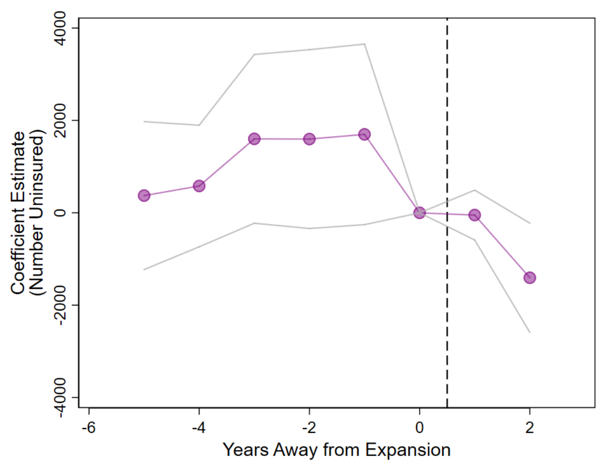
(b) 2006-2018

Note: Figure repeats Figure 2b in panel (a). In panel (b), the same estimates are produced but the sample period is extended from 2006-2018 with the result that I can display 24 quarters prior and 24 quarter post expansion. See the note to Figure 2b for more details.

Figure A4: Medicaid Expansion and Health Insurance



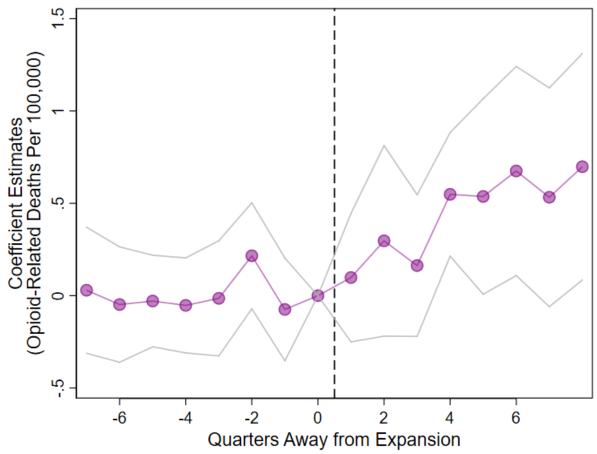
(a) Trends



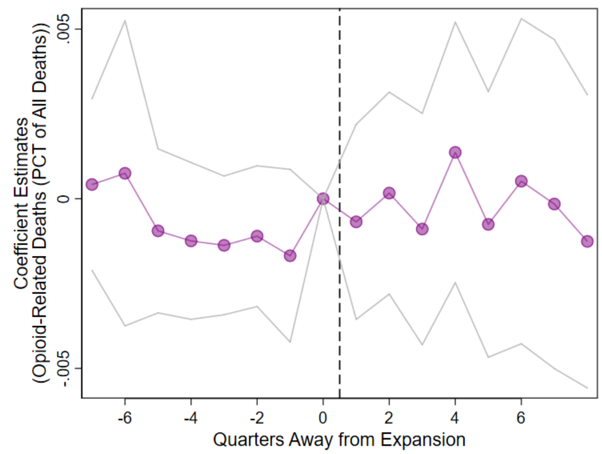
(b) DD Estimates

Note: Panel (a) shows annual trends in the counts of uninsured individuals separately for counties in states that expanded Medicaid and those that did not. Observations are at the county-year level. Expansions begin in January of 2014. Panel (b) shows estimates of the impact of Medicaid expansion on health insurance. Estimates are obtained by estimating Equation 2 using counts of uninsured individuals as the dependent variable. The specification includes county fixed effects, as well as calendar year fixed effects. In both panels, the sample is restricted to individuals aged 18 to 65 whose income is at or below 138 percent of the Federal Poverty Line. (This demographic became newly eligible under the Medicaid expansion licensed by the Affordable Care Act.) The source is SAHIE (2010-2016).

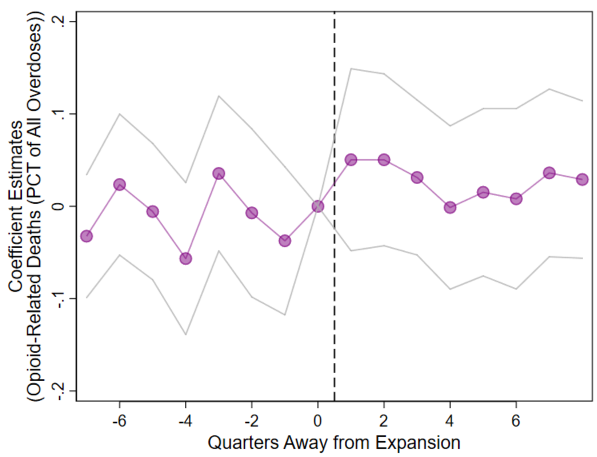
Figure A5: Impact of Medicaid Expansion on Opioid-Related Deaths: Exploring Measures



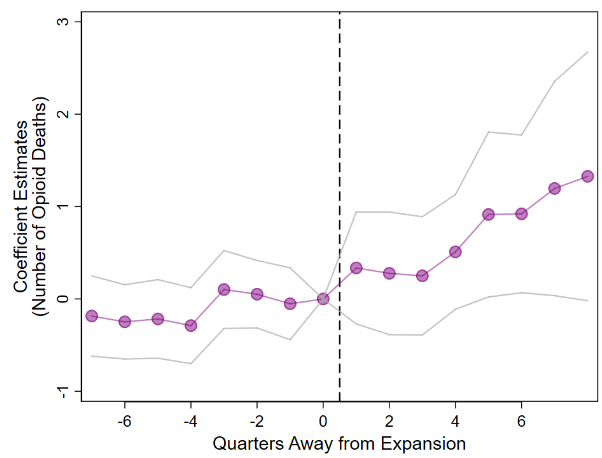
(a) Rate Per 100,000 Population (Main Diagram)



(b) Rate Per 100 Deaths



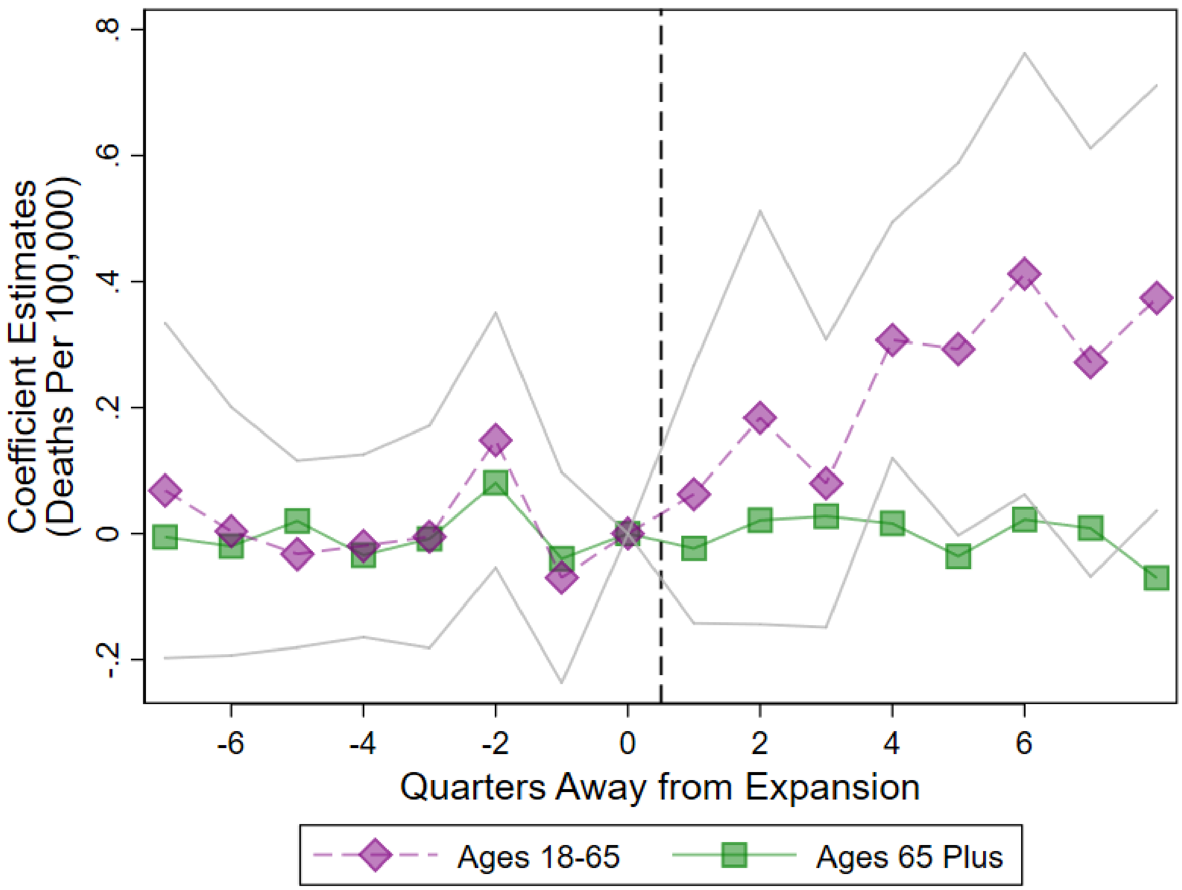
(c) Rate Per All Overdose Deaths



(d) Raw Counts

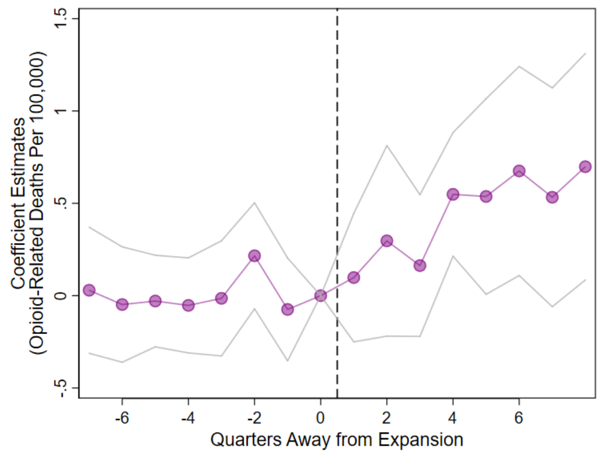
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with different measures of opioid-related deaths as the dependent variable. Panel (a) repeats Main Figure 2b, where the denominator is the total county population. Panels (b)-(c) repeat this estimation using an outcome variable in which the denominator are total deaths (b) and total overdose deaths (c), respectively. Panel (d) repeats this estimation using counts of opioid deaths as the outcome variable. Standard errors are clustered at the state level. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A6: Impact of Medicaid Expansion on Opioid-Related Deaths: Age Breakdowns

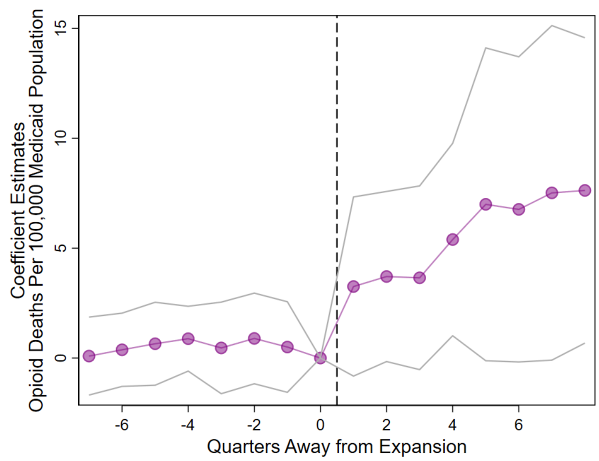


Note: Figure shows the coefficient estimates and 95% confidence interval from an OLS regression of Equation 2 where the dependent variable is an age specific rate of opioid related-deaths. Standard errors are clustered at the state level. Confidence intervals are given for the "ages 18-65" age group estimates. Observations are at the county-quarter level. In Figure 2b, comparable estimates are reported for opioid-related deaths for all ages pooled together. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016).

Figure A7: Impact of Medicaid Expansion on Opioid-Related Deaths: Medicaid Population Weighted



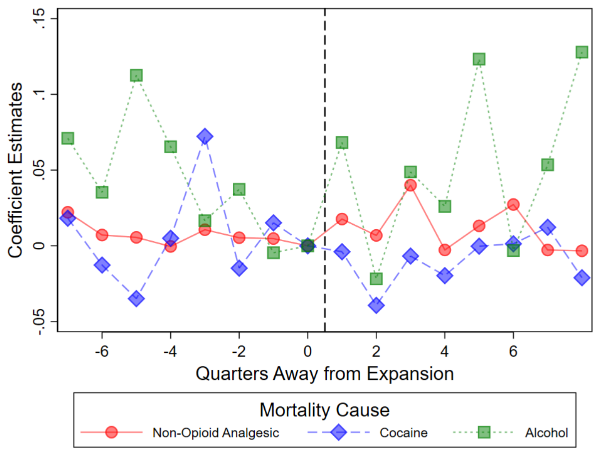
(a) Rate Per Population (Main Diagram)



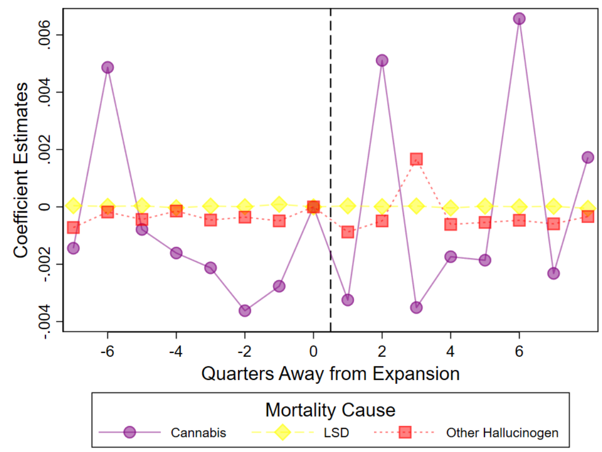
(b) Rate Per Medicaid Population

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with different measures of opioid-related deaths as the dependent variable. Panel (a) repeats Main Figure 2b. Panel (b) repeats this estimation but uses the county-year Medicaid population as the denominator and weights by the 2013 county-level counts of the Medicaid population. The "Medicaid population" is measured by the county-year population that is both uninsured and also at or below 138% of the federal poverty line. The county-year Medicaid population is taken from SAHIE. Standard errors are clustered at the state level. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

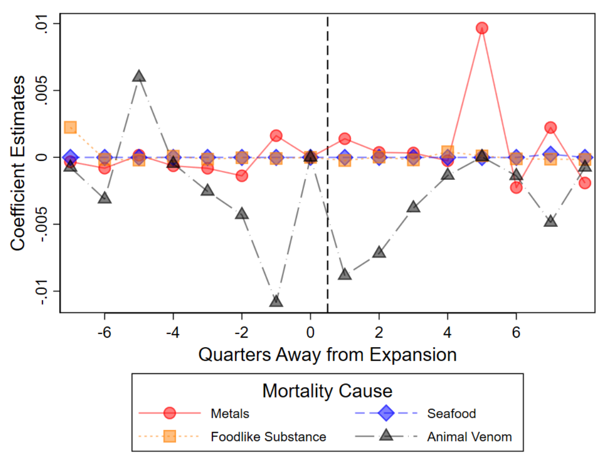
Figure A8: Impact of Medicaid Expansion on Opioid-Related Deaths: Placebo Mortality Types



(a) Other Drug Mortalities



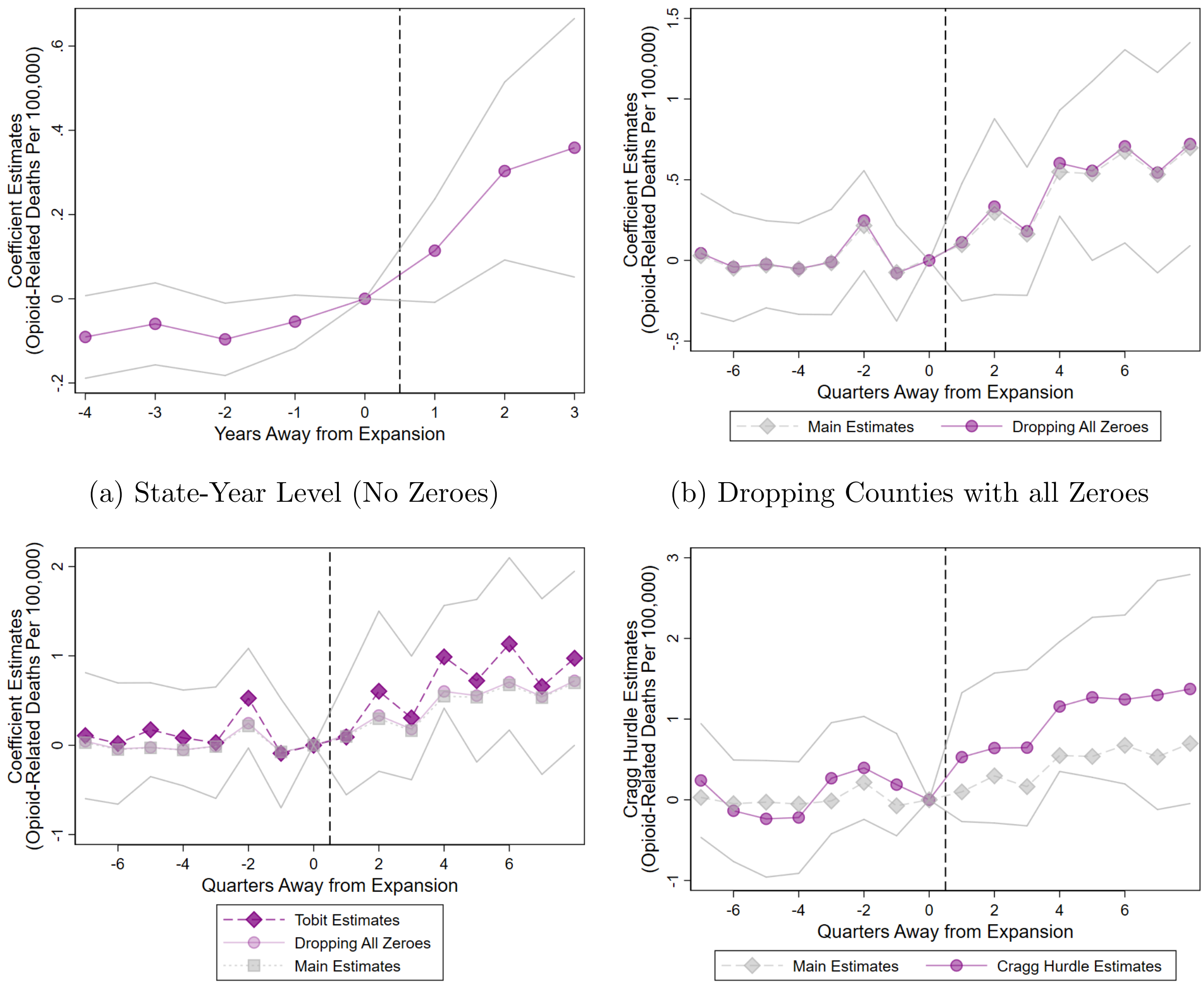
(b) Other Drug Mortalities, Cont



(c) Other Mortality Causes

Note: Figure shows the coefficient estimates from an OLS regression of Equation 2 where the dependent variable is the rate of deaths attributable to various, non-opioid sources. In the main estimates in this paper, three types of opioid-related deaths are counted as "opioid deaths": ICD-10 codes T40.1 "Heroin", T40.2 "Other Opioids" and T40.4 "Other Synthetic Narcotics". Panel a shows estimates where the dependent variable is the rate of deaths attributed to "Poisoning by nonopioid analgesics, antipyretics and antiheumatics" (T39s), Cocaine (T40.5), and the "Toxic effects of alcohol" (T51s). Panel b shows estimates where the dependent variable is the rate of deaths attributed to "Cannabis" (T40.7), "LSD" (T40.8), "Other and unspecified hallucinogens" (T40.9). Panel c shows estimates where the dependent variable is the rate of deaths attributed to the "Toxic effect of metals" such as lead and mercury (T56s), the "Toxic effects of seafood" such as ciguatera, scombroid, and shellfish (T61s), the "Toxic effect of substances eaten as food" such as mushrooms and berries (T62s), the "Toxic effect of contact with venomous animals" such as snakes and spiders. Standard errors are clustered at the state level. Observations are at the county-quarter level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016).

Figure A9: Impact of Medicaid Expansion on Opioid-Related Deaths: Exploring Zeroes



(a) State-Year Level (No Zeroes)

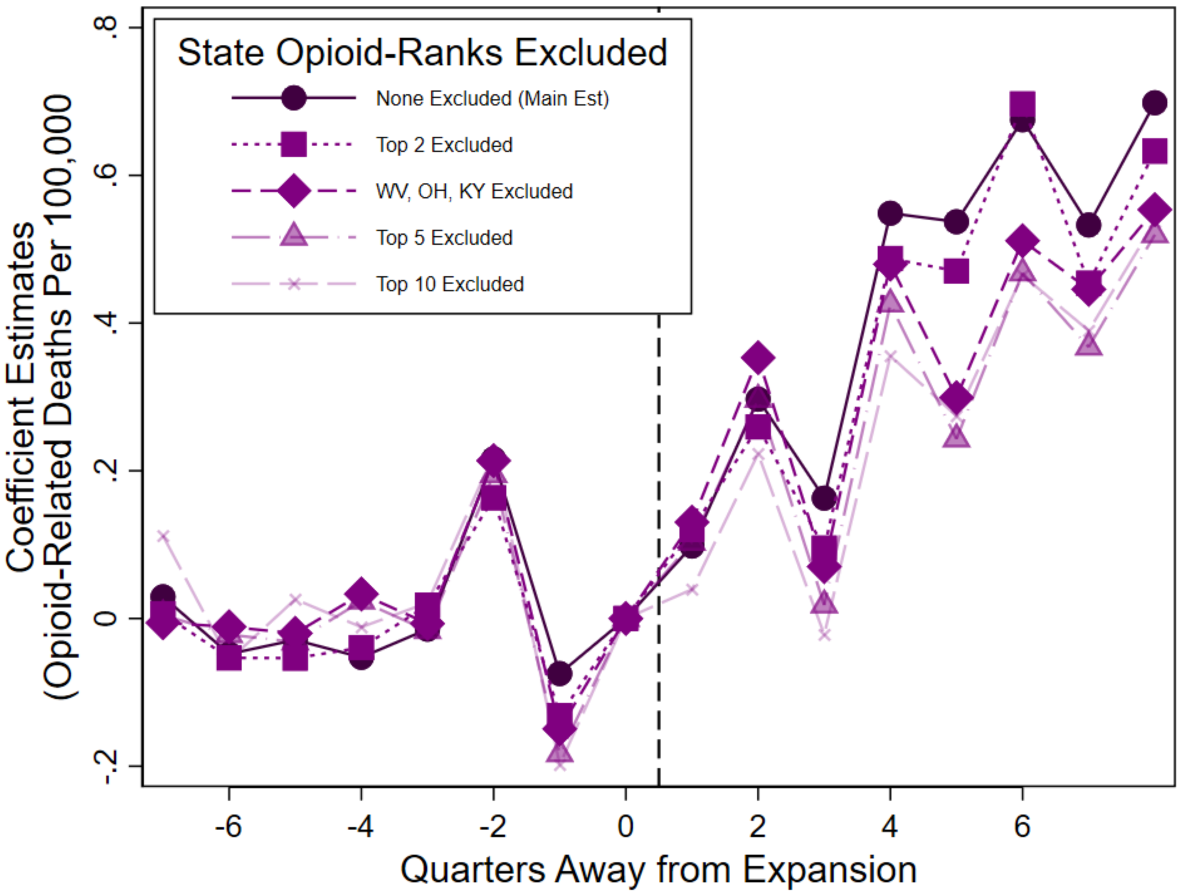
(b) Dropping Counties with all Zeroes

(c) Tobit Estimates

(d) Cragg Hurdle Estimates

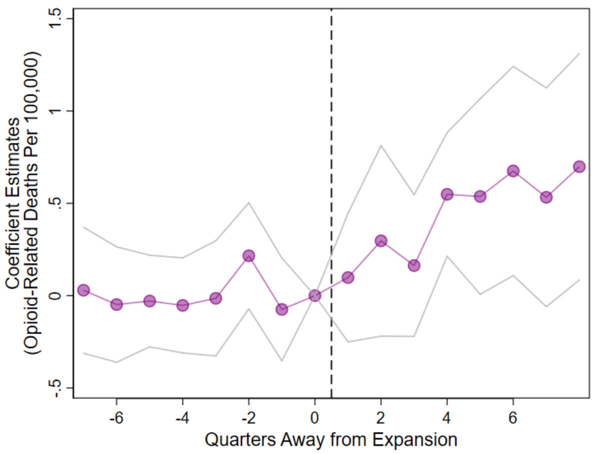
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 in different ways which address the presence of county-quarter observations with zero opioid-related deaths. Approximately 11% of counties show zero opioid-related deaths from 2012-2016. To address whether these zeroes are driving my results I perform four related robustness exercises. Panel (a) shows results at the state-year level, a level of aggregation at which there are no longer any zeroes. Panel (b) compares the main results from Figure 2b with estimates from a sample that drops all counties with zero deaths from 2012-2016 ("dropping all zeroes"). Panel (c) further compares the main results and the "dropping all zeroes" results with estimates from a tobit specification where the lower truncation limit is set to zero. The tobit specification includes the same county, quarter and year fixed effects as the main specification, clustering standard errors at the state level. Panel (d) compares main estimates with estimates from a linear Cragg Hurdle specification, in which the first stage selection estimation of the zero-bound is computed using the log of the county-year population. The Cragg Hurdle specification also includes the same county, quarter and year fixed effects as the main specification, clustering standard errors at the state level" The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files.

Figure A10: Impact of Medicaid Expansion on Opioid-Related Deaths: Excluding Highest Opioid States, Comparison

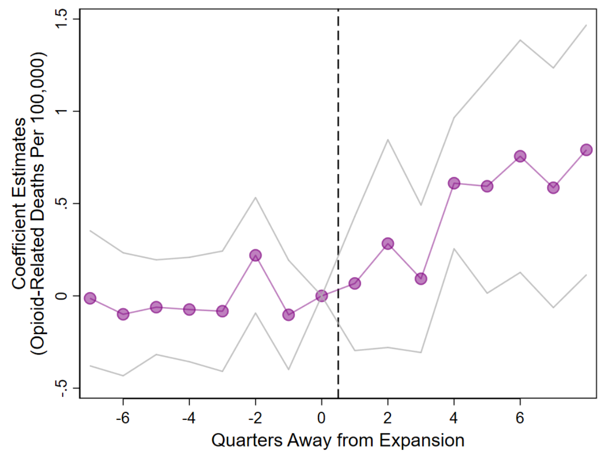


Note: Figure shows the coefficient estimates from estimating Equation 2 with different samples formed from dropping groups of states based on their overall opioid-death rates from 2012-2016. I form state opioid-ranks by dividing the total 2012-2016 opioid deaths by population for each state. I then rank states by this aggregate death rate. The top 5 highest opioid ranked states in ascending order are: RI (5), KY, OH, NH, WV (1). Standard errors are always clustered at the state level. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

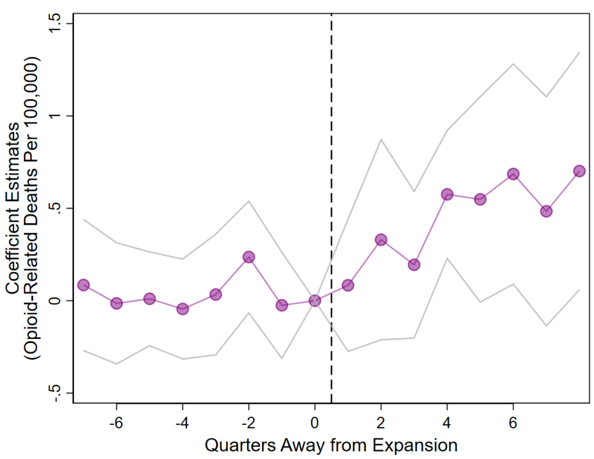
Figure A11: Impact of Medicaid Expansion on Opioid-Related Deaths: Exploring Treatment Contrasts



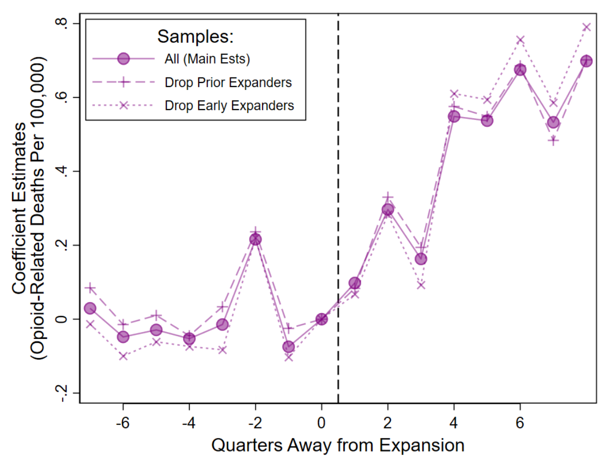
(a) All States (Main Estimates)



(b) Dropping Early Expanders



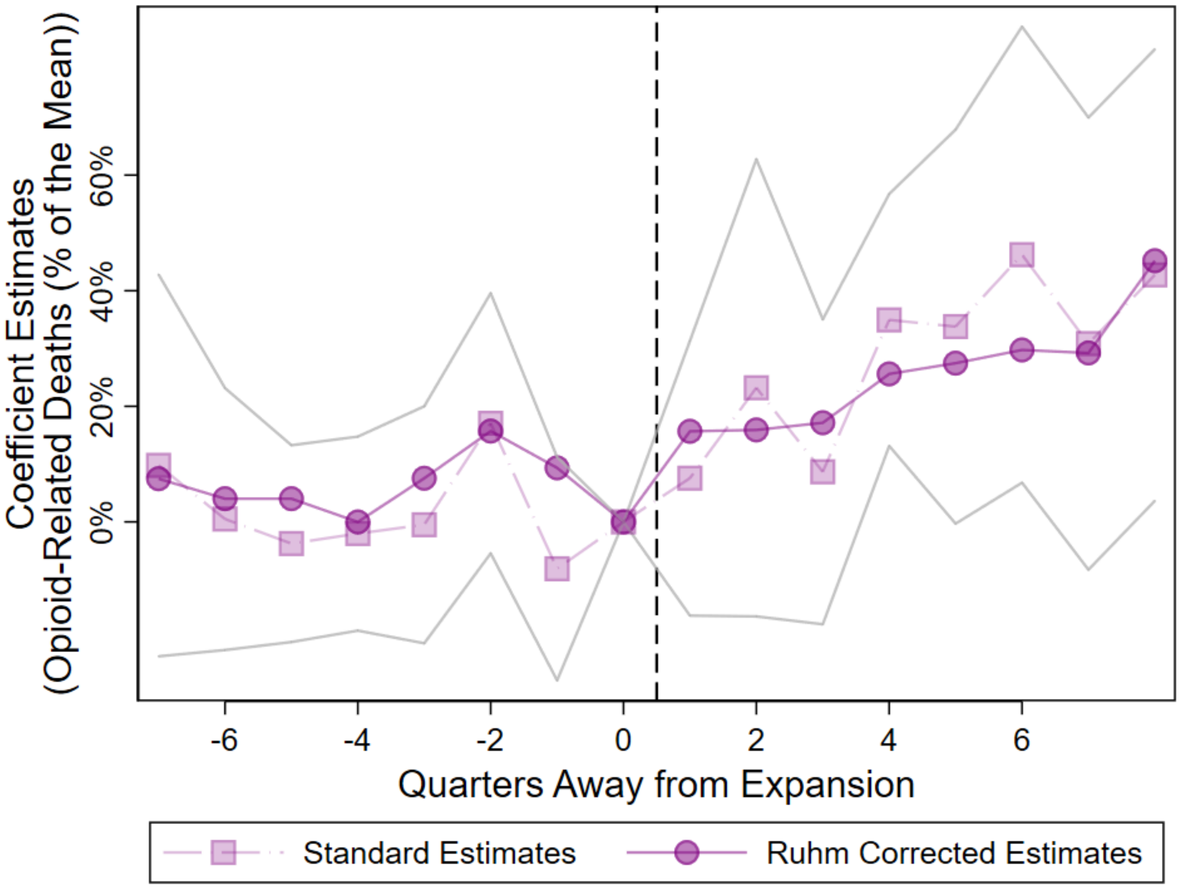
(c) Dropping Prior Expanders



(d) Combined

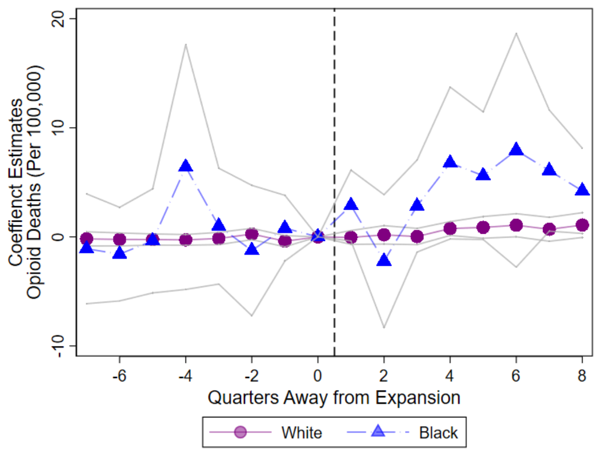
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with different samples formed from different treatment contrasts. Following Denham and Veazie (2019), I distinguish 4 "prior expander" states which had relatively generous pre-2014 eligibility levels (NY, VT, MA, DE). Following Sommers et al. (2013), I distinguish 6 "early expander" states which engaged in expansion efforts prior to 2014 (CA, CT, D.C., MN, NJ, WA.) Panel (a) repeats Main Figure 2b. Panel (b) repeats the same specification as in (a) but drops the 6 early expander states from the sample. Panel (c) repeats the same specification as in (a) but drops the 4 prior expander states from the sample. Panel (d) compares the point estimates from a-c in one diagram. Standard errors are always clustered at the state level. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A12: Impact of Medicaid Expansion on Opioid-Related Deaths: Ruhm Corrections

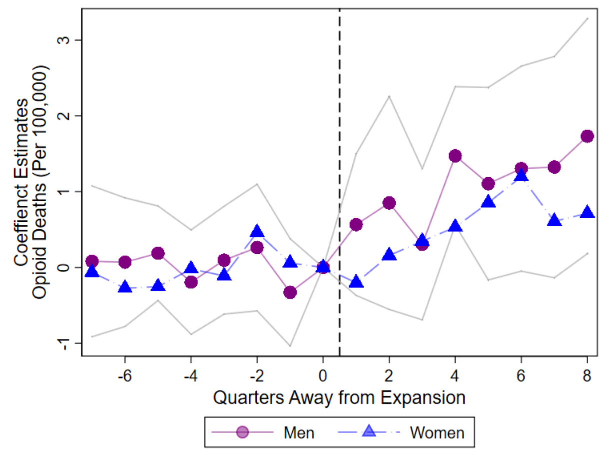


Note: Figure shows the coefficient estimates and 95% confidence interval from an OLS regression of Equation 2 where the dependent variable is the rate of opioid related-deaths. Standard errors are clustered at the state level. Observations are at the county-quarter level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016). The series "Standard Estimates" repeats the estimates and confidence intervals in Figure 2b, while the series "Ruhm Corrected Estimates" produces the same estimates after using Ruhm (2018)'s methods for correcting the overdoes measures in the restricted files. To ease visual comparison both series are presented as percentages of their respective pre-2014 means.

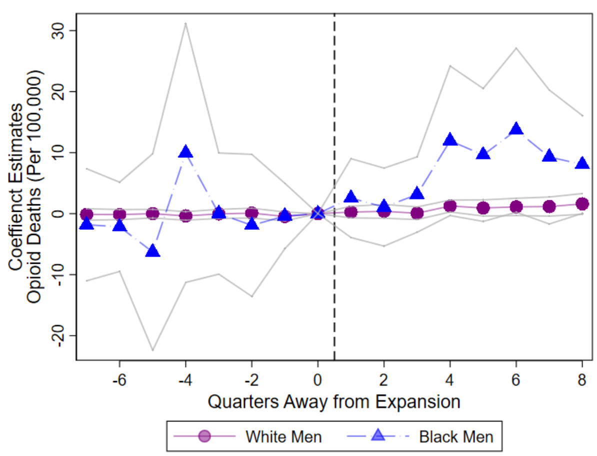
Figure A13: Impact of Medicaid Expansion on Opioid-Related Deaths: Heterogeneity



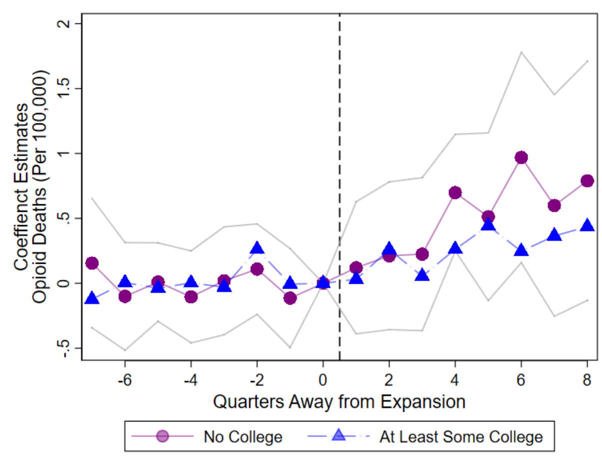
(a) By Race



(b) By Gender



(c) Men, By Race



(d) By Education

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with demographic specific rates of opioid-related deaths as the outcome variable. For example, the rate of white male opioid-related deaths is calculated per 100,000 white men. The rate of "no college" and "at least some college" opioid-related deaths is calculated per 100,000 population. Standard errors are clustered at the state level. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states but restricts to various subgroups by race, by gender, by the interaction of race and gender as well as by education, respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

Appendix Tables

Table A1: Impact of Medicaid Expansion on Opioid-Related Deaths: Robustness to Unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid Expansion	0.437**	0.410**	0.409**	0.403***	0.413***	0.414***
	(0.165)	(0.155)	(0.156)	(0.148)	(0.143)	(0.143)
Observations	38100	38100	38100	36100	34100	34100
Mean (Pre-Period)	1.553	1.553	1.553	1.553	1.553	1.553
Unemployment Specification		Linear	Quadratic	One Lag	Two Lags	Quadratic Two Lags

Note: Each column reports a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-quarter level. The dependent variable is the rate of opioid-related deaths. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Sample includes deaths of all US Residents. All specifications include county fixed effects as well as calendar quarter and year fixed effects. Column (1) repeat the main estimate from column (3) of Table 2. All other columns add to this specification various functions of the county-year unemployment rate. In columns (2) and (3), contemporaneous unemployment and then unemployment and its square are included, respectively, in an estimation of Equation 1. In columns (4) and (5) one and two lags of unemployment, respectively, are included together with contemporaneous unemployment. Lastly, in column (6), contemporaneous unemployment, its square, as well as the first lag and its square, and the second lag and its square all are included in the specification. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A2: Impact of Medicaid Expansion on Opioid-Related Deaths: DDD Estimates

	(1) Main DD	(2) DDD
Medicaid Expansion	0.437**	0.171
	(0.164)	(0.182)
High Uninsured		0.062
		(0.142)
Expansion × High Uninsured		0.501**
		(0.227)
Observations	38100	38100
Mean (Pre-Period)	1.553	1.553

Note: Each column reports a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. In column (1) the main estimates of the impact of Medicaid expansion on the rate of opioid-related deaths is repeated from Column (3) of Table 2. Column (2) reports the results from a specification of Equation 1 which includes all the pairwise interactions between an indicator for being in a state after Medicaid expansion and an indicator for being in a "High Uninsurance" county . I define a "High Uninsurance" county as a county which has an above median uninsured adult population with income at or above 138% of the Federal Poverty Line in the year prior to Medicaid expansion. (Adults in this group became newly eligible for Medicaid under the expansion.) Counties above the median averaged 12,500 uninsured individuals (which is 39% of the county population who are at or below 138% in the year prior to expansion) while counties below the median averaged 650 uninsured individuals (which is 36% of the county population at or below 138% of the poverty line in the year prior to expansion). All specifications include county fixed effects as well as calendar quarter and year fixed effects. Standard errors are clustered at the state level. Observations are at the county-quarter level. The sample includes deaths of all US Residents. The source is CDC Individual-Level Mortality Files (2012-2016), as well as SAHIE estimates of the uninsured (2010-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix B: Treatment Heterogeneity: Bacon Decomposition, Sun (2020) and Chaisemartin (2018)

Recent econometric work shows that when there is a staggered introduction of a treatment such as Medicaid expansion, standard two-way fixed effects specifications provide event study estimates that are a weighted average of the state expansion-cohort specific effects (i.e., the effect from states which expanded in quarter 1 of 2014, in quarter 2 of 2014,..., in quarter 3 of 2016.). If the effects of these differentially timed expansions are heterogeneous, the standard two-way fixed effects estimator not only misses this heterogeneity, but could exhibit "contamination bias" in which the estimator incorrectly estimates the overall treatment effect or incorrectly estimates pre-trends with the result that there are pre-treatment indicators that appear to be zero (i.e., no pre-trends) but are non-zero once the contamination of effects from other different expansion-cohorts has been accounted for. To address these concerns, I follow the recent literature in performing several tests.

1. I perform a Bacon decomposition to ascertain how much of the estimate arises from comparing never-treated to treated groups and how much arises from comparing treated groups which are treated at different times.

2. I implement a specification with a simple and intuitive control group that is not contaminated. To accomplish this, I restrict the control group to states which never expanded in the sample period. This estimate is identified off a treatment contrast that doesn't contain expansion-cohort heterogeneity.

3. I implement Sun (2020)'s techniques to build "interaction-weights" by event study indicator (i.e., each lead and lag) and state expansion-cohort groups. These weights are then used to test for evidence of heterogeneity.

4. I implement Chaisemartin (2018)'s techniques to produce dynamic treatment effect estimates which accommodate treatment heterogeneity.

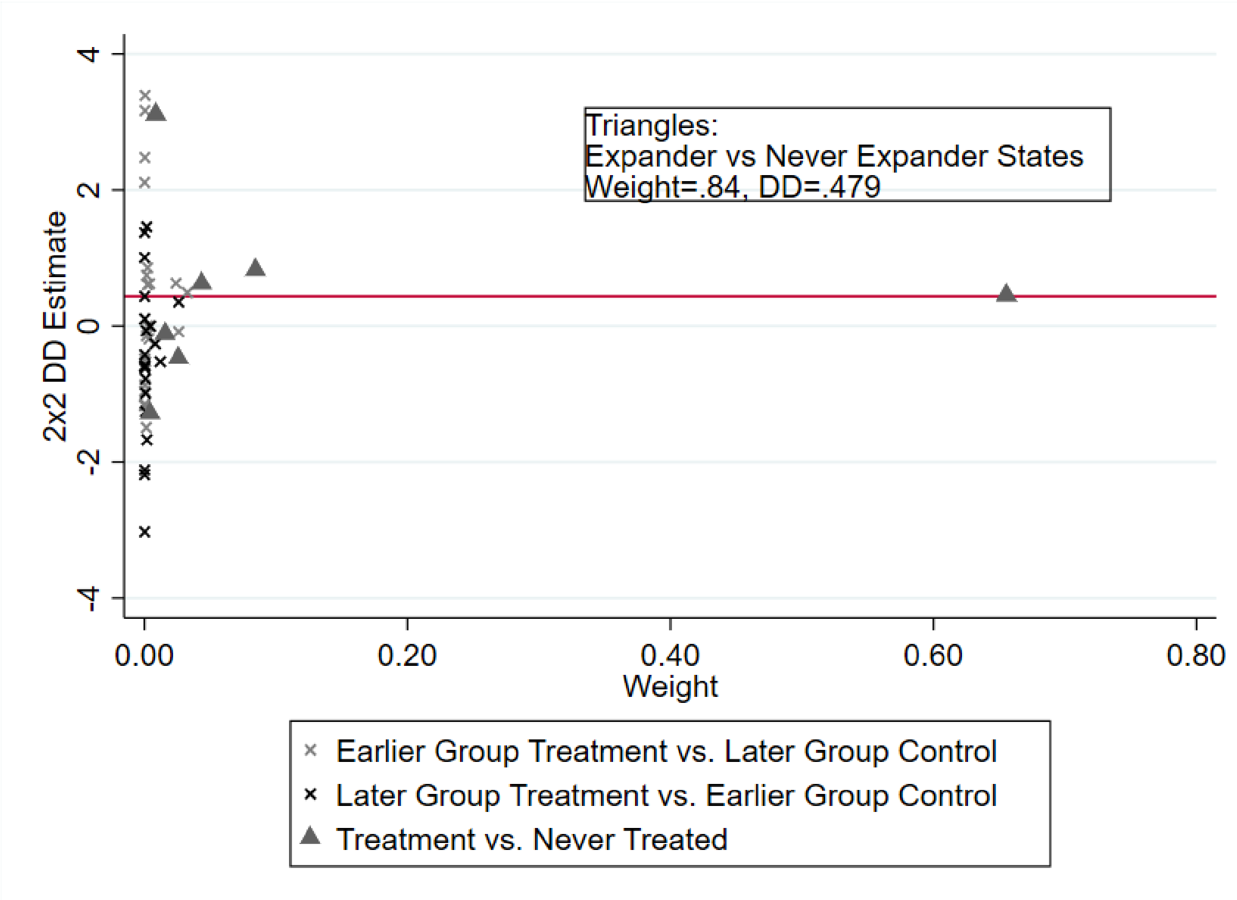
Overall, I find no evidence of contamination bias; because most of the treatment variation is driven by the the first state expansion-cohort, the results are driven largely by one large treatment, rather a series of smaller, staggered treatments. If anything, the decomposition suggests that slight heterogeneity across state expansion-cohorts is *very slightly* attenuating the standard estimates towards zero.

B1. Bacon Decomposition and "Clean" Controls

I follow Goodman-Bacon (2018) and compute how much of the standard DD estimate (Col (3) of Table 2) is attributable to a given treatment contrast, distinguishing, for example, the DD estimate contrasting never-expander states and expander states from the DD estimate contrasting expansion-only states which expanded at different times. I plot the different DD's arising from each treatment contrast against the weight they are implicitly given in the standard DD, distinguishing three distinct treatment contrasts (Figure B1).

I find that the contrast between expander states and states that never expanded ("treatment vs never treated") receives 83% of the weight in the overall DD estimate. (See the triangles in Figure B1). The decomposed estimate based on on this treatment contrast is .479 and is slightly larger than the standard two way fixed effects estimate, which is .437 (Col (3) of Main Table 2 and the red horizontal line in Figure B1).

Figure B1: Bacon Decomposition - Impact of Medicaid Expansion on Opioid Mortality

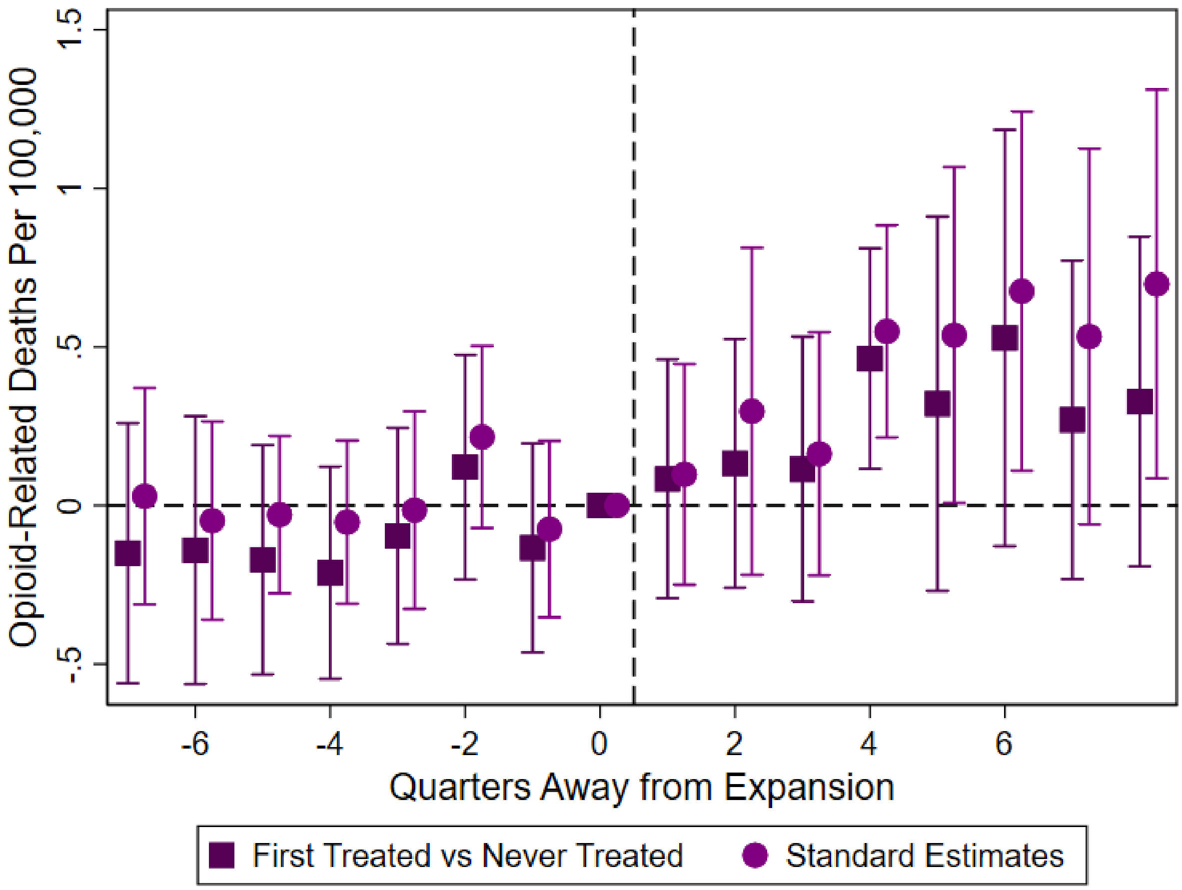


Note: Figure shows the Bacon-Goodman decomposition of the difference in difference estimates reported in Table 2. The solid red line shows the standard difference in difference estimate, which is reported Col (3) of Table 2 (.437).

The Bacon decomposition suggests that a DD estimate based entirely on the contrast between states which never expanded in the sample period that those which expanded first should give results very similar to the standard DD estimator. These results are of interest since they are free of expansion-cohort heterogeneity, and are intuitively straightforward to interpret.

Figure B2 displays these results. In the "First Treated vs Never Treated" estimates I omit the states which, in the standard dynamic estimates (Main Figure 2b), were initially in the control group and became treated later in the sample period. In other words, in the "First Treated vs Never Treated" specification, the control group remains untreated throughout the entire sample period. As Figure B2 shows, the "clean" dynamic results are similar to the main results (Figure 2b) and suggest a nearly identical average treatment effect.

Figure B2: Impact of Medicaid Expansion on Opioid-Related Deaths: “Clean Controls”



Note: Figure shows the coefficient estimates and 95% confidence interval from an OLS regression of Equation 2 where the dependent variable is the rate of opioid related-deaths. Standard errors are clustered at the state level. Observations are at the county-quarter level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016). The series ”Standard Estimates” repeats the estimates and confidence intervals in Figure 2b, while the series ”First Treated vs Never Treated” produces estimates which contrast only those states which expanded Medicaid first against those which never expanded Medicaid in the sample period. In other words, “First Treated vs Never Treated” estimates omit the states which, in the standard estimates, were initially in the control group and became treated later in the sample period.

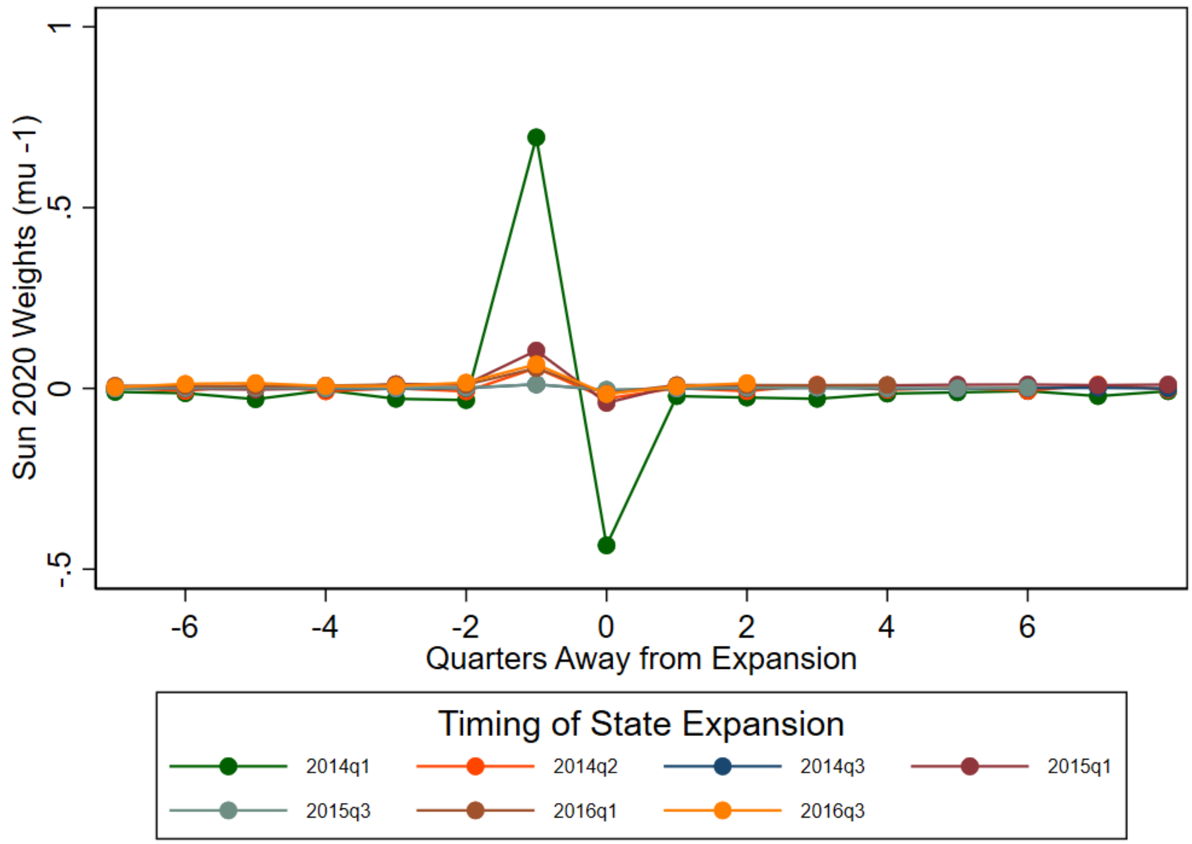
B2. Contamination Bias using IW Weights (Sun 2020)

An alternative approach to diagnosing the importance of treatment group heterogeneity is to evaluate the contribution of different state expansion-cohorts and lead and lag effects to the estimate of each lead and lag coefficient (Sun 2020). Intuitively, if lead and lag effects are contributing in meaningful ways to the identification of other leads and lags, it implies that contamination bias is present. I follow Sun (2020) and produce IW weights for the -1 estimator (two periods before treatment) to investigate the presence of "contamination bias" (Figure B3 below). I confirm that, for the -1 estimator, the sum of the IW weights over treatment lags is zero for each rollout-cohort.

The procedure is as follows: I implement Sun (2020)'s techniques to build "interaction-weights" (IW) by event study indicator and state expansion-cohort. An IW is a measure of a state's expansion-cohort's contribution to the estimation of a given coefficient in the standard event study specification. For example, for the -1 estimator (two periods before treatment), I compute an IW weight for the first state expansion-cohort for each event study time period (-6, -5, etc.) and then for the second state expansion-cohort, and so on. IW weights could show, e.g., that, in a given event study year, the first state expansion-cohort is contributing more to the -1 estimate than the second state expansion-cohort. I use these IW weights to test whether contamination bias is occurring. If, for example, I find significant weight placed on event study time period 2, 3, or 4 (in identifying the estimate at -1), that would suggest contamination bias.

Figure B3 shows the IW weights. In the main DD specification, I pool 6 state expansion-cohorts together, but for IW weights I break them apart. Following Sun (2020), I report the weights for the estimate of the indicator variable for two periods before treatment (-1 in event study time) across expansion-cohorts. First, I note that the weights for each expansion-cohort are largest at event time=-1 with the first expansion-cohort contributing the most weight. Since these are weights for the -1 estimator, we expect each cohort to contribute a positive weight to the estimates of the -1 coefficient, and we expect the first expansion-cohort to have the most weight since more of the treated sample experienced treatment in the first period. Secondly, I also expect the weights at 0 to be negative, since this is the excluded period in this specification. Finally, I note that the weights on the post-treatment indicators hover around zero and the sum of the post-treatment weights is very nearly zero (i.e. the sum of the IWs from our event study time =1 to =8). As Sun and Abraham (2020) notes, if the "weights are non-negative for lags of treatment", then this might suggest that the standard event study estimate for two periods before treatment is "sensitive to estimates of the dynamics effects … and does not isolate the pre-trends" (p.33). Overall, these IWs suggest that my estimate of the -1 indicator is not contaminated by bias arising from expansion-cohort heterogeneity.

Figure B3: Impact of Medicaid Expansion on Opioid-Related Deaths: Sun Weights



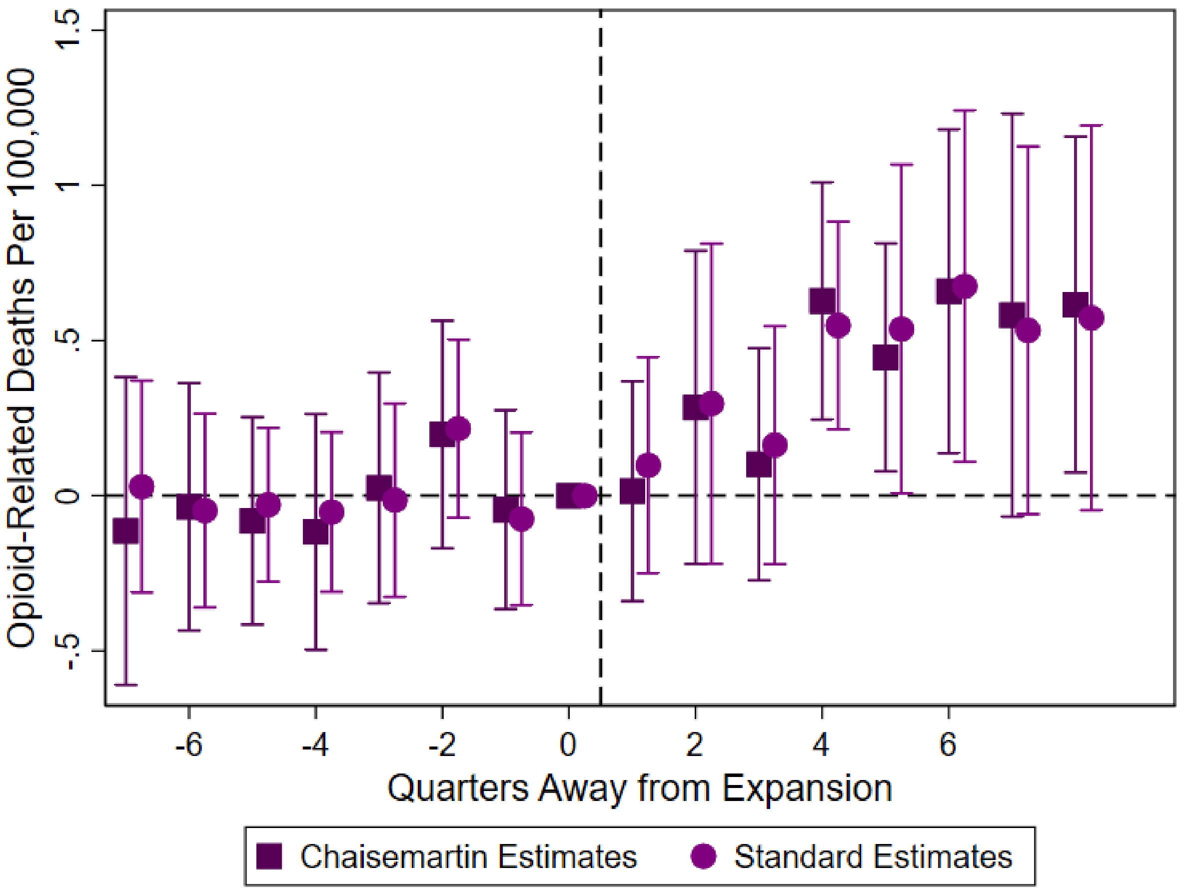
Note: Figure shows IW weights following Sun (2020). 0 is the excluded period.

B3. Chaisemartin Accounting for Treatment Heterogeneity

In this section, I have provided evidence suggesting that the main DD estimates are not biased due to treatment effect heterogeneity. Another approach is to account for any treatment effect heterogeneity there may be directly in the estimation procedure. To achieve this, I implement the techniques of Chaisemartin (2018) to produce dynamic treatment effect estimates which directly estimate heterogeneous dynamic treatment effects and weights the corresponding effects together into the estimates (Figure B4).

Overall, Chaisemartin estimates for pre-treatment periods do not suggest differential movement of treatment and control groups prior to treatment. The Chaisemartin estimates exhibit the same overall pattern as the standard estimates, although they suggest a slightly slower growth in treatment effects over time.

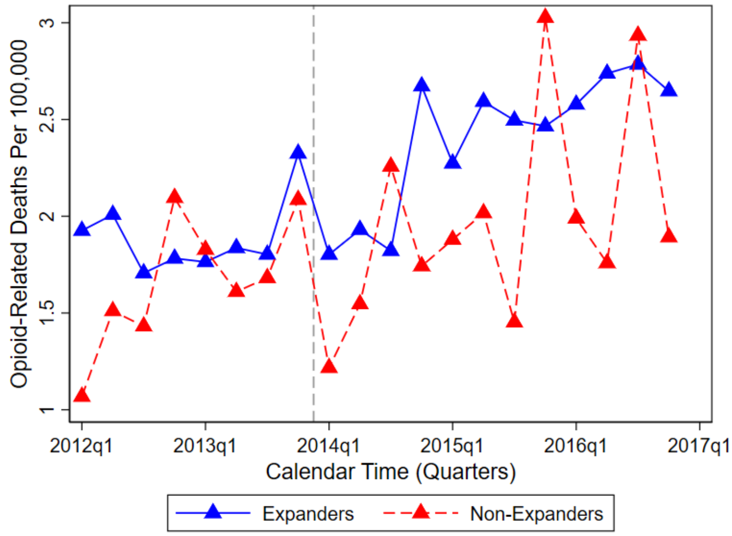
Figure B4: Impact of Medicaid Expansion on Opioid-Related Deaths: Chaisemartin Corrections



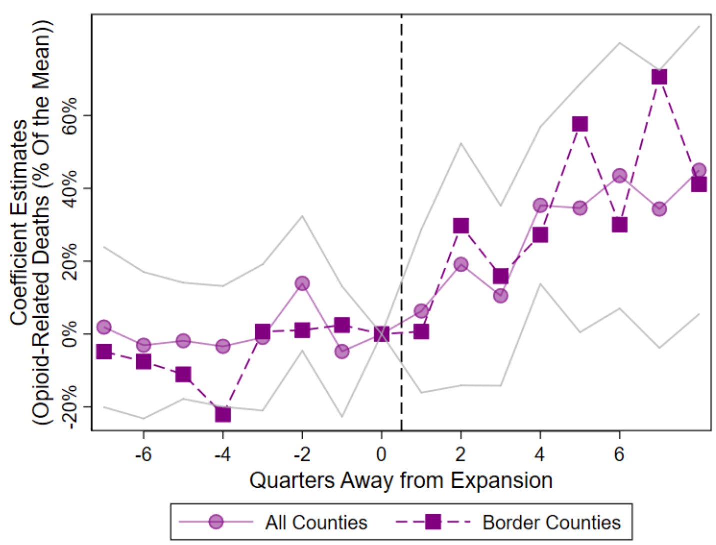
Note: Figure shows the coefficient estimates and 95% confidence interval from an OLS regression of Equation 2 where the dependent variable is the rate of opioid related-deaths. Standard errors are clustered at the state level. Observations are at the county-quarter level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016). The series "Standard Estimates" repeats the estimates and confidence intervals in Figure 2b, while the series "Chaisemartin Estimates" produces analogous estimates following techniques in Chaisemartin (2018).

Appendix C: Results Restricted to Border Counties

Figure C1: Medicaid Expansion and Opioid-Related Deaths: Border Sample



(a) Trends



(b) DD Estimates

Note: Panel (a) shows trends in opioid-related deaths separately for counties in states that expanded Medicaid and those that did not. Expansions began in January of 2014. The sample is restricted to counties on the geographic border between states which expanded and those which did not. Panel (b) shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with the rate of opioid-related deaths as the dependent variable. Results are reported for the sample of counties on the border between states which expanded and those which did not and compared to estimates using all counties. Estimates for border counties are weighted by each border county's share of its state population. To ease visual comparison, estimates for both samples are reported as percents of their respective pre-expansion means. Standard errors are always clustered at the state level. In both panels, observations are at the county-quarter level. All specifications includes county fixed effects, as well as calendar quarter and year fixed effects. The source is CDC Individual-Level Mortality Files (2012-2016).

Table C1: Impact of Medicaid Expansion on Opioid-Related Deaths: Border Sample

	(1) PCT Uninsured	(2) Death Rate
Border Counties Only		
Medicaid Expansion	-7.107***	0.841*
	(1.290)	(0.442)
Observations	3192	5800
Mean (Pre-Period)	39	1.744
Percent Effect CI	[12%,25%]	[-2%,97%]
All Counties		
Medicaid Expansion	-6.813***	0.437**
	(1.064)	(0.165)
Observations	22334	38100
Mean (Pre-Period)	40	1.553
Percent Effect CI	[12%,23%]	[7%,48%]

Note: Each cell reports estimates from a separate regression with standard errors reported in parentheses. In column (1), the share of uninsured individuals aged 18-65 with income at or below 138% of the Federal Poverty Line is the dependent variable in an OLS estimation of Equation 1. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Observations are at the county-year level. The source is SAHIE estimates of the uninsured (2010-2016) In column (2), the dependent variable is the rate of opioid-related deaths per 100,000 county population. Observations are at the county-quarter level, and rounded to the nearest hundred for disclosure reasons. In the first panel, the sample is restricted to counties on the borders between states which expanded Medicaid and those which did not. Estimates for border counties are weighted by each border county's share of its state population. In the second panel, all counties are included; the second panel repeats columns (1) and (3) of Table 2. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix D: Results Restricted to States on Margin of Medicaid Expansion

To further address any endogeneity concerns not addressed by examining the pre-trends and pre-period "effect" estimates, I also consider a restricted sample I call the "propensity sample". This sample is obtained by first estimating Equation 3 and plotting the distribution of these predicated values separately for expander and non-expander states. I then construct a sample where the two distributions overlap, and name this sample the "propensity sample". By construction, this sample includes only those states whose estimates of expansion likelihood was numerically comparable to the expansion likelihood of another state whose expansion status was different. In other words, the "propensity sample" contains only expander states whose expansion likelihood was numerically equal to or less than the expansion likelihood of some state that failed to expand, and contains only non-expander states whose expansion likelihood was numerically equal to or greater than the expansion likelihood of some state that expanded. This sample of states ought to be free from any sort of state-level selection concerns such as the concern that less Republican, more wealthy states who expanded could have differed in unobservable ways from more Republican, less wealthy states who failed to expand and that these unobservable differences are somehow driving the difference-in-difference estimates.[32]

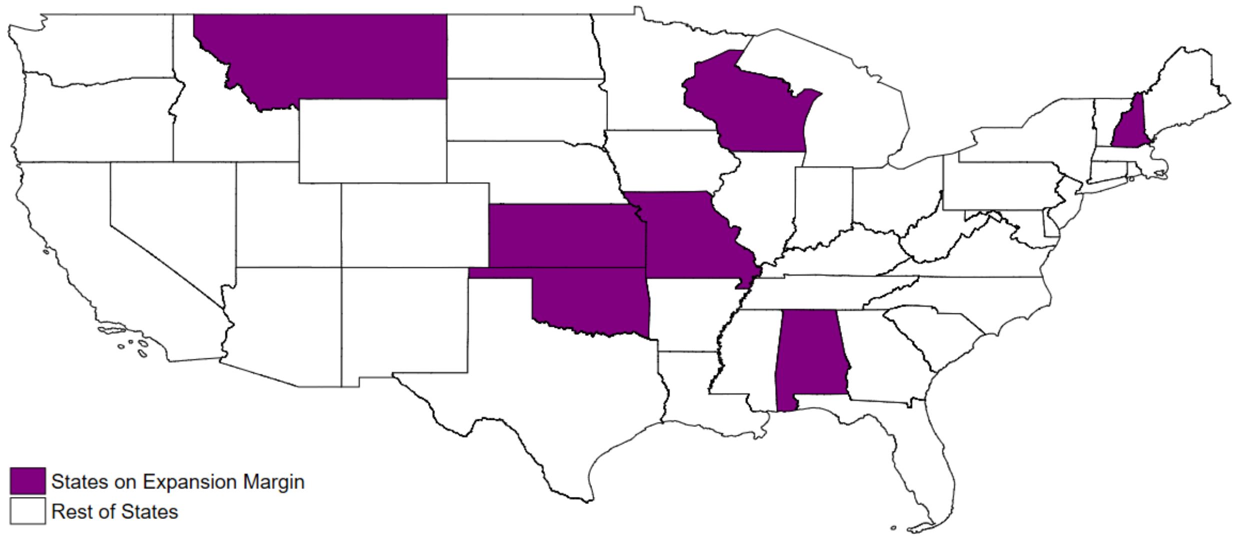
Figure D1 shows the propensity sample (excluding Hawaii and Alaska[33]), while Table 3 shows propensity balancing in the propensity sample. Finally, Figure D2a show calendar year trends in opioid-related deaths for the "propensity sample", while Figure D2b shows dynamic estimates. We can see that prior to the expansion starting in January of 2014, expander

[32]Note that for such an endogeneity story to be successful the timing of the impact of these unobserved factors would also have to correlate with the timing of Medicaid expansion and its staggered rollout.

[33]Hawaii expanded in 2014, while Alaska expanded in the third quarter of 2015

and non-expander states in this sample tracked each other well, but that expander states, on average, show large increases relative to non-expander states after expansion. Column (2) of Table D2 contains the results of Equation 1 restricted to the "propensity sample" (which includes only those states on the margin of Medicaid expansion) and compares them to the main sample (which includes all counties). I find again that Medicaid expansion is associated with sizeable increases in opioid-related mortality; the point estimates from the "propensity sample" are larger in magnitude than those from the overall sample, and the "propensity sample" estimates represent a much larger increase off the mean than the estimates from the overall sample. However, the "propensity sample" is one sixth the size of the overall sample and, consequently, has standard errors that are six times as large.

Figure D1: Distribution of Propensity Estimates: Map



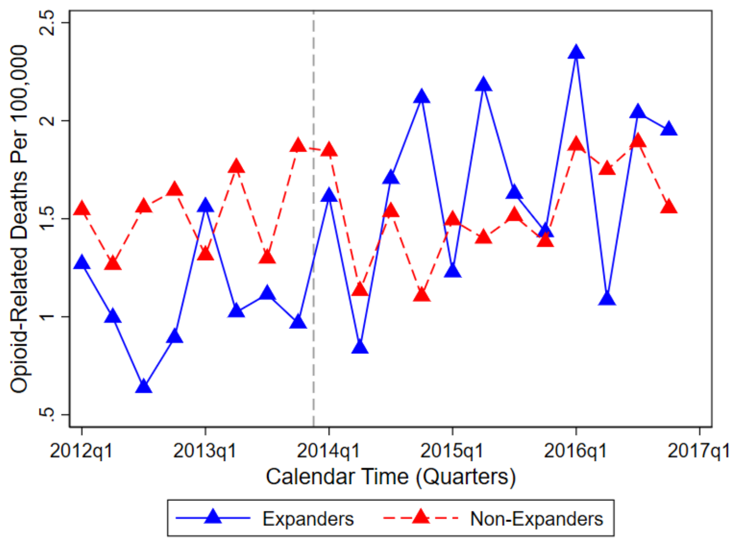
Note: Figure shows the propensity sample, which consists of States on the margin of Expansion. The expansion margin is computed by capturing the predicted values from equation 3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the propensity sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state.

Table D1: Predicted Propensities of State Level Expansion: Propensity Sample

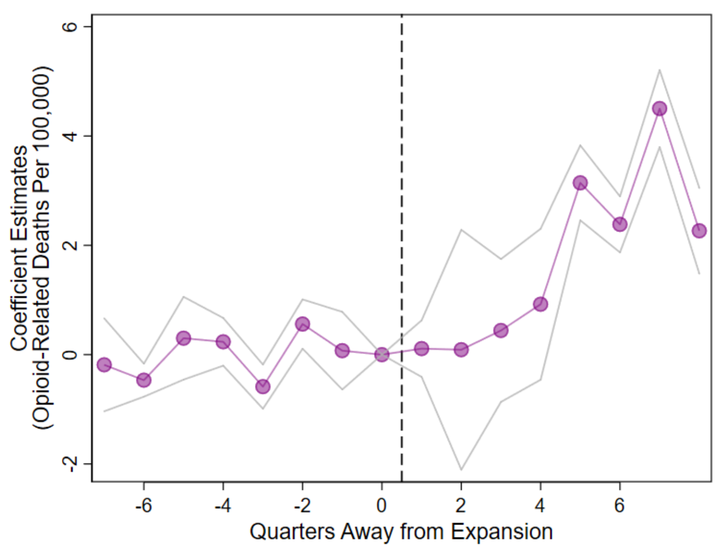
	All States		
	Expander	Non-Expander	Δ
Pr(Expansion)	.623	.306	.317***
	Propensity Sample		
	Expander	Non-Expander	Δ
Pr(Expansion)	.433	.418	.014***

Note: Table shows differences (Δ) in predicted likelihood of expansion across all states in the top panel and restricted to the propensity sample in the bottom panel. When we restrict to the propensity sample, states which expanded have expansion probabilities comparable to those states who did not expand. The expansion margin is computed by capturing the predicted values from Equation 3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the propensity sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state.

Figure D2: Medicaid Expansion and Opioid-Related Deaths: Propensity Overlapping Sample



(a) Trends



(b) DD Estimates

Note: Panel (a) shows trends in opioid-related deaths separately for counties in states that expanded Medicaid and those that did not. Expansions began in January of 2014. Panel (b) shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with the rate of opioid-related deaths as the dependent variable. Standard errors are clustered at the state level. In both panels, observations are at the county-quarter level. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. In both panels, the sample is restricted to states on the margin of expanding Medicaid (the "propensity sample"). The expansion margin is computed by capturing the predicted values from equation 3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the "propensity sample" just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. The source is CDC Individual-Level Mortality Files (2012-2016).

Table D2: Impact of Medicaid Expansion on Opioid-Related Deaths: Propensity Sample

	(1) PCT Uninsured	(2) Death Rate
Propensity Sample Only		
Medicaid Expansion	-9.420***	0.739
	(2.234)	(1.022)
Observations	3563	6200
Mean (Pre-Period)	40	1.44
Percent Effect CI	[13%,35%]	[-88%,190%]
All Counties		
Medicaid Expansion	-6.813***	0.437**
	(1.064)	(0.165)
Observations	22334	38100
Mean (Pre-Period)	40	1.553
Percent Effect CI	[12%,23%]	[7%,48%]

Note: Each cell reports estimates from a separate regression with standard errors reported in parentheses. In column (1), the share of uninsured individuals aged 18-65 with income at or below 138% of the Federal Poverty Line is the dependent variable in an OLS estimation of Equation 1. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Observations are at the county-year level. The source is SAHIE estimates of the uninsured (2010-2016) In column (2), the dependent variable is the rate of opioid-related deaths per 100,000 county population. Observations are at the county-quarter level, and rounded to the nearest hundred for disclosure reasons. In the first panel, The sample is restricted to states on the margin of expanding Medicaid (the propensity sample). The expansion margin is computed by capturing the predicted values from equation 3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the propensity sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. In the second panel, all counties are included; the second panel repeat columns (1) and (3) of Table 2. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix E: Medicaid Expansion and Marijuana Legalization as Separate Shocks

As discussed in Section 5.2 above, Figure 6 shows the cumulative percent of the population exposed to the interaction of Medicaid expansion and marijuana legalization. Appendix Tables E1- E2 shows the summary statistics of the variables containing the policy variation.

Is the state-level decision to legalize marijuana connected with the state-level decision to expand Medicaid? Table E3 shows estimates of the extent to which marijuana legalization can successfully predict Medicaid expansion. We see that the success marijuana legalization has in predicting Medicaid expansion is largely soaked up when other endogenous state-level variables are taken into account (See columns (1) and (2) of Table E3). Furthermore, we see that marijuana legalization is not successful in predicting the timing of Medicaid expansion (See columns (3) and (4) of Table E3). I conclude that, at least for the purposes of understanding opioid-related mortality, these two policy shocks are separate from each other and can be used as independent sources of policy variation.

Table E1: Summary Statistics: Further Breakdowns

	Marijuana Legalization	No Marijuana Legalization		Total
		Expander	Non-Expander	
Opioid Death Variables				
Opioid Related Deaths (Counts)	0.82	1.23	0.51	0.88
	(2.06)	(3.66)	(1.75)	(2.87)
PCT Counties w/ Zero Opioid Deaths	0.11	0.08	0.14	0.11
	(0.31)	(0.28)	(0.34)	(0.31)
Observations	7800	54400	50500	112800

Note: Means reported with standard errors in parentheses. Observations are at county-month level. Distributions are reported separately based both upon the state-level decision to expand Medicaid, upon the state-level decision to legalize recreational marijuana, and the interaction of these policies. All states that legalized recreational marijuana expanded Medicaid at some time. Source: CDC Individual-Level Mortality Files (2010-2016) and state legal databases.

Table E2: Summary Statistics: Treatment Variables

Medicaid Expansion	0.621 (0.490)
Date of Medicaid Expansion	March 2014 (6 months)
Recreational Marijuana	0.0528 (0.226)
Date of Recreational Legalization	November 2013 (15 months)
Observations	51

Note: Means reported with standard errors in parentheses. Source: State legal databases. Observations are at the state-level. 2010 state-level populations are used as population weights. Expansion and legalization means (rows 1,3 and 5) are to be interpreted as the average percent of the population exposed to those policies across all states (from 2010-2016). Date variables are in units of months and standard errors are to be interpreted as months away from the reported mean date. By the end of 2016, 32 states in the sample expanded Medicaid. By the end of 2016, 4 states had legalized recreational marijuana and 12 had legalized either recreational or medical marijuana. All states that legalized recreational marijuana expanded Medicaid at some point.

Table E3: Correlations between Medicaid Expansion and Marijuana Legalization

	(1) Expansion	(2) Expansion	(3) Exp Date	(4) Exp Date
Legalized Marijuana	0.435***	0.064	-2.751*	2.725
	(0.117)	(0.087)	(1.576)	(6.400)
Observations	112800	110000	62200	62100
Mean	0.586	0.589	April 2014	April 2014
Covariates		X		X

Note: Each column reports a separate OLS regression of a measure of a given state's decision concerning Medicaid expansion against an indicator for whether the state legalized recreational marijuana. Standard errors are clustered at the state level and reported in parentheses. In columns (1) and (2) the dependent variable is an indicator for whether the state expanded Medicaid, and in columns (3) and (4) the dependent variable is the date of Medicaid expansion (conditional upon expansion). All estimates measure the extent to which the state-level decision to legalize recreational marijuana predicts Medicaid expansion. Coefficients in columns (1) and (2) measure predicted impact on the likelihood of expansion; coefficients in columns (3) and (4) measure the number of months before or after the average expansion date an average state expanded as predicted by the state-level decision to legalize marijuana. Observations are always at the county-month level ranging from 2010 to 2016 and all regressions are weighted by 2010 population. Specifications in columns (2) and (4) include county level covariates from 2010, which are listed in Table 3 (some of these covariates were not available in 2016, which shrank the sample). The data sources are state legal databases. * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix F: Impact on Prescriptions Using Non-Medicaid Related Opioid Prescription Data

One concern with the estimates in Figure 3 is that, because they rely on data which was generated by prescription reports within the Medicaid system, if there were substantial payment substitution between Medicaid and pre-existing sources of finance, then the increase in opioid drugs attributed to Medicaid *might* merely reflect this substitution in financing while the state-level volume of opioid drugs prescribed remains unchanged. However, studying prescriptions drugs more broadly, Ghosh (2017) finds that following Medicaid expansion Medicaid-paid prescription utilization increased by 19 percent in expansion states relative to states that did not expand.[34] Mahendraratnum (2017) finds similar results. Indeed, Ghosh (2017) does not find reductions in uninsured or privately insured prescriptions and concludes that increased prescriptions filed under Medicaid did not substitute for pre-existing sources of payment.

To further address this issue of funding source substitution, I turn to data sources which are genereated outside the Medicaid system. In particular, to corroborate results found using CMS data on Medicaid associated opioid prescriptions, I use ARCOS data on overall volume of drugs distribution. ARCOS data is at the state-quarter level and reports measures of drug distibuted in gram weights. ARCOS measures are in no way related to the CMS system which records the data on opioid prescriptions associated with Medicaid used in Table 3 above. Using ARCOS data, I estimate sizable increases of Medicaid expansion on opioid drug distribution, estimates which are significant only at the ten percent level (column (1) of Table F1).

Back of the envelope calculations show that Table F1 estimates are consist with the view that Medicaid expansion increased the overall volume of opioids accessed in a state and did *not* merely cause Medicaid to refinance existing opioid prescriptions. Table 2 column 1 shows that Medicaid expansion caused 2,884 newly insured individuals (a 6.8% increase). This together with Table F1 column 1 (which shows Expansion caused a 5% increase in total volume of opioids distributed) suggests that 73.5% (5/6.8) of the increase is likely attributable to opioids being distributed to individuals newly insured by Medicaid. These estimates are, of course, consistent with the view that *all* of the 5% increase in distributed opioids went to the 2,884 (6.8%) newly insured individuals, the distribution ranging from new enrollees who never received any opioids to those who received well above the average.

Likewise, column 2 of Table 2, which is repeated in column 2 of Table F1 shows that Medicaid expansion caused .009 more opioid prescriptions or .4514 (28.5%) more opioid units per person to be filed through Medicaid. Approximately 21% of the population is enrolled in Medicaid[35], and so, based on these estimates, we would expect this increase in Medicaid

[34] This works out to approximately seven additional prescriptions per year per newly enrolled beneficiary. While this increase is for *all* prescription drugs, and Ghosh (2017) does not offer a specific breakdown for opioids, the overall increase is present in the category into which opioid prescriptions would fall ("Other"). See Table 2, row labelled "Other", 19%.

[35] Author calculations using public ACS data.

financed units per person to yield an increase of 5.98% (21 percent of 28.5%). This is consistent with the 5.3% increase off the mean we find in column 1 of Table F1 (.0031/.058), and suggests that 89% (5.3/5.98) of the newly distributed opioids were financed through Medicaid.

Table F1: Impact of Medicaid Expansion on Opioid Prescriptions - ARCOS Drug Distribution Data

	(1) ARCOS	(2) CMS
Medicaid Expansion	0.0031*	0.009***
	(0.0017)	(0.002)
Observations	1020	1530
Mean (Pre-Period)	.058	.024
Percent Effect CI	[-.4%,11.1%]	[21%,49%]

Note: Table reports estimates from an OLS regression with standard errors clustered at the state-level and reported in parentheses. The reported coefficient of interest is an indicator for whether the observation is in a state which expanded Medicaid. The reported percent effect confidence interval (CI) is the 95% confidence interval of the estimate divided by the mean of the outcome variable over years prior to Medicaid expansion. In column (1) the total volume of opioids (measured in gram weight) per person distributed to the state is the dependent variable an OLS estimation of Equation 1. Observations are at the state-quarter level. The source is ARCOS reports of quarterly drug distribution (2012-2016). In column (2), the rate of opioid prescriptions filed through Medicaid per person is the dependent variable in an OLS estimation of Equation 1. Observations are at the state-quarter level. The source is CMS state-drug utilization (2010-2017). * $p < .1$, ** $p < .05$, *** $p < .01$

Appendix G: Comparing Results to Mathur and Ruhm (2022)

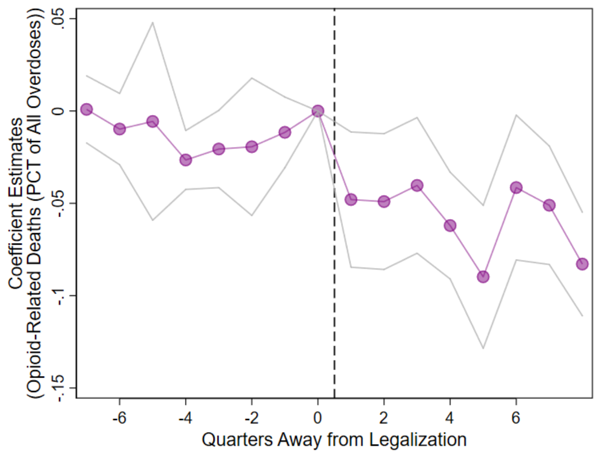
The main goal of this paper is to understand the impact of Medicaid expansion on opioid mortality. I estimate the impact of marijuana access on opioid mortality in order to understand how marijuana access policies interact with Medicaid expansion in the determination of opioid mortality.

In this Appendix, I compare my estimates of the impact of marijuana legislation with those in Mathur and Ruhm (2022). Both of our studies use restricted microdata, but my estimates use data only from 2012-2016, while Mathur and Ruhm (2022) uses data from 1999-2019. When I replicated Mathur and Ruhm (2022) using my data from 2012-2016 I also found that the policy variation we leverage is slightly different. The results in Figure 7 of this paper identify off variation in recreational marijuana legislation from Wen and Hockenberry (2018) that specifies the date the law became effective as distinct from the date the law permitted use, from the date the law was signed, and from the date the dispensaries opened. Mathur and Ruhm (2022), by contrast, offers four different sorts of policy variation: date of recreational legalization, date of recreational dispensary opening, date of medical legalisation and date of medical dispensary opening (Appendix Table A1).

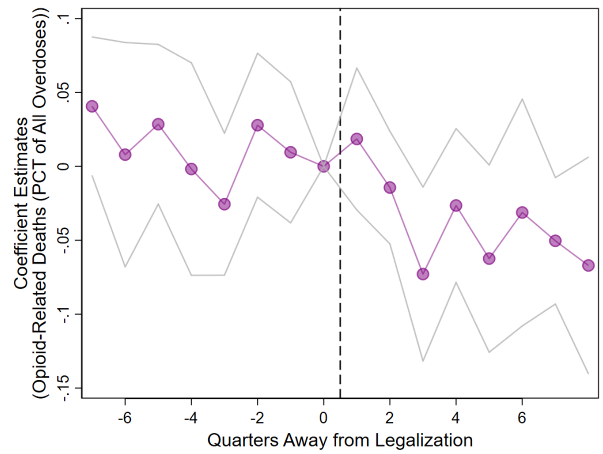
Therefore, to compare estimates, I utilized the exact policy variation in Mathur and Ruhm (2022) (Appendix Table A1) and ran specifications of equation 2 with three opioid mortality measures from 2012-2016. Overall, I find results broadly consistent with Mathur and Ruhm (2022): recreational marijuana access (as identified off both legalization date and dispensary opening date) seems to decrease opioid mortality (Figures G1-G2), but medical marijuana legalization seems to increase opioid mortality (Figure G3). In my data from 2012-2016, the decreases associated with recreational marijuana legalization are perhaps more convincing than those associated with recreational dispensary openings. Mathur and Ruhm (2022) also finds that, "these predicted effects are relative to the counterfactual of no legal cannabis. If we instead compare them to the situation where the state is adding recreational marijuana to already legal medical cannabis ..., the results ... suggest that recreational marijuana without retail sales reverses the deleterious effects of medical cannabis with dispensaries, but that the availability of retail recreational marijuana sales undoes these benefits."

It is important to emphasize that the results in Section 5.2.1, which utilize county-level variation in pre-Medicaid marijuana dispensary presence from Smith (2017), hold regardless of the choice of state-year variation in marijuana legalization. Thus, regardless of the estimates using recreational marijuana access policies, the Section 5.2.1 results using county-level dispensary presence still suggest that the opioid mortality impacts of Medicaid expansion were mitigated by marijuana access.

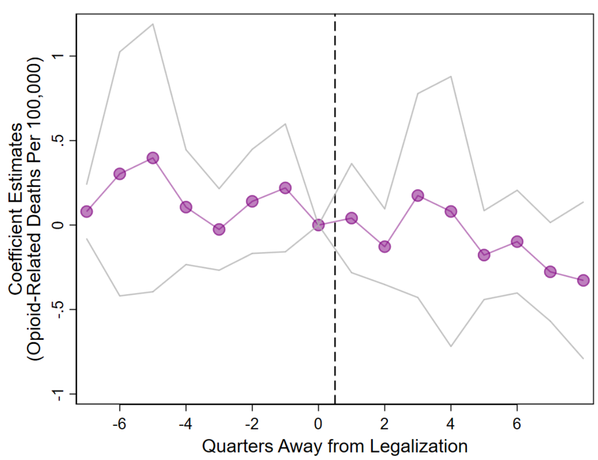
Figure G1: Impact of Marijuana Legalisation on Opioid-Related Deaths: Comparison to Mathur and Ruhm (2022) Recreational Legalization



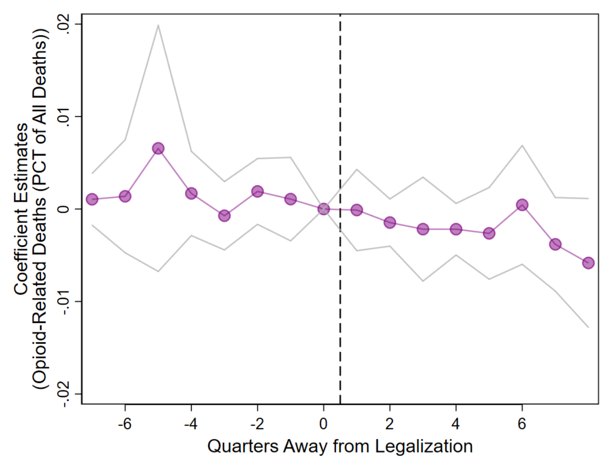
(a) Figure 7 This Paper



(b) Mathur and Ruhm (2022) Variation



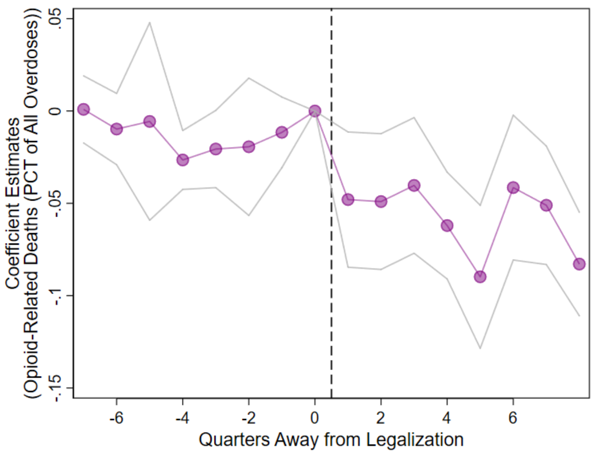
(c) Mathur and Ruhm (2022) Variation



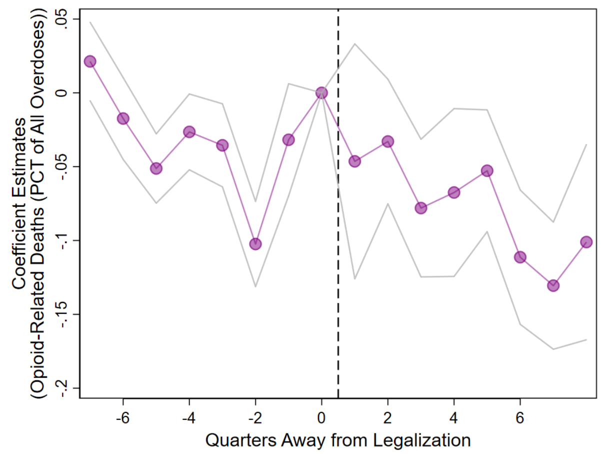
(d) Mathur and Ruhm (2022) Variation

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Panel a repeats Figure 7. This specification uses variation from Wen and Hockenberry (2018) that specifies the date the law became effective (as distinct from the date the law permitted use, from the date the law was signed, and from the date the dispensaries opened. Panels c-d use policy variation in Mathur and Ruhm (2022), which offers 4 different sorts of policy variation: date of recreational legalization, date of recreational dispensary opening, date of medical legalisation and date of medical dispensary opening (Appendix Table A1). Panels c-d each use the date of recreational legalization as recorded in Mathur and Ruhm (2022), and consider three different measures of opioid mortality: deaths as a share of all overdose deaths, deaths as a share of all deaths, and deaths per 100,000 population. The source is CDC Individual-Level Mortality Files (2012-2016).

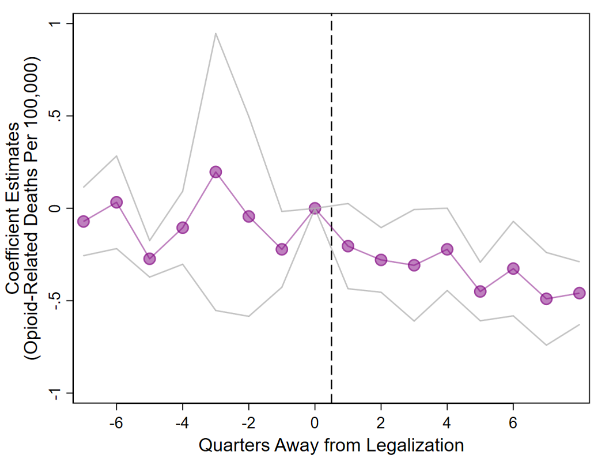
Figure G2: Impact of Marijuana Legalisation on Opioid-Related Deaths: Comparison to Mathur and Ruhm (2022) Recreational Dispensaries



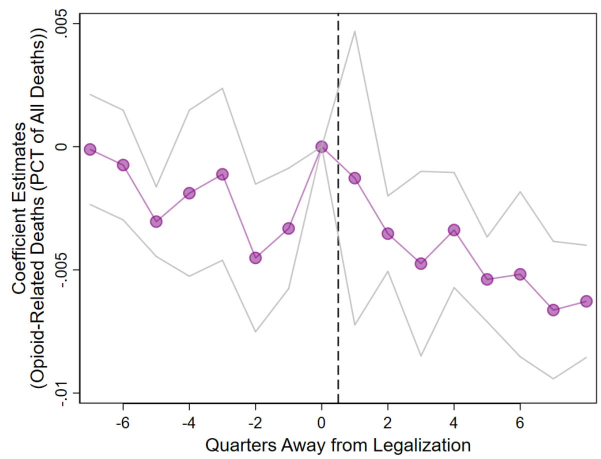
(a) Figure 7 This Paper



(b) Mathur and Ruhm (2022) Variation



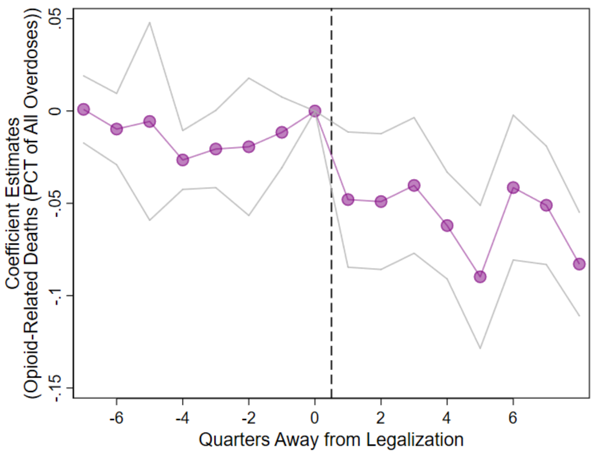
(c) Mathur and Ruhm (2022) Variation



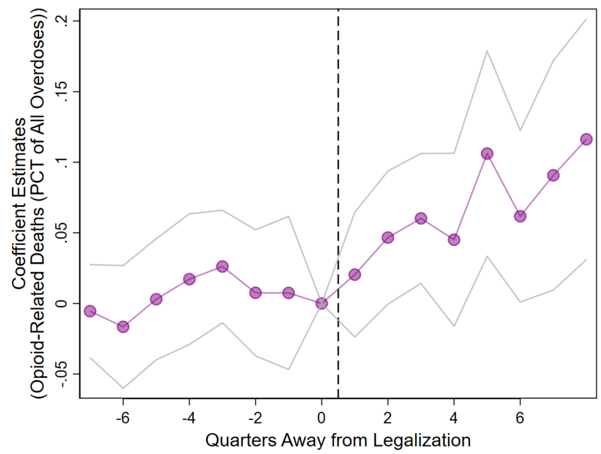
(d) Mathur and Ruhm (2022) Variation

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Panel a repeats Figure 7. This specification uses variation from Wen and Hockenberry (2018) that specifies the date the law became effective (as distinct from the date the law permitted use, from the date the law was signed, and from the date the dispensaries opened. Panels c-d use policy variation in Mathur and Ruhm (2022), which offers 4 different sorts of policy variation: date of recreational legalization, date of recreational dispensary opening, date of medical legalisation and date of medical dispensary opening (Appendix Table A1). Panels c-d each use the date of recreational dispensary opening as recorded in Mathur and Ruhm (2022), and consider three different measures of opioid mortality: deaths as a share of all overdose deaths, deaths as a share of all deaths, and deaths per 100,000 population. The source is CDC Individual-Level Mortality Files (2012-2016).

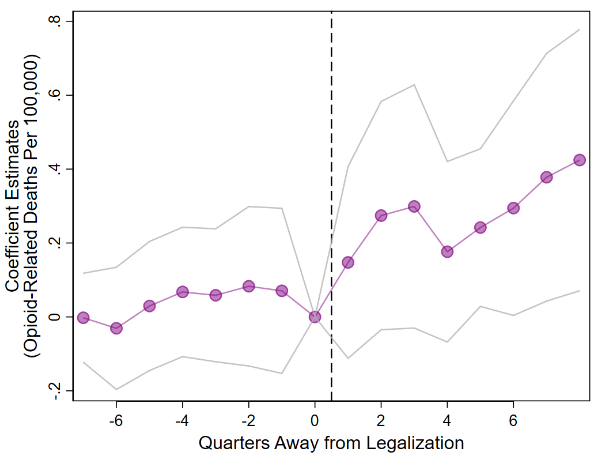
Figure G3: Impact of Marijuana Legalisation on Opioid-Related Deaths: Comparison to Mathur and Ruhm (2022) Medical Variation



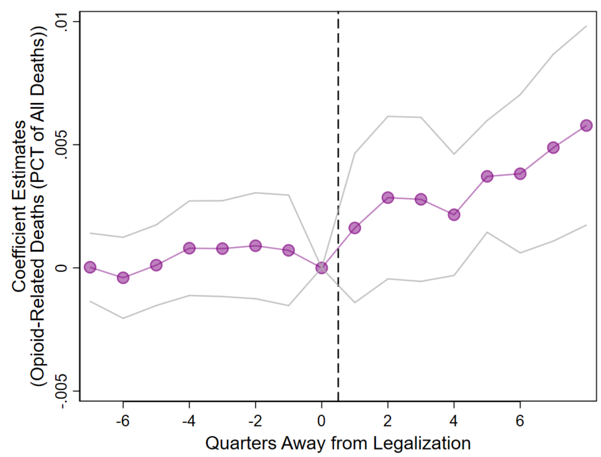
(a) Figure 7 This Paper (Recreational)



(b) Mathur and Ruhm (2022) Variation



(c) Mathur and Ruhm (2022) Variation



(d) Mathur and Ruhm (2022) Variation

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Panel a repeats Figure 7. This specification uses variation from Wen and Hockenberry (2018) that specifies the date the law became effective (as distinct from the date the law permitted use, from the date the law was signed, and from the date the dispensaries opened. Panels c-d use policy variation in Mathur and Ruhm (2022), which offers 4 different sorts of policy variation: date of recreational legalization, date of recreational dispensary opening, date of medical legalisation and date of medical dispensary opening (Appendix Table A1). Panels c-d each use the date of medical legalization as recorded in Mathur and Ruhm (2022), and consider three different measures of opioid mortality: deaths as a share of all overdose deaths, deaths as a share of all deaths, and deaths per 100,000 population. The source is CDC Individual-Level Mortality Files (2012-2016).

Appendix H: Comparing Results to Averett et al. 2019

Averett et al. (2019) finds “no evidence that Medicaid expansion is related to opioid deaths”.

In this Appendix, I compare my estimates to Averett et al. (2019) to understand why our estimates differ. Table H1 lists key differences I discovered in attempting to perform replication exercises. These differences are the choice of the sample period (I use 2012-2016, Averett uses 2010-2017), the level at which to collapse the microdata (I use county-quarter, Averett uses state-year), the choice of the outcome variable (I use the opioid death rate, Averett uses the log of the rate), the choice of which mortality codes to count as opioid death (I include deaths from heroin, while Averett excludes heroin), and the choice of whether to include other policy variation in the specification (I do not include other variation in my main specs, while Averett includes a wide set of control variables). While I was able to partially replicate most of Averett et al. (2019)’s choices, I found that none of these different choices changes the results substantially. Figures H2-H5 below show these results. The only replicable difference that I can find is that while I include all expansion states in the treatment group, Averett et al. (2019) explores several different, more narrow treatment groups which are taken from Kaestner et al. (2017) and Sharp et al. (2018).

The only aspect of Averett et al. (2019) that I could not replicate involves the choice to include as a control variable the maximum EITC payment received by a 3 person family (sourced from the UKCPSR). The issue I encountered is that, unlike the other control variables Averett et al. (2019) chose, EITC variation is at the nation-year level rather than the state-year level. Since Averett et al. (2019) and my paper are both using two way fixed effects estimators, we want to include year fixed effects as regressors, but, since EITC varies only by year, EITC variation is collinear with year fixed effects. Thus, I cannot include both ETIC variation and year fixed effects on the right hand side. Table 1 of Averett et al. (2019) reports coefficient estimates on the variable for EITC which are always “0.000 (0.000)” across all columns, even though all columns include year fixed effects. I do not understand how these are estimated, since, on my reading of the specifications, the EITC variation should in each specification already be absorbed into the year fixed effects. It is possible that this collinearity of EITC and year fixed effects is driving the differences between this paper and Averett et al. (2019). I tried to reconcile these differences by dropping EITC and then year fixed effects, separately and failed (Figure H5). All replication checks I show below in Figures H2-H3 include year fixed effects but not the EITC variation. In all specifications, I adopt Averett’s sample period of 2010-2017.

Overall, I still find evidence of an effect:

1. Averett’s raw trends suggest an effect, just as my trends do.

2. Averett’s event study estimates using Sharp et al. (2018) variation suggest an effect (panels c and d in Figure H1), just as my estimates do (Figure H2).

3. Averett’s event study estimates using Kaestner et al (2017)’s full variation (panel a in Figure H1) suggest at best weak evidence of an effect.

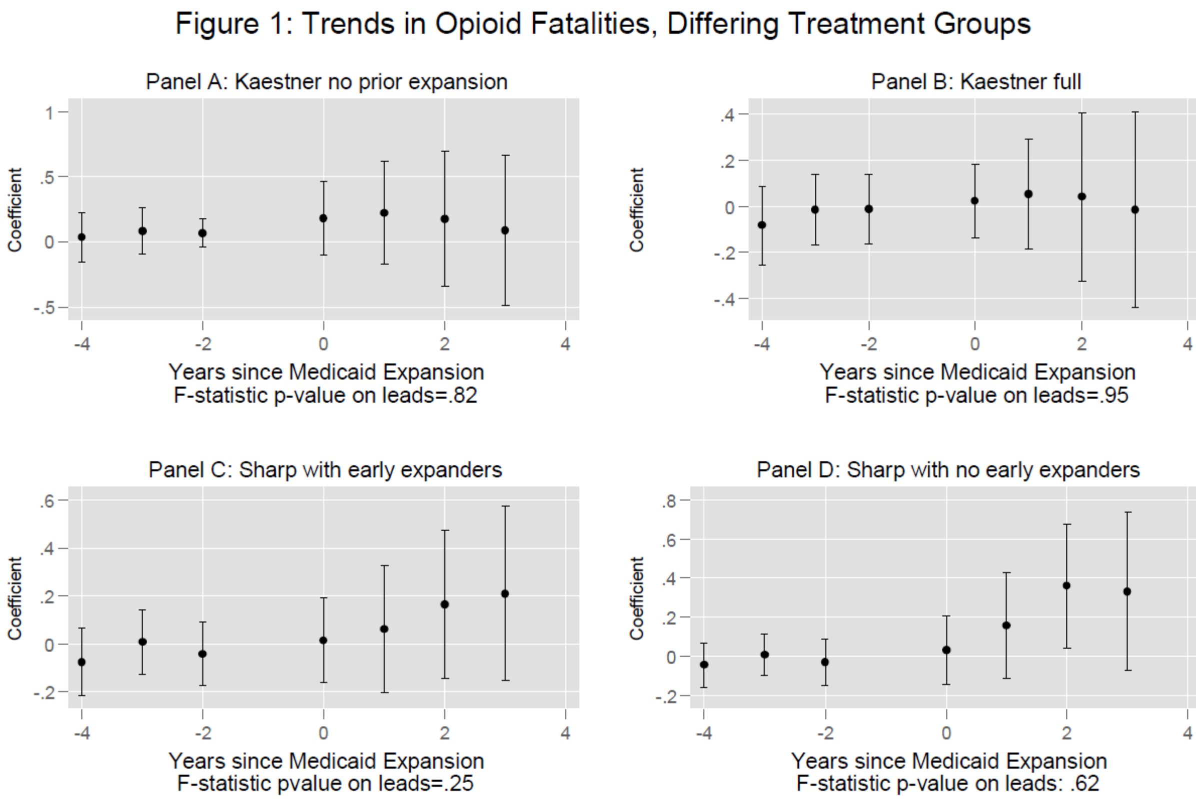
4. The main differences I discovered between Averett et al (2019) and my estimates is connected with the choice to include time fixed effects (Figure H5).

While I disagree with Averett's conclusions, I do concede that if we believed that Kaestner et al (2017)'s "full" variation were *strongly* preferable to every other treatment contrast, the evidence for an effect would become weaker (as in panel d of Figure H5).

Table H1: This Paper Compared to Averett et al. (2019)

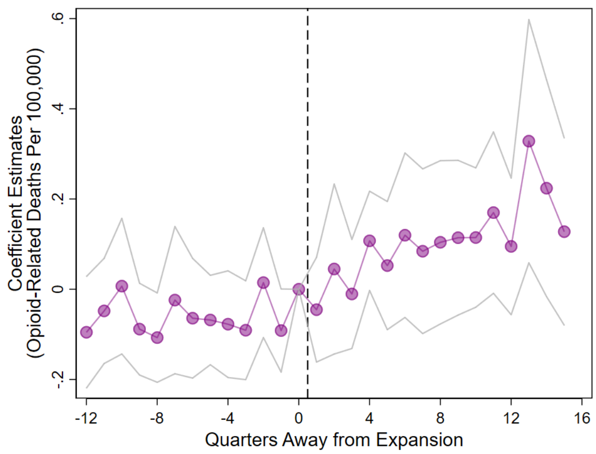
	Choice			
	This Paper	Averett et al.	Replication Success	Does Difference Drive Results?
Sample:	2012-2016	2010-2017	Yes	No
Level:	County-Quarter	State-Year	Yes	No
Outcome:	Rate	Log(Rate)	Yes	No
Opioids:	Heroin	No Heroin	Yes	No
Treatment:	All Variation	Kaestern/Sharp	Partial	Partially
Controls:	None	Covars & Confounders	No	Yes

Figure H1: Comparison to Averett et al 2019: Averett estimates

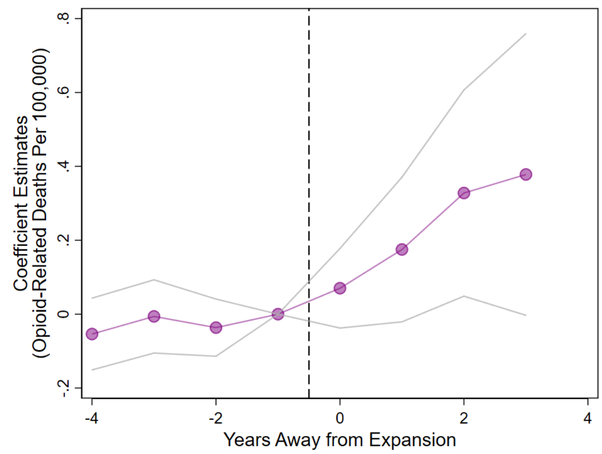


Note: Figure shows the coefficient estimates and 95% confidence interval reported by Averett et al. (2019)

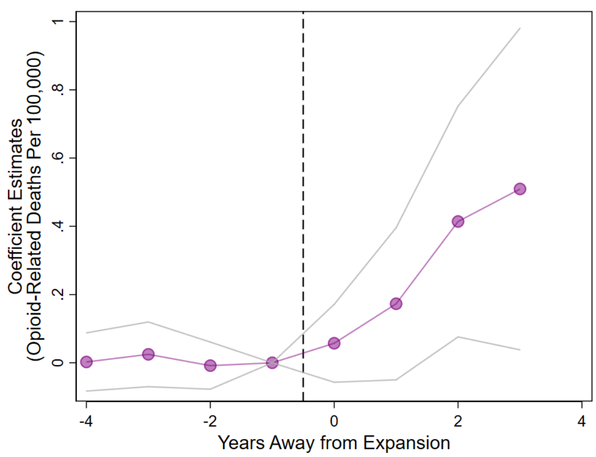
Figure H2: Comparison to Averett et al. (2019): Contrast Levels, Heroin, Logs



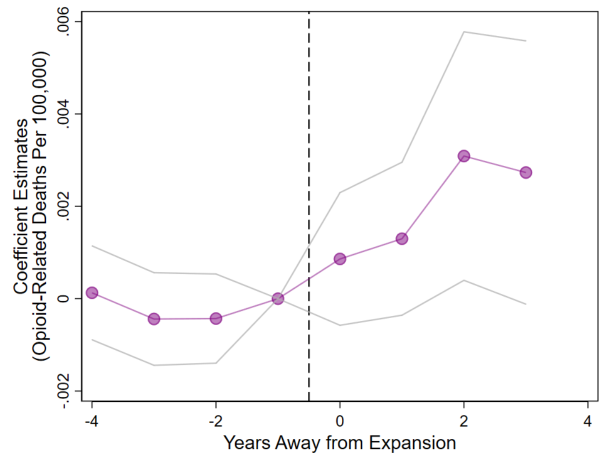
(a) County-Quarter; 0=base quarter; Heroin



(b) State-Year; -1=base-year; Heroin



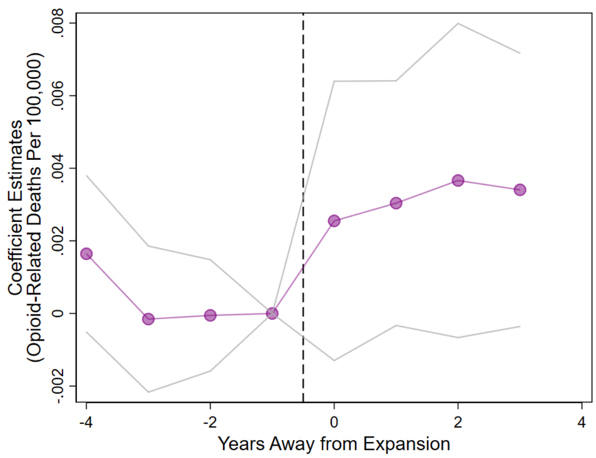
(c) State-Year; -1=base-year; No Heroin



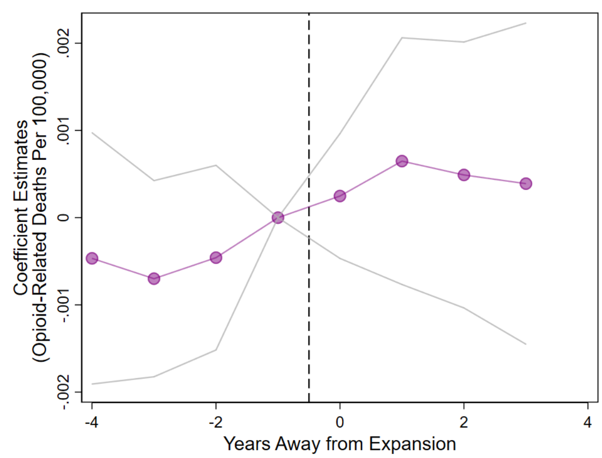
(d) State-Year; -1=base-year; No Heroin; Log

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths or their log as the outcome variable. All panels use the full expansion variation, rather than Kastner et al and Sharp more limited variation. Panel a repeats main specs from this paper using Averett's sample from 2010-2017. Date are at the county-quarter level. As in the main paper, "0" indicates the base quarter. Panel b collapses the data to the state-year level, following Averett. I follow Averett in assigning "0" to be the year of expansion and "-1" to be the base year. Panel c repeats panel b but follows Averett in excluding deaths attributed to Heroin from counting as opioid-related deaths. Panel d repeats panel c, but follows Averett in using the log of the opioid-death rate as the outcome variable. The source is always CDC Individual-Level Mortality Files (2010-2017).

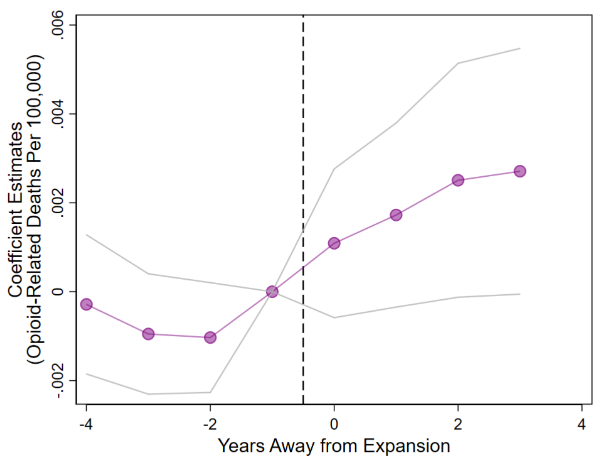
Figure H3: Comparison to Averett et al. (2019): Different Treatment Contrasts, Logs



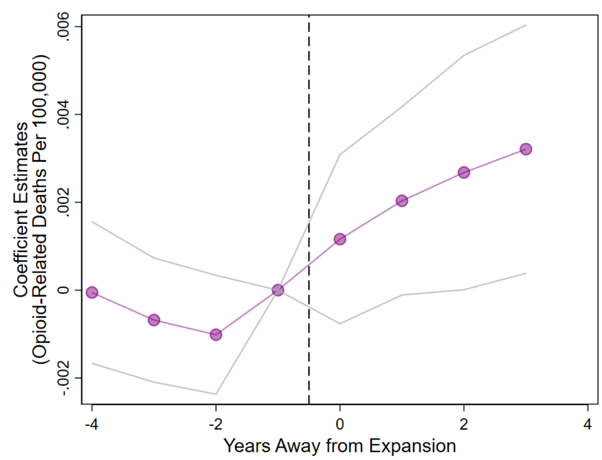
(a) Kaestner no prior; No Heroin; Logs



(b) Kaestner full; No Heroin; Logs



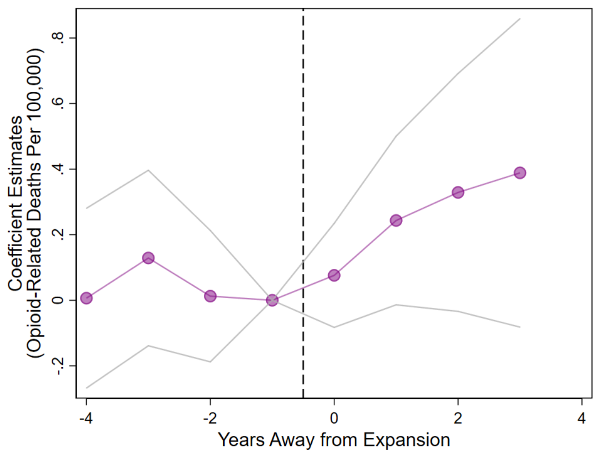
(c) Sharp with early; No Heroin; Logs



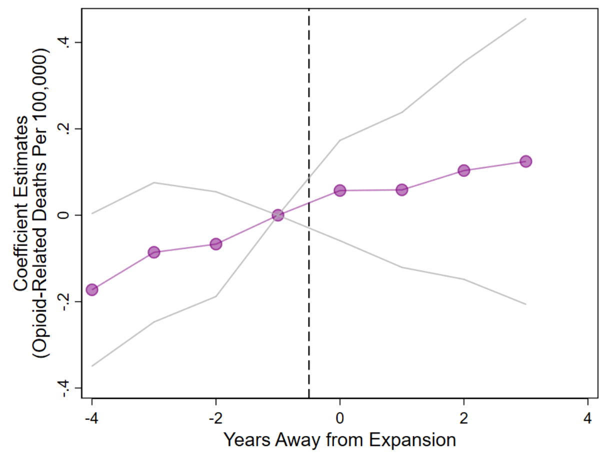
(d) Sharp w/o early; No Heroin; Logs

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Each panel shows separate treatment groups, following the order in Averett et al's Figure H1 above. Date are at the state-year level, following Averett et al (2019). The source is always CDC Individual-Level Mortality Files (2010-2017).

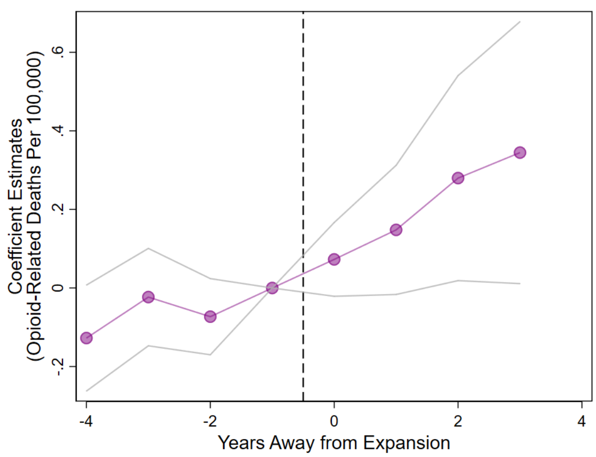
Figure H4: Comparison to Averett et al. (2019): Different Treatment Contrasts, Level Rates



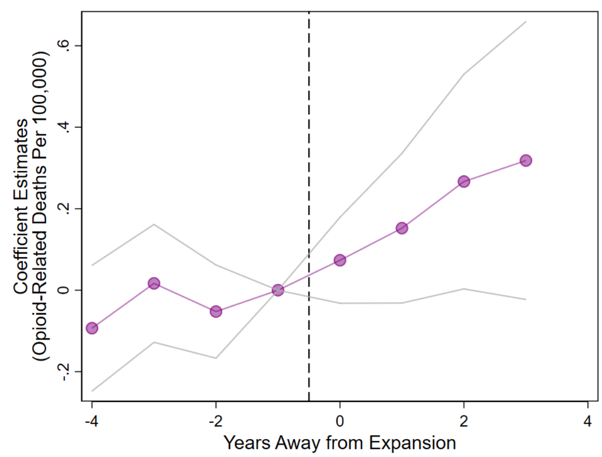
(a) Kaestner no prior; Heroin



(b) Kaestner full; Heroin



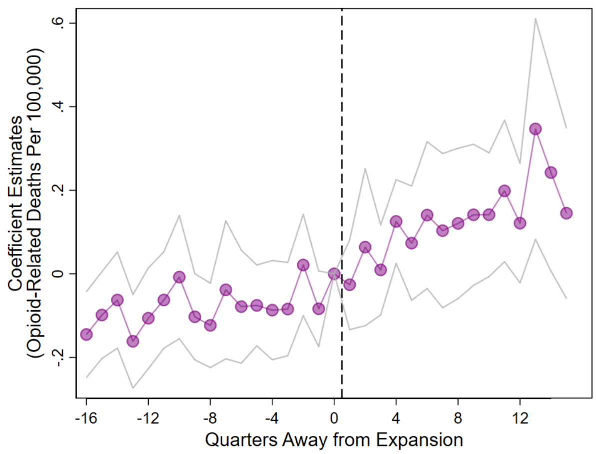
(c) Sharp w early; Heroin



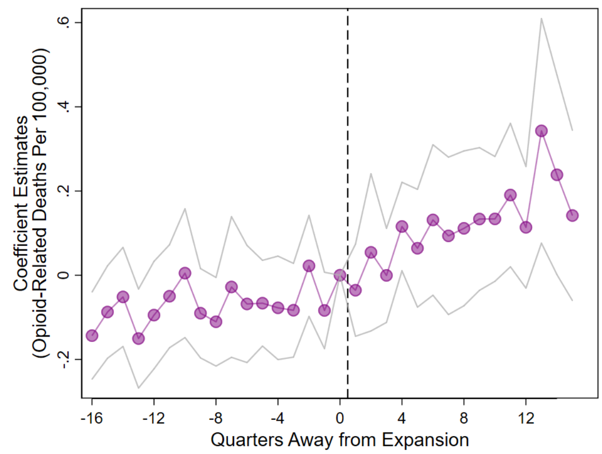
(d) Sharp w/o early; Heroin

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Each panel shows separate treatment groups, following the order in Averett et al's Figure H1 above. Date are at the state-year level, following Averett et al (2019). The source is always CDC Individual-Level Mortality Files (2010-2017).

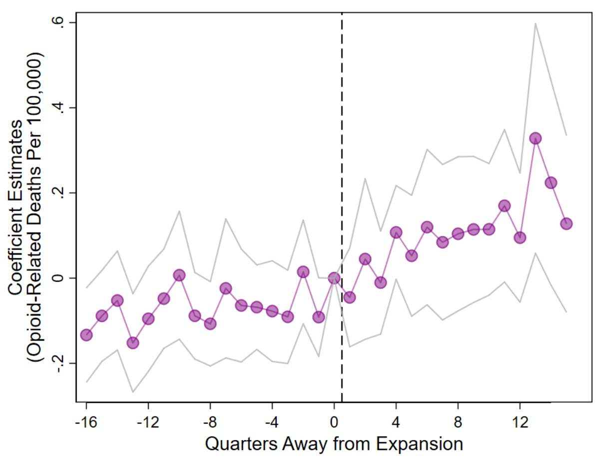
Figure H5: Comparison to Averett et al. (2019): Control Variables, EITC Variable, Year FE's



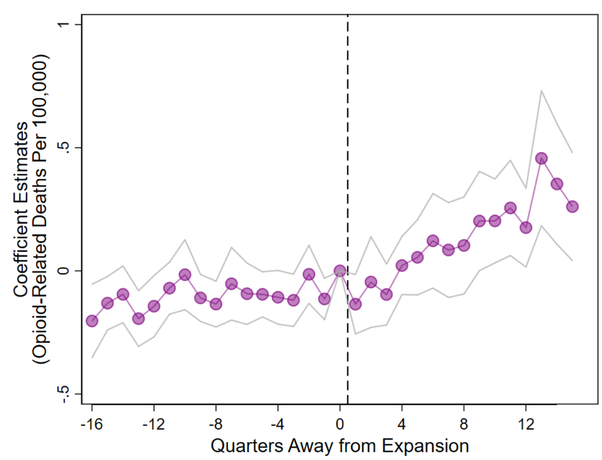
(a) Controls (NO EITC)



(b) Controls (NO EITC); Marijuana



(c) Controls (NO EITC); Marijuana, PDMPs



(d) Controls (WITH EITC); Marijuana, PDMPs, NO Year FEs

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 with rates of opioid-related deaths as the outcome variable. Data are always at the county-quarter level using the 2010-2017 sample. Panel a repeats main specs but follows Averett in including a set of control variables sourced from UKCPSR. I exclude the measure of EITC generosity, since it is collinear with year fixed effects. As in the main paper, "0" indicates the base quarter. Panel b repeats panel a but adds Averett's control variable for whether a state legalized marijuana (recreational or medical) Panel c repeats panel b but also includes variation in PDMP legislation from Jena et al. (2014) Panel d repeats panel c, but includes the EITC variable while dropping the year fixed effects. The source is CDC Individual-Level Mortality Files (2010-2017).

Data Appendix

To determine which specific NDC drug product codes count as opioids in analyzing the CMS state-drug utilization data, I follow Jena et al. (2014) in including the following products. Often the products involve combinations of opioids with other well known over the counter painkillers such as Acetaminophen or Aspirin.

List of Product Names of Opioid Products:

Acetaminophen, Caffeine, Cihydrocodeine
Buprenorphine HCl
Buprenorphine HCl and Naloxone HCl Dihydrate
Butalbital, Acetaminophen, Caffeine, Codeine
Butalbital, Aspirin, Caffeine, Codeine
Butorphanol Tatrtrate
Fentanyl
Hydrocodone with Acetaminophen
Hydrocodone with Ibuprofen
Hydromorphone HCl
Meperidine HCl
Methadone HCl
Morphine Sulfate
Morphine-Naltrexone
Nalbuphine HCl
Oxycodone HCl
Oxycodone with Acetaminophen
Oxycodone with Aspirin
Oxycodone with Ibuprofen
Oxymorphone HCl
Pentazocine with Naloxone
Pentazocine with Acetaminophen
Propoxyphene HCl
Propoxyphene HCl with Acetaminohen
Propoxyphene Napsylate with Acetaminophen
Tapentadol HCl
Tramadol HCl
Tramadol HCl with Acetaminophen

Jena et al. (2014) find that the vast majority of prescriptions are for a small subset of the above products. The percentages gives are the percent of claims for opioids the authors find in medical claims taken from a 20% random sample of Medicare beneficiaries in 2010.

List of Often Prescribed Opioid Products:

Hydrocodone with acetaminophen (paracetamol) (42.9% of all claims)
Oxycodone with acetaminophen (11.6%)
Tramadol (11.9%)
Oxycodone (7.4%)
Morphine sulfate (4.5%)
Fentanyl (4.2%)