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# ESSAYS ON THE ROLE OF POLICIES IN MAJOR PUBLIC HEALTH ISSUES

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# ESSAYS ON THE ROLE OF POLICIES IN MAJOR PUBLIC HEALTH ISSUES

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the College of Business and Economics at the University of Kentucky

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ABSTRACT OF DISSERTATION

# ESSAYS ON THE ROLE OF POLICIES IN MAJOR PUBLIC HEALTH ISSUES

This dissertation explores the role of policy on health outcomes and behaviors that relate to major public health concerns. Essay 1 and Essay 2 investigate the effects of Medicaid expansions on substance-use outcomes. Essay 3 examines the impacts of school reopenings in Texas on COVID-19 and mobility outcomes.

Essay 1 studies the effect of the Affordable Care Act Medicaid expansion. Using State Drug Utilization Data 2011-2017, I find that the Medicaid expansion is associated with an increase of 40-60 Medicaid-paid opioid prescriptions per 1,000 people aged 19–64. However, the results suggest that post-expansion prescriptions are, on average, shorter or prescribed in lower doses. Analyses of commonly misused opioids show that hydrocodone is the most affected substance, which makes up more than 50 percent of all Medicaid-paid opioid prescriptions. I do not find evidence that the Medicaid expansion is associated with the fentanyl epidemic.

Essay 2 studies the impact of Medicaid expansions on discharge outcomes of substance-use-disorder treatment and racial disparities in treatment completion. Using data from the Substance Abuse and Mental Health Services Administration 2008-2018 and event-study analysis, I do not find evidence that Medicaid expansions affect treatment completion rate in public-funded specialty treatment facilities. Analyses on racial subsamples, however, show some evidence of a negative effect on treatment completion among Black patients, while there is little effect among White and Hispanic patients.

Essay 3 examines the effect of fall 2020 school reopenings in Texas on county-level COVID-19 cases and fatalities. Analyses from hand-collected data imply that school reopenings led to at least 43,000 additional COVID-19 cases and 800 additional fatalities within the first two months. Results on mobility, using Safegraph data to provide evidence that spillovers to adults' behaviors contributed to these large effects. Median time spent outside the home on a typical weekday increased substantially in neighborhoods with large numbers of school-age children, suggesting a return to in-person work or increased outside-of-home leisure activities among parents.

KEYWORDS: Medicaid Expansion; Health Insurance; Substance Use; Racial Disparities; COVID-19

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AUGUST 8, 2021

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ESSAYS ON THE ROLE OF POLICIES IN MAJOR PUBLIC HEALTH ISSUES

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To my husband, Huy.

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# Chapter 1

# Effect of the ACA Medicaid Expansion on Prescription Opioid Utilization Patterns

## 1.1 Introduction

Opioid abuse is a significant public health concern in the United States. According to the National Institute of Drug Abuse (NIDA), in 2018, 128 people in the United States died from an opioid overdose every day (NIDA, 2020). The epidemic started in the late 1990s with increasing prescription opioid overdose deaths and led to rapid increases in deaths involving heroin, starting in 2010, and synthetic opioids, starting in 2013. This opioid crisis not only creates pressure on public health but also impacts social and economic welfare. Florence et al. (2016) estimate a cost of $78.5 billion for prescription opioid abuse in 2013, which includes healthcare, addiction treatment, productivity loss, and legal enforcement.

The medical use of prescription opioids as an analgesic for acute and post-surgical pain is not of serious concern. However, prolonged opioid use can lead to drug dependence and addiction. Moreover, the ready availability of prescription opioids is a primary factor leading to the initiation of non-medical use, and the risk of using prescription opioids for non-medical purposes is not limited to the individuals for whom the drugs are prescribed. More than 70 percent of non-medical opioid users obtained the drugs from a friend or relative (Jones et al., 2014; Lankenau et al., 2012). The Centers for Disease Control and Prevention (CDC) estimate that 40 percent of deaths from opioid overdose are related to a prescription opioid (CDC, 2020). Prescription opioids and non-prescription opioids, such as heroin, are pharmacologically similar, which suggests a higher probability of heroin initiation among prescription opioid users compared to non-users. There have been numerous studies regarding the relationship between prescription opioids and heroin consumption (Jones, 2013; Compton et al. 2016). Although most studies are descriptive, they find a consistent, positive association between the use of these two types of opioids. Jones (2013) finds that over 80 percent of heroin users used prescription opioids before trying heroin.

Since the rise of the opioid epidemic, there have been substantial efforts to manage prescription opioid use. Much attention has been paid to regulations and programs designed to control prescription opioid use, including prescription administering programs such as the state Prescription Drug Monitoring Programs (PDMPs), pain management clinic laws, and supply-side strategies such as adding abuse-deterrent controlled-release properties to drug formulas (e.g., OxyContin and Butrans). Health insurance coverage is another important channel that could affect prescription opioid use. The literature has long shown that health insurance increases the use of health services (Currie & Gruber, 1996a & b; Card, Dobkin, & Maestas, 2008) and prescription drugs (Duggan & Morton, 2010; Ketcham & Simon, 2008; Lichtenberg & Sun, 2007).

This paper starts by studying the link between an increase in health insurance access through the Affordable Care Act (ACA) Medicaid expansion and prescription opioid utilization among the Medicaid population. The ACA has been the largest-scale health care reform in the United States since the introduction of Medicaid and Medicare in the 1960s. Since its implementation, the U.S. has experienced a nationwide increase in health insurance coverage. Studies that focus on the earlier years after the ACA find consistent increases in health insurance coverage in both expansion and non–expansion states. Initially, the Medicaid expansion was intended to occur nationwide. However, a Supreme Court decision in 2012 allowed states to adopt the expansions optionally. At the time of the ACA's implementation (January 1, 2014), 25 states decided to expand Medicaid.

Empirically, assessing the relationship between health insurance and any type of health care utilization is challenging due to potential reverse causality, and prescription opioid use is no exception. On the one hand, having health insurance coverage increases access to opioid medication. On the other hand, individuals who demand prescription opioids are more likely to seek health insurance. The second empirical challenge that comes with estimating the effects of health insurance on prescription opioid use is omitted variable bias. Individuals with bad health are likely to have a higher demand for both health insurance and painkillers. The ACA's Medicaid expansion aims to target disadvantaged populations, which have less insurance coverage and, in general, have worse health. However, the fact that not every state adopted the Medicaid expansions provides plausibly exogenous variation in Medicaid eligibility for individuals in expansion and non-expansion states, which can mitigate the issues mentioned above.

This essay contributes to the existing literature in several ways. First, the paper provides a comprehensive analysis of the causal relationship between the Medicaid expansion, prescriptions (measured by per-population prescriptions and MMEs), and Medicaid spending on prescription opioids that were reimbursed by Medicaid. Research looking at the association between the Medicaid expansion and opioid prescriptions mainly focuses on per-enrollee prescriptions. However, it is necessary to investigate both measures, because changes in per-enrollee utilization could only capture the differences between the pre-expansion and post-expansion Medicaid populations, as the number of enrollees also increases in expansion states. Per population estimates are more relevant when compared to total opioid prescriptions across all payers or to other government-funded programs.

This essay also contributes by being the first to examine the heterogeneity among substances and to provide an implication of the Medicaid expansion's role in the on-going fentanyl and synthetic opioid epidemic. Identifying the changes among substances is important due to the fact that these substances differ in potencies and thus in prescribing patterns (i.e. strong opioids are not prescribed to opioid-naive patients). Moreover, opioid substances also differ in their mechanism of action, the way each substance interacts with opioid receptors, which can lead to discrepancies in their ability to induce addiction, as described by Stoeber et al. (2018). Thus, examining the heterogeneity among opioids can reveal if the change in utilization mainly reflects the change in the Medicaid-eligible population or if it suggests signs of opioid misuse.

Using combined data from the State Drug Utilization Data (SDUD) and the National Drug Code Directory, I employ a generalized difference-in-differences (GDD) framework to estimate the effects of the Medicaid expansion on Medicaid-paid prescription opioid use. In general, I find that the Medicaid expansion is associated with an increase of opioid prescriptions paid by Medicaid per 1,000 adults under 65. However, the average post-expansion Medicaid enrollee is not necessarily using more opioids compared to the average enrollee in the pre-expansion population, as results for per-enrollee opioid prescriptions are not robust.

Next, I look at separate samples that contain all morphine, hydrocodone, oxycodone, and fentanyl prescriptions. Among these substances, hydrocodone is shown to have the largest increase in the number of prescriptions and MMEs. The results translate to about 32 prescriptions and 11,700 MMEs for every 1,000 people ages 19–64 (1.83 standard deviations). The effects on fentanyl, a highly potent synthetic opioid, are relatively small, about 1.6 prescriptions per 1,000 (0.5 standard deviations). Therefore, this increase in fentanyl prescriptions through Medicaid is not likely to have directly contributed to the fentanyl epidemic.

## 1.2 Background and Literature of the ACA

The ACA was implemented in 2014, with a goal to achieve nearly universal health insurance coverage in the United States. The ACA consists of three main parts that are commonly known as a "three-legged stool." The first leg consists of regulations that guarantee coverage for individuals. Insurance companies are required to base their premiums on community rating and issue coverage regardless of pre-existing health conditions. The second leg, or the individual mandate, includes regulations to prevent the potential death spiral[1] that the first component could cause. The third leg addresses concerns about affordability, which consists of subsidies and the Medicaid expansions. Individuals with incomes between 100 and 400 percent of the FPL, who are not eligible for Medicaid or employer-sponsored insurance, would qualify for premium subsidies. In addition to this nationwide subsidy program, states have the option to expand their Medicaid programs to individuals with incomes below 138 percent FPL at almost zero cost.[2]

### 1.2.1 Effects of the ACA Medicaid Expansion on Healthcare and Health-Related Outcomes

Since the ACA's implementation, the literature on this healthcare reform has been rapidly growing among researchers and policymakers. Mazurenko et al. (2018), Antonisse et al. (2018), and Gruber & Sommers (2019) provide systematic reviews of the ACA-related studies. Studies generally find that the ACA, especially the Medicaid expansion, is associated with an increase in health insurance coverage (Sommers et al., 2014; Sommers et al., 2015; Buchmueller et al., 2016; Wherry

[1] When premiums are based on community rating, people with worse health conditions are more likely to sign up than healthy people. As a result, premiums would eventually rise, which would further discourage healthy people to sign up, and so on.

[2] To learn more about the institutional details of the ACA, see Courtemanche et al. (2017) and Frean et al. (2017).

and Miller, 2016; Courtemanche et al., 2017-2019; Duggan, Goda, & Jackson, 2017; Frean et al., 2017; Kaestner et al., 2017).

There is mixed evidence of changes in health service utilization, including preventive care (Sabik, Tarazi & Bradley, 2015; Wherry and Miller, 2016; Simon, Soni, & Cawley, 2017; Courtemanche et al., 2017), hospital use (Akosa Antwi et al., 2015; Admon et al., 2019; Anderson et al., 2016), emergency services (Nikpay et al., 2017; Sabik et al., 2017; Pines et al., 2016; Courtemanche, Friedson, & Rees, 2019). There are also a vast number of studies investigating the association between the Medicaid expansion and specialized services (Singhal et al., 2017; Soni et al., 2018).

Studies exploring the link between the components of the ACA and prescription drug use document a general increase. Ghosh, Simon, & Sommers (2019) and Mahendraratnam et al. (2017) find that after the Medicaid expansion, aggregate prescription drug use increased about 17–19 percent. Mahendraratnam et al. (2017) also find that after one year of the expansion, Medicaid spending increased more than one-third in expansion states. Amuedo-Dorantes & Yaya (2016) also find an increase in prescription drug access due to the ACA's expansion of dependent coverage.

Higher utilization, however, does not necessarily translate to better health. Studies that examine the impact of the ACA on self-assessed health find mixed results. While a number of studies document improved self-reported health (Sommers et al., 2015; Sommers et al., 2016; Simon et al., 2017; Cawley et al., 2018), other papers find mixed results and insignificant changes in health measures (Courtemanche et al., 2018; Wherry and Miller, 2016). Most studies do not find an association between the ACA and risky behaviors or the use of risky-behavior-related products, including alcohol and tobacco (Courtemanche et al., 2018 & 2019; Cawley et al., 2018; Cotti et al., 2019). However, Maclean et al. (2019) find evidence that the Medicaid expansions provide higher access to smoking cessation medications.

### 1.2.2 The Role of Health Insurance in the Opioid Epidemic

The association between health insurance and opioid analgesic use also poses an important question yet has not been fully understood. Having health coverage provides individuals with acute pain issues, such as post-surgical pain and late-stage cancer pain, with the necessary pain relievers. However, having access to opioid analgesics at a lower cost can lead to moral hazard among individuals who do not need such medication. An increase in demand for opioids can also lead to spillovers. Powell et al. (2020) examine the impact of Medicare Part D on opioid supply and find an increase in opioid abuse treatment admissions and opioid-related mortality among the Medicare-ineligible population. Soni (2018) documents a substitution effect between over-the-counter pain relievers and prescription pain relievers among the Medicare-eligible population. The structure of a health insurance program could also affect opioid analgesic utilization. Baker et al. (2018) find that enrollment in Medicare Advantage reduces the likelihood that beneficiaries fill an opioid prescription.

Several studies focus on the impact of ACA Medicaid expansion on opioid-related outcomes. The current literature focuses on the two channels by which the Medicaid expansion can affect the opioid epidemic. The first channel relies on the theory that, by increasing access to drug-dependence and opioid-addiction treatments to the target population, the Medicaid expansion could lower the number of opioid-dependent individuals and reduce opioid-involved deaths. Most studies in this area find that the Medicaid expansion is associated with higher utilization of treatment services and admissions for opioid use disorder (Wen et al., 2017; McKenna, 2017; Andrews et al., 2018; Meinhofer & Witman, 2018; Sharp et al., 2018; McCarty et al., 2019; Maclean & Saloner, 2019). Meinhofer & Witman (2018) also find no evidence that the increase in treatment admissions from Medicaid beneficiaries is crowding out of other types of health insurance. Feder et al. (2017), however, find no evidence of changes in treatment service use among people with heroin use disorder, despite higher coverage. Olfson et al. (2018) find no changes in treatment service use among the low-income population.

The second channel focuses on the fact that Medicaid has increased access to care. Given that the eligible population is less healthy and more prone to conditions that require analgesic medications, the Medicaid expansion could be efficient in serving the target population. However, opioid analgesics can initiate addiction and abusive behaviors among prescribed and non-prescribed users. Results have been mixed among these studies. Sharp et al. (2018) find a negative but statistically insignificant association between the Medicaid expansion and per-enrollee number of prescriptions, while Cher et al. (2019) find a positive but insignificant impact on the same measure. Saloner et al. (2018) also find an increase in the number of opioid prescriptions paid by Medicaid, using data from California, Maryland, Washington, Florida, and Georgia. Given that the two channels discussed above could happen concurrently and create opposite effects on the level of opioid usage, estimates of the association between the ACA and opioid use represent the net effect of the two channels.

Studies that investigate the effects of the Medicaid expansion on opioid-related fatalities also find mixed results. McInerney (2017) and Kravitz-Wirtz et al. (2020) find a reduction in opioid-related deaths and death rates associated with the Medicaid expansion. However, the association between the Medicaid expansion and opioid-related deaths is unclear, according to Abouk et al. (2019) and Averett, Smith, & Wang (2019). The mixed results and the dynamic nature of opioid-related issues necessitate a thorough analysis of the utilization patterns of prescription opioids under the Medicaid expansion.

## 1.3 Data

### 1.3.1 Measuring Medicaid Opioid Utilization

The main data come from the State Drug Utilization Data (SDUD) collected by the Centers for Medicare and Medicaid Services (CMS). Since the start of the Medicaid Drug Rebate Program (MDRP) in 1991, the CMS has required states to submit records of Medicaid prescription drug utilization. Under the MDRP, participating drug manufacturers are required to provide rebates to the states and the federal government in exchange for coverage by Medicaid. The SDUD records quarterly information of state-reported prescription drugs that are reimbursed by Medicaid, including the number of prescriptions and total reimbursements by the covered National Drug Codes (NDC). Reimbursement data contain the amounts that Medicaid paid to providers, which does not account for manufacturer rebates.[3] The data used in this essay span the years 2011–2017.

I do not include data prior to 2011 for two reasons. First, the ACA Young Adult Coverage extension implemented in 2010 has been shown to have impacted opioid-related outcomes among the population ages 18–25 (Wettstein, 2019). Excluding data from 2010 and earlier could avoid capturing the short-term shocks in prescription opioid use that could have come from this extension. Second, it was not until the enactment of the ACA in March 2010 that the CMS required states to report prescriptions for Medicaid patients who are enrolled through managed care organizations (MCO). Prior to this time, drug manufacturers were only obligated to provide rebates to states for prescriptions that were purchased through

[3] The SDUD does not include drugs from state-only programs and other federal programs such as the 340B Drug Pricing Program.

the fee-for-service (FFS) scheme. Missing data of MCO prescriptions in 2010 and earlier could be an estimation threat because states are differential in terms of FFS – MCO structure. Without utilization from MCOs, reported data would incorrectly present the actual number of Medicaid prescriptions in each state and create bias.

To compile utilization data for prescription opioids, I create a list of prescription opioid NDCs by matching brand-name and generic opioid drug names with all corresponding NDCs from the FDA's National Drug Code Directory. To account for new drugs that enter the CMS system, I match the drug names with the Medicaid Opioid Drug lists provided by the CMS. [4] The goal is to include all opioid substances and avoid the potential bias coming from the heterogeneity across substances. Table A.3 presents a list of common prescription opioids. Next, the opioid NDCs are linked to the SDUD to create a Medicaid utilization dataset for opioid prescriptions only. I exclude medications that are commonly prescribed for addiction treatments such as buprenorphine, naltrexone, and naloxone. Data are aggregated by state and year. The SDUD has two limitations. First, NDCs with fewer than 11 prescriptions in a state-quarter cell are suppressed. Second, the reported quarter may represent the time the NDC was dispensed or paid by the state rather than the time of actual utilization. Aggregating utilization by year could provide a more precise measurement.

For each outcome category of Medicaid opioid utilization (number of prescriptions and Medicaid reimbursements), I construct two measures: per population aged 19-64, and per-enrollee using state population data from the US Census Bureau and Medicaid enrollment data from the CMS Medicaid Budget and Expen-

[4] https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/Medicare-Provider-Charge-Data/OpioidMap_Medicaid_State

diture System (MBES) and the Henry J. Kaiser Family Foundation (KFF).[5] Data on Medicaid reimbursements are adjusted to 2011 dollars.

### 1.3.2 Treatment Variable

States receive a treatment status from the year that they implemented a Medicaid expansion, either through the ACA or through their own programs. Accordingly, treatment states can be divided into three groups. The first group consists of states that expanded in 01/2014 and did not have prior expansions. The second group consists of states that expanded Medicaid prior to 2014, under Section 1115 waivers (California, Connecticut, Delaware, DC, Minnesota, New Jersey, and Washington) and/or had their own expansions with similar criteria to the ACA Medicaid expansions (Delaware, Massachusetts, New York, Vermont, and DC). These states are defined to have a treatment status before 2014. States that already have similar criteria to the ACA Medicaid expansions are considered "early" expansion states. The third group consists of states that expanded after 01/01/2014. These states are Alaska, Indiana, Louisiana, Michigan, Montana, New Hampshire, Pennsylvania, Virginia, Maine, Idaho, Utah, and Nebraska.[6] At the time of this essay, 37 states including DC have adopted the expansion (KFF 2020).[7]

### 1.3.3 Control Variables

Numerous factors can affect prescription opioid use. Therefore, when estimating the effects of the Medicaid expansion on prescription opioids, it is crucial to account for potential confounders that can affect state-level opioid utilization. The paper's main model includes two sets of controls. The first set includes variables

[5]The MBES did not start reporting Medicaid enrollment data until January 2014.
[6]See table A.1 for details of states' expansion years.
[7]States expanded after 2017, Virginia, Maine, Idaho, Utah, and Nebraska, are treated as non-expansion states in this essay.

that control for state demographic characteristics (such as the share of individuals that is white and the share of the female population), using data from the ACS, and economic conditions, including state poverty rate of the population ages 19–64 (KFF), unemployment rate, and minimum wage (UKCPR Welfare Data).

The second set of covariates controls for major state prescription opioid regulations. Since the rise of opioid use, there has been substantial public effort to control the epidemic. State opioid policies have been shown to have some impact on prescription opioid use, which can create bias in the estimates if they are correlated with these states' decision to expand Medicaid. I include an indicator for states' adaptation of the Prescription Drug Monitoring Programs-mandate (PDMP-mandate)[8] collected from the Prescription Drug Abuse Policy System (PDAPS). Although state PDMP consist of multiple components, the PDMP-mandate has been shown to impact prescription opioid use, compared to other parts of the regulation (Wen et al., 2017).[9] Other regulations include the Pain Management Clinic Laws[10] and the Prescription Drug Time and Dosage Limit Laws. Data on states' implementation of these programs are collected from the PDMP Training and Technical Assistant Center. I also include state recreational marijuana law status, following Meinhofer and Witman (2018). Table A.2 reports the effective time of these regulations.

### 1.4 Methodology

The empirical strategy of this essay aims to identify the effect of the Medicaid expansions on Medicaid utilization and reimbursements. The variation in states'

[8]PDMP are state-implemented programs that record patients' prescription history to monitor potential fraudulent behaviors such as "doctor shopping," a behavior marked when a patient obtains prescriptions from five or more prescribers (Buchmueller and Carey, 2018).

[9]A state is defined as having the PDMP-mandate when it requires a healthcare professional to check with the system before prescribing or dispensing opioids.

[10]Pain clinic laws refer to regulations on pain management clinics such as facility and staffing certification or supply limit regulations (PDAPS.org).

decisions to expand Medicaid provides a natural identification for a difference-in-differences (DD) design. However, not all states that expanded Medicaid did so in January 2014. Previous literature has taken this variation into account in many ways, including dropping the early expansion states or assuming that the impact coming from these states is negligible. To both estimate the effects coming from the early and late expansion states and maintain the sample size, in the baseline model, I follow a generalized difference-in-differences framework described in equation 1.1. I later include estimation results of the main model but exclude states with their own pre-ACA expansions as a robustness check.

$$Y_{st} = \alpha + \beta Medicaid_{st} + \gamma X_{st} + \delta P_{st} + \eta_s + \lambda_t + \varepsilon_{st} \tag{1.1}$$

The outcome variable of interest, $Y_{st}$, is a measure of Medicaid prescription opioid utilization or reimbursement in state $s$ and year $t$. $Medicaid_{st}$ is the treatment indicator, which equals 1 if period $t$ is the year state $s$ expanded Medicaid and after. $X_{st}$ is a vector of covariates that control for time-varying state-specific demographic and economic conditions that could have influenced both state opioid utilization and the decision to expand Medicaid, and $P_{st}$ is a vector of opioid-related policies, as described in Section 1.3.3. The model also includes state and year fixed effects, $\eta_s$ and $\lambda_t$, to account for heterogeneity across states and year-specific unobservables, respectively, and $\varepsilon_{ist}$ is the error term. All regressions are weighted by state population ages 19–64. Standard errors are adjusted for heteroskedasticity and clustered by state. I also include results from unweighted regressions in the robustness check section.

The coefficient of interest, $\beta$, measures the differential change in Medicaid opioid utilization or reimbursements between control and treatment states. For per-enrollee outcomes, $\beta$ represents the average change in utilization between the initial Medicaid population and the new Medicaid population. For per-population outcomes, $\beta$ represents the average change in Medicaid-reimbursed opioid prescriptions among the young adult (ages 19-64) population.

## 1.5 Results

### 1.5.1 Summary Statistics

Table 1.1 presents the summary statistics in 2013, the pre-treatment period for most states. The average per population (in thousands) opioid utilization in a state is about 122 prescriptions, which corresponds with an average of approximately $4,800 in Medicaid spending and over 111,000 MMEs. Oxycodone accounts for the largest share: about 30 percent of all opioid prescriptions. Before the ACA, the average uninsured rate was approximately 20 percent. On average, demographic characteristics were similar between the control and treatment states. However, economic and political characteristics were different between the two groups. Medicaid expansion states have a higher minimum wage but also a higher unemployment rate. Sixty percent of house and senate seats in expansion states are Democratic, compared to about 33 percent in non-expansion states. Expansion states also differ from non-expansion states in terms of opioid policies. For example, by 2013, 24 percent of expansion states implemented the PDMP-mandate, compared to 8 percent of the non-expansion states.

Figures 1.1 and 1.2 show total opioid prescriptions (per 1,000 people ages 19–64) by state in 2013 and 2014, respectively. For ease of comparison, the scale in figure 1.2 is kept similar to figure 1.1. Between the two years, prescription rates increased in most states, including states that did not expand in 2014.

To further access the trends between expansion and non-expansion states, panels a and b of figure 1.3 present the average opioid utilization and reimbursement rate (per 1,000 people ages 19–64) in expansion and non-expansion states. The

SDUD provides utilization data at the end of each period; therefore, the time of the expansion (January 1, 2014) corresponds with the year 2013 on the graphs, instead of 2014. The data show a slightly increasing pre-trend in both expansion and non-expansion states (figure 1.3 - panel a.) After the ACA's implementation, there is a sharp increase in the overall trend, but Medicaid expansion states experience a larger change. The fact that there is an increase in utilization even in non-expansion states can be potentially explained by the "welcome mat" effect, or the "woodwork effect," described in Frean et al. (2017).[11] The pre-trends of Medicaid spending in figure 1.3 - panel b, on the other hand, do not look similar. However, the graphs present the time series trends rather than the identification assumption that the model relies on, which are conditional on the covariates. I later examine the trends using an event study design and control for pre-trends in a robustness check.

It is also noticeable that the trends of prescriptions and reimbursement amounts do not move in tandem, which suggests that underlying factors that influence Medicaid spending can be different from those affecting the number of opioid prescriptions. These discrepancies are not unexpected because, although Medicaid reimbursements are calculated by average wholesale price rather than retail price, there is evidence that manufacturers could respond to policies (Dranove et. al, 2017). Moreover, drug prices can change due to new drug entrance, including generic drugs.

Medicaid opioid prescriptions and reimbursement seem to peak in 2015 and start to decrease in 2016 and 2017. These changes could reflect a nationwide

[11] The Medicaid expansion can affect both Medicaid newly- and previously-eligible individuals. For individuals that are eligible for Medicaid coverage before the expansion, there are no official changes to their eligibility for Medicaid after 2014. However, a large-scale health care reform such as the ACA can increase awareness among eligible non-enrollees. Also, there will be a reduction in social stigma with receiving Medicaid because the expansion increases income eligibility (in most states) and the Medicaid population. For this reason, the "welcome-mat" effects are present in both expansion and non-expansion states.

awareness and effort to manage prescription opioid use. Another possible explanation is that in July 2016, the Comprehensive Addiction and Recovery Act (CARA) was signed into law. This is the most comprehensive addiction policy in the US within 40 years.[12] Its main goal is to increase prevention and recovery support to localities, especially areas that are more affected by addiction issues. Along with other concurring state and local policies, the CARA might have played a role in the downward trend of opioid prescriptions.

### 1.5.2 Total Opioid Utilization

Table 1.2 - panel A presents the baseline model estimates of the effects of the Medicaid expansion on opioid prescriptions paid by Medicaid. Measures of the dependent variable include number of prescriptions per population age 19–64 and per enrollee (both in thousands). All specifications observe a positive and statistically significant association between the Medicaid expansion and opioid use. Column 1 reports estimates the specification that includes state and year fixed effects only. Column 2 includes state economic and demographic controls. The preferred specification is reported in Column 3, which includes state characteristics and opioid-related policy variables. The estimates do not seem to have drastic changes across specifications. However, including state characteristics and opioid-related policies seems to slightly increase the magnitude and precision of the estimates. Results from the preferred specification show an increase of 60.3 Medicaid-paid opioid prescriptions per 1,000 people and 77.5 prescriptions per 1,000 enrollees when states expand Medicaid, about 49.5 percent and 17.1 percent from the 2013 full sample means, respectively.

Table 1.2 - panel B presents the results for Medicaid reimbursements. There is evidence that Medicaid spending also increases due to the expansion. Spending

[12] For further details of the CARA, see `https://www.congress.gov/bill/114th-congress/senate-bill/524`

per 1,000 people (age 19–64) increases by $973.5, which is 20.37 percent of the 2013 full-sample mean. However, estimates for spending per 1,000 enrollees become small and statistically insignificant. One potential explanation is that prescriptions for newly enrolled beneficiaries contain lower dosages compared to existing beneficiaries. Scaling the estimates by the number of enrollees would decrease the magnitude. Another explanation is that there are other mechanisms from the Medicaid expansion, aside from prescription utilization, that could influence reimbursement amounts, such as manufacturer response and managed care organizations (MCOs). In one of the robustness analyses, I control for the ratio of prescriptions administered by MCOs to evaluate the extent to which MCOs could influence opioid use and spending.

## 1.6 Extended Measures of Opioid Use

### 1.6.1 Morphine Milligram Equivalent Units

Next, I examine the results accounting for substance strength. Specifically, I calculate a new outcome variable that measures potency in "units," using the CDC's guide to obtain the "opioid oral morphine milligram equivalent conversion factors," or MME factors. The formula is described in equation 1.2.

$$MMEs = drug\ strength \times MME\ factors \times total\ units \tag{1.2}$$

where *MMEs* stands for morphine milligram equivalent units, *drug strength* is the unit strength of the substance associated with each NDC, *total units* is the number of units reimbursed through Medicaid. MMEs measure total morphine milligram equivalent units of opioid reimbursed by Medicaid. Ideally, researchers would also wish to observe daily dosage,[13] but due to data limitations, I could

[13]Daily dosage is measure by MMEs divided by the number of days prescribed.

only calculate potency based on substances and the number of reimbursed units. However, looking at opioid use using MMEs is important for two reasons. First, opioids (both prescription and illicit) differ not only by strength but also by substance. Higher-dosage and higher-potency substances increase the risk of addiction and overdose. Thus, it is useful to also understand the amount of opioid used in addition to the number of prescriptions. Second, if the change in the number of prescriptions is disproportionately distributed among lower-dosage prescriptions, the effect on prescriptions will be higher than the effects on potency units.

Table 1.2 - panel C presents the results of re-estimating the baseline model using the number of reimbursed MMEs as an outcome variable. The Medicaid expansion is associated with higher MMEs prescribed per 1,000 people. The preferred estimate in column 3 shows an increase of 26,070 MMEs when states expand Medicaid.[14] When scaling the number of MMEs by the number of beneficiaries, similar to the results for Medicaid spending, the coefficients become small and indistinguishable from zero. Again, if new enrollees are prescribed with less potent substances and/or for shorter periods, per-enrollee measures will become smaller than per-population measures. I explore this possibility in the next section.

### 1.6.2 Effects of the Medicaid Expansion by Common Opioids

Opioid potency varies across substances. Certain drugs are also more often involved in opioid abuse and death incidents than others. Among all prescription opioids, hydrocodone, oxycodone, morphine, and fentanyl are related to the most overdose deaths and abuse. According to the morphine milligram equivalent factor scale, morphine and hydrocodone are equally potent, with MME factors of 1; oxycodone has an MME factor of 1.5; and fentanyl has MME factors ranging

[14] Because the data does not contain prescription lengths, it is not possible to compare the estimates to a typical or unusual opioid dose.

from 0.13 to 7.2, depending on the form.[15] Fentanyl is an important substance to investigate due to not only its potency[16] but also the severe impact of illegal fentanyl starting in 2013.[17] Prescribed fentanyl can take the form of tablets/lozenges, liquid (oral or nasal spray), injections, and transdermal patches. The most commonly prescribed forms are transdermal patches (52.82%) and injections (47.15%). Injectible fentanyl, however, is more common in inpatient settings. This form of fentanyl is excluded in MME analyses due to unavailable MME factor. Fentanyl patches, especially in extended-release forms, can be dangerous if misused due to the high concentration. Although up to the time of this essay is written, most fentanyl-related incidents involve illicit fentanyl rather than prescription fentanyl, it is still essential to answer the question of whether the Medicaid expansion is associated with the fentanyl epidemic through prescription fentanyl.

There also has been some evidence that oxycodone has a higher misuse tendency compared to morphine and hydrocodone (Wightman et al., 2012). Accessing the heterogeneity in utilization among these drugs is relevant since the results can inform policymakers about areas that need more attention. To examine the heterogeneity, I re-estimate equation 1.1 separately, allowing the outcome variables to be measures of each drug above. Each drug category includes utilization data for both the corresponding brand-names and generics. Figures 1.4 to 1.7 (both a & b) present trends in the number of prescriptions and Medicaid reimbursements by commonly misused opioids.

Table 1.3 reports the estimates for commonly misused substances. Panel A presents the results of per-population measures, and panel B presents per-enrollee results. Columns (1) to (4) contain estimates for samples that include only pre-

[15] Fentanyl is measured in mcg, while others are measured in mg.

[16] Fentanyl is 50 to 100 times more potent than morphine (CDC 2020).

[17] From 2013 to 2017, the number of overdose deaths that involved fentanyl has increased the sharpest, compared to other opioids. In 2017, fentanyl accounted for 30,000 of 72,000 overdose deaths (NIDA, 2018), although most fentanyl-related deaths come from "street" fentanyl rather than prescription fentanyl.

scriptions of morphine, hydrocodone, oxycodone, and fentanyl. Column (5) lists results for other opioids for comparison. The effects of the Medicaid expansion vary across substances. In panel A, hydrocodone observes the largest impact, with an increase of 32 prescriptions per 1,000 people, which is almost 1.5 times the 2013 mean. The effects on oxycodone and fentanyl are both positive, but are smaller, about 36 percent of the baseline mean. There is no significant impact on morphine prescriptions. In terms of Medicaid spending and MMEs, the impact remains largest for hydrocodone. Compared to other substances, hydrocodone has a lower potency (MME factor = 1) and is usually prescribed in lower doses. Therefore, hydrocodone is more common among opioid-naive patients (Jeffery et al., 2018). Results for Medicaid-paid hydrocodone prescriptions continue to be positive among per-enrollee measures, while estimates for other substances become smaller and statistically insignificant (panel B), and oxycodone took the largest share of all opioids in 2013. These results imply that increase in opioid prescriptions paid by Medicaid mostly consists of hydrocodone prescriptions. Results for per-enrollee prescriptions across substances also imply that post-expansion Medicaid population, on average, are prescribed more hydrocodone prescriptions. However, because there is little change in the amount of opioids used (in MMEs), the average per-enrollee opioid use is not necessarily higher among the post-expansion population.

Although there is a small increase in the number of fentanyl prescriptions, the estimates are not significantly different from zero in other measures. Fentanyl is usually prescribed to patients with more serious pain problems such as cancer pain, which are not likely to have large responses to the Medicaid expansion given that the new enrollees are, on average, healthier than the former Medicaid population. In other words, there is no evidence that the Medicaid expansion is associated with the increase in fentanyl-related overdose deaths through prescription fentanyl.

### 1.7 Event-Study Model

For the parameter $\beta$ in equation 1.1 to be valid, changes in prescription opioid utilization through Medicaid are assumed to be similar in the treatment and control groups in the absence of the Medicaid expansion, conditional on the observables. This assumption can be violated if there are unobservables that influence the supply and demand for prescription opioids yet correlate with the ACA Medicaid expansion. There are some cases in figures 1.3– 1.7 where the pre-trends in Medicaid reimbursement are not similar between the control and treatment groups. However, it is necessary to examine the identifying assumption conditioning on the covariates. I employ a flexible event-study framework, which brings two advantages. First, it can assess pre-trends while allowing the time of treatment to vary by state. Second, the model can investigate the overall dynamic impacts after the expansions. This is useful because there has recently been evidence of a gradual impact of the ACA on health-related outcomes (Courtemanche et al., 2019). The event study model is described in equation 1.3:

$$Y_{st} = \alpha + \sum_{\tau=-2,\tau\neq-1}^{2} \beta_\tau(Medicaid_{s\tau}) + \beta_{-3}Medicaid_{-3} + \beta_3 Medicaid_3 + \gamma X_{st} + \delta P_{st} + \eta_s + \lambda_t + \varepsilon_{st} \tag{1.3}$$

where $Y_{st}$ is an outcome of opioid utilization. $\beta_\tau$ parameters measure the effect of the Medicaid expansions on state $s$ prescription opioid use if year $t$ is $\tau$ years after state $s$ expanded Medicaid. $\beta_{-3}$ and $\beta_3$ are indicators if year $t$ is three to six years before and three to six years after state $s$ expanded Medicaid, respectively.

Because most expansion states adopted the expansion in 2014, I group the further periods to minimize state compositional effects from the early and late expansion states. The other terms, $X_{st}$, $P_{st}$, $\eta_s$, $\lambda_t$, and $\varepsilon_{st}$, are defined as in equation 1.1. Under the parallel trend assumption, $\beta_k$ (for all $k < 0$) would equal zero. In other words, the test for pre-treatment trends is equivalent to the t-test that $\beta_{-3}$ to $\beta_{-2}$ equal zero. I omit the year before state $s$ expanded Medicaid $(\tau = -1)$ as the reference year.

Figures 1.8 to 1.10 present the event study results for different utilization measures of the dependent variables: per 1,000 people (age 19–64) and per 1,000 enrollees. All of the estimated coefficients associated with pre-treatment years are not statistically different from zero. The test of joint significance, where the null hypothesis is $\hat{\beta}_{-3}$ equals $\hat{\beta}_{-2}$ and zero, also fails to reject the null hypothesis. These results indicate that conditional on the covariates, the parallel-trend assumption is not violated, even if the visual evidence does not show identical pre-trends. After the expansion, there are continued effects of the Medicaid expansions on per-population prescriptions. Total opioid utilization observes a steadily positive and significant impact every year for $t > 0$. The results for Medicaid spending and total MMEs, however, are smaller and less precise. Figures A.1 to A.4 present the event-study results that assess the parallel trend assumption for individual substances.

### 1.8 Robustness Checks

In this section, I examine the sensitivity of results from the baseline model. The robustness checks fall into two main categories: (1) changes in the covariates that account for potential policy- and market-related confounding factors and (2) changes in the model specifications.

Table 1.4 reports the estimates from group (1) sensitivity checks. I first consider a channel of the ACA that could potentially influence Medicaid prescription drug utilization. Dranove et. al (2017) find evidence of a reduction in Medicaid spending as states increase the share of drug benefit administered by MCOs, which happens partly as a response to the change in the ACA's manufacturer rebate rule. Therefore, MCOs may play a role in opioid prescribing and spending patterns. To account for changes in MCO penetration, I construct a variable that equals the number of prescriptions utilized through MCOs divided by the total number of prescriptions for each state-year pair. States' political characteristics are also potential confounders. It is widely known that blue states are more likely to expand Medicaid compared to red states. To the extent that state pollical characteristics could influence opioid use, I include the shares of state house and senate that are Democratic obtained from the UKCPR Welfare Data.[18] DC and Nebraska were excluded from this specification because they are unicameral.

Population composition might also affect prescription opioid use if (1) the newly eligible population in expansion states systematically has a higher demand for prescription opioids due to age composition or (2) individuals who are in higher need of opioid medications and eligible for Medicaid coverage but not living in an expansion state migrate to such states for coverage. To account for population composition, I control for states' share of individuals aged 45—64 of the Medicaid population[19]. According to Bernstein and Minor (2017), Medicaid recipients aged between 45 and 64 use the most prescription opioids. A CDC report by Schieber et al. (2020) also shows that this age group, 45-64, has the largest share of individuals with at least one opioid prescription filled and the largest rate of opioid prescription per patient[20]. The results are reported in table 1.4,

[18]Other political variables were excluded due to lack of variation.

[19]Obtained from the ACS 2011-2017.

[20]Compared to the full ACA Medicaid eligible age range, 26-64, this age range also better isolates the ages of individuals who gain coverage through the Medicaid expansions because younger beneficiaries from non-expansion states could gain coverage via pregnancy.

column (3). The estimates are smaller in magnitude, with an increase of about 40 Medicaid-paid opioid prescriptions per 1,000 individuals aged 19-64. This result suggest that changes in the Medicaid population age composition could explain part of the increase in utilization. In another sensitivity check that accounts for migration, I re-estimate equation 1.1, using population shares of individuals aged 19–64 as a dependent variable. The results suggest that migration driven by the Medicaid expansion, conditional on the covariates.[21]

In the next part, I consider the sensitivity of the estimates to the model itself. First, I include interaction terms of US Census Division and year indicators to further control for trends in existing regional-specific characteristics, such as opioid-related (both legal and illegal) or economic conditions, that might have influenced Medicaid utilization. Results are shown in table 1.5, column 1. Column 2 reports the results from the sample that excludes states with their own pre-ACA Medicaid expansions. The estimates from both specifications are smaller in magnitude than the main estimates. However, the changes are small, and the sign and significance remain similar in most cases. One exception is that the effects on per-population reimbursements (reported in panel A) become statistically insignificant. I present the results when re-estimating equation 1.1 without weighting in column (3) as a reference. Last, column (4) reports the estimates when controlling for state-specific pre-trends. The estimates are quantitatively similar to those in Table 1.2 in most cases, with an exception of MMEs used per enrollee in Table 1.5 - Panel B, where the estimates are noisy and sensitive to different specifications due to large standard errors. However, there are no specific patterns across the specification.

[21] Results are not reported for brevity.

### 1.9 Discussion and Conclusion

This essay investigates the effects of the Medicaid expansions on different measures of opioid analgesic utilization. The findings add to the understanding of the Medicaid expansion's role in the opioid epidemic and the ongoing discussion of the ACA repeal. I find that the Medicaid expansion is also associated with an increase in opioid analgesic prescriptions that were paid by Medicaid. There is a consistently positive effect of the Medicaid expansion on per-population, Medicaid-paid opioid prescriptions, with about a 30-50 percent increase in the number of opioid prescriptions per 1,000 people ages 19–64. Per-enrollee prescription use observes an increase of about 16 percent; however, in line with previous studies, the per-enrollee estimates are not as robust. These results suggest it is likely that the Medicaid expansion provides access to opioid painkillers to more people, and the post-expansion enrollees were not particularly utilizing more prescriptions. As the ACA Medicaid expansions targets low-income adults aged 19-64, the increase in Medicaid-paid opioid prescriptions also reflects the change in the composition of the post-expansion Medicaid population.

Results on Medicaid spending and MMEs also follow a similar pattern, with evidence of higher per-population utilization but mixed results among per-enrollee estimates. The estimates show relatively larger changes in the number of prescriptions compared to Medicaid spending and MME outcomes, which suggests shorter or lower-dose prescriptions among the post-expansion Medicaid population. Separate analyses of individual substances reveal that the increase in prescriptions mostly comes from hydrocodone (more than 50 percent of all opioids), which is less potent and more common for shorter prescriptions, compared to other commonly prescribed opioids. Although the Medicaid expansion coincides with a sharp increase in fentanyl-related overdose deaths, this essay does not find evidence that the expansion is associated with the fentanyl epidemic through prescription fentanyl.

The findings can be evaluated in several layers. First, the Medicaid expansion provides health care coverage at almost zero cost to lower-income individuals, and it is well understood that low-income individuals are less healthy and are likely to have higher prescription drug utilization, including opioid analgesic use. In this sense, the expansion has served the target sub-population. The increase in utilization may also come from beneficiaries who had other types of health insurance before they were eligible for Medicaid, which represents a switch of payment sources for prescription opioids. Under this mechanism, the Medicaid expansion is less efficient in terms of serving the target population due to crowding out. However, Saloner et al. (2018) find that the crowding-out effects on opioid prescriptions filled by other payment sources (cash, private insurance, and Medicare) are relatively small and not statistically significant.

The results do not suggest an unusual utilization pattern in opioid prescriptions. In agreement with Goodman-Bacon and Sandoe (2017), the findings do not conclude that the Medicaid expansions cause the opioid epidemic. However, from the cost and benefit perspective, although it is important that the Medicaid expansion has provided access to opioid misuse treatment and pain relievers, it is as important for policymakers to pay attention to the risk of opioid usage. In general, greater access to opioids comes with the risks of addiction, overdosing, and possibly lead to increased addiction treatment utilization. These risks are not limited to prescribed users but also extended to non-prescribed users.

This essays have some limitations. First, the data measure utilization only, so I cannot control for beneficiaries' characteristics or explicitly identify whether a prescription is prescribed to a new patient or is a refill. The results, thus, do not distinguish between changes in the intensive and the extensive margin. Second, because the SDUD only contains Medicaid utilization data, the results are limited to the Medicaid population, which may not represent the effects of the ACA at the national level. Another limitation is that this essay does not account for the potential response from suppliers such as advertisements, physician incentives, and new drug entrance. The scope of this essay also does not cover potential spillover effects on non-opioid analgesic or illicit opioids. These points suggest the next natural questions for future studies that aim for a deeper understanding of the role of health insurance in the opioid epidemic.

### 1.10 Tables

Table 1.1: Descriptive statistics (2013) - by Medicaid expansion status

| | (1) Full sample | | (2) Expansion | | (3) Non-expansion | |
|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd |
| Opioid utilization | | | | | | |
| *Per 1,000 people ages 19–64* | | | | | | |
| Total prescriptions | 121.71 | 48.37 | 134.69 | 52.96 | 107.12 | 38.68 |
| Morphine prescriptions | 8.01 | 4.92 | 8.55 | 5.34 | 6.96 | 3.79 |
| Hydrocodone prescriptions | 21.85 | 12.32 | 22.26 | 12.60 | 21.17 | 11.83 |
| Oxycodone prescriptions | 40.44 | 23.76 | 46.98 | 24.77 | 27.79 | 15.09 |
| Fentanyl prescriptions | 4.51 | 3.39 | 4.05 | 2.94 | 5.27 | 3.91 |
| Total Reimbursements | 4,780 | 5,178 | 4,256 | 2,649 | 5,369 | 7,047 |
| Total MMEs | 111,057 | 59,356 | 124,295 | 62,836 | 88,760 | 46,369 |
| *Per 1,000 enrollees* | | | | | | |
| Total prescriptions | 453.68 | 148.20 | 471.73 | 145.39 | 433.37 | 151.78 |
| Total Reimbursements | 18,616 | 22,422 | 14,878 | 7,760 | 22,823 | 31,463 |
| Total MMEs | 412,241 | 189,761 | 444,270 | 196,677 | 358,298 | 168,853 |
| State characteristics | | | | | | |
| Unemployment rate | 6.79 | 1.75 | 7.23 | 1.87 | 6.36 | 1.54 |
| Poverty rate | 0.12 | 0.03 | 0.12 | 0.03 | 0.13 | 0.04 |
| Percent female | 0.51 | 0.01 | 0.51 | 0.01 | 0.51 | 0.01 |
| Percent white | 0.77 | 0.14 | 0.75 | 0.16 | 0.79 | 0.11 |
| Percent uninsured | 18.71 | 5.49 | 16.89 | 5.84 | 20.46 | 4.60 |
| State minimum wage | 7.42 | 0.71 | 7.68 | 0.72 | 7.17 | 0.62 |
| Fr. of state house that is Democratic | 0.47 | 0.18 | 0.60 | 0.15 | 0.34 | 0.10 |
| Fr. of state senate that is Democratic | 0.46 | 0.20 | 0.60 | 0.17 | 0.33 | 0.11 |
| Share MCO | 0.38 | 0.37 | 0.48 | 0.38 | 0.28 | 0.34 |
| Opioid-related policies | | | | | | |
| PDMP-mandate | 0.16 | 0.37 | 0.24 | 0.44 | 0.08 | 0.27 |
| Pain clinic laws | 0.20 | 0.40 | 0.13 | 0.34 | 0.32 | 0.48 |
| Recreational marijuana laws | 0.04 | 0.20 | 0.06 | 0.25 | 0.00 | 0.00 |
| Observations | 51 | | 25 | | 26 | |

*Notes*: Expansion status is based on whether the state's Medicaid is expanded in January 2014. Medicaid reimbursements are measured in 2011 dollars. Table excludes prescription drug time and dosage limit laws because states did not start to adopt these laws until 2016.

Table 1.2: The effect of the Medicaid expansion on opioid utilization

| | (1) | (2) | (3) |
|---|---|---|---|
| Panel A | | | |
| Prescriptions per 1,000 people | 55.6*** | 59.4*** | 60.3*** |
| | (14.4) | (11.9) | (11.8) |
| Prescriptions per 1,000 enrollees | 71.1** | 74.6** | 77.4** |
| | (28.1) | (29.1) | (29.0) |
| Panel B | | | |
| Reimbursement per 1,000 people | 957.9** | 928.6* | 973.5** |
| | (451.3) | (492.1) | (468.1) |
| Reimbursement per 1,000 enrollees | 249.1 | 8.8 | 196.2 |
| | (1570.2) | (1663.0) | (1545.2) |
| Panel C | | | |
| MMEs per 1,000 people | 25,010.8*** | 25,387.2*** | 26,070.0*** |
| | (7,960.8) | (8,543.9) | (8,456.1) |
| MMEs per 1,000 enrollees | 2,262.0 | -247.1 | 2,282.2 |
| | (27,300.8) | (29,931.7) | (29,256.6) |
| State & year FEs | Y | Y | Y |
| State characteristics | N | Y | Y |
| Opioid-related policies | N | N | Y |
| Observations | 357 | 357 | 357 |

*Notes:* All specifications are weighted by state population ages 19–40. Standard errors in parentheses are adjusted for heteroskedasticity and are clustered by state. Reimbursement is measured in 2011 dollars. MMEs are calculated as: *drug strength*$\times$*morphine equivalent factors*$\times$*total units.*
*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 1.3: The effect of the Medicaid expansion on opioid utilization by commonly prescribed drugs

| | (1) Morphine | (2) Hydrocodone | (3) Oxycodone | (4) Fentanyl | (5) Other |
|---|---|---|---|---|---|
| Panel A - Per-population measures | | | | | |
| Prescriptions | 1.2 | 32.0*** | 14.6*** | 1.7** | 10.9** |
| | (0.8) | (8.5) | (5.4) | (0.7) | (4.5) |
| *Mean 2013* | 8.0 | 21.9 | 40.4 | 4.5 | |
| | (4.9) | (12.3) | (23.8) | (3.4) | |
| Reimbursement | -56.3 | 468.5*** | 515.8* | -80.0 | 125.1 |
| | (122.7) | (153.0) | (261.0) | (146.4) | (101.3) |
| *Mean 2013* | 465.8 | 336.6 | 2,057.8 | 697.8 | |
| | (522.4) | (186.9) | (1,182.7) | (1,859.1) | |
| MMEs | -716.4 | 11,729.3** | 12,239.7** | -103.0 | 2,909.3* |
| | (2,115.5) | (4,809.6) | (4,766.9) | (831.4) | (1,653.8) |
| *Mean 2013* | 18,679.8 | 10,531.8 | 59,057.3 | 9,092.5 | |
| | (13,854.0) | (6,423.6) | (35,317.5) | (6,122.1) | |
| Panel B - Per-enrollee measures | | | | | |
| Prescriptions | -2.4 | 61.2*** | 9.9 | 2.8 | 5.9 |
| | (2.2) | (21.89) | (9.4) | (1.7) | (12.4) |
| *Mean 2013* | 29.8 | 80.8 | 151.5 | 18.1 | |
| | (16.7) | (45.3) | (83.2) | (15.6) | |
| Reimbursement | -540.9 | 872.1** | 410.3 | -587.3 | 37.7 |
| | (416.2) | (394.4) | (663.9) | (670.5) | (292.2) |
| *Mean 2013* | 1,693.7 | 1,248.9 | 7,877.0 | 2,913.6 | |
| | (1,591.5) | (745.8) | (4,603.8) | (8,298.9) | |
| MMEs | -13,378.8* | 20,826.3 | -1,775.9 | -4,055.7 | 594.0 |
| | (7,366.2) | (12,619.9) | (11,479.6) | (3,064.7) | (4,802.2) |
| *Mean 2013* | 67,629.8 | 38,787.0 | 221,823.5 | 33,146.3 | |
| | 43,490.0 | 23,521.0 | 124,039.2 | 18,443.1 | |
| Observations | 357 | 357 | 357 | 357 | 357 |

*Notes:* All specifications are weighted by state population ages 19–40. Standard errors in parentheses are adjusted for heteroskedasticity and are clustered by state. MMEs are calculated as: *drug strength* × *morphine equivalent factors* × *total units.* Full-sample 2013 means and standard deviations are reported for reference.
*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 1.4: Robustness checks for potential confounding factors

| | Share MCO | Political controls | Share of Medicaid population ages 45-64 |
|---|---|---|---|
| Panel A - Per-population measures | | | |
| Prescriptions | 58.6*** | 59.7*** | 39.8*** |
| | (12.1) | (11.4) | (10.7) |
| Reimbursements | 1,024.6* | 930.1* | 720.4* |
| | (565.5) | (468.3) | (392.8) |
| MMEs | 26,304*** | 25,829*** | 16826.5* |
| | (8,185) | (8,007) | (9290.1) |
| Panel B - Per-enrollee measures | | | |
| Prescriptions | 74.6** | 76.7** | 49.6 |
| | (31.4) | (29.0) | (29.8) |
| Reimbursements | 97.8 | 15.4 | -95.7 |
| | (1,897) | (1,618) | (1608.8) |
| MMEs | 1,735 | 1,662 | -13857.2 |
| | (29,371) | (29,711) | (31241.9) |
| Observations | 357 | 343 | 357 |

Notes: All specifications control for state opioid-related policies and include state and year fixed effects. Regressions are weighted by state population ages 19–40. Standard errors in parentheses are adjusted for heteroskedasticity and are clustered by state. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 1.5: Specification sensitivity checks

| | Excluded early expansion states | Census Div. & year interactions | Unweighted | Control for pre-trends |
|---|---|---|---|---|
| Panel A - Per-population measures | | | | |
| Prescriptions | 56.9*** | 49.3*** | 66.6*** | 56.0*** |
| | (11.6) | (11.0) | (13.1) | (11.9) |
| Reimbursements | 872.2* | 866.6 | 1,145.1 | 1,018.3** |
| | (478.5) | (633.3) | (1071.5) | (390.5) |
| MMEs | 24,841*** | 21,175*** | 28,172*** | 23,513** |
| | (8,829) | (7,207) | (8,470) | (10,977) |
| Panel B - Per enrollee measures | | | | |
| Prescriptions | 70.7** | 28.6 | 68.7** | 76.3** |
| | (28.6) | (22.7) | (27.4) | (29.3) |
| Reimbursements | 89.8 | -904.6 | -1,441.2 | 721.0 |
| | (1,539.9) | (1,826.0) | (3,336.0) | (1,260.6) |
| MMEs | -475 | -24,229 | -15,258 | 5,456 |
| | (29,832) | (22,574) | (24,135) | (23,645) |
| Observations | 322 | 357 | 357 | 357 |

*Notes:* All specifications control for state opioid-related policies and include state and year fixed effects. Regressions are weighted by state population ages 19–40. Standard errors in parentheses are adjusted for heteroskedasticity and are clustered by state.
*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

### 1.11 Figures

Figure 1.1: Medicaid opioid prescriptions per population (1,000s) ages 19–64: 2013

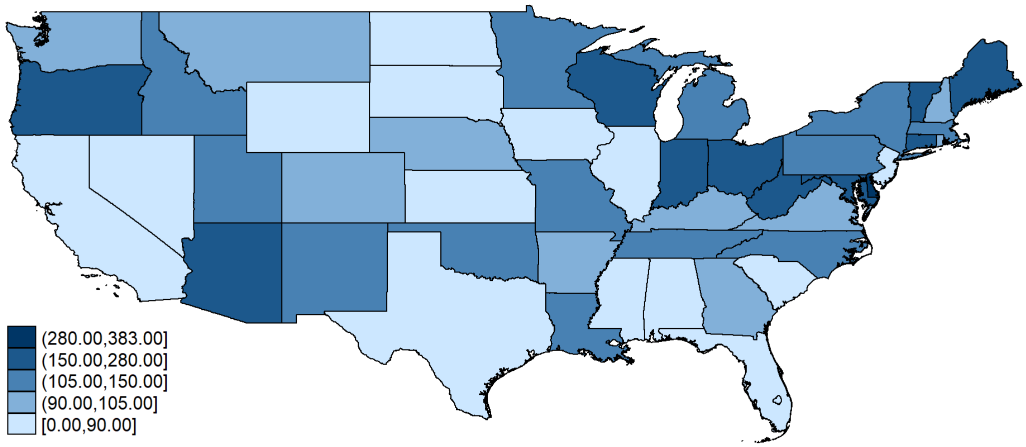



Figure 1.2: Medicaid opioid prescriptions per population (1,000s) ages 19–64: 2014

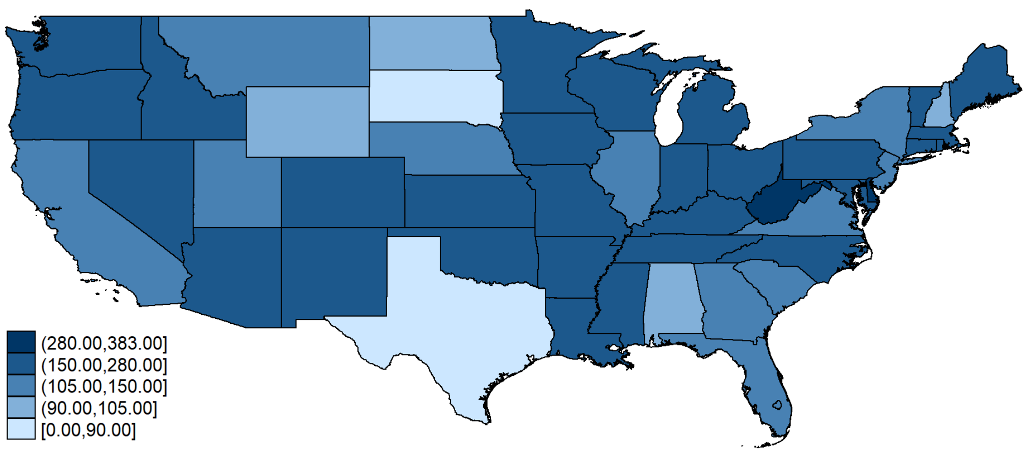
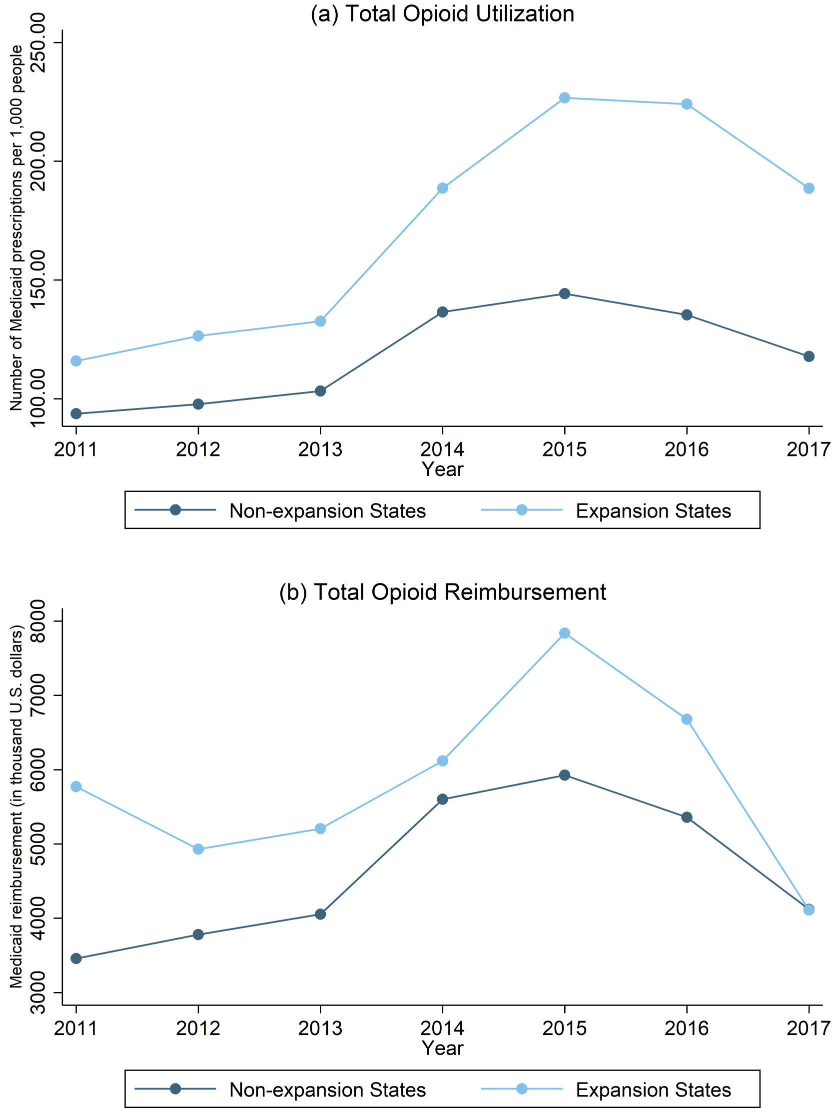
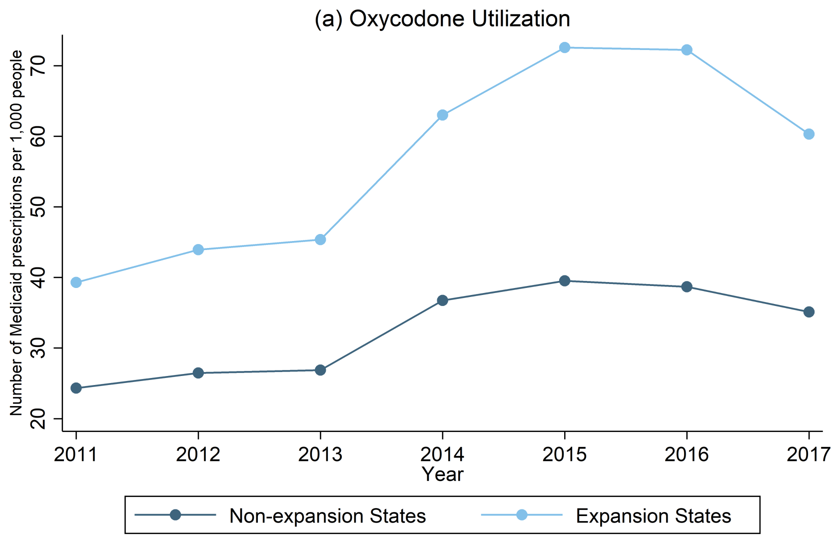


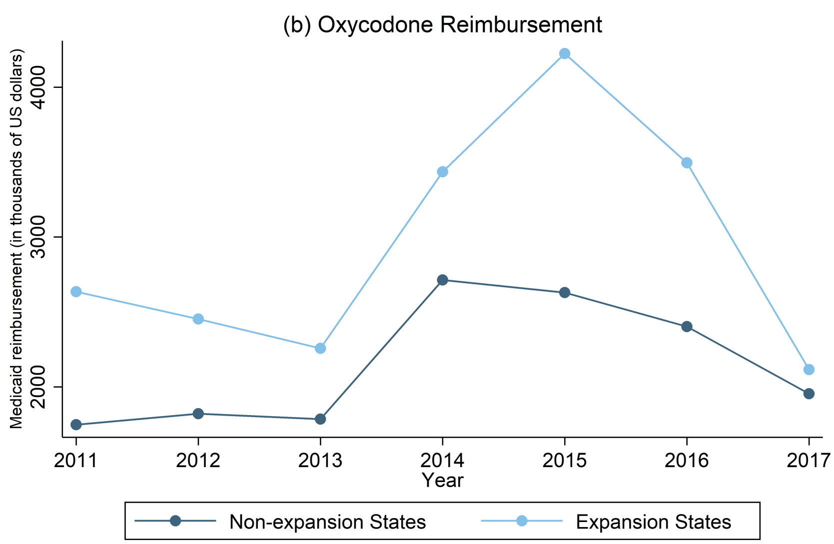
Figure 1.3: Medicaid utilization and reimbursement of opioids among treatment and control states, 2011–2017: Total




Notes: Medicaid expansion status follows Henry Kaiser Family Foundation (2019). Utilization measures are weighted by state population ages 19–64.

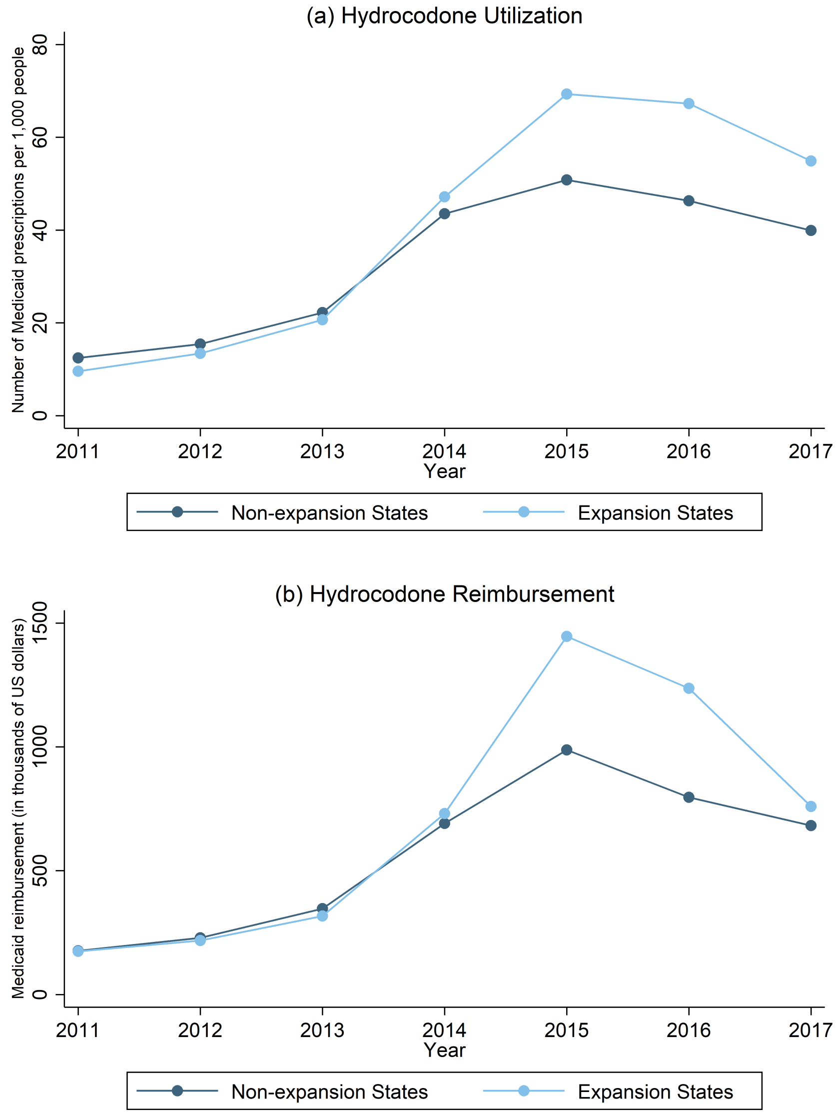
Figure 1.4: Medicaid utilization and reimbursement of opioids among treatment and control states, 2011–2017, by drug: Oxycodone



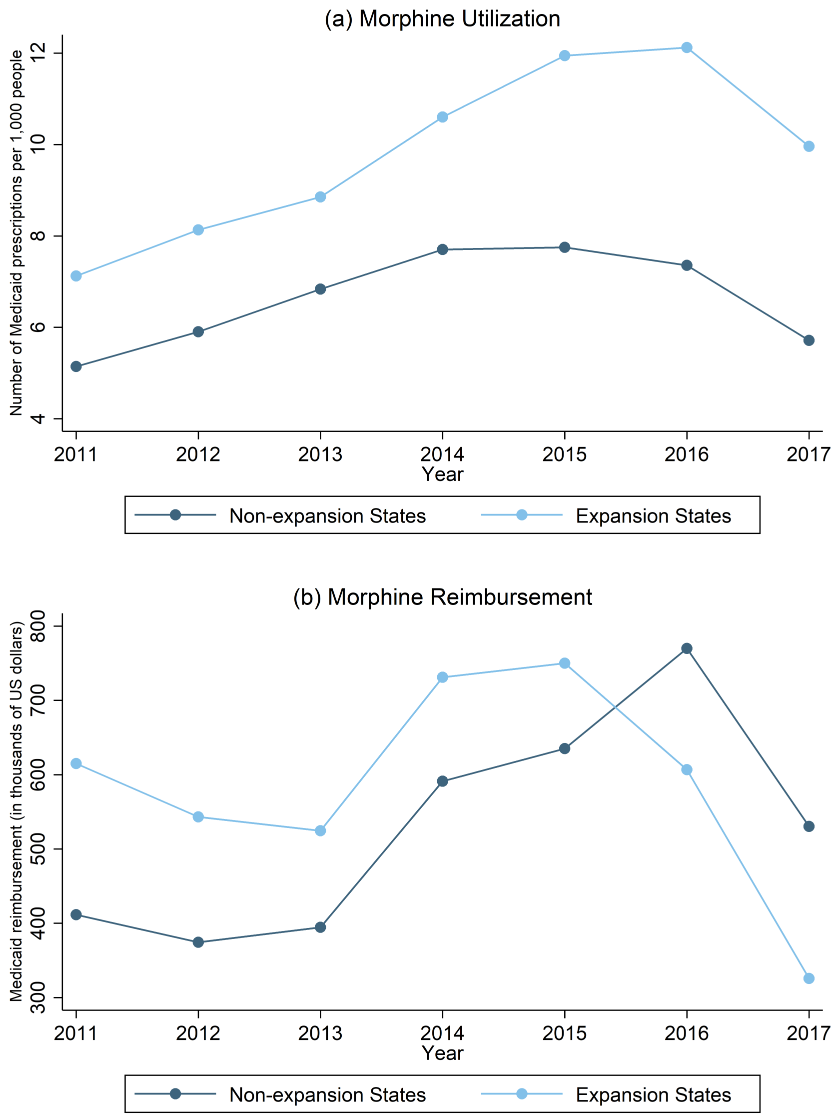

*N*otes: Medicaid expansion status follows Henry Kaiser Family Foundation (2019). Utilization measures are weighted by state population ages 19 - 64.

Figure 1.5: Medicaid utilization and reimbursement of opioids among treatment and control states, 2011–2017, by drug: Hydrocodone



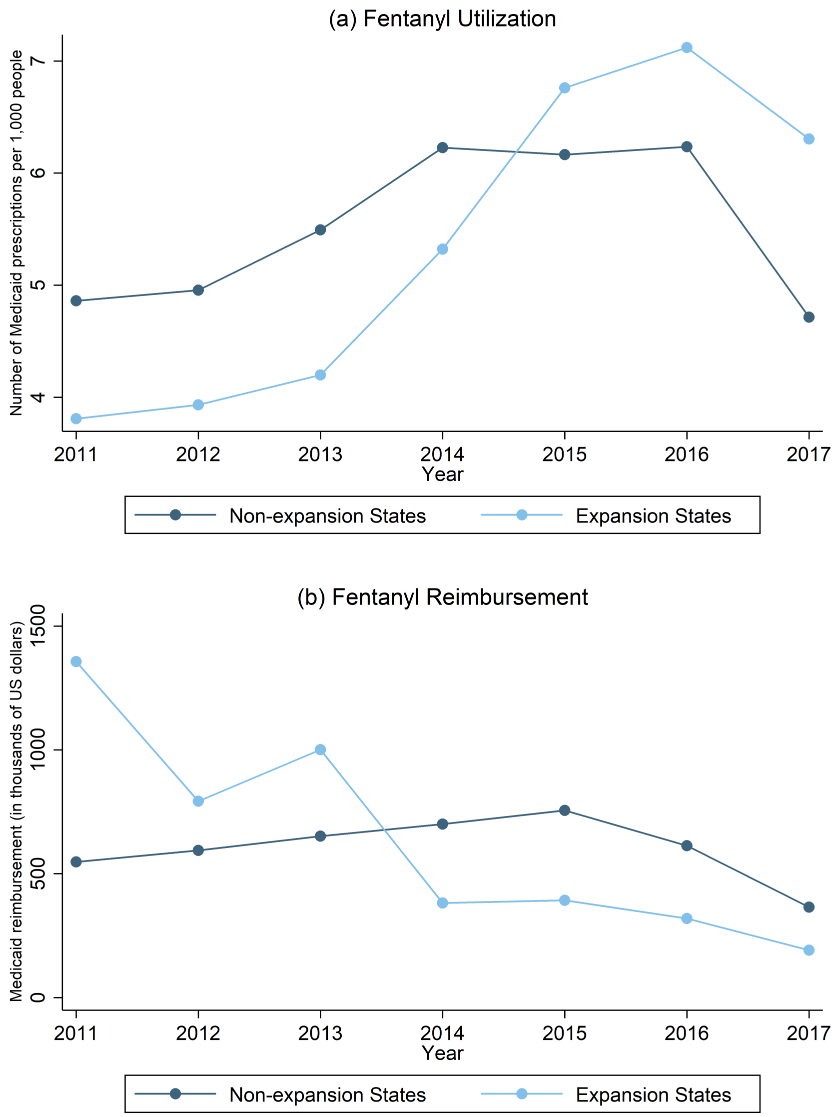

*N*otes: Medicaid expansion status follows Henry Kaiser Family Foundation (2019). Utilization measures are weighted by state population ages 19 - 64.

Figure 1.6: Medicaid utilization and reimbursement of opioids among treatment and control states, 2011–2017, by drug: Morphine



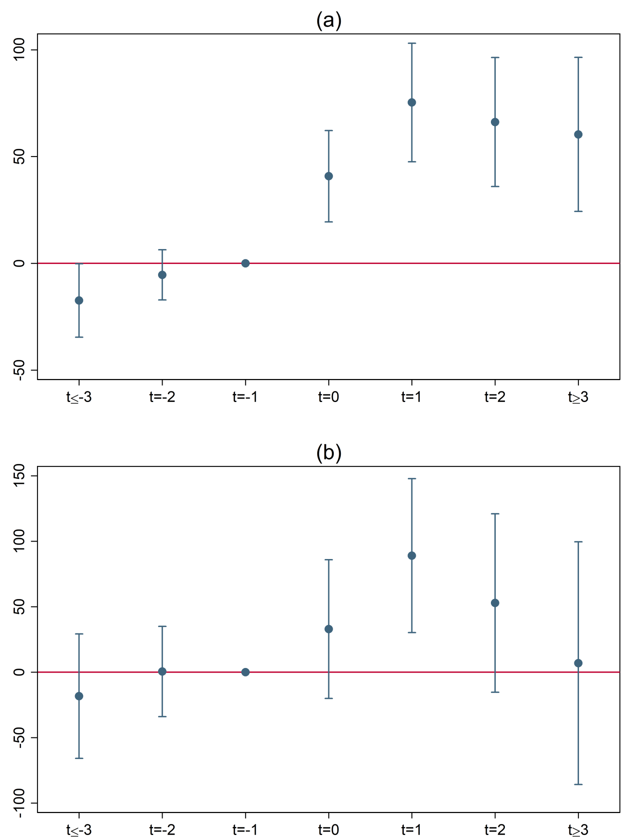

*N*otes: Medicaid expansion status follows Henry Kaiser Family Foundation (2019). Utilization measures are weighted by state population ages 19–64.

Figure 1.7: Medicaid utilization and reimbursement of opioids among treatment and control states, 2011–2017, by drug: Fentanyl



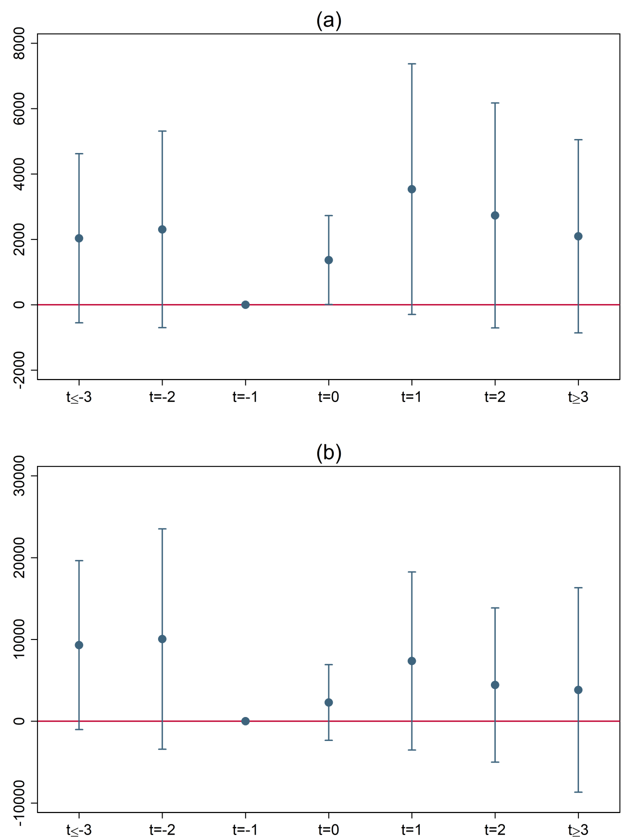

*N*otes: Medicaid expansion status follows Henry Kaiser Family Foundation (2019). Utilization measures are weighted by state population ages 19–64.

Figure 1.8: Event-study results: Effect of the Medicaid expansions on opioid prescriptions.



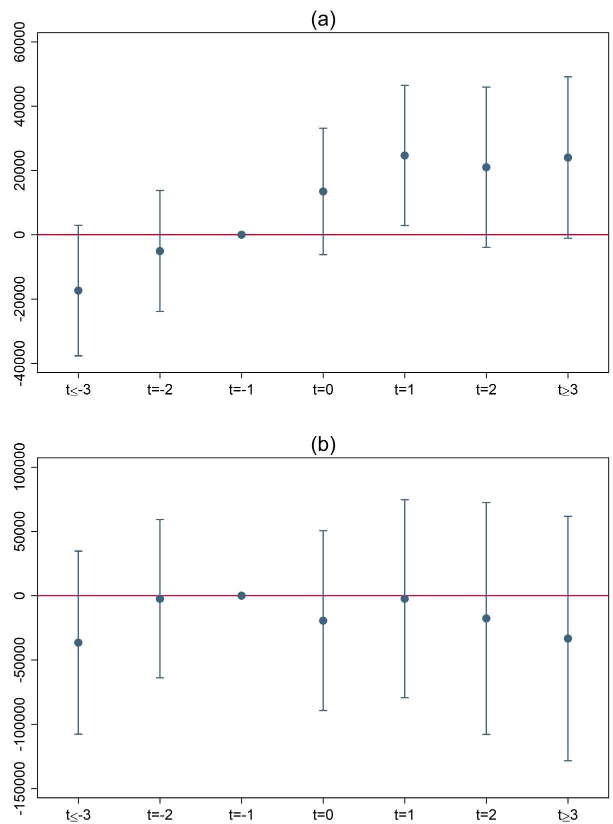

Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. *t=0* is the year of expansion, *t=-1* is the reference year. Dependent variables are state-year counts of prescriptions and are *(a) divided by state population ages 19–64 (1,000s), (b) and divided by the number of enrollees (1,000s)*.

Figure 1.9: Event-study results: Effect of the Medicaid expansions on opioid reimbursement.




Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. Reimbursement is measured in 2011 dollars. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables are state-year aggregate Medicaid reimbursement and *(a) divided by state population ages 19–64 (1,000s), (b) and divided by the number of enrollees (1,000s).*

Figure 1.10: Event-study results: Effect of the Medicaid expansions on opioid use, in MMEs.




Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. MMEs are calculated as: *drug strength*×*morphine equivalent factors*×*total units*. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables are sum of state-year opioid use measured in morphine milligram equivalents (MMEs) and are *(a) divided by state population ages 19–64 (1,000s), (b) and divided by the number of enrollees (1,000s).*

# Chapter 2

## Do Medicaid Expansions Affect Treatment Completion and Racial Disparities in Substance-Use-Disorder Treatment Facilities?

### 2.1 Introduction

Substance use and its related issues are major concerns in the US, not only from a public health perspective but also in economics and public security. Substance use disorder (SUD) is characterized as a medical illness in which individuals who suffer from this disease are incapable of controlling their use of drugs (legal or illegal) and alcohol, which leads to negative consequences. Costs related to substance use issues involve not only medical expenses and loss of life but also costs related to crimes, loss of productivity, etc. Furthermore, individuals who suffer from SUD are more likely to experience co-occurring mental issues. For individuals with existing mental health conditions, the misuse of substances is likely to worsen psychotic symptoms (Ross and Peselow, 2012).

Besides policies that aim to control and prevent substance-use-related issues, providing treatments for people with SUD is also critical for public health and safety (Bondurant et al., 2018; Chandler et al., 2009; McInerney, 2018). According to the Centers for Medicare & Medicaid Services, health care costs for individuals with untreated alcohol use disorders are twice as those who were treated. It is also estimated that methadone treatment returns $4 to $5 for every $1 invested (Medicaid.gov). However, only a small portion of individuals who are in need of SUD treatment actually receive them. According to the Substance Abuse and Mental Health Services Administration (SAMSHA) 2016 Short Report, 21.7 million people who are 12 and older had a SUD in the past year. It is estimated that only about 10.8 percent of them received SUD treatment (SAMHSA, 2016).

Data from the SAMHSA also show that, among people who are admitted to a SUD treatment facility, about 44 percent finish the treatment, which is less than 5 percent of individuals with SUD.

There are often barriers that prevent people with SUD from seeking the treatment they need. Barriers to treatment include, but are not limited to, treatment availability, access to diagnoses, stigma, lack of readiness, and financial resources (Ali et al., 2017; Stringer and Baker, 2015). Beyond these resource-related barriers, socio-demographic characteristics also affect individuals' getting and completing the appropriate SUD treatment. Racial disparities in health and healthcare have been an issue of concern and research has generally found evidence of disparities among people of color who are in need or who have had SUD treatment. Saloner and Cook (2013) find that Blacks and Hispanics are less likely than Whites to complete treatment for alcohol and drug misuse. Matsuzaka and Knapp (2019) find that people of color have more treatment barriers compared to their White counterparts. Lewis et al. (2018) report that African Americans experience longer delays in treatment entry than Caucasians. Although there are many reasons for discrepancies in treatment outcomes that are not easy to improve with a single policy, health insurance can play a role in reducing financial barriers among racial groups.

This essay aims to investigate to what extent an increase in insurance coverage such as the Affordable Care Act (ACA) Medicaid expansions could affect treatment discharge outcomes in SUD treatment facilities. It has been well-established in the literature that Medicaid expansions increase health coverage among the uninsured population. In terms of SUD treatment, earlier studies have found that Medicaid expansions are associated with higher Medicaid coverage among people who receive the treatment. There is also evidence of higher utilization of outpatient medication-assisted treatment among people with opioid use disorder specifically (Meinhofer and Witman, 2018; Maclean and Saloner, 2019). However, it is unclear how the increase in SUD treatment utilization translates to treatment completion or incompletion in specialty facilities. Unlike most types of healthcare services, SUD treatment has low completion and treatment retention rates. Because treatment completion depends on a variety of factors, coming from both the patient and the facility (White et al., 2005), looking into completion/incompletion outcomes deepens the understanding of the dynamic within specialty facilities.

Using data from the SAMSHA, I examine the effects of Medicaid expansions on two main sets of outcomes: treatment discharge outcomes and disparities across racial groups. Based on the variation in states' decisions to expand Medicaid, I employ a difference-in-differences strategy in the form of an event-study design. In general, I find little evidence that the treatment completion rate in SUD treatment facilities is affected by Medicaid expansions. When looking into treatment outcomes across subsamples of White, Black, and Hispanic patients, I find some evidence of heterogeneous effects. Results from the White subsample follow similar patterns to the full sample, while the treatment completion rate of Black patients gradually decreases in the years after expansion. Further investigations of other discharge outcomes reveal that Medicaid expansions do not affect the percentage of patients who dropped out of treatment. However, there is suggestive evidence that the percentage of patients whose treatments were terminated by the facility may have gone up. Although this result should be interpreted with caution due to pre-trends in some cases, the pattern is consistent for all racial/ethnicity subsample analyses.

## 2.2 Background

### 2.2.1 Mechanisms Medicaid Expansions Can Affect Admissions and Treatment Outcomes

Medicaid expansions, along with the other parts of the ACA, may create responses from both the supply and demand of SUD treatment. An increase in health insurance coverage can increase demand for SUD treatment by reducing the financial constraints for previously uninsured individuals who seek SUD treatment. Based on the findings by Ali et al. (2016), affordability is a prevalent constraint among the uninsured population. Besides increasing access to treatment via admissions, health insurance could also increase completion rate by providing a continuous source of payment. For instance, uninsured individuals may drop out of treatment because of financial instability. Also, having health insurance could provide a financial safety net for other types of care and encourage patients to follow through with their treatment, as individuals who suffer from SUD are more likely to have other co-morbid diseases. However, other factors could influence treatment admissions and outcomes. Individuals who gain Medicaid coverage under Medicaid expansions but were previously insured under private insurance may face fewer choices of facilities and treatment programs, although existing evidence suggest that the number of individuals in this category are likely small[1]. Also, moral hazard or inertia could make individuals less likely to complete treatment if they rely on re-admissions due to lower costs.

Responses, or lack of responses, from the supply side can contribute to SUD treatment outcomes through several channels. First, facilities that choose to accept Medicaid can reach certain levels of capacity that prevent them from admitting

[1] Kaestner et al. (2017) find little evidence that the Medicaid expansions were associated with changes in work effort. Abramowitz (2018) finds some evidence of switching coverage from employer-sponsored to Medicaid. However, the estimate was small.

additional patients. Crowded facilities may also face other constraints, such as staffing and technology, which can affect the quality of treatment among admitted patients. Second, facilities may have to adapt to the new mix of admitted patients. These changes can lead to spillovers to treatment outcomes of patients that are covered by other insurance types (i.e., patients with private or Medicare coverage, etc.) and patients who were previously eligible for Medicaid. Third, because the Affordable Care Act characterizes SUD treatment as an essential health benefit that insurers are required to cover[2], facilities may face a general increase in demand, which can create additional pressure in those that are located in an expansion state.

For patients from minority racial groups, there could be factors, other than those discussed above, that influence SUD treatment outcomes in the presence of an increase in Medicaid coverage. Availability of treatment coverage may not be obvious to minority patients due to language barriers or lack of facilities/referral systems in areas that have a higher minority population and potential discrimination against patients from minority racial groups (Matsuzaka and Knapp, 2019).

### 2.2.2 Prior Literature

***Medicaid Expansions and SUD Treatment***.— Studies on the association between health insurance and SUD treatments have been largely focusing on opioid treatment outcomes, many of which find that Medicaid expansions are associated with higher Medicaid coverage for SUD admissions (Wen et al., 2017; McKenna, 2017; Andrews et al., 2018; Maclean & Saloner, 2019). Maclean and Saloner (2019) also find a decrease in the share of individuals who did not have health insurance or those who received payment support from other state and local programs.

Several other papers find an increase in utilization of treatment services and

[2]For a list of 10 categories of health benefit that insurers must cover, see `https://www.healthcare.gov/coverage/what-marketplace-plans-cover/`

admissions for opioid use disorder (Meinhofer & Witman, 2018; Maclean & Saloner, 2019; Saloner &Maclean, 2020). Meinhofer & Witman (2018) also find no evidence that the increase in treatment admissions from Medicaid beneficiaries is crowding out of other types of health insurance. Saloner & Maclean (2020) find that by the fourth year after Medicaid expansions, total admissions to SUD treatment facilities increase by 36 percent. Treatment admissions for alcohol and opioid disorders also increase due to Medicaid expansions.

There has also been evidence that Medicaid expansions are associated with a higher number of specialty facilities that offer medication-assisted treatment such as injectable naltrexone and buprenorphine for opioid use disorder (Abraham et al., 2020) and antidepressants for psychiatric treatment (Shover et al., 2019). However, Abraham et al. (2020) also note that the increase in naltrexone and buprenorphine treatment mostly comes from nonprofit and for-profit programs, which take up less than 10 percent of the treatment system.

On the contrary, there are also studies that do not find Medicaid expansions are associated with a change in SUD treatment service utilization. Feder et al. (2017), find no evidence of changes in treatment service use among people with heroin use disorder, despite higher coverage. Olfson et al. (2018) find no changes in treatment service use among the low-income population. A study by Andrews et al. (2018) also finds no evidence that Medicaid expansions are associated with a change in the numbers of clients despite an increase in Medicaid-insured patients.

***Medicaid Expansions and Racial Disparities in Healthcare***.— Studies that examine the relationship between the ACA/Medicaid expansion and health coverage have reported a general reduction in the coverage gap among racial groups (Sommers et al., 2015; Buchmueller et al., 2016; Courtemanche et al., 2019; Buchmueller and Levy, 2020). However, results for the racial gap of health care access and health-related outcomes are mixed. Yue et al. (2018) find a widening gap in healthcare access among Hispanics. Sommers et al. (2017) find that lower quality of care remains an issue in health disparities across racial groups. Breathett et al. (2017) find an increase in heart transplant rate among African American patients in expansion states, while there is no significant change among Caucasian and Hispanic patients.

***Racial Disparities in SUD Treatment.***— Papers in this area have found evidence of racial gaps in both SUD treatment progression and completion. Saloner and Cook (2013) find that Blacks and Hispanics were 3.5–8.1 percentage points less likely than whites to complete treatment for alcohol and drugs, and Native Americans were 4.7 percentage points less likely to complete alcohol misuse treatment. Mennis et al. (2018) find that Blacks and Hispanics are less likely to complete treatment, and they also take longer to complete treatment. These results motivate the question of whether Medicaid expansions have an impact on treatment access and quality among minorities with substance use disorder issues, which can provide a better understanding of how to improve treatment outcomes for minority patients.

The mixed results among papers that study the association between Medicaid expansions and SUD outcomes reflect the complexity of SUD treatment systems. They could also reflect that different groups in the population respond differently. Moreover, because completion rates in SUD treatment facilities are generally low (less than 50 percent), it is important to investigate whether the Medicaid expansions lead to a change in treatment completion.

This essay contributes to the literature by examining the role of Medicaid expansions in specialty treatment facilities beyond admissions. Studying the association between the Medicaid expansion and treatment completion and other discharge outcomes also helps expand the understanding of responses from both the demand and supply for SUD treatment. By examining additional years after states expanded Medicaid, results from this study provide a closer look at longer-term effects of Medicaid expansions as compared to existing works. To the best of my knowledge, this is the first paper that explores the role of health insurance in racial disparities in SUD treatment completion outcomes. Because such disparities in health and healthcare, especially in SUD treatment, is still a prominent issue, this essay informs policymakers of areas that need additional attention in terms of increasing treatment effectiveness, especially for vulnerable populations.

### 2.3 Data

Data come from the Treatment Episode Data Set (TEDS) discharge data by the Substance Abuse and Mental Health Services Administration (SAMHSA). The TEDS is a data system that records yearly admissions (TEDS-A) and discharges (TEDS-D) from SUD facilities that receive public funding. Data are reported by state and year. This essay uses data recorded in the TEDS-D dataset to focus on discharge-related outcomes. The TEDS do not include data from all SUD facilities. According to the National Survey of Substance Abuse Treatment Services (N-SSATS), in 2013, 57% of all SUD treatment facilities receive received federal, state, or local government funds or grants. The study period ranges from 2008 to 2018. I exclude data prior to 2008 to minimize the potential changes associated with the Mental Health Parity and Addiction Equity Act (MHPAEA) in 2008[3].

[3]The MHPAEA requires health insurance providers to cover the same benefit level of mental and/or SUD treatment as what they cover for medical and surgical services.

### 2.3.1 Outcome Variables

Outcomes variables for SUD discharges are based on the questions asked in the TEDS data. The first set of outcome variables is built upon the reasons for discharge recorded in the data. These reasons include finishing treatment, dropping out of treatment, termination by facility, transfer to other programs, incarceration, death, and other unstated reasons. Based on these classifications, I define completion of treatment as when patients' reasons of discharge are categorized under "finish treatment." Other discharge reasons are referred to as incompletion of treatment. One thing to note in the TEDS data is that individuals represent admissions rather than unique patients (i.e., an individual that was admitted twice shows up as two separate admissions in the data). Therefore, throughout essay paper, the term "patient" represents admissions, unless stated otherwise.

Next, based on information about patients' time of stay at the facility, I construct 2 additional outcomes: length of stay (measured in days) if the stay is 30 days or fewer and an indicator of whether the patient stays more than 30 days[4] I also construct subsamples based on the substance disorder which the patients reported at admission. These substances of treatment include alcohol, heroin, cocaine, marijuana, and other opiates, including prescription opioids and non-prescription methadone.

### 2.3.2 Control Variables

Control variables in individual analyses can be grouped into 3 categories. The first category contains individual socio-demographic characteristics. The TEDS include age categories (below 18, 18-24, 25-54, and over 55), education levels (less than high school, high school, and more than high school), employment, gender,

[4]Length of stay information is recorded by days if the stay lasts less than 30 days but is recorded in 7 to 15 days intervals if the stay lasts more than 30 days.

race, and whether the patient is Hispanic. The second control group includes individual treatment-related characteristics: whether the patient was admitted with a narcotic disorder[5], whether the patient has co-occurring substance misuse and mental disorder, and the number of substances reported at admission. The third group controls for state characteristics: the number of SUD treatment centers in each state (obtained from the N-SSATS) and the fractions of State House or Senate that are Democratic (National Welfare Data, collected by the University of Kentucky Center for Poverty Research).

## 2.4 Methodology

States' treatment status depends on the years states expand their Medicaid, either through the ACA or equivalent programs. States that expanded Medicaid will remain treated throughout the study period. Similar to the previous chapter[6], treatment states include those that expanded Medicaid before, during, and after 2014. States that did not expand Medicaid remain control states for the whole period.

Difference-in-differences designs rely on the assumption that in absence of Medicaid expansions, treatment outcomes in expansion and non-expansion states would follow similar trajectories, conditional on the observables. Therefore, allowing expansion time to vary years before and after expansions, in the form of an event-study, can examine the validity of this assumption. This strategy brings another advantage. Compared to most other types of health service utilization, SUD treatment outcomes can experience a more gradual effect due to the complexity of the treatment system (i.e., referral system). Other barriers such as stigma and

[5]I classify admissions as associated with a narcotic disorder when individuals are reported to have a misuse disorder with at least one of the following substance: heroin, non-prescription methadone, prescription, and other opioids.

[6]See Table A.1.

procrastination in seeking treatment could also slow down the potential effects. An event-study model will allow a continual observance of the post-period. The model is described in equation 2.1.

$$Y_{ist} = \alpha + \sum_{\tau=-3,\tau\neq-1}^{3} \beta_\tau(Medicaid_{s\tau}) + \beta_{-4}Medicaid_{-4} + \beta_4 Medicaid_4 + \gamma X_{ist} + \delta P_{st} + \eta_s + \lambda_t + \varepsilon_{ist} \tag{2.1}$$

$Y_{ist}$ is a measure of individual $i$'s treatment quality or completion. The $\beta$ parameters, for $-3 \leq \tau \geq 3$, measure the effect of Medicaid expansions on individual treatment quality or completion if year $t$ is $\tau$ years after state $s$ expanded Medicaid. Appendix Table B.1 shows how the treatment (Medicaid expansion) periods are distributed based on states' expansion years. Accordingly, $\tau < 0$ indicate pre-expansion periods. I limit the event-study to a window of four years before and after expansion, which spans the most states in the sample. $Medicaid_{-4}$ and $Medicaid_4$ are indicators that equal 1 if period $t$ is 4 or more year before and after the expansion, respectively[7]. The identification assumption for a difference-in-differences model means that $\beta_\tau$ (for all $\tau < 0$) would equal zero. In other words, the test for pre-treatment trends is equivalent to the t-test that $\beta_{-3}$ to $\beta_{-2}$ equal zero, as $\tau = -1$ is omitted as the reference year. $X_{ist}$ is a vector of individual characteristics. $P_{st}$ controls for state characteristics. $\delta_s$ and $\alpha_t$ control for state fixed effects and year fixed effects, respectively, and $\varepsilon_{ist}$ is the error term. All specifications are weighted by states' 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

[7] These two indicators can be influenced by compositional characteristics of late or early expanders. Thus, I will exclude them from the interpretations.

## 2.5 Results

### 2.5.1 Descriptive Statistics

Figure 2.1 shows the breakdowns of all discharge reasons. Throughout the study period, 43.9 percent of admissions were discharged through completion. About 25.8 percent of admitted patients drop out of treatment against professional advice, and 6.9 percent of admissions were terminated by the facility. Facilities also record transfers to other facilities as discharges, which take about 16.3 percent of all admissions.

Table 2.1 summarizes important characteristics of admissions included in the dataset, from 2008 to 2013. In the full sample, patients admitted to specialty facilities in expansion states have similar completion rate to those in non-expansion states. Around 43-44 percent of admitted patients finish their treatment. The proportions of non-completion discharge are relatively similar between the two groups, except for that admitted patients in expansion states are more likely to drop out of treatment than those in non-expansion states (30 percent vs. 21 percent). Demographic characteristics, such as age and gender compositions, are comparable between the two groups. However, "currently working" status is slightly more prevalent among patients in non-expansion states. Treatment discharge reasons in the White subsample follow similar patterns to the full sample, although patients in expansion states are more likely to have higher education. The discrepancies in discharge reasons between expansion and non-expansion states are more prominent in the Black and Hispanic subsamples.

Table 2.2 provides the summary statistics of state characteristics for the period 2008 to 2013. On average, the number of SUD treatment centers in expansion states almost doubles the number of centers in non-expansion states (682 vs. 346). Based on this large difference, I decide to include the number of facilities as a control in later analyses. Political characteristics are also drastically different between the two groups, with expansion states have a higher rate of Democratic members in state House and Senate.

### 2.5.2 Treatment Completion

Figure 2.2 reports the event-study results for the effects of Medicaid expansions on SUD treatment completion. There is no evidence that Medicaid expansions have affected the treatment completion rate. Throughout the post-period, the estimates are close to zero and are statistically insignificant. The pre-period estimates are also being statistically insignificant, which confirms the validity of the baseline model.

Table 2.3 shows the association between patients' characteristics and treatment completion. Older patients appear to have higher completion rate than younger patients. Higher education and employment also increase completion rate. Female individuals are less likely to complete SUD treatment (-2.82 percentage points), while White individuals, on average, are more likely to complete treatment (5.29 percentage points). Among substance-use-related characteristics, having a narcotic disorder is associated with a reduction of 12.91 percentage points in completion rate, and having a co-occurring mental disorder is associated with a 3.74-percentage-point reduction in treatment completion.

***Treatment Completion By Substance.***— In this section, I examine whether Medicaid expansions affect the treatment completion rate of each substance disorder. Figure 2.3 presents the portions of patients who reported having a use disorder of each substance. Individuals can report up to three substances at admission. Alcohol is the most common substance. More than 50% of SUD treatment admissions involve alcohol use disorder. The second common substance is marijuana, which appears in 37% of admissions. Figure 2.4 reports the results for disorder treatment that involve alcohol, cocaine, and marijuana. Figure 2.5 reports the results for disorder treatment that involve heroin and other opiates. Treatment completion among patients who have a cocaine, marijuana, or heroin disorder increase slightly when states expand Medicaid. However, these effects are not substantial.

### 2.5.3 Other Discharge Outcomes

I then turn to investigate whether Medicaid expansions influences non-completion discharge outcomes (Results shown in Figures 2.6 and 2.7). There is no significant effect on the percentage of patients who drop out of their treatment or who are transferred to another facility. However, I find suggestive evidence that the percentage of patients whose treatments are terminated by the facility, also referred to as administrative discharge, has increased due to Medicaid expansions. Results also show a negative association between Medicaid expansions and the percentage that patients are discharge due to incarceration, which is lowered by about 0.36 percentage points, or 16 percent, by the fourth year of expansion.

Figures 2.8 and 2.9 show the results for the length of stay, measured in days if the stay is shorter than 30 days, and the probability that the stay last more than 30 days. Figure 2.8 shows no change in the length of stays that lasts fewer than 30 days. Results for stays that last longer than 30 days, however, are inconclusive due to pre-trends.

### 2.5.4 Racial Disparities in Treatment Completion and Discharge Outcomes

***Treatment Completion.***— To examine whether Medicaid expansions influence disparities in SUD outcomes, I re-estimate the main model for each of the racial/ethnicity subsamples: White, Black, and Hispanic. Results are shown in Figure 2.10. Similar to the estimates from the full sample, treatment completion among White and Hispanic patients do not seem to change due to Medicaid expansions. Among Black patients, the treatment completion rate seems to go down after expansion, although the estimates are not significant during earlier years. By the fourth year after the expansion, the completion rate among Black residents has gone down by 4.13 percentage points, which is 9.58 percent from the pre-expansion mean. In a study that examines the effect of Medicaid expansions on admissions to SUD facilities, Saloner and Maclean (2020) find an increase in aggregate admissions during the third and fourth years after Medicaid expansions among White and Hispanic patients, but there is no change in admissions among Black patients. Therefore, in this case, it is worth noting that there is some negative effect on the treatment completion rate among Black patients. On one hand, these results suggest that there may be some underlying reasons that adversely influence treatment completion among Black patients. On the other hand, the results may come from the compositional differences in Medicaid eligibility and price sensitivity for SUD treatment across racial groups.

To further investigate to what extend the compositional differences affect the results, I first examine whether Medicaid expansions are associated with a change in the racial distribution across patients. Specifically, I run three regressions, following a difference-in-differences framework, with patient racial indicators as dependent variables. Results are reported in Table 2.4. Accordingly, Medicaid expansions are not associated with the probability that a patient is White or Hispanic. However, the probability that a patient is Black lowers by 2.5 percentage points. These results are consistent with the findings by Saloner and Maclean (2020), which suggest that the lower treatment completion rate associated with Medicaid expansions is not likely to come from an influx of Black patients. Second, I turn to test whether Medicaid expansions influence patient characteristics that are related to Medicaid eligibility or SUD treatment demand elasticity. Medicaid eligibility characteristics include whether the patient's age is between 18-54[8], patients' level of education and employment status (as proxies for income eligibility). Patient characteristics that can influence the price elasticity of SUD treatment include the number of substances and indicators of the substances with which the patient was involved. Results in Table 2.5 show almost no evidence that Medicaid expansions are associated with changes in relevant patient characteristics, and the difference between racial groups is minimal. These results suggest that the potential effects coming from patient composition would not be substantial in this case.

***Other Discharge Outcomes*.**— Results for other, non-completion discharge reasons are shown in Appendix Figures B.1 to B.4. For all subsamples, Medicaid expansion does not seem to affect the rate of patients who drop out of treatment against professional advice, which is consistent with full-sample results. There is also evidence that the rate of administrative discharge increases when states expand Medicaid. Although pre-trend exists in the Black subsample, for the White and Hispanic subsample, in which there are no pre-trends, the estimates show a positive association between Medicaid expansions and the percentage of administrative discharge.

### 2.5.5 Robustness Checks

In this section, I consider alternative specifications to assess the model's validity and results' robustness. Panels (a) to (d) of Figure 2.11 present specifications that test for potential issues of omitted variable bias. In Panel (a), the specification includes only state and year dummies as covariates. Panel (b) excludes political

[8]Under the ACA, Medicaid expansions mostly affect individuals aged between 19 and 64. However, the TEDS records patient age in categories: less than 18, 18-24, 24-54, and over 55. Therefore, I define eligible age based on the categories closest to 19-64.

controls. Panel (c) controls for states' economic characteristics by adding unemployment and poverty rates to the baseline model. Panel (d) controls for Prescription Drug Monitoring Programs, which could be a confounder between states' underlying substance-use conditions and their decisions to expand Medicaid.

In Panel (e), I exclude early expansion states from the sample to check for potential heterogeneity in effects between states that expanded early and states that expanded under the ACA Medicaid expansions. The specification in Panel (f) accounts for the fact that the number of existing treatment centers in a state may affect the trend in treatment outcomes. I include interactions of the number of SUD specialty centers in 2010 (time-invariant) and a time trend. In Panel (g), I present the results from a specification that allows a longer pre-expansion period and a more balanced sample. To avoid dropping too many states, I exclude early expansion states (CA, CT, D.C., MN, NJ, and WA), states that had their own prior expansions (DE, MA, NY, and VT), and states that expanded after 2015 (LA and MT). Results from this specification show a slight decrease by the third year after expansion. However, the effect becomes insignificant during the following year. All specifications in Figure 2.11 show robust results that the treatment completion rate is not affected by Medicaid expansions. There are also no significant pre-trends among these specifications.

For racial subsamples of White, Black, and Hispanic patients, I present the results of robustness checks in Appendix Figures B.5 to B.9. From Appendix Figures B.5 to B.8, results are consistent that there are no significant changes in completion rate of White patients. It is also consistent that Black patients experience a reduction in completion rate. For the Hispanic subsamples, pre-trends are present in some cases, and the results seem to be sensitive to certain specifications. In the cases with no significant pre-trends, the effects from Medicaid expansions are generally around zero. In Appendix Figure B.9, the estimates for the White and Black samples are visually similar to the results for all racial groups in Figure 2.11. There is, however, some pre-trends for the Hispanic sample.

I include another set of robustness checks for the racial subsamples, in which specifications are weighted by states' White, Black, and Hispanic populations, respectively. This approach redistributes weights according to groups' population instead of states' population. Results are reported in Appendix Figure B.10. White and Hispanic subsamples continue to show no effect. Results from the Black subsample continues to show a negative effect with a larger magnitude of the estimates, compared to the main model.

### 2.6 Discussion

By providing health insurance coverage to low-income individuals, Medicaid expansions have provided access to many types of health services, including treatment for individuals with SUD. Previous works have found that Medicaid expansions are associated with higher number of admissions to SUD treatment facilities. This essay investigates how Medicaid expansions affect the completion rate and treatment discharge outcomes of people who are admitted to these specialty facilities.

Analyses of all admitted patients within SUD treatment facilites show no significant association between Medicaid expansions and the treatment completion rate. However, in subsample analyses, there is some evidence of a heterogeneity in the treatment completion rate across racial/ethnicity groups. While Medicaid expansions have no significant effects on treatment completion of White patients, the results suggest a reduction in SUD treatment completion rate among Black patients.

Analyses on non-completion discharge outcomes find suggestive evidence that the percentage of administrative discharges may have gone up in states that expanded Medicaid. The percentage of patients who dropped out of the treatment, however, does not seem to be affected by Medicaid expansions, regardless of the patient's race or ethnicity. These results suggest that moral hazard is not likely to be the leading cause of a lower treatment completion rate in certain populations, and the completion rate could be improved with appropriate adjustments in the treatment system.

This study has some limitations. The TEDS does not include data from all SUD treatment facilities. Therefore, interpretations only apply to facilities that receive public funds. However, these facilities are more relevant in terms of policies, compared to those that do not receive public funds.

Overall, treatment completion among minority groups appear to be more sensitive to the changes associated with Medicaid expansions. Among these subpopulations, the treatment completion rate tends to go down in subsequent years rather than immediately after Medicaid expansions. This pattern is parallel to the finding of gradual increase in admissions to specialty facilities by Saloner & Maclean (2020).

Based on findings from existing works, the lack of existing treatment/referral network and lower quality of care may have contributed to health disparities across racial groups. However, to unravel these underlying reasons requires further investigations that are outside the scope of this essay. Although results from this essay do not show substantial differences among racial groups,the results suggest that better-targeted attention can help improve SUD treatment outcomes for minority patients.

### 2.7 Tables

Table 2.1: Summary statistics: individual characteristics

| | Full Sample | | | | White | | | | Black | | | | Hispanic | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Exp | | Non-exp | | Exp | | Non-exp | | Exp | | Non-exp | | Exp | | Non-exp | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Finish treatment (%) | 44.01 | 49.64 | 43.19 | 49.53 | 45.97 | 49.84 | 43.02 | 49.51 | 43.31 | 49.55 | 38.78 | 48.73 | 41.04 | 49.19 | 50.72 | 49.99 |
| Dropped out (%) | 30.49 | 46.04 | 21.05 | 40.76 | 28.06 | 44.93 | 21.49 | 41.08 | 31.75 | 46.55 | 23.11 | 42.15 | 35.08 | 47.72 | 15.74 | 36.42 |
| Facility terminated (%) | 5.39 | 22.59 | 8.27 | 27.54 | 5.63 | 23.05 | 6.91 | 25.36 | 6.92 | 25.38 | 8.50 | 27.89 | 3.74 | 18.98 | 13.24 | 33.90 |
| Transferred (%) | 14.85 | 35.56 | 16.96 | 37.53 | 14.85 | 35.56 | 18.89 | 39.14 | 12.78 | 33.39 | 17.99 | 38.41 | 15.83 | 36.51 | 8.52 | 27.91 |
| Incarcerated (%) | 2.24 | 14.78 | 2.27 | 14.89 | 2.00 | 14.01 | 2.22 | 14.74 | 2.41 | 15.34 | 2.52 | 15.66 | 2.63 | 16.01 | 1.95 | 13.82 |
| Death (%) | 0.21 | 4.63 | 0.32 | 5.60 | 0.22 | 4.71 | 0.35 | 5.94 | 0.19 | 4.38 | 0.28 | 5.28 | 0.22 | 4.70 | 0.13 | 3.65 |
| Length of stay ($\leq$30 days) | 23.30 | 13.42 | 22.59 | 13.52 | 23.01 | 13.42 | 21.81 | 13.70 | 22.06 | 13.77 | 23.35 | 13.41 | 24.79 | 13.02 | 24.48 | 12.61 |
| Stay one month | 0.55 | 0.50 | 0.53 | 0.50 | 0.53 | 0.50 | 0.50 | 0.50 | 0.51 | 0.50 | 0.56 | 0.50 | 0.60 | 0.49 | 0.60 | 0.49 |
| Prior treatment | 0.66 | 0.48 | 0.48 | 0.50 | 0.68 | 0.47 | 0.50 | 0.50 | 0.70 | 0.46 | 0.47 | 0.50 | 0.59 | 0.49 | 0.36 | 0.48 |
| Number of substances | 1.75 | 0.72 | 1.75 | 0.82 | 1.79 | 0.75 | 1.79 | 0.83 | 1.78 | 0.71 | 1.68 | 0.80 | 1.67 | 0.66 | 1.73 | 0.79 |
| Narcotic | 0.31 | 0.46 | 0.24 | 0.43 | 0.38 | 0.48 | 0.30 | 0.46 | 0.20 | 0.40 | 0.09 | 0.29 | 0.28 | 0.45 | 0.23 | 0.42 |
| Mental Disorder | 0.24 | 0.43 | 0.41 | 0.49 | 0.29 | 0.45 | 0.42 | 0.49 | 0.24 | 0.43 | 0.36 | 0.48 | 0.16 | 0.36 | 0.46 | 0.50 |
| Ages less than 18 | 0.01 | 0.12 | 0.02 | 0.12 | 0.01 | 0.08 | 0.01 | 0.10 | 0.01 | 0.11 | 0.01 | 0.12 | 0.03 | 0.18 | 0.04 | 0.20 |
| Ages 18-24 | 0.23 | 0.42 | 0.26 | 0.44 | 0.24 | 0.43 | 0.24 | 0.43 | 0.16 | 0.36 | 0.22 | 0.42 | 0.27 | 0.45 | 0.38 | 0.48 |
| Ages 25-54 | 0.60 | 0.49 | 0.61 | 0.49 | 0.61 | 0.49 | 0.64 | 0.48 | 0.61 | 0.49 | 0.60 | 0.49 | 0.59 | 0.49 | 0.52 | 0.50 |
| Ages over 55 | 0.15 | 0.36 | 0.12 | 0.32 | 0.14 | 0.35 | 0.12 | 0.32 | 0.23 | 0.42 | 0.16 | 0.37 | 0.11 | 0.31 | 0.06 | 0.24 |
| Less than highschool | 0.38 | 0.48 | 0.37 | 0.48 | 0.27 | 0.44 | 0.31 | 0.46 | 0.43 | 0.50 | 0.42 | 0.49 | 0.56 | 0.50 | 0.55 | 0.50 |
| Highschool | 0.40 | 0.49 | 0.40 | 0.49 | 0.44 | 0.50 | 0.43 | 0.49 | 0.39 | 0.49 | 0.39 | 0.49 | 0.33 | 0.47 | 0.31 | 0.46 |
| More than highschool | 0.22 | 0.42 | 0.23 | 0.42 | 0.29 | 0.45 | 0.27 | 0.44 | 0.17 | 0.38 | 0.19 | 0.39 | 0.11 | 0.32 | 0.14 | 0.35 |
| Currently working | 0.19 | 0.39 | 0.23 | 0.42 | 0.23 | 0.42 | 0.24 | 0.43 | 0.10 | 0.31 | 0.18 | 0.38 | 0.17 | 0.37 | 0.24 | 0.42 |
| Female | 0.32 | 0.47 | 0.37 | 0.48 | 0.35 | 0.48 | 0.40 | 0.49 | 0.29 | 0.45 | 0.32 | 0.47 | 0.29 | 0.45 | 0.33 | 0.47 |
| Observations | 5,725,503 | | 1,588,956 | | 3,260,708 | | 1,095,115 | | 1,175,821 | | 301,071 | | 905,335 | | 85,716 | |

Notes: Statisics are based on individual-level TEDS-D data 2008-2013. Data are weighted by state Census 2010 population.

Table 2.2: Summary statistics: state characteristics

| | (1) Expansion | | (2) Non-expansion | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| Number of treatment centers in state | 682.24 | 547.03 | 346.41 | 173.20 |
| Unemployment rate | 8.57 | 2.00 | 7.64 | 1.94 |
| Poverty rate | 14.13 | 2.76 | 14.92 | 2.73 |
| State minimum wage | 7.53 | 0.61 | 6.87 | 0.74 |
| Fraction of State House that is Democrat | 0.64 | 0.08 | 0.40 | 0.08 |
| Fraction of State Senate that is Democrat | 0.57 | 0.11 | 0.38 | 0.09 |
| Observations | 172 | | 107 | |

Notes: Data come from TEDS-D 2008-2013. Data are weighted by state Census 2010 population.

Table 2.3: Effect of Medicaid expansions on SUD treatment completion: individual controls

| | |
|---|---|
| Aged 18-24 | 2.07** |
| | (0.90) |
| Aged 25-54 | 6.46*** |
| | (1.70) |
| Aged over 55 | 10.56*** |
| | (1.63) |
| Highschool | 4.17*** |
| | (0.48) |
| More than highschool | 7.62*** |
| | (0.94) |
| Currently working | 2.43* |
| | (1.21) |
| Female | -2.82*** |
| | (0.28) |
| White | 5.29*** |
| | (0.91) |
| Reported a narcotic substance | -12.91*** |
| | (2.14) |
| Having a co-occuring mental disorder | -3.74*** |
| | (0.51) |
| No. of substances report at admission | -0.62 |
| | (0.84) |
| No. of treatment centers in state | 0.00 |
| | (0.01) |
| Constant | 36.24*** |
| | (5.99) |
| Observations | 12,234,293 |

Notes: Table reports estimates of individual characteristics control in equation 2.1. Specification is weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 2.4: Effect of Medicaid expansions on patients' racial indicators.

| | White | Black | Hispanic |
|---|---|---|---|
| Medicaid Expansion | 0.012 | -0.025*** | 0.003 |
| | (0.008) | (0.006) | (0.005) |
| Mean | 0.533 | 0.218 | 0.226 |
| Observations | 12,234,293 | 12,234,293 | 12,234,293 |

Notes: Results from difference-in-differences analysis. Regressions control for state, year fixed effects, and state characteristics. Data is weighted by 2010 Census population. Standard errors in parentheses are adjusted for heteroscedasticity and are clustered by state. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

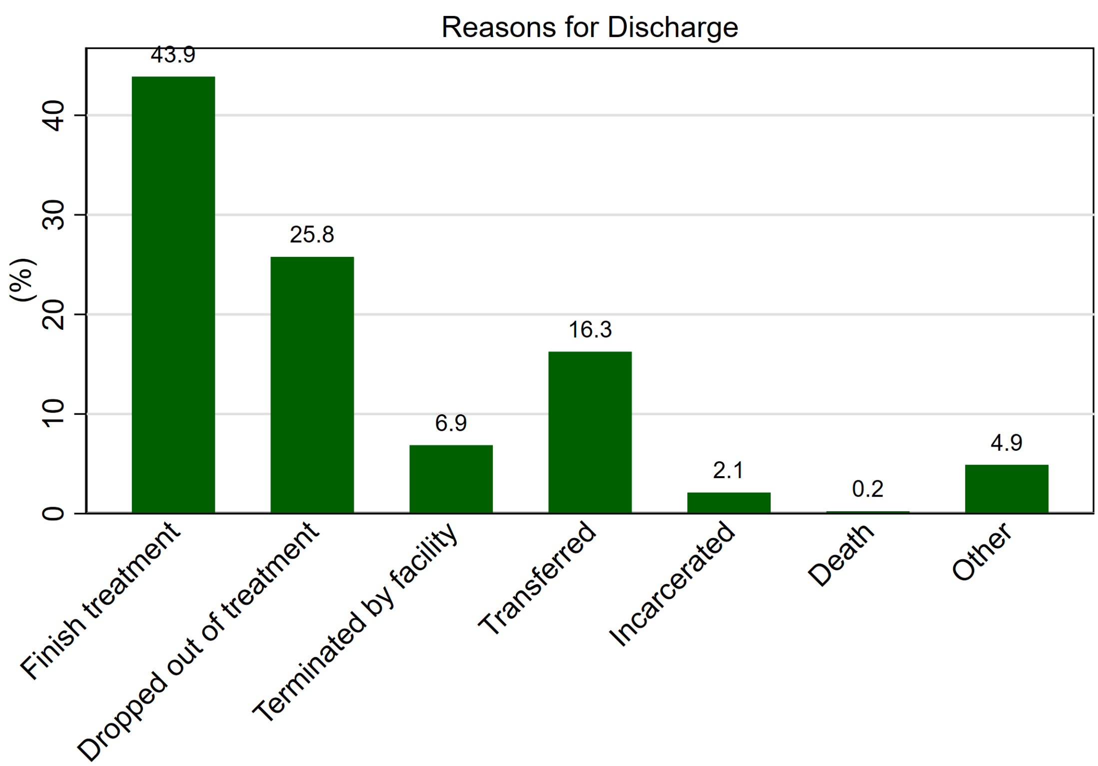
Table 2.5: Effect of Medicaid expansions on patient characteristics, by race/ethnicity.

| | White | Black | Hispanic |
|---|---|---|---|
| Age 18-54 | -0.001 | 0.005 | -0.017 |
| | (0.006) | (0.009) | (0.016) |
| Mean | 0.853 | 0.762 | 0.860 |
| Currently Working | 0.005 | -0.005 | 0.012 |
| | (0.005) | (0.007) | (0.010) |
| Mean | 0.232 | 0.105 | 0.168 |
| More than Highschool | 0.007 | -0.002 | 0.000 |
| | (0.008) | (0.006) | (0.005) |
| Mean | 0.287 | 0.172 | 0.113 |
| Number of substances involved | 0.027 | 0.009 | 0.016 |
| | (0.023) | (0.023) | (0.013) |
| Mean | 1.785 | 1.780 | 1.674 |
| Alcohol | -0.003 | -0.010 | -0.010 |
| | (0.014) | (0.014) | (0.012) |
| Mean | 0.540 | 0.590 | 0.485 |
| Cocaine | 0.010 | 0.003 | 0.019** |
| | (0.008) | (0.011) | (0.008) |
| Mean | 0.183 | 0.456 | 0.196 |
| Marijuana | 0.020 | 0.004 | 0.006 |
| | (0.015) | (0.012) | (0.007) |
| Mean | 0.335 | 0.429 | 0.391 |
| Heroin | 0.001 | 0.007 | -0.005 |
| | (0.011) | (0.009) | (0.010) |
| Mean | 0.243 | 0.177 | 0.247 |
| Non-Rx Methadone | -0.002 | -0.000 | 0.000 |
| | (0.001) | (0.000) | (0.000) |
| Mean | 0.008 | 0.003 | 0.004 |
| Other Opioids | -0.011 | -0.004 | -0.011*** |
| | (0.009) | (0.002) | (0.002) |
| Mean | 0.169 | 0.024 | 0.038 |
| Observations | 7,884,896 | 2,398,425 | 1,172,734 |

Notes: Results from difference-in-differences analysis. Regressions control for state, year fixed effects, and state characteristics. Reported means are calculated using data from the TEDS 2008-2013. Data is weighted by 2010 Census population. Standard errors in parentheses are adjusted for heteroscedasticity and are clustered by state. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

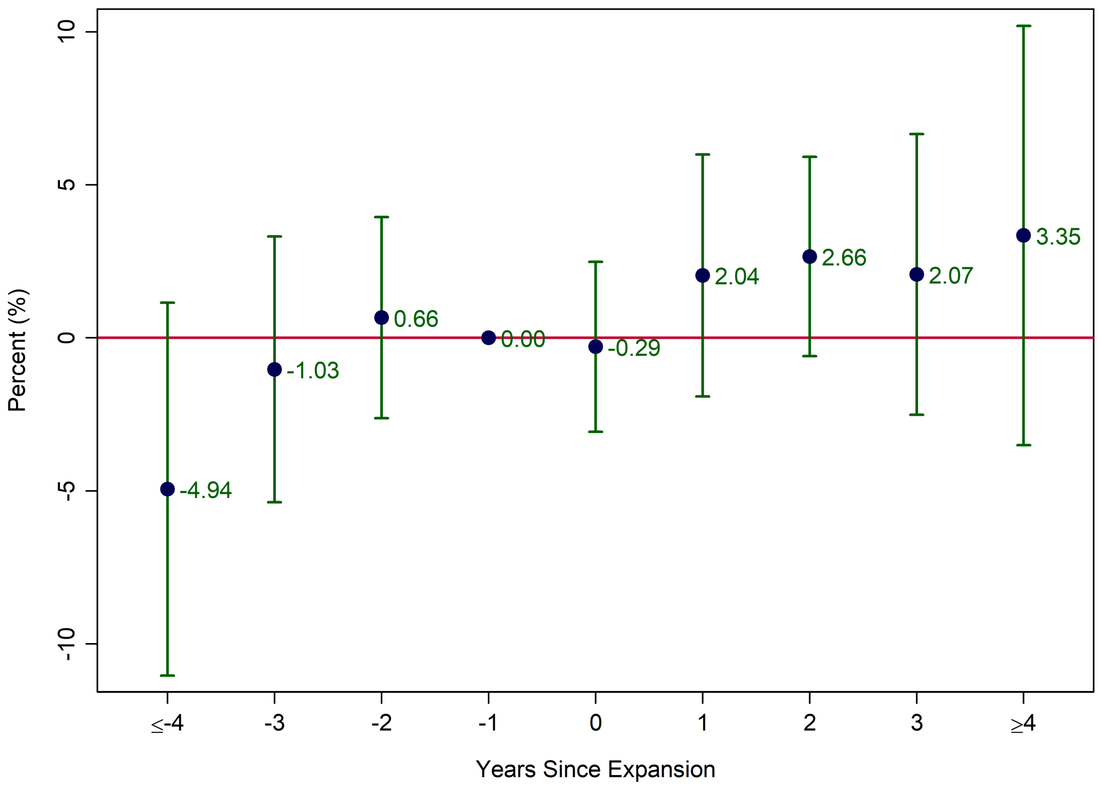
## 2.8 Figures

Figure 2.1: Breakdown of discharge reasons



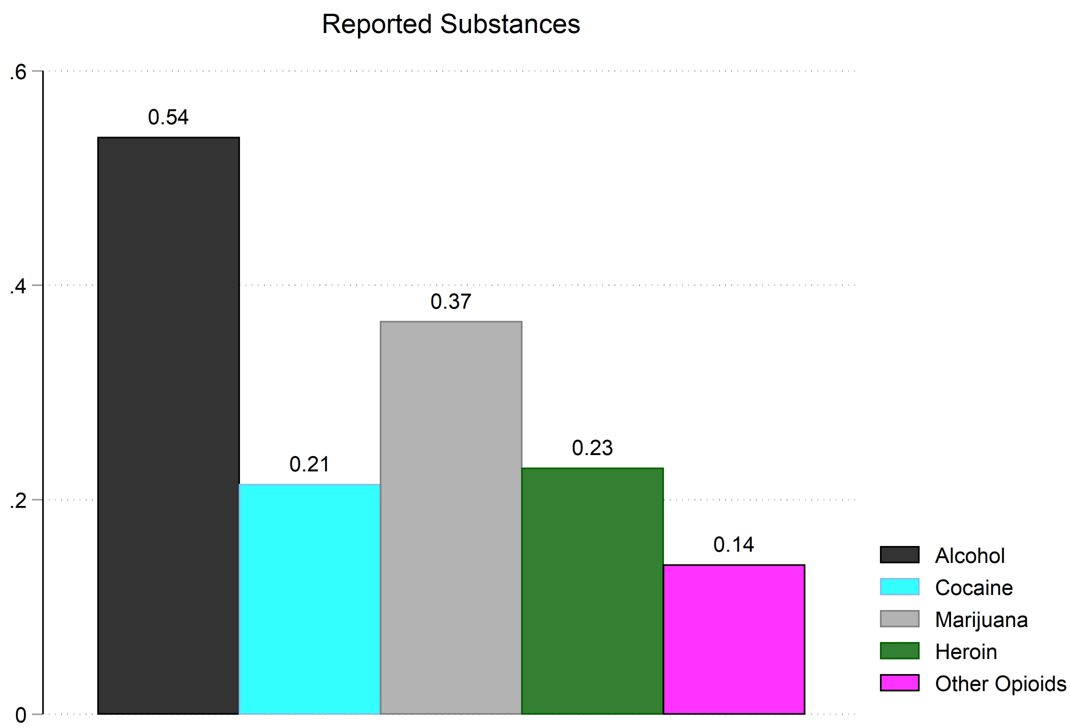

Notes: Data come from TEDS-D 2008-2018.

Figure 2.2: Effect of Medicaid Expansions on SUD treatment completion



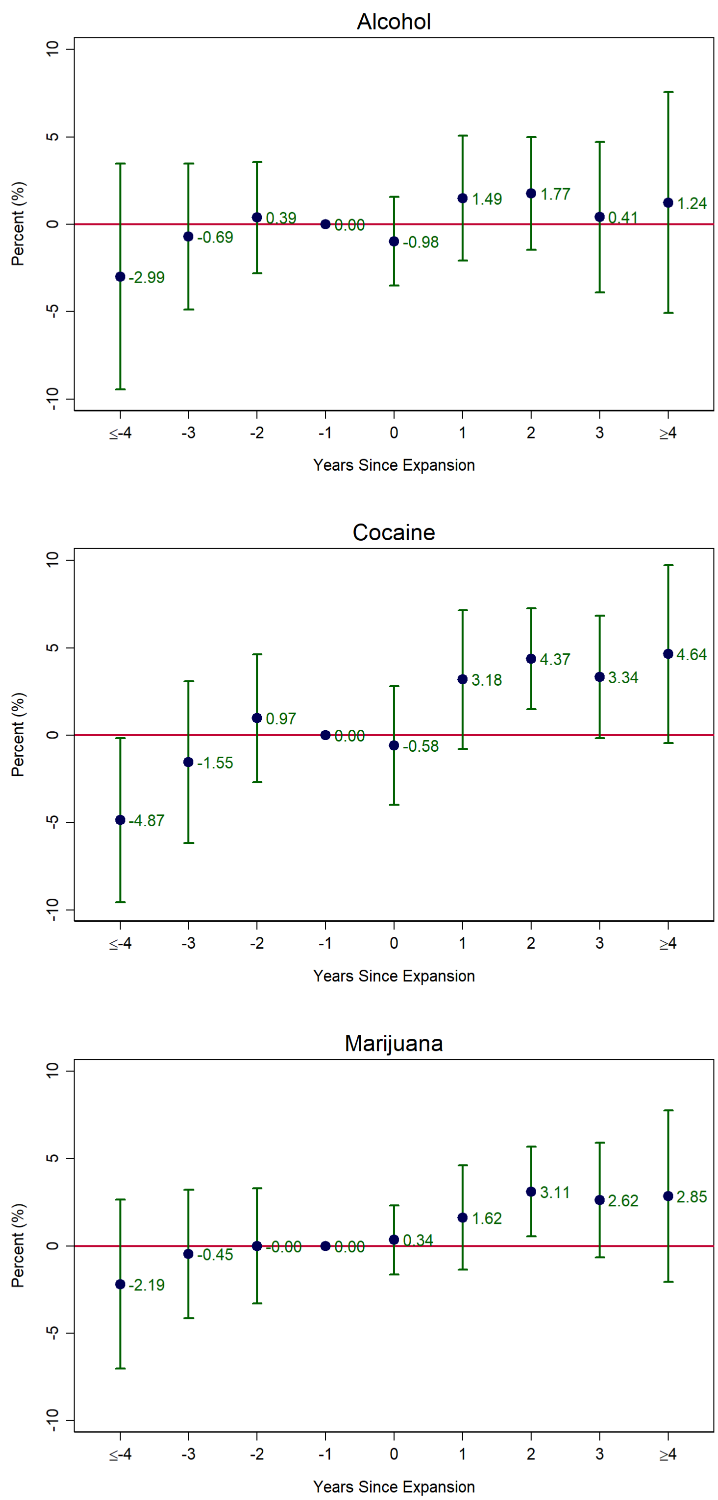

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specification is weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.3: Substance use disorder reported at admission



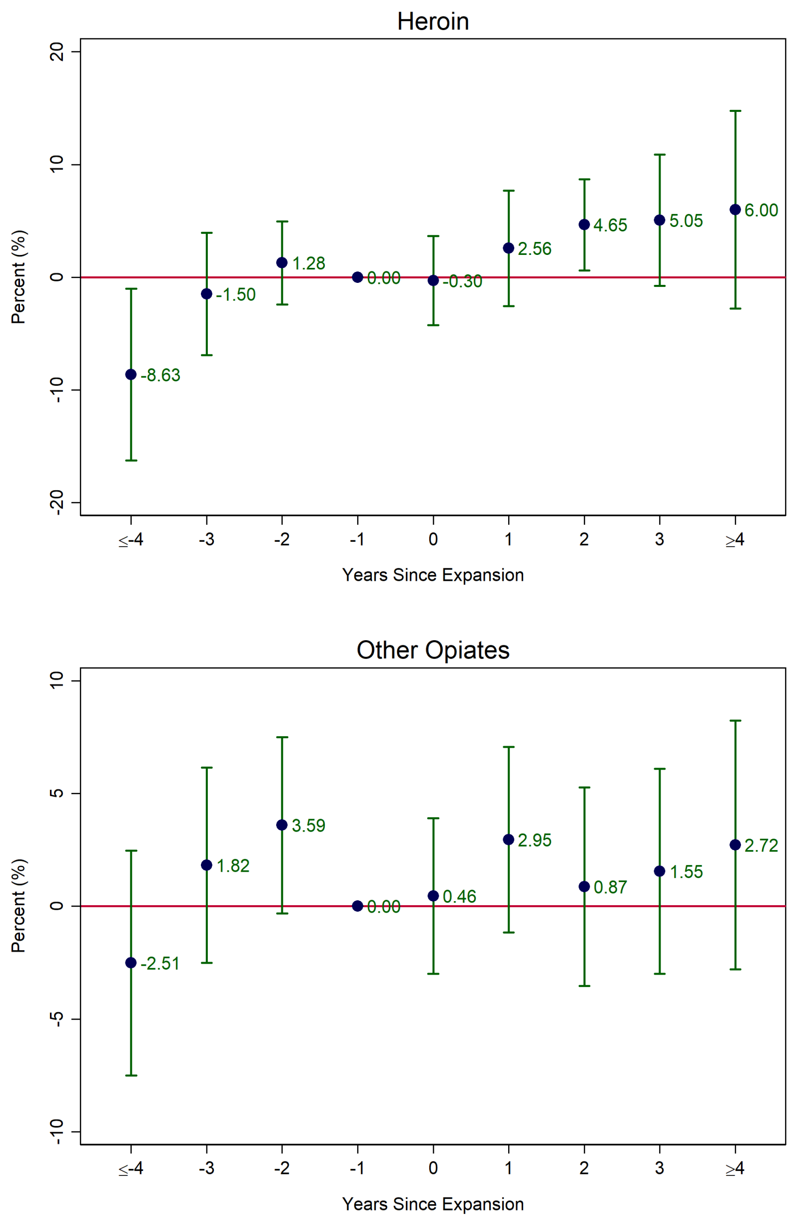

Notes: Data come from TEDS-D 2008-2018. Graph show the frequency a substance being reported. An individual can report up to three substances of use disorder.

Figure 2.4: Effect of Medicaid expansions on SUD treatment completion - by substance



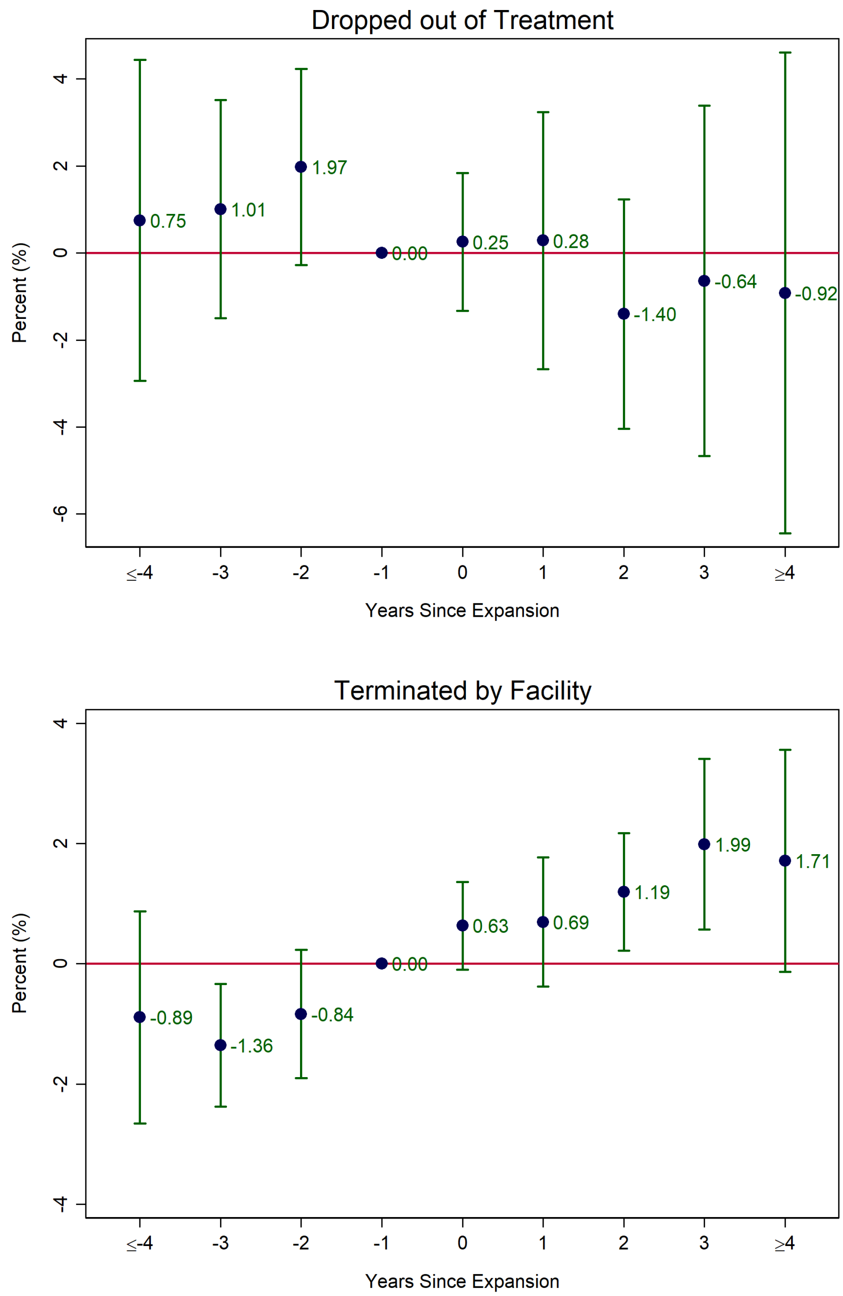

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.5: Effects of Medicaid expansions on SUD treatment completion - by substance (cont.)



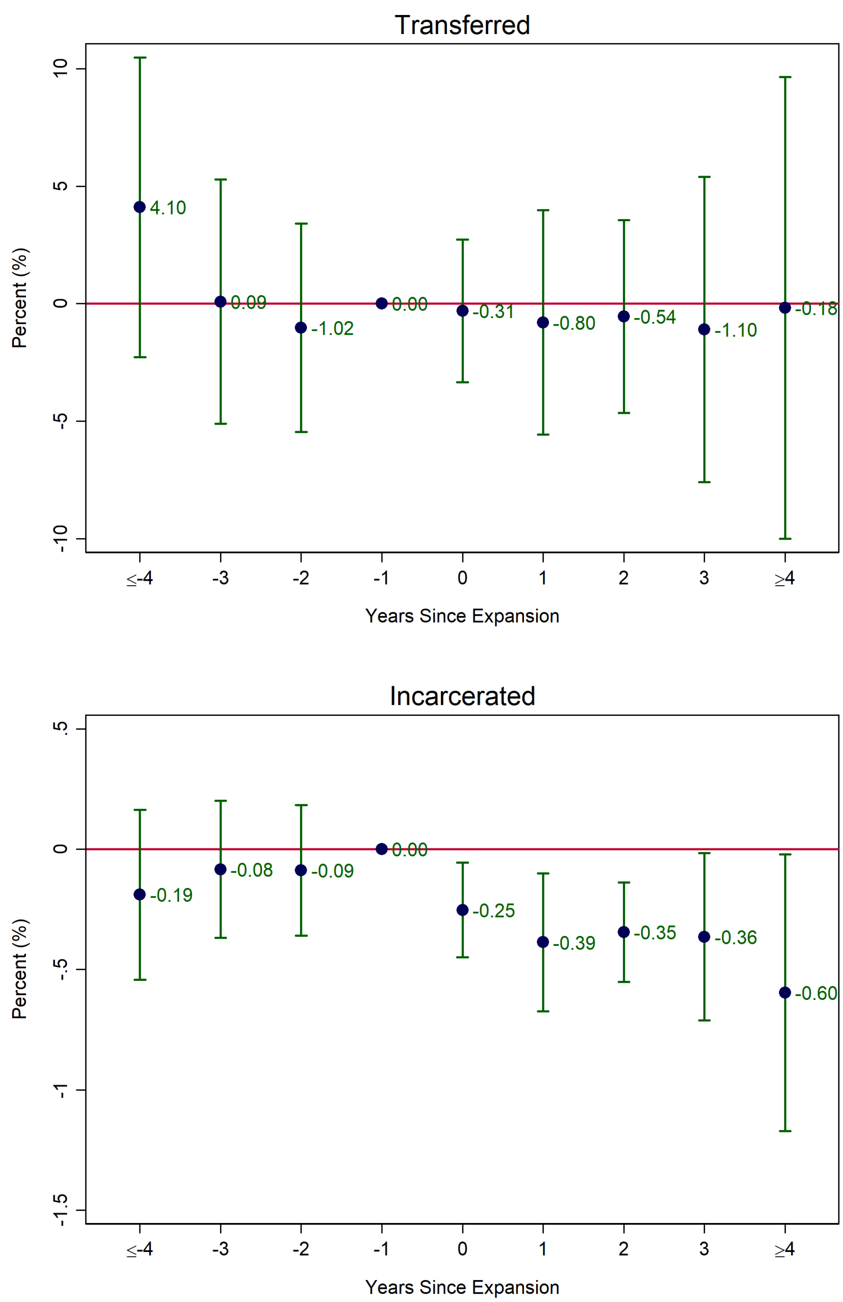

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.6: Effect of the Medicaid expansions on non-completion discharge



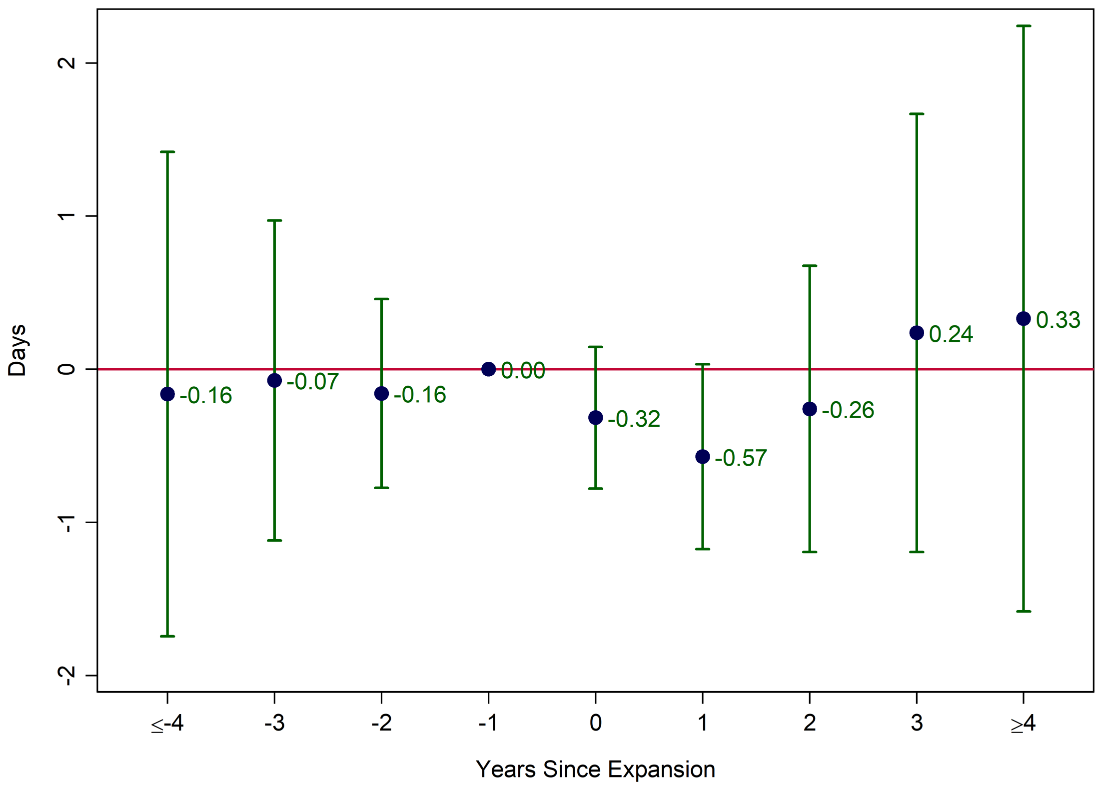

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.7: Effect of the Medicaid expansions on non-completion discharge



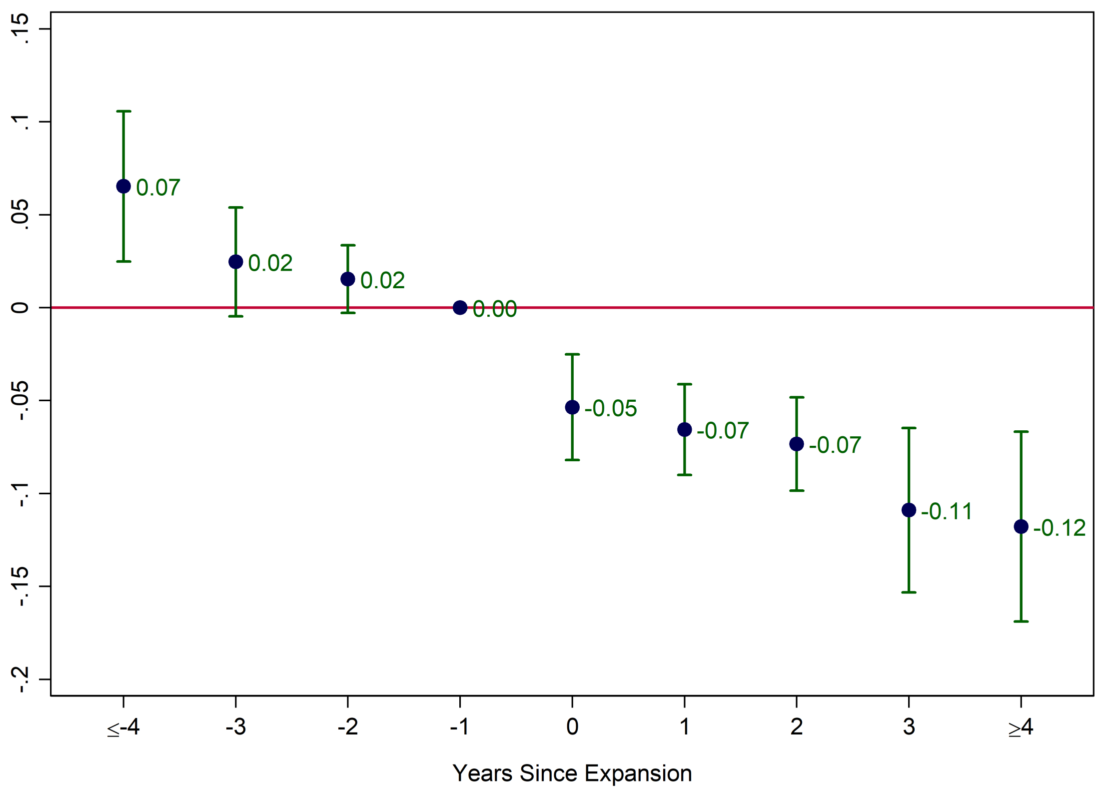

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.8: Effect of the Medicaid expansions on length of stay (≤30 days)



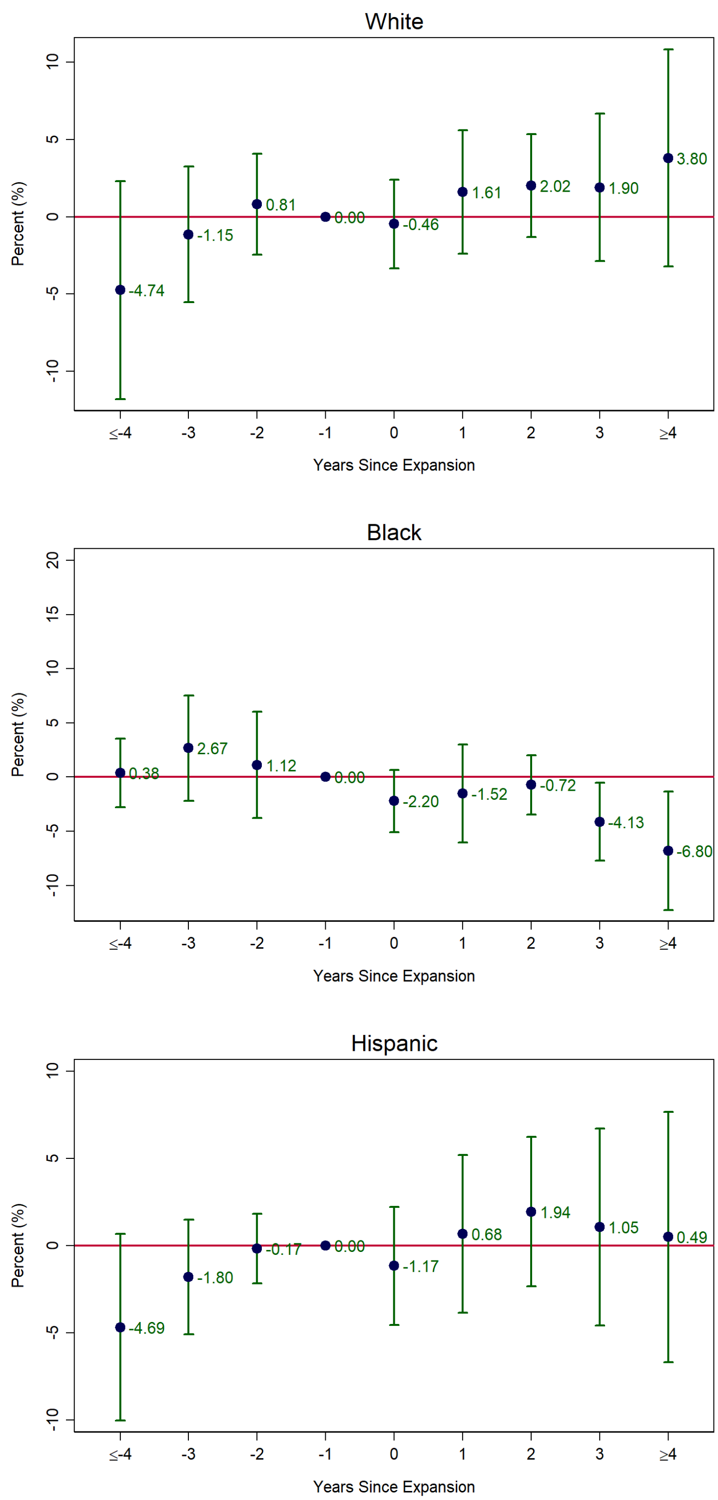

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specification is weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.9: Effect of the Medicaid expansions on stay >30 days



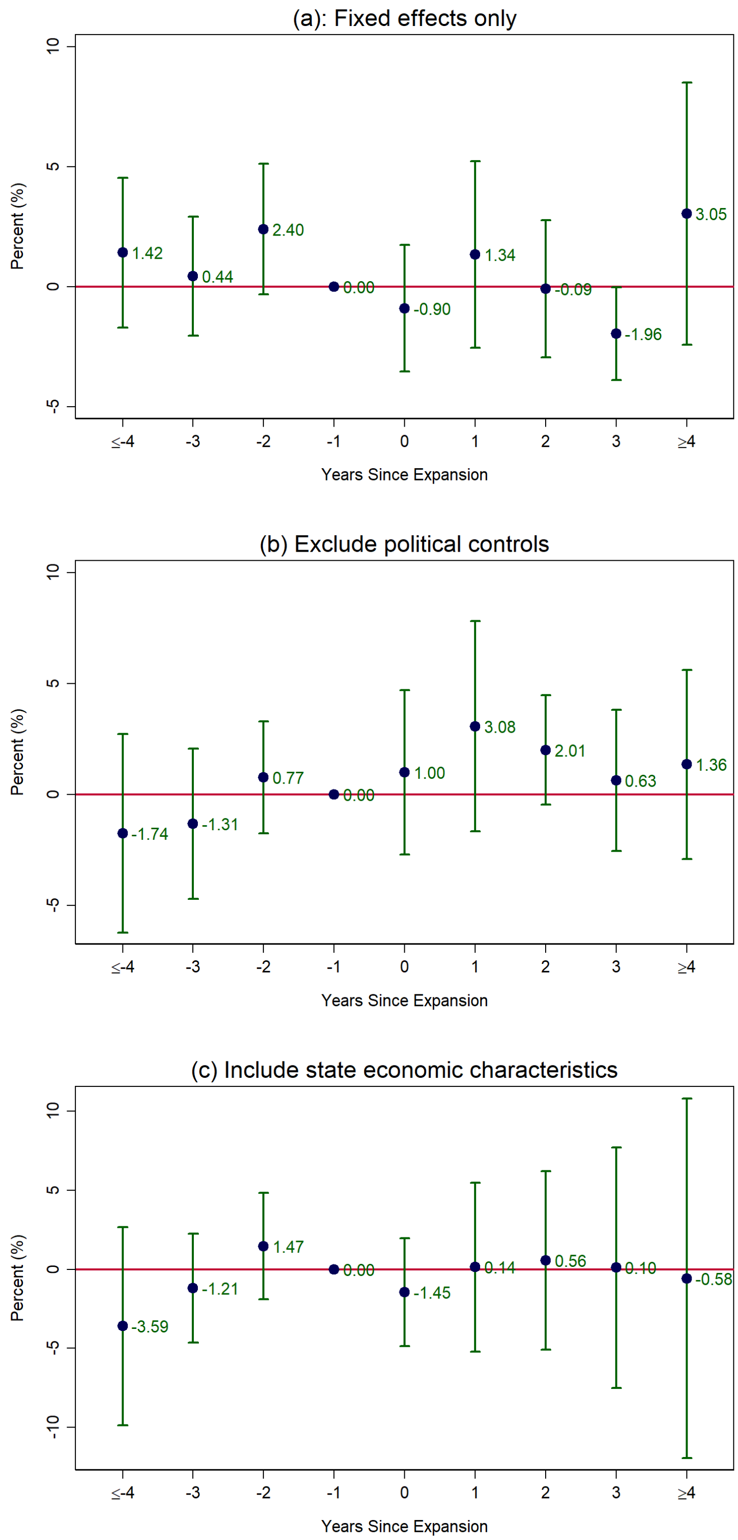

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specification is weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.10: Effect of the Medicaid expansions on racial disparities in treatment completion



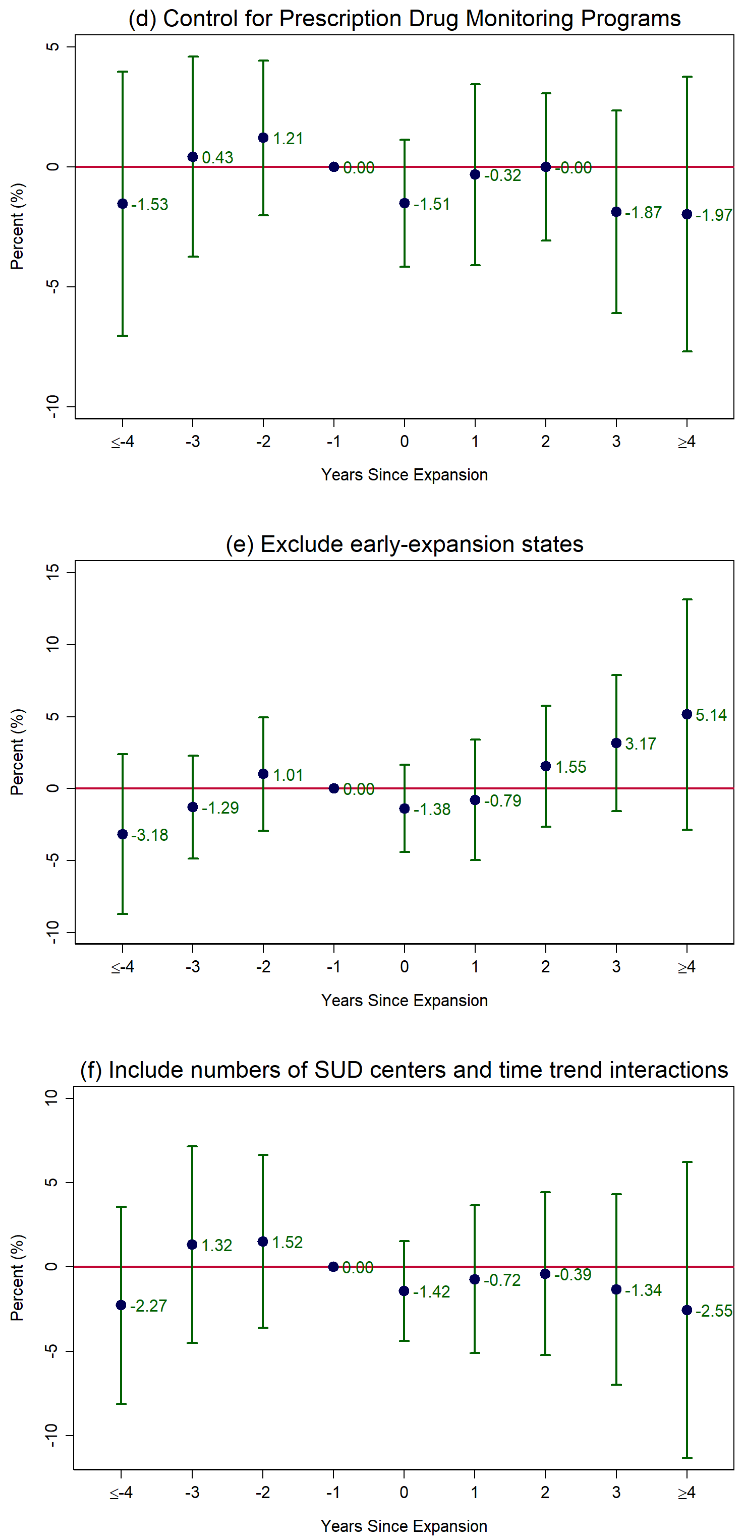

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.11: Robustness checks



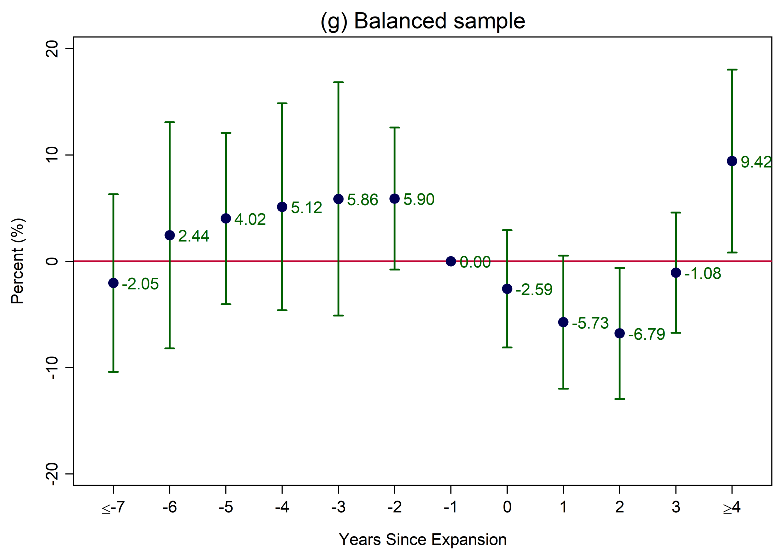

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.11: Robustness checks (cont.)




Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure 2.11: Robustness checks (cont.)




Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state. Sample excludes: CA, CT, DE, D.C., LA, MA, MN, MT, NJ, NY, VT, WA.

# Chapter 3

## School Reopenings, Mobility, and COVID-19 Spread: Evidence from Texas[1]

### 3.1 Introduction

The COVID-19 pandemic has led to gut-wrenching decisions about whether and when to open schools for in-person instruction. Ideally, these decisions would be made from an evidence-based cost-benefit analysis. However, initially there was very little evidence to make these decisions, and only recently has more information become available. On the benefit side, recent research suggests that remote learning leads to significant learning loss, especially among disadvantaged populations (Kuhfeld et al., 2020; Kuhfield and Tarasawa, 2020; Maldonado and Witte, 2020). Remote learning also could lead to delayed social and emotional development and reduced detection of child abuse as teachers are often at the front lines of detection (Schmidt and Natanson, 2020). In addition, remote learning could lead families to make difficult decisions between working and staying home with young children, which could dampen the speed of the economic recovery (Green et al, 2020; Council of Economic Advisers, 2020). Together, this suggests that opening schools could improve student learning and social and emotional development while minimizing the possibility of child abuse.

On the cost side, there are concerns of health risks for students, staff, and the larger community as the openings could further spread COVID-19. These concerns have been championed by teacher unions, which argue that schools should open only when they are safe (Hurt, Ball, and Wedell, 2020). However, a Centers for Disease Control and Prevention (CDC) report examining 17 rural Wisconsin schools using contact tracing found minimal transmission both within and out-

[1]This chapter is joint work with Charles Courtemanche, Aaron Yelowitz, and Ronald Zimmer.

side of the schools (Falk et al., 2021). Other investigations of known cases among students and staff – such as Doyle et al.'s (2021) study of Florida and Emily Oster's K-12 COVID-19 dashboard – tend to reach similar conclusions.[2] Accordingly, the CDC recently concluded that in-person instruction can be carried out safely as long as masks are worn, social distancing is maintained, community spread is low, and other community restrictions (e.g., on restaurants) remain in place (Honein et al., 2021).

However, contact-tracing-based evidence alone is insufficient to fully understand the health implications of reopening schools. Contact tracing is widely known to be inadequate in the U.S. due to insufficient staff and resources to keep up with large numbers of new cases, as well as resistance to provide information among those contacted. For instance, a National Public Radio story found that 27 percent of cases and 43 percent of contacts lacked phone numbers in Delaware, only 44 percent of new cases were reached within 24 hours in New Jersey, and only 4.5 percent and 25 percent of cases could be traced to known contacts in Washington, D.C. and Delaware, respectively (Simmons-Duffin, 2020). Moreover, econometric evidence from Dave et al. (2020) linked more than 100,000 cases to the Sturgis Motorcycle Rally in South Dakota, compared to just 328 identified by contact tracing. This illustrates the potential for contact tracing to substantially underestimate the total number of cases resulting from a particular event after several rounds of exponential spread. One missing link in the contact tracing chain prevents the attribution to the event of any people infected by the missing link, any people those people subsequently infected, and so on.

On the other hand, the number of known cases resulting from in-school spread could overstate the net increase in the number of cases, as it does not account for the counterfactual activities students and staff would be engaging in if schools

[2]Oster's dashboard is available at `https://covidschooldashboard.com/`.

were closed. While some students and staff would stay at home and face little risk, others would go to day care facilities, parks, restaurants, virtual school pods at friends' houses, or other places where adherence to mitigation measures could be lower than in schools (Courtemanche et al., 2020).

Additionally, focusing only on net changes in risk among those attending school misses a potentially important part of the story: spillover effects on the behaviors of parents or others in the community. Kids returning to in-person school may allow parents or other caregivers to return to in-person work or outside-the-home activities, leading to COVID-19 spread in the community even if there is minimal spread in the schools. Spillovers could even extend beyond families directly affected by the return to school, as school openings could signal to the community that it is safe to return to normal activities, again fueling spread (Glaeser et al., 2020). Alternatively, spillovers could reduce spread if people foresee danger from school reopenings and cut back on other activities.

Econometric studies can provide answers to these debates by estimating reduced-form effects that encompass all mechanisms through which reopening schools influences the spread of COVID-19. Three concurrent working papers examine the effects of school openings in Germany (Isphording et al, 2021), Michigan and Washington (Goldhaber et al., 2021, hereinafter we refer to as CALDER study) and the U.S. as a whole (Harris et al., 2021, hereinafter we refer to as Tulane study). While the current versions of these studies find little evidence that reopening schools increases COVID-19 spread on average, the Tulane and CALDER studies find some evidence that this may not be the case in communities with high levels of preexisting transmission.

The above discussion highlights a key distinction: the consensus that schools *can open safely with low community spread and proper safeguards* is not the same as saying that all schools are opening safely. To examine what can happen in a less idealized scenario, we focus on the state of Texas. All of the school districts in Texas reopened for in-person instruction at some point during the 2020 fall semester. Many did so when COVID-19 rates in the community were relatively high, generally without staggered or hybrid strategies to limit the number of students attending at one time.

We estimate the impact of school reopenings in Texas on COVID-19 spread using hand-collected information on school districts' instructional modality and start dates combined with weekly county-level data on confirmed COVID-19 cases and fatalities. Our baseline model is an event study that separately estimates effects for each week in a four-month bandwidth surrounding reopenings. This allows us to assess pre-treatment trends while also allowing impacts to emerge gradually due to incubation periods, testing delays, multiple rounds of subsequent spread, and the fact that COVID-19 deaths tend not to occur quickly. We find that school reopenings in Texas gradually but substantially increased the per capita numbers of new weekly COVID-19 cases and deaths. To illustrate, 95 percent confidence intervals from the baseline regression imply that school reopenings across Texas led to *at least* 43,000 additional COVID-19 cases and *at leas*t 800 additional fatalities after two months. These magnitudes represent 12 percent and 17 percent, respectively, of the total numbers of cases and deaths in the state during that period. Results are qualitatively similar across a wide range of robustness checks, including those that address newly discovered issues with staggered-treatment-time two-way-fixed-effects research designs. Using similar event-study models and SafeGraph data (which tracks the movement of individuals aged 16 and older by using cell phone data), we show that time spent outside the home by adults rose sharply in communities with the largest numbers of children after school reopenings. Some evidence also suggests increased mobility in communities with large numbers of seniors, consistent with signaling effects on those not directly affected by the reopenings.

Overall, we find convincing evidence that opening schools led to community spread, and was likely facilitated by increased mobility, which could arise both directly in schools but also indirectly through the behaviors of parents or other adults. Although the recent distribution of effective vaccines is changing the cost-benefit of the calculations policymakers are making, difficult decisions about schools will likely continue into the 2021-2022 academic year. Children under sixteen years old cannot yet be vaccinated, there are broad geographic pockets across the country with low adult vaccination rates, and the emerging variant B.1.1.7 infects children more easily than prior strains.[3,4]

## 3.2 Background

### 3.2.1 School Reopenings in Texas

On July 7, 2020, the Texas Education Agency (TEA) issued school reopening guidelines, which covered topics such as COVID-19 prevention, responses, mitigation, and information dissemination.[5] These guidelines covered the wearing of masks, reporting of positive cases, and screening of staff, teachers, and students. Most importantly, it provided the following guidance for reopening schools: "during a period up to the first four weeks of school, which can be extended by an additional four weeks by vote of the school board, school systems may temporarily limit access to on-campus instruction."

These instructions were further clarified by a July 17, 2020 joint statement

[3] https://health.usnews.com/health-care/patient-advice/articles/when-will-there-be-a-covid-19-vaccine-for-kids.

[4] https://www.nydailynews.com/coronavirus/ny-covid-variants-michael-osterholm-new-york-20210404-73bhzmgpzremnpr5hirw2eo724-story.html

[5] https://www.wfaa.com/article/news/education/texas-students-must-wear-face-masks-at-school-tea-says/287-e2ef67ef-6ec7-4827-9a80-43fb83932564

from Governor Greg Abbot, Lt. Governor Dan Patrick, Speaker Dennis Bonnen, Senate Education Chairman Larry Taylor, and House Education Chairman Dan Huberty. They stated that local school districts have the constitutional authority to decide when and how schools safely open and noted that local school boards have the authority to set the start date which could be in in "August, September, or even later."[6] They also noted that local school boards can make these decisions "on advice and recommendations by local public health authorities but are not bound by those recommendations." Importantly, the statement also clarified that not only could school districts start the first four weeks as a "back to school transition" with remote instruction, but school districts could extend their back-to-school transition an additional four weeks with a vote of the school board and a waiver from the state. After eight weeks, school districts could ask for an addition extension as the result of health concerns related to COVID-19 and the TEA will decide those requests on a case-by-case basis. Finally, the guidance from TEA noted that school districts must provide the option for families of remote instruction, even if the school district provides in-person instruction. However, because of the challenges of the logistics of providing both in-person and remote instruction, school districts could restrict families to switching their choice of instructional modality only at the end of grading periods.

With this policy context as background, Figure 3.1 displays the start date of opening schools for in-person instruction for school districts in the 2020-21 school year relative to the start date of opening schools in the 2019-20 school year.[7] About two-thirds of school districts opened schools in 2020-21 within one week of the start date of 2019-20 in spite of the widely documented surge in COVID-19 cases

[6] `https://gov.texas.gov/news/post/governor-abbott-lt-governor-patrick-speaker-bonnen-chairman-taylor-chairman-huberty-release-statement-on-school-re-openings`

[7] In most districts, we were able to determine the 2019-20 start date. However, in the cases where we were not able to identify the 2019-20 start date we either used the prior year start date (e.g., 2018-19) or the median 2019-20 start date within the county.

in Texas in the summer of 2020. Moreover, less than two percent of school districts delayed the reopening by more than eight weeks, possibly because of the requirements imposed by the state to obtain an exemption to remain virtual longer than eight weeks. To the extent that state directives trumped local caseloads or politics in influencing reopening decisions, that would help to alleviate endogeneity concerns in our econometric analysis.

### 3.2.2 Econometric Evidence on Schools and COVID-19

As the pandemic began to unfold during the spring of 2020, very little was known about the likelihood of spread among young populations and whether schools could safely operate with in-person instruction. Three early studies that controlled for other accompanying restrictions like restaurant closures and shelter-in-place orders did not find evidence that school closings slowed the spread of COVID-19 (Courtemanche et al., 2020; Hsiang et al., 2020; Flaxman et al., 2020). However, a fourth study that did not control for these other restrictions did find evidence of a sizeable effect (Auger et al., 2020). These prior studies are of limited usefulness for reopening decisions as almost all the spring school closures in the United States occurred within one week of each other, leading to little identifying variation and generally imprecise estimates. While controlling for other types of restrictions is important for causal inference, it further strains the available identifying variation, perhaps explaining the null findings from studies that did so. Further, it is not clear that closings and openings should have symmetric effects. Much more was known about mitigation strategies in fall 2020 compared to spring, but community spread was also much greater in the fall.

Only recently has econometric evidence on reopening schools begun to emerge. Isphording et al. (2020) leveraged variation in the timing of school start dates and found little evidence of effects on community spread in Germany.[8] However, the relevance of this finding for a U.S. population with different attitudes toward COVID-19 and different mitigation policies, both inside and outside of schools, is unclear. Tulane researchers used national insurance claims data and U.S. Department of Health and Human Services (HHS) hospitalization data along with national data on school reopenings to examine the impact of school reopenings on hospitalization (Harris et al., 2021). Overall, they found no association between school reopenings and hospitalization. However, they noted that in areas with higher pre-opening COVID-19 hospitalization rates, the results are less conclusive with some evidence indicating that in these areas, school openings could lead to greater hospitalizations. Given that the data for the study was only collected through mid-fall 2020—prior to much of the national surge of hospitalizations—the study's findings do not necessarily extrapolate to later in the pandemic. Moreover, the sample period only allows for six weeks of post-treatment data, which may not be enough time for meaningful increases in hospitalizations to occur given incubation periods and the potential need for multiple rounds of spread outward from schools before reaching the vulnerable individuals who are most likely to require hospitalization.

Another study, released by a research consortium named CALDER, examined monthly county level COVID-19 cases using school reopening information provided by Michigan and Washington's departments of education (Goldhaber et al., 2021). The researchers noted that in Washington, only 10 percent of districts (almost entirely rural) and only 2 percent of the student population was attending either a school operating with hybrid or in-person instruction. In Michigan, the percentages were higher, with 76 percent of schools operating either with hybrid or in-person instruction. Like the Tulane study, this study examined COVID-19

[8]This result is consistent with two descriptive studies of small sets of schools in France and Helsinki that also found little evidence of spread (Dub et al., 2020; Fontanet et al., 2020).

cases prior to much of the surge of cases in the winter of 2020-21. The research team found that in-person modality options are not associated with increased spread of COVID-19 at low levels of pre-existing COVID-19 cases but did find that cases increase at moderate to high pre-existing COVID-19 rates. Again, the analysis raises questions as to which set of results are more relevant to the COVID-19 conditions of the winter.

We complement these other concurrent studies by examining a state where conditions may have been less than ideal for a safe reopening. First, Texas had relatively high rates of COVID-19 spread in early fall of 2020 that roughly mirrored the national conditions that would emerge toward the end of the semester. To illustrate, Figure 3.2 shows weekly COVID-19 cases per 100,000 residents in Texas compared to Washington, Michigan, and the U.S. as a whole during the latter half of 2020. The vertical lines delineate the weeks of June 20 through October 16 – a four-month period centered on the modal reopening date in our Texas data, and a similar period to that used in the Tulane and CALDER studies. Texas' rate of new cases was substantially greater than those of Washington, Michigan, and the overall U.S. during early fall when the bulk of Texas' schools reopened for in-person instruction.

Additionally, many states opened schools using hybrid models where only partial numbers of students attended schools each day to allow for greater social distancing. In contrast, most Texas schools opened at near capacity. For instance, in reviewing school opening plans of Texas school districts, our best estimate is that over 90 percent of school districts opened fully in-person without any staggered or phased-in attendance. This is in contrast to 42 percent nationally (Harris et al., 2021). In addition, using Texas' Department of State Health Services data on the proportion of students attending in person by late September, we found that out of the 1,049 school districts, 358 had over 90 percent of their students attending in person with 27 having 100 percent.[9] Moreover, studies have shown that residents of politically conservative areas – such as the majority of Texas – are less likely to follow social distancing and mask-wearing recommendations than those in politically liberal areas (Milosh et al, 2020). This could influence the impact of reopening schools on COVID-19 spread in several ways, possibly including weaker enforcement of guidelines at schools and extracurricular activities, greater increases in mobility among parents, a stronger signal to the community that life can return to normal, and less willingness to impose compensatory other aspects of life.

The Texas context offers other advantages as well. Texas allowed districts more discretion in when to open schools than many other states, allowing us to examine the effect of variation in timing of school openings in the context of common statewide mitigation policy, thereby reducing the possibility of omitted variable bias from other restrictions.[10] In addition, in contrast to the Tulane and CALDER studies, which observed only a portion of schools open, every school district in the state eventually opened schools during the time frame of our study. This allows for an examination of wide-ranging school districts including rural and urban, large and small, and non-diverse and racially diverse. In the CALDER and Tulane studies, open schools were disproportionally rural. Finally, Texas has a large number of school districts and counties, which provides statistical power to detect effects. As a source of comparison, while Texas has 254 counties and 1,049 school districts,[11] Michigan has 83 counties and 810 school districts, and Washington has 39 counties and 286 school districts.

[9] `https://dshs.texas.gov/coronavirus/schools/texas-education-agency/`. We excluded charter schools from the analysis.

[10] Restrictions can vary within states, but we were unable to find any instances of cities or counties in Texas imposing or eliminating policies like shelter-in-place orders or mask mandates during our sample period.

[11] One county (Loving) has no schools within the county.

### 3.3 Data

To collect information for each school district's start date and modality, we performed Google searches in which a team of assistants searched for key terms using district name and the phrase "back to school plan".[12] The vast majority of districts had a back-to-school plan and it often included both the district's modality plan for instruction and the school district start date. If the school district started with virtual instruction, the back-to-school plan often listed the planned date for in-person instruction.[13] In cases in which the start date was not listed, the team of assistants searched for the school district's academic calendar. In cases where back-to-school plans or calendars were not available, we also conducted newspaper and Facebook searches to identify this information through news stories and school district's Facebook posts.[14] Even in cases where a back-to-school plan and/or academic calendars were available, we often conducted additional newspaper or Facebook searches to verify the district's start date and modality of instruction.

Because COVID-19 cases and fatalities are only available at the county level, we need to aggregate the school reopening variable from the district to the county

[12] We did not include charter or private schools primarily because they represent a small minority of the total students in the states and also because it would have been difficult to ascertain this information.

[13] In some cases, districts phased in in-person attendance (e.g., Kindergarten through 3rd grade could attend in person one week and the following week the rest of the grades could attend in person). In these cases, we used the first date students were allowed on campus. If the district only allowed special education students on campus, we did not count this as in-person instruction given the small number of students on campus.

[14] After these steps, there were only 11 school districts in which we could not identify the start date and only 17 school districts we could not identify the modality of instruction. We tried to follow up with each district with a phone call. Through these phone calls, we were able to identify the start date for seven of the 11 missing dates for school districts and the missing modality information for 12 of the 17 school districts. Therefore, we had missing dates for four school districts, which we imputed based on the median start date within their county. For modality, we had missing dates of five school districts, which we imputed as the majority instructional modality of the school districts within the county. These are very small districts, with the average size of the missing start date districts being 78 students and the average size of the districts with missing modality information being 177 students. Since our data will be population-weighted, these districts are effectively inconsequential to the results.

level, which requires accounting for the fact that not all districts within a county opened at the same time. In the Tulane study, the researchers defined treatment as occurring when the first district within a county reopened. However, for many districts in Texas, this definition would result in a county being labeled "treated" when only a small fraction of schools is actually open. Consider Bexar County, a large county that includes San Antonio. Southwest Independent School District (ISD), which represents less than 5 percent of the county's student enrollment, was the first district to open schools on August 24, 2020. However, there were some districts within the county that opened up schools as late as seven weeks later and six school districts representing 75 percent of the county's student population opened on September 8, 2020. In this case, defining treatment based on the earliest opening school district would effectively lead to it being assigned two weeks too early relative to the most consequential shock. Therefore, our primary treatment definition is the week in which the county had the largest jump in percentage of county students who could attend a school in person. In the case of Bexar County, that would be the week of September 8th.[15] We should also note that treatment begins for our empirical analysis once schools open for any type of in-person instruction including fully in-person, phased-in (e.g., a subset of grades open for in-person instruction with gradual number of grades eligible to attend in-person over time), or as a hybrid model (e.g., students attending in person part of the week and attending virtually the rest of the week). However, as discussed previously, phased-in and hybrid reopenings were rare in Texas.

It should also be noted that in opening schools for in-person instruction, districts almost uniformly allowed families to choose to attend in-person or remotely. However, districts had to prepare for the possibility that all or nearly all students

[15]Later, we present a series of analyses that suggests our results are robust to alternative definitions of treatment including using the first school district that opened schools in person, 50 percent of the county enrollment is open for in-person instruction, and 20 percent of the county enrollment is open for in-person instruction.

could attend in person. Therefore, our main treatment variable could be thought of as "intent-to-treat" (ITT) analysis as a district's decision to provide in-person instruction is providing the opportunity for all students to attend in person. That said, schools in Texas tended to open at relatively close to full capacity as nearly 60 percent of all school districts had 80 percent or more of their students enrolled for in-person instruction by the end of September. Our analysis is also an ITT analysis in a second way. Once treatment begins by a school district opening schools for in-person instruction, we consider the school opened throughout the analysis, even if the school has a temporary shutdown as a result of an outbreak. In defining treatment in this way, our estimates should be seen as conservative estimates.

Our COVID-19 data come from the Texas Department of State Health Services (TDSHS).[16] Numbers of COVID-19 cases, fatalities, and tests are recorded daily at the county level from May 3, 2020 through January 3, 2021. We use weekly (Sunday through Saturday) data instead of daily data because not all labs are open daily or do not report daily (e.g., many labs are not open on weekends) and can have duplicate numbers or reporting errors, which can lead to oscillating numbers from one day to the next. By using weekly numbers, we are largely able to smooth out these fluctuations.[17] To account for variations in county population, we calculated COVID-19 cases, fatalities, and tests per 100,000 residents using 2019 county population estimates from the Census Bureau.[18] These cases and

[16] `https://dshs.texas.gov/coronavirus/additionaldata.aspx`

[17] It should be noted that some data errors within the TDSHS data systems have been discovered over time as documented by media accounts: `https://www.khou.com/article/news/health/coronavirus/texass-record-high-covid-positivity-rate-falls-after-data-experts-investigate/287-ffc19167-0d47-4be9-8c06-8648229288ef` and `https://www.texastribune.org/2020/09/24/texas-coronavirus-response-data/`. Corrections to these errors could cause accumulated cases or tests to decrease over time as the data are corrected. These anomalies should create noise, but not bias and should largely be accounted for in our analysis using week fixed effects.

[18] `https://www.census.gov/data/datasets/time-series/demo/popest/2010s-counties-total.html`

fatalities variables will be our main outcome variables, while the testing variable will be a control in the cases regressions.

To help understand potential spillover effects of school reopenings on adult mobility, we utilize Social Distancing Metrics (Version 2.1, "SDM") data provided by SafeGraph, Inc., from May 3, 2020 to January 3, 2021.[19] SafeGraph collects information on almost 45 million cellular phone users, including about 10 percent of devices in the U.S. The sample correlates very highly with the true Census populations with respect to distribution by county, educational attainment, and income.[20] These data are aggregated from GPS pings provided by cellular devices that have opted-in to location sharing services from smartphone applications. The device data is aggregated by Census Block Group (CBG) and day, based on a device's "home" location.[21] In our timeframe, there were 15,705 CBG's overall in the Texas SDM; on an average day, more than 1.9 million devices were followed in Texas. For our analysis, we restricted the sample to a balanced panel of 14,580 CBG's (with more than 1.6 million overall devices on an average day).[22] The typical CBG had approximately 112 devices. We created samples at the weekly

[19] `https://www.safegraph.com/blog/stopping-covid-19-with-new-social-distancing-dataset`

[20] `https://www.safegraph.com/blog/what-about-bias-in-the-safegraph-dataset`

[21] To impute a "home" location for a cellphone user, SafeGraph considers a common nighttime location of each mobile device. In the entire United States, the SDM is aggregated to approximately 220,000 CBGs. To enhance privacy, CBG's are excluded if they have fewer than five devices observed in a month.

[22] CBG's were excluded if (a) the CBG was not observed for all days in our sample period, (b) the CBG could not be merged to demographic information from the 2018 American Community Survey (ACS) 5-year estimates, (c) the CBG's population – according to the 2018 ACS – was in the bottom or top 1 percent of the full distribution (corresponding to 391 and 7150, respectively), or (d) over the course of the panel, relative to the mean device count in the CBG, any specific CBG-day observation had a device count that more than twice the mean or less than half the mean. By restricting to CBG's with relatively stable numbers of devices over the long panel, we hope to avoid complications related to installation and removal of apps, inactive devices, and sample attrition highlighted in some other studies (Andersen et al., 2020; Allcott et al., 2020). Although Safegraph reports that some apps implement GPS collection methods that depend on the movement of the device (rather than a fixed time interval), this would likely affect levels of certain metrics (e.g., completely home all day) but not changes.

level for the full week (Monday through Sunday), for weekdays (Monday through Friday), and for weekends (Saturday and Sunday).

We utilize four of the mobility measures provided in the SDM that are often used in other studies. The most commonly used measure is the fraction of devices that do not leave their home location during a given day ("Percent Completely Home").[23] We also use two "work" measures. SafeGraph defines "work" as either the fraction of devices that spent more than 6 hours at a non-home location between 8am-6pm ("Percent Full Time") or fraction of devices that spent between 4-6 hours at a non-home location between 8am-6pm ("Percent Part Time").[24] Finally, several studies have examined median time spent away from home (or at home).[25] These measures are based on the observed minutes outside of home (or at home) throughout the day, regardless of whether these time episodes are contiguous. The time during which a smartphone is turned off is not counted towards the measures.

Finally, for some of our analyses, we utilize county-level variables from other sources. The county's college enrollment is available from the U.S. Department of Education National Center for Educational Statistics (NCES).[26] Percent of voters who voted for President Trump in the 2016 presidential election comes from the

[23]See Bailey et al. (2020), Bullinger et al. (forthcoming), Cronin and Evans (2020), Allcott et al. (2020), Dave et al. (2020a), Simonov et al. (2020), Dave et al. (2021), Friedson et al. (Forthcoming), and Gupta et al. (2020).

[24]See Bullinger et al. (forthcoming) and Simonov et al. (2020).

[25]See Allcott et al. (2020), Dave et al. (2020a), Cotti et al. (forthcoming), and Gupta et al. (2020).

[26]These data were collected at `http://nces.ed.gov/ccd/elsi/`. The reporting years of enrollment ranged from 2013-2017. As part of the data cleaning process, for residential campuses only, we assumed all enrolled students could attend classes in person and therefore, we calculated the maximum weekly proportion of the total county population that could be on campus by dividing the number of enrolled students by the county population. To calculate the daily proportion of college students of the total county population, we assumed that no students were on campus during the summer (nearly all colleges did online instruction over the summer). We also assumed all residential colleges had in-person classes for the fall semester. For those colleges with no residential students, we assumed the colleges were providing instruction either online or had minimal student interactions. Using Google searches of academic calendars, we identified the start date for each college, which is the day we assumed students began interacting on campus. In many counties, there are multiple colleges with different start dates, which means the college proportion changes over time as more and more colleges start their fall sessions.

MIT Election and Data Science Lab (2018). We control for average weekly temperature, precipitation, and snowfall using data collected by the National Oceanic and Atmospheric Administration (NOAA) and the Global Historical Climatology Network.

Our main analysis sample contains a balanced event-time window surrounding treatment, i.e. the week of the county's largest increase in percentage of students who can attend in-person school. For the COVID-19 outcomes, we include eight weeks prior to treatment, the treatment week, and eight weeks after treatment. A lengthy post-treatment period allows for multiple rounds of spread (e.g. from student to parent to grandparent), incubation periods, time to receive and obtain results from a test, and the fact that deaths can occur weeks after infection. On the other hand, a long post-treatment period faces a relatively high risk of confounding from other concurrent shocks. In our case, the holiday break – which started in many Texas districts after the week of December 13 – is a particular concern, as schools being "reopened" should not influence spread when they are not in session. In our view, an eight week post-treatment window best balances these considerations. It is long enough to plausibly capture much of the dynamics of the treatment effect. At the same time, it is short enough to avoid sample windows that stretch past the week of December 13 for all but two small counties (Starr and Zavala) that will have little influence in our population-weighted sample. For the SafeGraph mobility outcomes, there is not a clear reason to expect a lag before treatment effects emerge, so we limit the event-time window to six weeks on each side, thereby ensuring that the sample window does not extend past the week of December 13 for any county.

Table 3.1 shows means and standard deviations for our outcome variables in both the pre- and post-treatment periods, weighted by population. Interestingly, new cases per capita were about the same in the pre- and post-treatment periods, while death rates went down by almost 50 percent. This was in spite of a moderate increase in mobility across all four measures. Of course, numerous factors affect these flat or downward trends, including better understanding of preventive measures such as mask-wearing, advancements in treatments, and the average age of cases gradually becoming younger. A finer-grained econometric analysis is necessary to disentangle the causal effects of school reopenings from these underlying trends.

Table 3.2 shows results from a simple cross-sectional regression of week of reopening (ranging from 14 to 28, with week 1 being the week of May 3) on several county-level variables that might be expected to influence reopening decisions: President Trump's 2016 vote share, percent Hispanic, percent Black, county population, and percent of the SafeGraph sample who stayed completely at home for the day in the four weeks prior to any schools reopening (a proxy for compliance with public health guidelines), and average weekly new cases per capita in the four weeks prior to any schools reopening. We standardize the covariates to allow a direct interpretation of the magnitudes. Trump vote share is the dominant predictor, which is consistent with previous research that showed politics drove school opening decisions (Valant, 2020). Each standard deviation increase in Trump vote share is associated with schools reopening 1.22 weeks sooner. In contrast, none of the other variables are statistically significant, and none have a magnitude greater than 0.17 weeks. The coefficient for pre-school-year caseloads is nearly zero, and its p-value is nearly 0.9. Therefore, reopening decisions appear to have been driven much more heavily by politics than public health considerations, which may be surprising but is consistent with prior research (Valant, 2020). This can be seen as favorable for an econometric analysis, as it suggests that reverse causality from caseloads influencing reopening decisions should not be a concern. We will be able to account for stable county characteristics such as political views by including county fixed effects.

### 3.4 Econometric Methods

We aim to identify the causal effects of school reopenings on new weekly COVID-19 cases and fatalities per 100,000 residents by estimating event-study regression models of the form

$$Y_{ct} = \beta_0 + \sum_{i=-8, i \neq -1}^{8} \beta_{1i}(Open_{c,t-i}) + \beta_2 Tests_{ct} + \alpha_c + \tau_t + \varepsilon_{ct} \tag{3.1}$$

where the subscripts c and t represent county and week; y is the case or fatality outcome; OPEN is the reopening indicator; TESTS is a control variable for the number of COVID-19 tests per 100,000 residents,[27] included since differential testing rates across locations and time can be an important driver of confirmed case numbers; $\alpha$ and $\tau$ are county and time fixed effects; and $\varepsilon$ is the error term. Observations are weighted by county population, and standard errors are robust to heteroskedasticity and clustered by county.

The summation term for the treatment variable reflects the inclusion of separate indicator variables for whether schools will reopen eight weeks after week t, seven weeks after, six weeks after, etc., down to two weeks after; whether schools reopened exactly in week t; and whether schools reopened one week before week t, two weeks before, etc., up to eight weeks before. The variable for whether schools will reopen one week from now is omitted as the reference period. The "lead" terms (weeks until school reopening) measure pre-treatment trends, while the "lag" terms (weeks after school reopening) measure the evolution of the treat-

[27] Since test results might not be recorded in the same week that the test was conducted, we experimented with including lags of the testing variable, finding that the contemporaneous value as well as two weekly lags were statistically significant. We therefore include all three of those variables in the regressions.

ment effects over time. As discussed above, we expect the effects on new cases to grow over time because of the incubation period, the lag between symptom onset and receiving a test, the time required to obtain test results, and the exponential nature of case growth. For fatalities, we expect an even longer lag since deaths typically occur after an extended battle with the illness.

We also estimate a number of variants of our baseline event-study specification as robustness checks. The first three checks add variables in an effort to address possible omitted variable bias concerns. Causal inference in our event-study model requires the assumption that case and death trajectories would have evolved similarly in early versus late reopening counties in the counterfactual in which schools did not reopen. The pre-treatment trends estimated using the lead terms in the event-study model are informative as to how case and death trajectories would have evolved in the counterfactual scenario. However, it is possible that some confounders did not emerge until the post-treatment period. For instance, most Texas colleges and universities opened for in-person instruction at the start of the fall semester. If these post-secondary reopenings fueled COVID-19 spread and if school reopening dates were also systematically correlated with the prevalence of college students in the county, this could bias our estimators for the school reopening coefficients. We therefore estimate a model that controls for college and university reopenings in a dose-response, event-study manner. Specifically, we construct a variable for the proportion of a county's population that attends an in-session post-secondary institution in a given week. We then interact this continuous "dosage" measure with indicators for each of the eight weeks before and after the first college reopening in the county. Our second robustness check controls for time-varying unobservables more generally by including linear county-specific time trends.

For our third check, recall that the results from Table 3.2 showed that vote share for President Trump was the dominant predictor of reopening week. Residents' political views are presumably fixed during a two-month sample period, meaning that they are captured by the county fixed effects. However, it is possible that political views could influence not only levels of new COVID-19 cases but also trends, and county fixed effects alone would not account for the latter. If heavily Republican counties opened schools relatively early and also developed steeper COVID-19 trajectories in the fall for reasons besides school reopenings, our estimated effects of reopenings would be biased upwards. We therefore estimate a model that adds interactions of time-invariant Trump vote share with each week fixed effect, thereby flexibly allowing for right- and left-leaning counties to have different COVID-19 trajectories.

The next series of robustness checks utilize alternate constructions of the key variables. First, instead of defining reopening as occurring in the week with the largest increase in the percentage of a county's students who attend schools that reopened for in-person learning, we use the week during which the county (a) crossed over the 50 percent threshold for students attending reopened schools, (b) crossed over the 20 percent threshold, and (c) had its first reopening. The latter is the treatment definition used by the Tulane study. Next, two checks consider alternate functional forms for the case and fatality outcomes: (a) exponential growth rate in cumulative cases (computed as the difference in the natural logs of cumulative cases from one week to the next) and (b) the natural log of the count of new cases.

Our next two checks vary the way in which we control for COVID-19 testing, since changes over time in the number of tests performed could be endogenous to the trajectory of new infections. First, we simply drop the testing variables. Second, we control for the number of new negative tests per 100,000 residents rather than total tests, as those might arguably reflect availably of tests rather than level of virus in the community.[28]

The next group of robustness checks varies sample construction. Two checks shorten the sample window from eight weeks on each side of treatment to six and four, respectively. Next, we test whether the results could be driven by a small number of unusual counties by dropping (a) the county of El Paso, which experienced dramatically more COVID-19 spread than any other county in Texas during our sample period, and (b) all six counties with more than a million residents. Finally, we consider a different way to ensure that the results are not driven exclusively by large, urban areas by re-estimating the baseline model without weighting observations by county population, thereby making the estimates reflective of effects in the average county (with each county counting equally), as opposed to average effects across Texas as a whole. We also examine the robustness of the findings by re-estimating the main model leaving out one county at a time, for the six largest counties with population exceeding one million.

Finally, an emerging literature documents problems with two-way fixed-effects (TWFE) models with staggered treatment times.[29] First, TWFE regressions give more weight to observations treated in the middle of the sample period, which can lead to unreliable estimates of the average treatment effect if treatment effects are heterogeneous. Using the event-study formulation with a balanced panel and a sample period centered around treatment time rather than calendar time alleviates this concern. Since each county has exactly eight pre-treatment observations, one observation during the treatment week, and exactly eight post-treatment observations, the variance of each treatment variable is identical for each county.

[28]Note that, since we did not control for testing in the baseline fatalities regression, we do not perform the robustness checks involving testing for that outcome.

[29]This literature includes Callaway and Sant' Anna (forthcoming), de Chaisemartin and D'Haultfoeuille (2020), Goodman-Bacon (forthcoming), and Sun and Abraham (2020). Our discussion in the remainder of this section is based on reviews of this emerging literature by Baker et al. (2021) and Cunningham (2021, pp. 461-510).

More troublesome in our context is that, in settings that rely exclusively on variation in treatment timing for identification as opposed to having control units, two-way fixed effects models implicitly use early treated units as controls for later treated units. This leads to bias when treatment effects are dynamic because the response of the early treated units is still evolving at the time that they are called upon to be controls, effectively leading to a violation of the parallel trends assumption for those particular late-versus-early comparisons. Event-study models do not necessarily alleviate this concern. Under the assumption that the treatment effect either strengthens or stays the same over time, the bias is toward zero and we can conclude that, if anything, our estimates are conservative. We find this assumption plausible for COVID-19 outcomes; as discussed above, all the reasons to expect treatment effects to evolve over time point towards them becoming stronger rather than weaker.

Nonetheless, we conduct two robustness checks that utilize newly developed methods that address this issue. Both of these methods perform well in simulations and applications conducted by Baker et al. (2021). First, we employ the "stacked regression" strategy used by Cengiz et al.'s (2019) study of four decades of state minimum wage increases. This method begins by constructing new datasets for each treatment event (each county's school reopening) along with corresponding "clean controls", defined as those counties whose school reopenings did not occur within eight weeks on either side of the reopening week of the focal county. Then, we combine the resulting datasets into a single "stacked" sample and re-run the baseline regression, except adding interactions of indicators for each underlying dataset with each of the county and week fixed effects (as well as, when COVID-19 cases is the outcome, the testing controls). Standard errors are clustered by county to prevent the duplication of data from leading to over-rejection of the null hypotheses. Our other robustness check implements the method of Callaway and Sant' Anna (forthcoming), which first estimates dynamic treatment effects for units treated at each time period, then combines them by weighting by sample share rather than treatment variance. This method also purges the potentially problematic late-treated versus early-treated-as-control comparisons from the identifying variation.[30,31]

### 3.5 Results

Figure 3.3 displays the event-study results for the baseline model with new COVID-19 cases per 100,000 residents as the outcome. The dots indicate the coefficient estimates for each week of event time relative to the reference period of one week before reopening. The bars represent 95 percent confidence intervals, meaning that a variable is statistically significant at the 5 percent level if its bar does not cross the horizontal zero line. As a point of reference for evaluating magnitudes, recall from Table 1 that the pre-treatment sample mean for the dependent variable is 147.7 cases per 100,000.

The results provide evidence of a positive, large, and causally interpretable effect of reopening schools on COVID-19 cases per 100,000 residents. The coefficient estimates associated with the negative event time terms show little evidence of

[30] To implement this method, we use the open-source STATA and R packages provided by Jonathan Roth and Pedro Sant'Anna `https://github.com/jonathandroth/staggered#stata-implementation`. For COVID-19 cases, the method requires us to drop three counties that are the only county treated in a particular week. For fatalities, we encounter a problem with singular variance matrix because small counties tend to have weeks in which there were zero deaths reported. We therefore limit the sample to counties with more than 19,000 residents and shorten the event study window to seven periods before and after reopening to avoid unbalanced treatment groups.

[31] Note that we do not also present results from the Goodman-Bacon (forthcoming) decomposition because that is designed for two-way fixed effects models with a single treatment variable, rather than for event-study models like ours with numerous treatment variables. That said, if we run a basic TWFE regression with a single treatment variable, the decomposition shows that the treatment effect estimate is driven roughly equally by early-treatment versus late-treated-as-control and late-treatment versus early-treated-as-control comparisons. The estimated treatment effect from the former is more strongly positive than that from the latter, consistent with dynamic treatment effects causing a bias toward zero when early-treated units are used as controls, as discussed above.

problematic pre-treatment trends. The line is nearly straight, the point estimates are all negative but never larger than 33.5 (22.7 percent of the sample mean), and only one of the eight estimates is statistically significant at the 5 percent level relative to the reference period. The coefficient estimates from the post-treatment period show that a statistically significant increase in cases emerges two weeks after reopening, consistent with the expected lag between exposure and confirmation as a case. The effect then grows over time before stabilizing in weeks six through eight at slightly over 100 new cases per 100,000 residents. This effect size is substantial, as it represents more than two-thirds of the pre-treatment sample mean. The confidence intervals are large, but even the low end of the 95 percent confidence interval for the week eight coefficient estimate would represent a non-trivial 17 percent increase relative to the pre-treatment mean.

The results from the robustness checks for new cases, shown in Appendix Figures C.1 through C.7, are broadly similar. In all regressions, the estimated effect of reopening schools is positive, with magnitudes and levels of statistical significance that exhibit the general pattern of strengthening over time (although individual coefficient estimates occasionally deviate from that pattern). In the thirteen robustness checks that have magnitudes that can be compared to those from the baseline specification (the exceptions being the two with dependent variables that have different scales), the coefficient estimates eight weeks after reopening range from 75 to 194 new cases per 100,000. The baseline estimate of 108 is therefore towards the more conservative end of this range.

Figure 3.4 shows the baseline results for weekly deaths per 100,000 residents, which has a pre-treatment mean of 3.51. As with cases, the results suggest a positive causal effect of school reopenings. No clear pattern emerges in the pre-treatment period, and none of the eight negative event time terms are statistically significant at the 5 percent level. A statistically significant increase in deaths emerges two weeks after reopening, and the effect strengthens over time, reaching 2.37 after eight weeks. This magnitude again represents more than two-thirds of the pre-treatment sample mean, and the low end of the 95 percent confidence interval is a still sizeable 0.97, or 28 percent of the pre-treatment mean.

The results from the robustness checks, presented in Appendix Figures C.8 through C.13, are again broadly similar in terms of signs and significance. In the regressions where magnitudes are directly comparable, the effect after eight weeks ranges from 0.88 to 4.6, putting our baseline estimate of 2.37 towards the middle. While the general pattern of positive and strengthening effects persists across specifications, the standard errors tend to be much larger (as a percent of the outcome mean) for the deaths regressions than the cases regressions, and individual coefficient estimates therefore lose statistical significance in some of the checks more frequently. In particular, the standard errors in the late event time periods are extremely large using the Callaway and Sant' Anna method, making it impossible for plausible effect sizes to be statistically significant. However, that regression nonetheless produces some of the largest point estimates out of all the robustness checks, with the increase in deaths per 100,000 residents reaching 4 after eight weeks.

Finally, in Appendix Figure C.14, we show the coefficient on the event study model from week +8, leaving out one large county at a time for the six counties in Texas with population exceeding one million. The conclusions for both cases and fatalities are very similar from all specifications, suggesting that the results are not driven by any one large county.

In order to help assess the practical significance of the results, we utilize the estimates from the baseline models for cases and fatalities to predict how Texas' COVID-19 trajectory would have evolved differently if schools had not reopened. As discussed above, the generally large confidence intervals associated with our estimates mean that relying exclusively on point estimates for these calculations could be misleading. We therefore also perform a more conservative simulation using the low end of the estimates' 95 percent confidence intervals.

First, we compute the predicted number of cases attributable to school reopenings. Our point estimates for reopening in the present week, the prior week, two weeks ago, and so on out to eight weeks ago, are 11.78, 20.96, 58.97, 42.35, 61.68, 58.27, 100.39, 109.91, and 109.99, respectively. These estimates imply that at the end of the reopening week, there would have been 11.78 fewer cases per 100,000 residents. The first full post-treatment week adds another 20.96 extra cases per 100,000 residents, for a total of 32.74. After eight post-treatment weeks, the cumulative number of extra cases is the sum of all nine coefficient estimates, which is 574.3 per 100,000 residents. Since our regression is weighted by population, our estimates are interpretable as average effects across all of Texas. Therefore, the total number of extra cases is given by multiplying 574.3 by the state's population of 28,995,712 and then dividing by 100,000, yielding 166,521. According to our data, there were a total of 373,323 new cases in Texas in the nine weeks included in our post-treatment window (including the treatment week itself). Therefore, the point estimates imply that Texas' caseload would have been almost 45 percent lower during that time had schools not reopened.

As stated above, we caution against a literal interpretation of that number given the relatively wide confidence intervals associated with our estimates. A safer interpretation can be obtained by instead using the low end of the 95 percent confidence interval to determine the minimum number of cases attributable to school reopenings implied by our results. The low end of the 95 percent confidence intervals associated with the variables for the treatment week and each of the eight post-treatment weeks are -2.88, -13.67, 9.39, -2.72, 12.74, 5.30, 51.12, 55.5, and 33.37, for a total of 148.15. Scaling up to the population of Texas yields a minimum of 42,956 cases attributable to school reopenings in the nine subsequent weeks, or 11.5 percent of the state's total caseload during that time.

The same process can be used to compute the number of fatalities attributable to school reopenings. The baseline regression's point estimates for the treatment week and eight post-treatment week variables are 0.35, 0.61, 0.91, 1.3, 1.21, 1.51, 1.98, and 2.36, for a total of 2.36 deaths per 100,000 residents, or 3,021 across the state of Texas. The corresponding low ends of the 95 percent confidence intervals are -0.05, 0.18, 0.35, 0.54, 0.44, 0.61, 0.83, and 1,07, which sum to 1.07 fatalities per 100,000 residents, or 818 total across the state. During the time frame, there were 4,796 COVID-19 fatalities in Texas, so the point estimates imply that 63 percent of them were due to school reopenings, while the confidence intervals imply that *at least* 17 percent of them were.

In sum, even under conservative assumptions, reopening schools had a meaningful impact on both COVID-19 cases and associated fatalities in Texas. It is noteworthy that the percentage impacts on both outcomes are roughly similar. Ex ante, one might have expected the increase in deaths to be much smaller proportionally than the rise in cases. COVID-19 mortality rates are nearly zero for children and are much smaller for the working-age adults who comprise the majority of school teachers and staff than they are for elderly or vulnerable adults. Our results therefore suggest that school-reopening-induced COVID-19 spread is reaching more vulnerable segments of the population. One possible explanation is secondary spread, where infected kids or employees spread the virus to older, more at-risk individuals. However, this explanation appears incomplete, as it would imply a several-week lag between new cases and new deaths, which we do not observe in the data. Another possibility is spillover effects, where schools opening signals to the community that it is safe to return to normal activities including returning to in-person work, leading to spread across all segments of the population that may not originate in schools. Such indirect effects could also help to explain the large effect sizes. The next section explores the possibility of spillovers more directly.

### 3.6 Spillover Effects on Mobility

We next use SafeGraph data to explore whether changes in mobility patterns among adults may help to explain the large sizes of the effects of school reopenings on COVID-19 cases and deaths. Our baseline regression is again an event-study model given by (1), with the reopening variable defined by the largest potential week-to-week increase in in-person enrollment. However, we make three small changes in order to customize the approach for mobility outcomes. First, in contrast to the lags inherent in COVID-19 cases and deaths, effects on mobility can emerge immediately, and it is not obvious that they will evolve over time. Therefore, we shorten the window on each side of treatment to six weeks rather than eight, which prevents any counties' post-treatment windows from extending into the holiday break. The analysis therefore uses 13 weeks of data, and given our numbering convention, goes from -6 weeks to +6 weeks (where we denote week 0 as the week of school reopening within the county). Second, we now arrange weeks from Monday to Sunday, rather than Sunday to Saturday as we did in our models for COVID-19 spread, so that we can also examine weekday mobility separately from weekend mobility in some specifications. Third, instead of omitting the lead one week prior to school reopening as the reference category, we omit the lead two weeks prior, since preparation for a return to school could plausibly increase mobility in the week prior to reopening. For example, families may return from vacations or may engage in more back-to-school shopping.

There are three primary ways in which school reopenings can lead to spillover effects beyond the students who attend school and the teachers and staff who work there. First – and most directly – in-person learning may increase transmission between students and teachers, ultimately leading to secondary spread into the larger community. The CDC guidelines emphasize ideal conditions for in-person learning to succeed, including low initial levels of community spread, adequate social distancing, vigilant mask wearing, and a host of other steps that are unlikely to be fully carried out in practice. Second, opening of schools is associated with other indirect changes for parents due to decreased childcare responsibilities. This could include either greater physical presence in workplaces or increased outside-the-home leisure activities, both of which could lead to greater transmission and community spread. Finally, reopening schools could send an incorrect signal to the larger community that normal activities are safe again, similar to the "learning by deregulation" concept described in Glaeser et al. (2020). Such a signaling effect could even extend to those – such as seniors – with no direct ties to students or school employees.

We examine SafeGraph mobility data to explore these possible mechanisms. We aggregate SafeGraph's SDM database to the weekly level (averaging mobility measures across the week), where our unit of observation is a CBG, which we will refer to as a "neighborhood." After a number of screens to the SDM data (discussed in the data section earlier), we examine 14,580 neighborhoods from 252 of the 254 Texas counties. Our four mobility measures, following SafeGraph's SDM conventions, are percentage of devices completely at home, percent part-time work, percent full-time work, and median minutes outside of the dwelling; SafeGraph's convention is to define part-time (full-time) "work" as spending 3-6 hours (6 or more hours) at one location other than home between 8 am and 6 pm local time.

Using these SafeGraph definitions, in the weeks prior to reopening, approximately 28 percent of devices were completely home on a given day, and nearly

8 percent were engaged in part-time work and 4 percent in full-time work on a daily basis. In addition, the median time spent outside of the home on a given day was 108 minutes.

The event-study specifications provide evidence of increased mobility. As illustrated in Figure 3.5, only one out of sixteen pre-trend coefficients from six to three weeks prior to reopening are significantly different from the lead term two weeks prior to reopening. There is some evidence of anticipation effects in the week immediately prior to reopening with significant increases in work behavior. Starting in the week of reopening, and essentially thereafter, there is strong evidence of increased mobility. In the first week of school reopening (week 0), there is a reduction in staying completely home of 0.7 percentage points, increases in part-time and full-time work of 0.4 percentage points, and increases in time outside the home of more than 8 minutes. These results persist – and all grow substantially larger – in the subsequent weeks. For example, in week 6, the mobility results are two to three times as large. and time outside the home increases by 20 minutes. By the end of the period, relative to the baseline prior to reopening, these are decreases of 5 percent in completely at home per day, increases of 12 percent and 21 percent in part- and full-time work, and increases of 18 percent in time outside the home.

Next, we examine these mobility patterns in another way. Schools operate during weekdays, not weekends. Thus, increases in daily mobility induced by school openings (e.g., children at school, parental labor supply) should be more prominent on weekdays. When we run the same event study models on weekdays only in Figure 3.6, the mobility effects are much stronger, suggesting these mechanisms are operating as expected. To illustrate, in the full-week model, recall that there was a reduction in staying completely home of 0.7 percentage points in week 0; when focused on weekdays, there is now a 1.1 percentage point reduction. In the full week model, time outside the home increased by more than 8 minutes in week 0; when focused on weekdays, it is now nearly 13 minutes. By week 6, time outside the home increases by 30 minutes per day, considerably higher than the 20 minutes in the full week model. Relative to the pre-treatment weekday mean of 116 minutes, this is an increase of 26 percent. When focusing on the weekend in Figure 3.7, we generally find modest reductions in mobility, which may represent an overall reallocation of activities as the school year begins. For example, time outside the home falls by between 2 to 5 minutes per day in the weeks after reopening, although many of the coefficients are insignificant. The overall net effect – as represented by the full week – is clearly higher mobility.

One key benefit of the SDM database is the level of granularity. The typical neighborhood in our sample has a population of approximately 1,500 people, and was merged to demographic data from ACS summary files for 2018 (which aggregates microdata from 2014-2018). Importantly, these neighborhood summary files contain detailed information on the age distribution. From this, we characterize neighborhoods in two different ways: whether they have significant numbers of school-age children (fraction of population aged 5 to 17) and whether they have significant numbers of elderly (fraction of population aged 65 and over). We re-estimate our models, restricting to neighborhoods in the top quintile of school-age children (neighborhoods where, on average, approximately 25 percent of the population is comprised of school-aged children). We also re-estimate models for the top quintile of neighborhoods with elderly (where, on average, nearly 18 percent of the population are senior citizens). By focusing on neighborhoods with many children and parents, we expect increases in mobility due to both the resumption of school and any signaling effects. In contrast, in neighborhoods with large numbers of elderly, the effects of reopening schools and increased physical work presence should be diminished, although the general signal that normal activities are safe could still apply.

The overall patterns in the event-study are somewhat stronger for the top quintile of neighborhoods with school-age children; as illustrated in Figure 3.8, the median time away from home at 6 weeks after opening is nearly 27 minutes, compared with 20 minutes in the full sample. The pre-trends for all mobility measures show little change in mobility until school reopening, and then a highly significant and sizable increase thereafter. In contrast, the overall results are more muted in the elderly sample in Figure 3.9. For example, median time away from home shows no significant increase after school reopenings, and the magnitude is substantively smaller; at 6 weeks post, time away insignificantly increases by 7 minutes. There is an increase in "full-time work" (recall, SafeGraph defines this based on extended stays outside the home, not whether the person is actually at work), yet the magnitudes are again considerably smaller than in neighborhoods with many children. Put differently, when focused on a sample that should largely be unaffected from reopenings or increased physical work presence, we see only limited evidence of mobility consistent with a signal of returning to normal. The results from this granular analysis would then suggest mobility-induced increases from opening schools and potential spillovers onto parental behavior, especially labor supply.

Finally, we re-examine our main mobility results with a series of robustness checks that largely mirror the results on COVID-19 in Appendix Figures C.15 through C.22. Appendix Figure C.15 modifies the event-study specification by additionally including county-specific time trends. All of the substantive findings remain. For example, 6 weeks after opening, median time away from home is 20 minutes, virtually identical to the main specification. In Appendix Figure C.16, the specification is amended to include weekly controls for average temperature, precipitation, and snowfall, factors that have been shown to affect mobility (Kapoor et al., 2020; Wilson, 2020). In all instances, none of the pre-trends from six to three weeks prior to reopening are different from the omitted lead of two week; additionally, the results on mobility virtually mirror the baseline, full-week results. Next, in Appendix Figure C.17, we shorten the window to 4 weeks on either side of the school reopening. The same general patterns emerge as in the baseline specification. For example, in this specification, time away from home 4 weeks after reopening significantly increases by more than 15 minutes; in the base specification, it was 17 minutes.

In Appendix Figures C.18- C.20, we modify the parameterization of school reopenings, by considering a county to be open if 50 percent, 20 percent, or any students had in-person learning offered to them. As the figures make clear, how one characterizes school reopening at the county level matters for the interpretation of the mobility results. In one case (50 percent threshold), there are essentially no mobility effects (and the pre-trends are generally insignificant). In other cases (20 percent threshold or greater than 0 percent), the pre-trends for many of the mobility measures are significant, yet there are no mobility impacts after the "opening". Next, in Appendix Figure C.21, we modify the baseline specification by including an interaction of Trump's 2016 county vote share with week fixed effects. The same general patterns remain as in the baseline, but some of the estimated impacts are smaller and insignificant. Thus, evolving attitudes of Trump voters appears to be related both to school openings and increased mobility. Finally, in Appendix Figure C.22, we display the coefficient from week +6 from event study models that leave out the six largest Texas counties, each with population exceeding one million, one at a time. The estimated impacts on mobility are quite similar to the exercise, suggesting that none of the large counties are driving our results.

Collectively, these results suggest that reopening schools leads to important spillover effects on adult mobility that may help to explain the large effect sizes for the COVID-19 outcomes. The evidence is consistent with parents going physically back to work and perhaps also increasing outside-the-home leisure activities. These effects could be due to lessened child care responsibilities, signaling about the safety of returning to normal activities, or a combination of both. In contrast, the evidence is not as strong for neighborhoods with large numbers of elderly residents or for the general population on weekends, which may suggest that the time-use mechanism is relatively more important than general signaling.

### 3.7 Conclusion

In this study, we examine the impact of opening Texas public schools for in-person instruction in fall 2020 on community spread of COVID-19 as well as fatalities. In the eight weeks after reopening, we conservatively estimate, based on lower bounds of confidence intervals, that there would have been at least 43,000 fewer COVID-19 cases and at least 800 fewer fatalities. These results hold across a variety of specifications and robustness checks. These results could be explained both by the direct effect of spread within the schools and the indirect effects of increased mobility within the community as our analysis of cellphone data suggests that six weeks after reopening, the median time spent outside of the home increased by 26 percent on weekdays. This suggests that decision makers need to think strategically about how to encourage behavior to mitigate spread of COVID-19 not only within schools, but within the community at large.

On the surface, our empirical findings diverge with several popular narratives that have emerged about school openings. Some studies – including a prominent CDC study from Wisconsin – rely on contract tracing efforts to quantify impacts of school reopening. The imperfections of run-of-the-mill contact tracing efforts

– including the inability to follow asymptomatic cases or lack of cooperation in finding all close contacts – suggests estimates of in-school spread may be a lower bound. Importantly, this approach does not account for inevitable, indirect behaviors – such as greater parental mobility including increased physical presence in the workplace – which also may contribute to community spread. Although other recent research teams (Tulane, CALDER) take methodological approaches closer to our approach and find overall more modest effects on COVID-19 spread, it is important to emphasize that the initial conditions in Texas were more ripe for community spread and schools opened more widely, more quickly, and generally, close to full capacity.

Although it is beyond the scope of our study to provide a cost-benefit analysis of school reopenings, our quantitative findings contribute a key input into such an analysis. Recent work by Kniesner and Sullivan (2020) estimate non-fatal economic losses of about $46,000 per case, and Department of Transportation apply an $11 million loss per fatality. Such health- and productivity-related losses from COVID-19 must be weighed against learning losses for children, as well as other ancillary effects related to child mental health and abuse and these losses could be substantial but will only become clear over time. Distributional considerations are also important, as benefits of school closures accrue disproportionately among older individuals, whereas the costs are largely borne by children.

Obviously, as vaccinations expand, the cost-benefit calculations of opening schools changes. As of early April 2021, approximately 33 percent of adults aged 18 and older have been at least partially vaccinated, and the percentage is considerably higher among the most vulnerable.[32] To the extent that the spread of new mutations of the virus are mitigated by vaccines, almost all policies that restrict mobility – including school closures – will eventually be unnecessary. Nonethe-

[32]https://www.nytimes.com/interactive/2020/us/covid-19-vaccine-doses.html

less, there continue to be pockets of high spread of COVID-19. Furthermore, among various groups, there is widespread vaccine hesitancy and mistrust of the medical system. For instance, in a survey conducted in February 2021, among white evangelical adults, 45 percent stated they would "definitely not" or "probably not" get the COVID-19 vaccine.[33] In addition, vaccination rates are stubbornly low in both the African-American and Hispanic communities,[34] potentially due to historic mistrust of medical providers (Alsan and Wanamaker, 2018). As of April 2021, Texas lags the national average in both partial and full vaccinations, as do many of the states in the South.[35] Collectively, this suggests that there will be significant pockets of communities where lack of restrictions – including the opening of schools – may still lead to considerable community spread moving forward. Additionally, the B.1.1.7 (alpha) variant that is gradually becoming the dominant strain in the U.S. infects children more easily than prior strains, and children under twelve years old cannot yet be vaccinated.[36,37] The newer variant, B.1.617.2 (delta), has also been increasingly responsible for new cases, which poses additional risks to the unvaccinated population.[38]

For these reasons, debate about school openings and mitigation strategies will therefore likely continue to persist into the 2021-2022 school year, and our results provide important information that can help inform that debate. In particular, the CDC guidelines say that schools can reopen if community spread is low and considerable precautions are taken. Our study is not necessarily at odds with that

[33] https://www.pewresearch.org/fact-tank/2021/03/23/10-facts-about-americans-and-coronavirus-vaccines/ft_21-03-18_vaccinefacts/

[34] https://www.kff.org/coronavirus-covid-19/issue-brief/latest-data-on-covid-19-vaccinations-race-ethnicity/

[35] https://www.nytimes.com/interactive/2020/us/covid-19-vaccine-doses.html

[36] https://www.nydailynews.com/coronavirus/ny-covid-variants-michael-osterholm-new-york-20210404-73bhzmgpzremnpr5hirw2eo724-story.html

[37] https://health.usnews.com/health-care/patient-advice/articles/when-will-there-be-a-covid-19-vaccine-for-kids

[38] https://covid.cdc.gov/covid-data-tracker/#variant-proportions

guidance; instead, it simply shows that school reopenings are not always safe if those conditions are not met.

### 3.8 Tables

Table 3.1: Means and standard deviations of outcome variables

| COVID Outcomes | (1)<br>Pre-reopening | (2)<br>Post-reopening |
|---|---|---|
| New cases per 100,000 residents | 147.73 | 139.74 |
| | (121.37) | (171.99) |
| New deaths per 100,000 residents | 3.51 | 1.8 |
| | (5.45) | (3.72) |
| Observations | 2,024 | 2,277 |
| **Mobility Outcomes** | **Pre-reopening** | **Post-reopening** |
| Time completely home (%) | 28.21 | 26.62 |
| | (5.94) | (5.72) |
| Part-time work (%) | 7.84 | 9 |
| | (2.25) | (2.65) |
| Full-time work (%) | 3.98 | 4.93 |
| | (1.35) | (1.69) |
| Median non-home dwelling time (minutes) | 107.76 | 128.25 |
| | (56.42) | (62.1) |
| Observations | 87,480 | 102,060 |

Notes: Standard deviations are in parentheses. The COVID outcomes utilize public county-by-week-level data, while the mobility outcomes are from census-block-group-by-week-level data from SafeGraph. Observations are weighted by county (census-block-group) population for the COVID (mobility) variables. The pre-reopening period refers to the eight (six) weeks prior to school reopenings for the COVID (mobility) variables. The post-reopening period refers to the reopening week along with the eight (six) weeks following reopening for the COVID (mobility) variables.

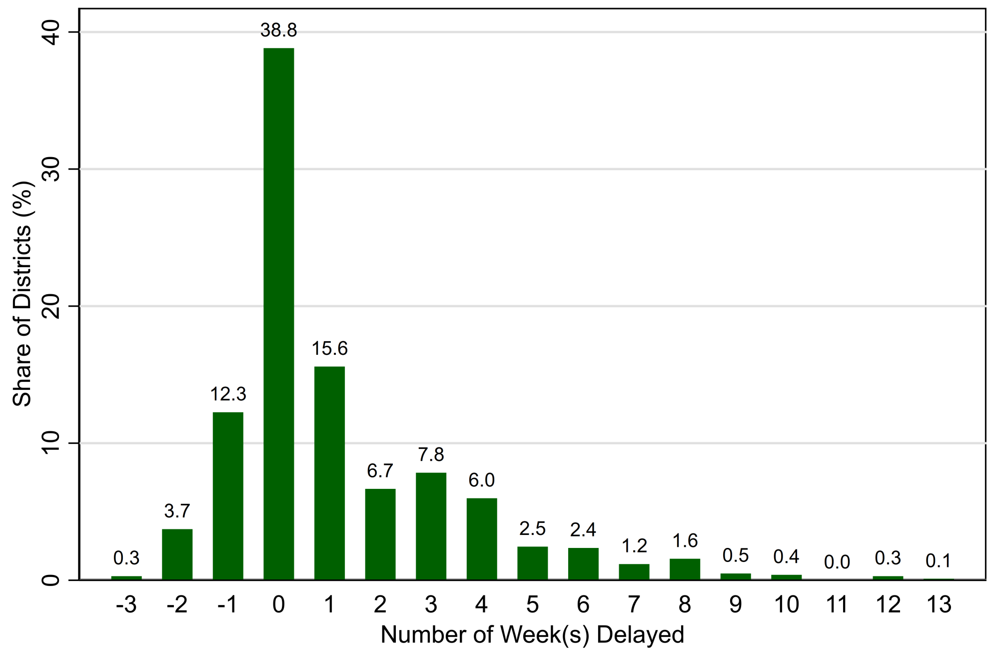
Table 3.2: Predictors of reopening week

| | Coefficient estimate (standard error) |
|---|---|
| Standardized 2016 percent of votes for President Trump | -1.18*** |
| | (0.3) |
| Standardized percent Hispanic | -0.19 |
| | (0.15) |
| Standardized percent Black | -0.15 |
| | (0.12) |
| Standardized population | 0.18 |
| | (0.16) |
| Standardized percent who stayed at home for full day | 0.07 |
| | (0.15) |
| Standardized new weekly cases per 100,000 | 0.32*** |
| | (0.12) |
| Constant | 17.06*** |
| | (0.19) |

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Results are from a cross-sectional county-level linear regression with week number of reopening (ranging from 14 to 28, with 1 indicating the week of May 3) as the outcome variable. The stay-at-home and new cases variables are pooled averages across the four weeks prior to the earliest school reopening (week numbers 10 through 13).

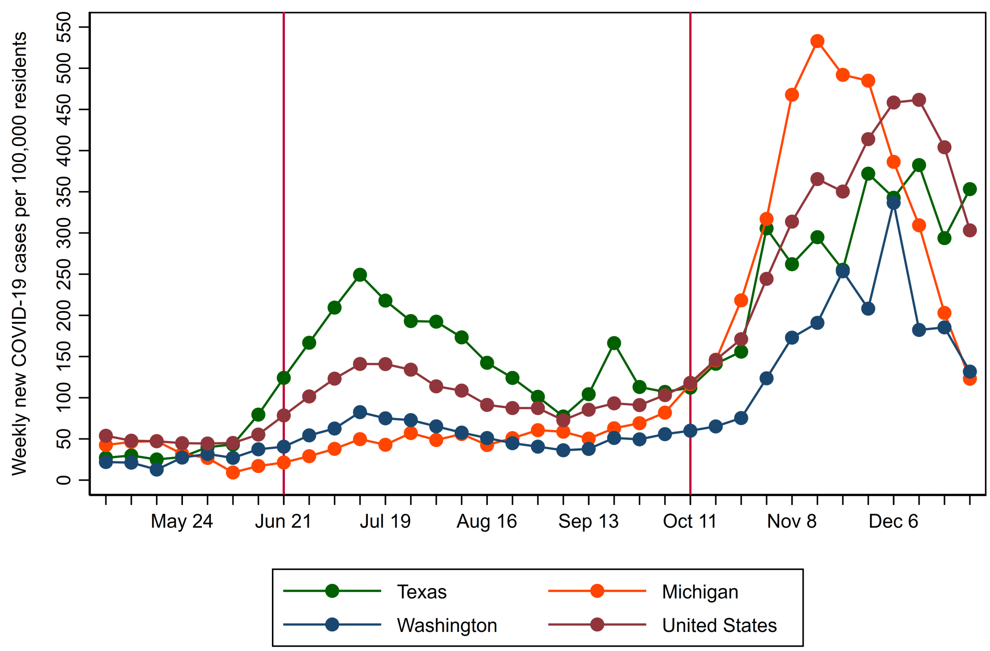
### 3.9 Figures

Figure 3.1: Relative start date of school district start date in 2020-21 school year relative to the 2019-20 school year



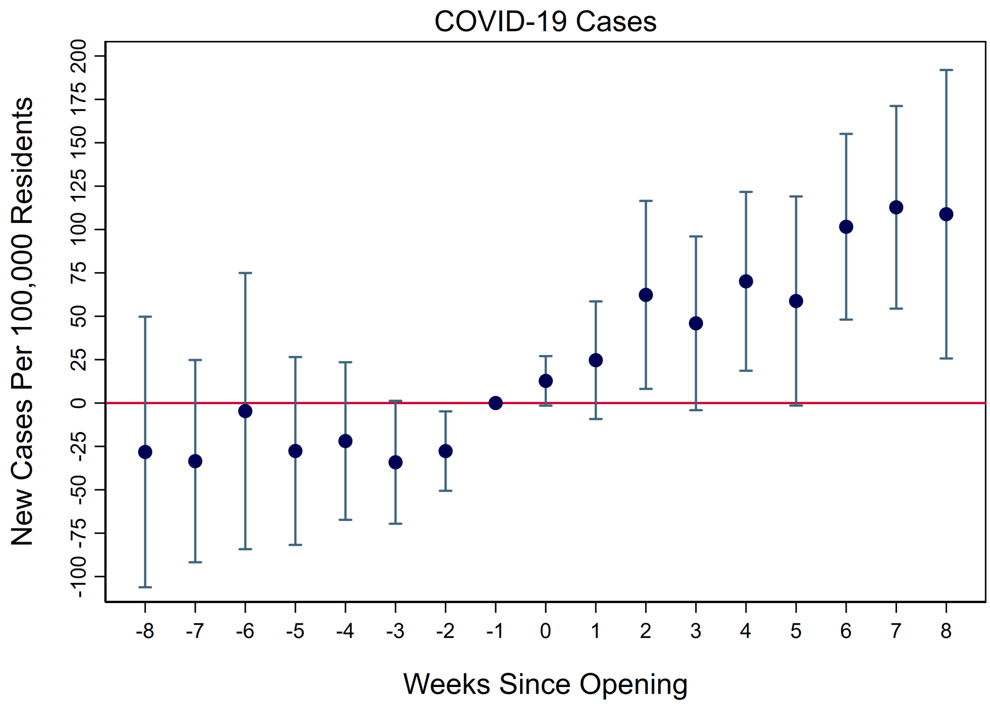

Note: In some cases, we do not have the 2019-20 start date for school districts. In these cases, we substitute a prior start date for any year we could find a record.

Figure 3.2: Weekly COVID-19 cases per 100,000 residents in Texas, Washington, Michigan, and the U.S.



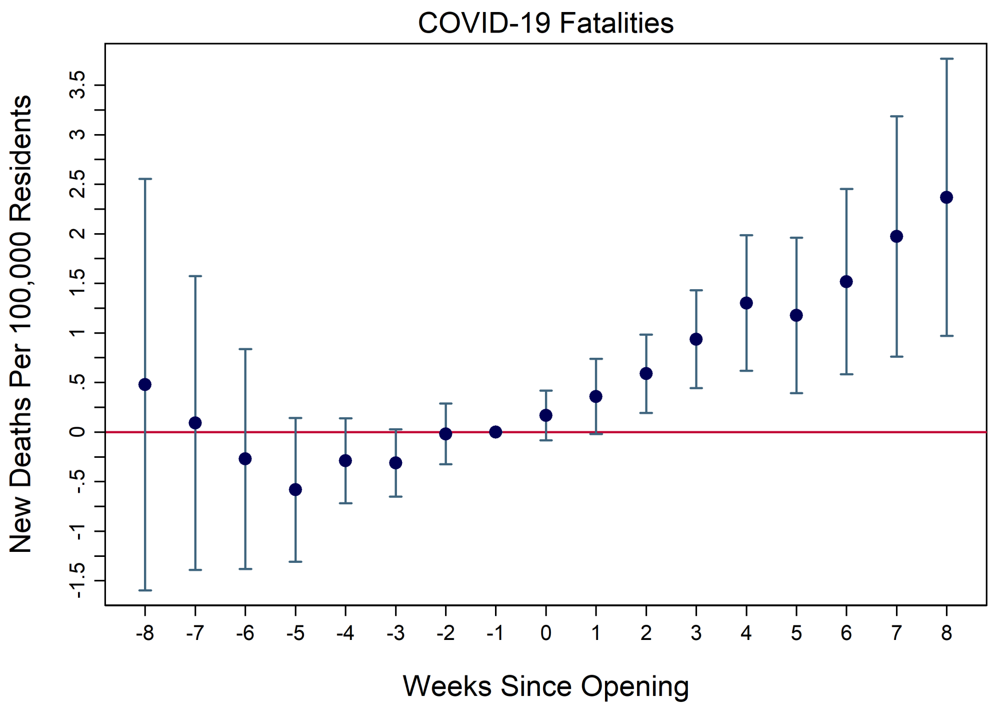

Note: Data from the Center for Systems Science and Engineering at Johns Hopkins Universiy.

Figure 3.3: Event-study regression results for effect of reopening schools on COVID-19 sases per 100,000 residents



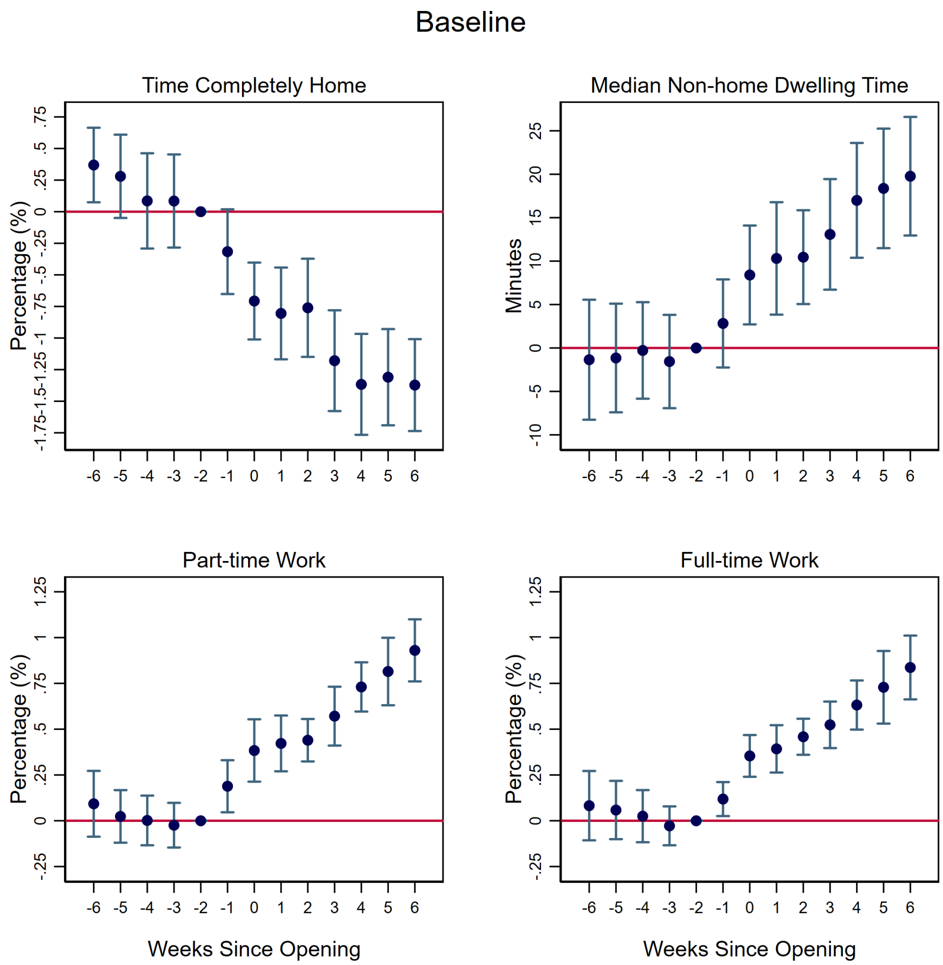

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regression is weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.4: Event-study regression results for effect of reopening schools on COVID-19 fatalities per 100,000 residents



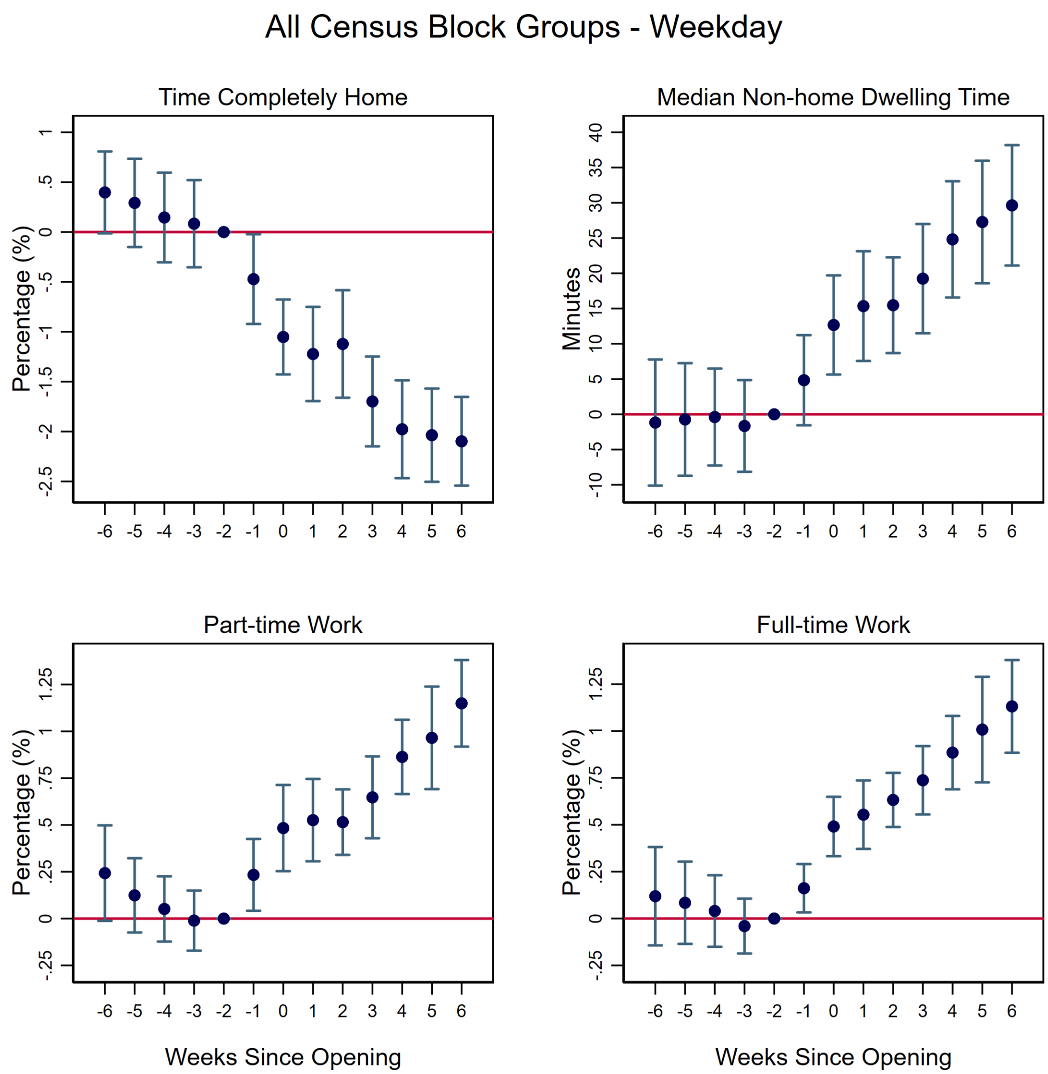

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regression is weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.5: Effects of school reopening on mobility - Baseline model (all CBGs, full-week)



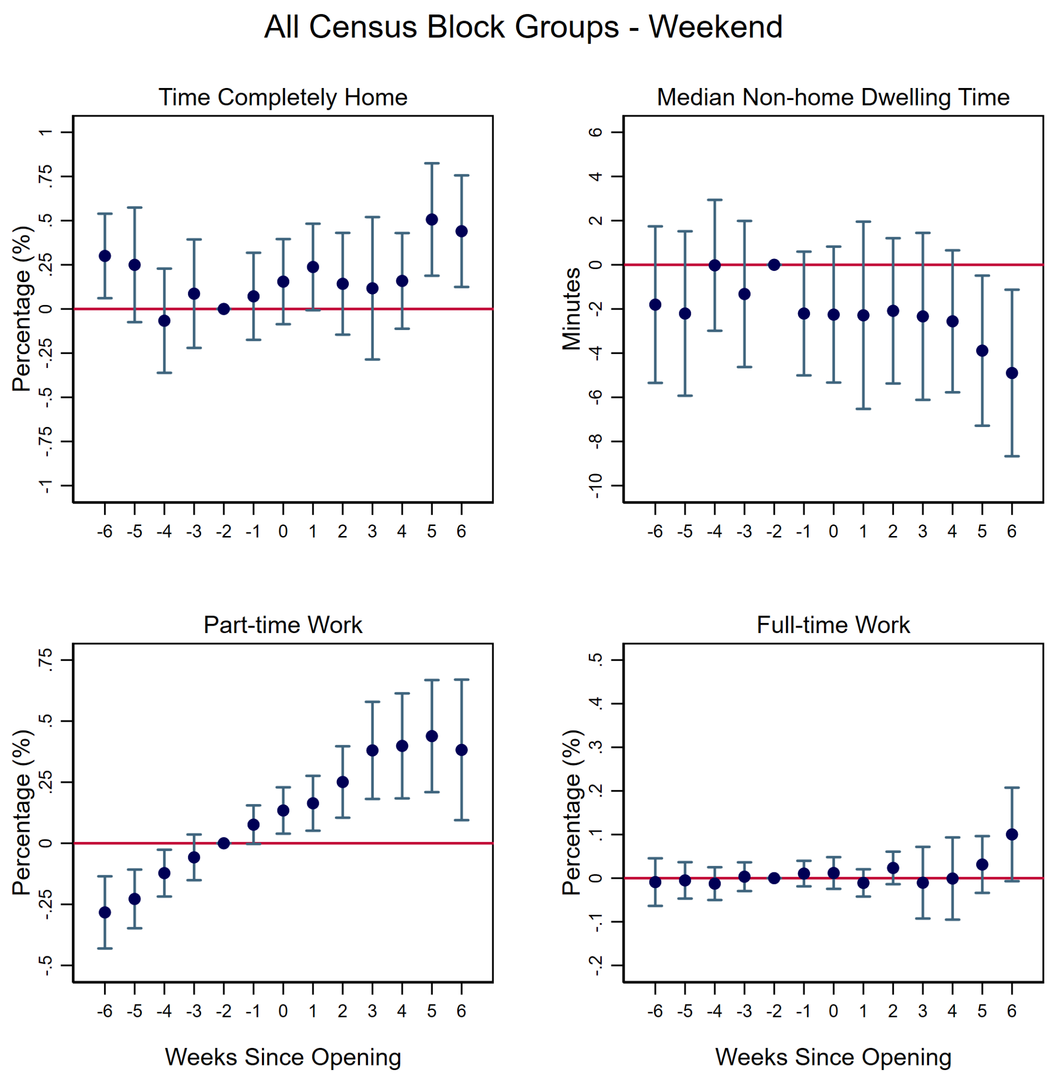

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.6: Effects of school reopening on mobility - All CBGs, Weekday



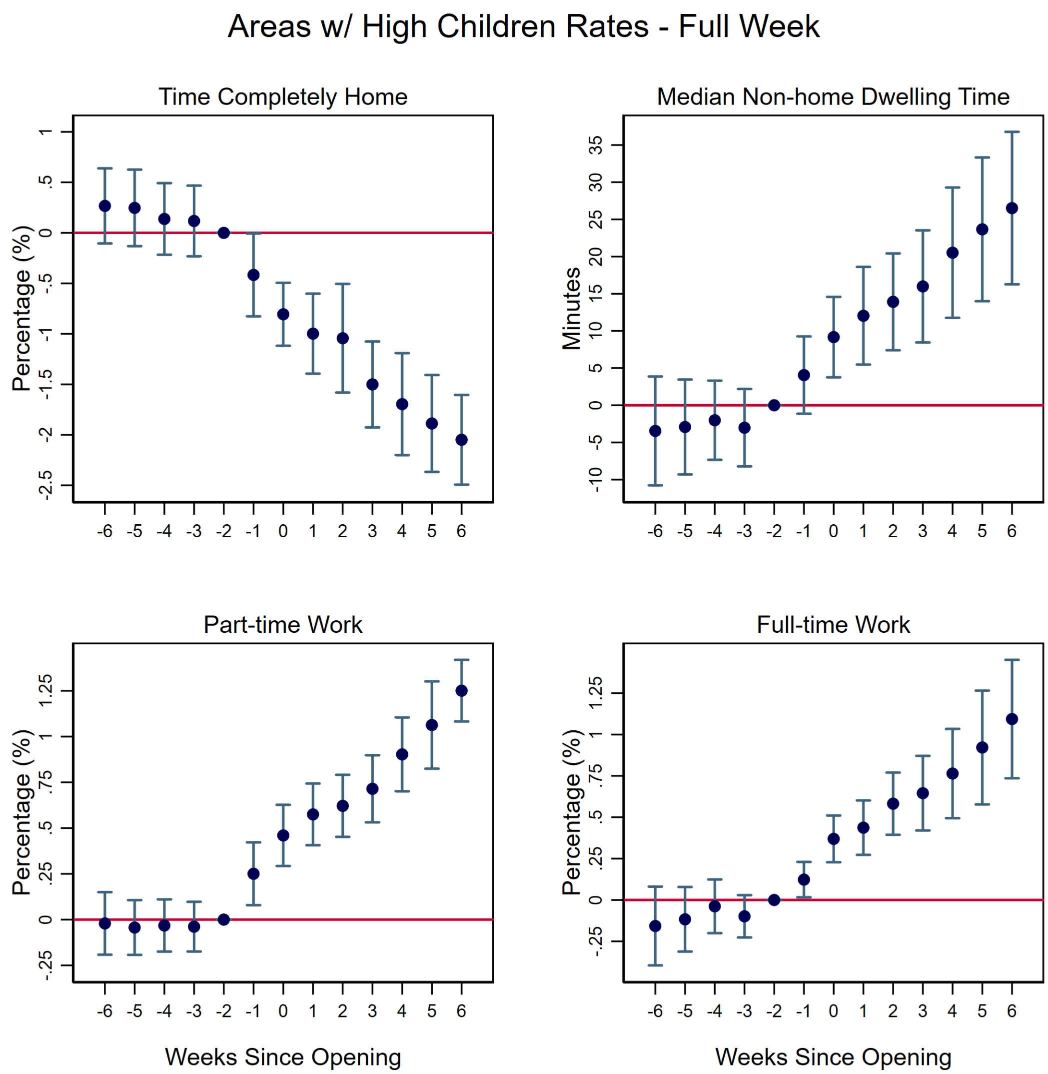

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.7: Effects of school reopening on mobility - All CBGs, Weekend



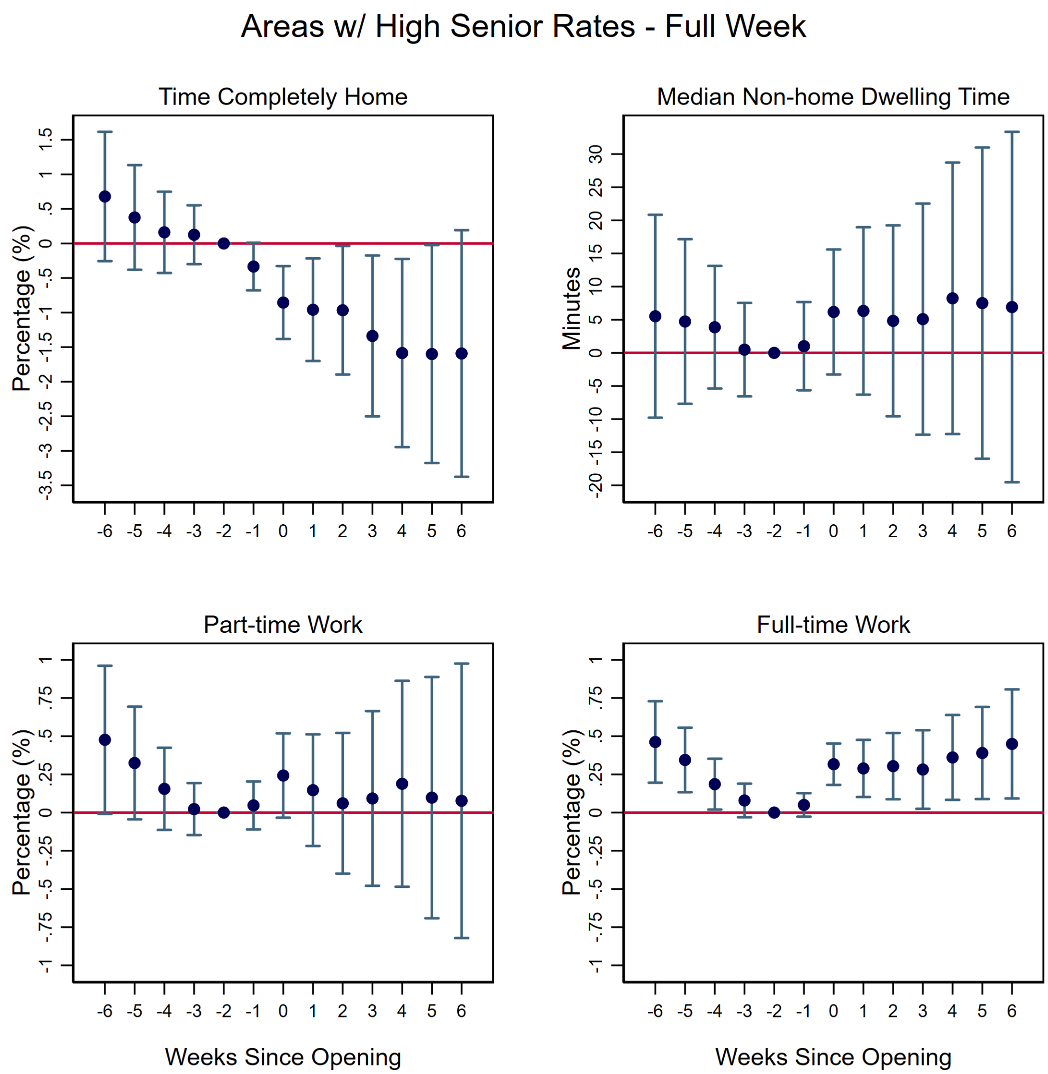

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.8: Effects of school reopening on mobility - Areas with high percentage of children




Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure 3.9: Effects of school reopening on mobility - Areas with high percentage of seniors




Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

# Appendix A

## Supplemental Material for Chapter 1

Table A.1: State Medicaid expansion years

| State | Year | State | Year | State | Year | State | Year |
|---|---|---|---|---|---|---|---|
| Alabama | N/A | Illinois | 2014 | Montana | 2016 | Rhode Island | 2014 |
| Alaska | 2015 | Indiana | 2015 | Nebraska | N/A | South Carolina | N/A |
| Arizona | 2014 | Iowa | 2014 | Nevada | 2014 | South Dakota | N/A |
| Arkansas | 2014 | Kansas | N/A | New Hampshire | 2014 | Tennessee | N/A |
| California | 2011 | Kentucky | 2014 | New Jersey | 2011 | Texas | N/A |
| Colorado | 2014 | Louisiana | 2016 | New Mexico | 2014 | Utah | N/A |
| Connecticut | 2010 | Maine | N/A | New York | 1997* | Vermont | 2006* |
| Delaware | 1996* | Maryland | 2014 | North Carolina | N/A | Virginia | N/A |
| D.C. | 2010 | Massachusetts | 2006* | North Dakota | 2014 | Washington | 2011 |
| Florida | N/A | Michigan | 2014 | Ohio | 2014 | West Virginia | 2014 |
| Georgia | N/A | Minnesota | 2011 | Oklahoma | N/A | Wisconsin | N/A |
| Hawaii | 2014 | Mississippi | N/A | Oregon | 2014 | Wyoming | N/A |
| Idaho | N/A | Missouri | N/A | Pennsylvania | 2015 | | |

Notes: The ACA Medicaid and state own expansion years obtained from The Henry and Kaiser Family Foundation and state Medicaid websites, respectively. CA, CT, D.C., MN, NJ, and WA are the early opt-in states via a Section 1115 Waiver.

* DE, MA, NY, and VT have their own expansion prior to the ACA and also expand Medicaid in 2014. They receive treatment status in 2014.

Table A.2: State opioid-related polices

| State | Prescription drug time and dosage limit laws | | | Must-access PDMP | Pain clinic Regulation | Recreational marijuana law |
|---|---|---|---|---|---|---|
| | Year | Day limit | Amount limit | | | |
| Alabama | | | | | 2013 | |
| Alaska | 2017 | 7 | | 2017 | | 2014 |
| Arizona | | | | | | |
| Arkansas | | | | 2017 | | |
| California | | | | | | 2016 |
| Colorado | | | | | | 2012 |
| Connecticut | 2016 | 7 | | 2015 | | |
| Delaware | 2017 | 7 | | 2012 | | |
| District of Columbia | | | | | | 2014 |
| Florida | | | | | 2010 | |
| Georgia | | | | 2014 | 2013 | |
| Hawaii | 2016 | 30 | | | | |
| Idaho | | | | | | |
| Illinois | 2012 | 30 | | 2018 | | |
| Indiana | 2017 | 7 | | 2014 | | |
| Iowa | | | | | | |
| Kansas | | | | | | |
| Kentucky | 2017 | 3 | | 2012 | 2012 | |
| Louisiana | 2017 | 7 | | 2008 | 2006 | |
| Maine | 2017 | 7 | 100 MME/day | | | 2016 |
| Maryland | 2017 | None | Lowest effective dose | 2018 | | |
| Massachusetts | 2016 | 7 | | 2014 | | 2016 |
| Michigan | | | | | | 2018 |
| Minnesota | 2017 | 4 | | 2017 | | |
| Mississippi | | | | | 2011 | |
| Missouri | 1988 | 30 | | | | |
| Montana | | | | | | |
| Nebraska | | | | | | |
| Nevada | 2017 | 14 | 90 MME/day | 2017 | | 2016 |
| New Hampshire | 2017 | 7 | Lowest effective dose | 2016 | | |
| New Jersey | 2017 | 5 | Lowest effective dose | 2015 | | |
| New Mexico | | | | 2015 | | |
| New York | 2016 | 7 | | | | |
| North Carolina | 2018 | 5 | | | | |
| North Dakota | | | | | | |
| Ohio | 2017 | 7 | 30 MME/day | 2015 | | |
| Oklahoma | | | | 2015 | | |
| Oregon | | | | | | 2014 |
| Pennsylvania | 2017 | 7 | | 2017 | | |
| Rhode Island | 2017 | 20 doses | 30 MME/day | 2016 | | |
| South Carolina | 2007 | 31 | | 2017 | | |
| South Dakota | | | | | | |
| Tennessee | 2013 | 30 | | 2013 | 2011 | |
| Texas | 2013 | 30 | | 2019 | 2009 | |
| Utah | 2017 | 7 | | 2017 | | |
| Vermont | 2017 | Varies | Varies | 2015 | | 2018 |
| Virginia | 2017 | 7 | | 2015 | | |
| Washington | | | | | | 2012 |
| West Virginia | | | | | 2012 | |
| Wisconsin | | | | | | |
| Wyoming | | | | | 2016 | |

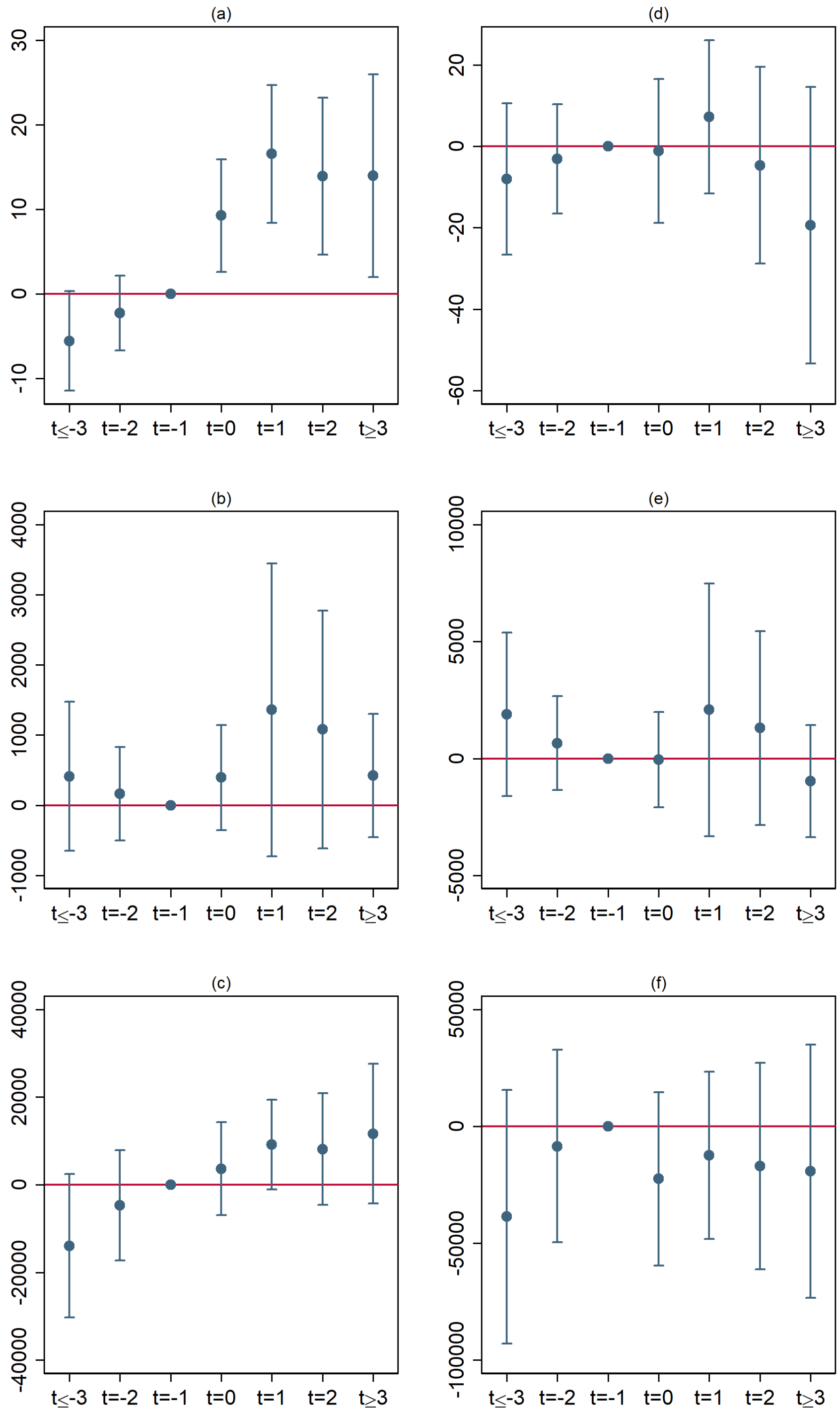
Notes: Table reports the effective years of states' opioid-related regulations. Prescription drug time and dosage limit laws limit the number of days and/or amount of opioids prescribed to first-time patients.

Table A.3: Common prescription opioids

| **Generic** | **Brand name** |
|---|---|
| buprenorphine | Belbuca |
| | Butrans |
| butorphanol | |
| codeine | |
| fentanyl | Actiq |
| | Duragesic |
| | Fentora |
| | Subsys |
| hydrocodone | Lortab |
| | Norco |
| | Vicodin |
| | Reprexain |
| hydrocodone bitartrate | Hysingla |
| | Zohydro |
| hydromorphone | Dilaudid |
| | Exalgo |
| meperidine | Demerol |
| methadone | Dolophine |
| morphine sulfate | Duramorph |
| | Infumorph |
| | MorphaBond |
| | Embeda |
| | MS Contin |
| nalbuphine | |
| oxycodone | Oxycontin |
| | Xartemis |
| | Percocet |
| | Xtampza |
| | Roxicodone |
| oxymorphone | Opana |
| tapentadol | Nucynta |
| tramadol | Ultram |

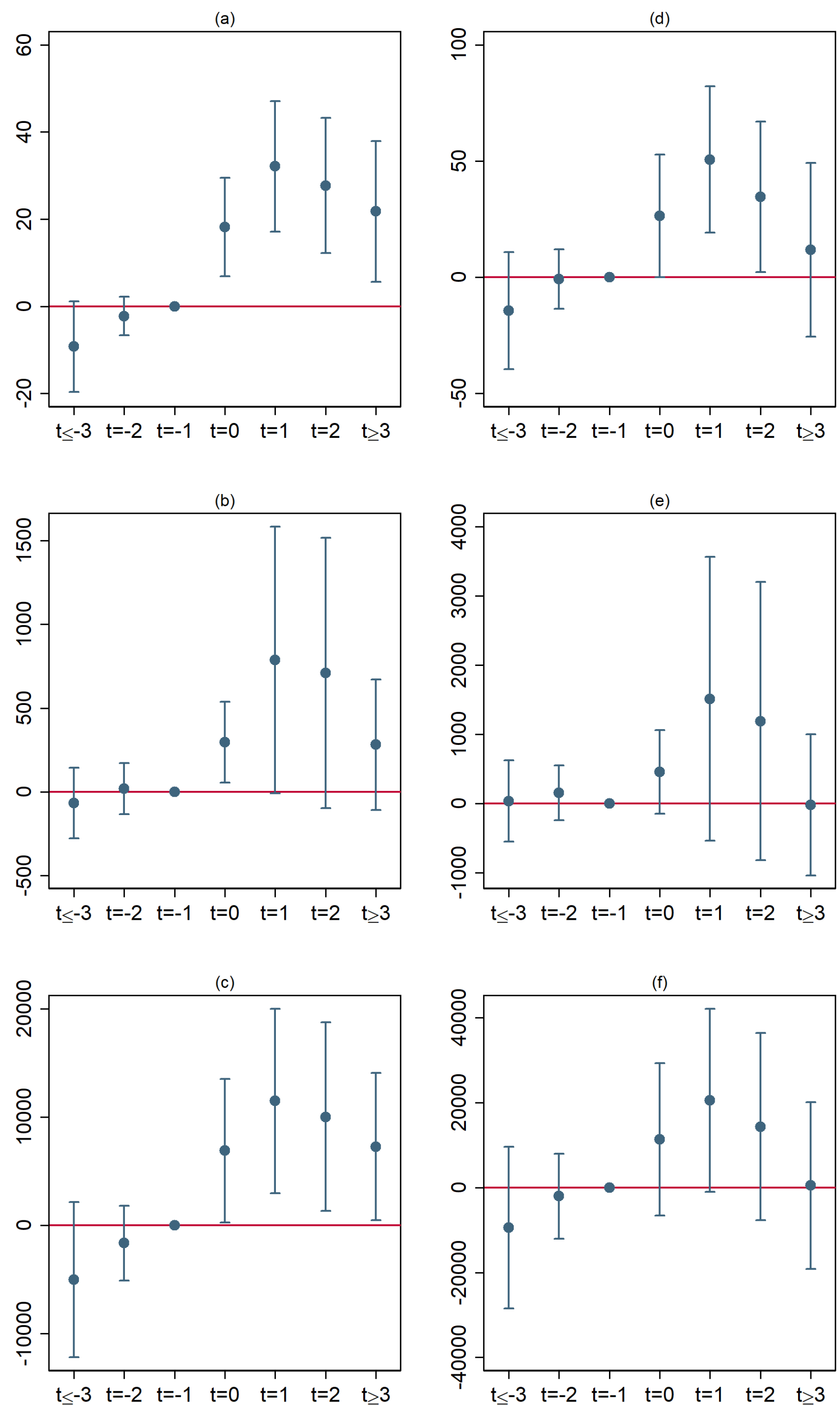
*Notes*: This list contains common opioid drug names with non-zero utilization. Substance names are obtained from Medicaid Opioid Drug Lists and the Monthly Prescribing Reference. For a full list of prescription opioids used in this essay, visit *www.cms.gov*.

Figure A.1: Event-study results: Effects of the Medicaid expansions on oxycodone use



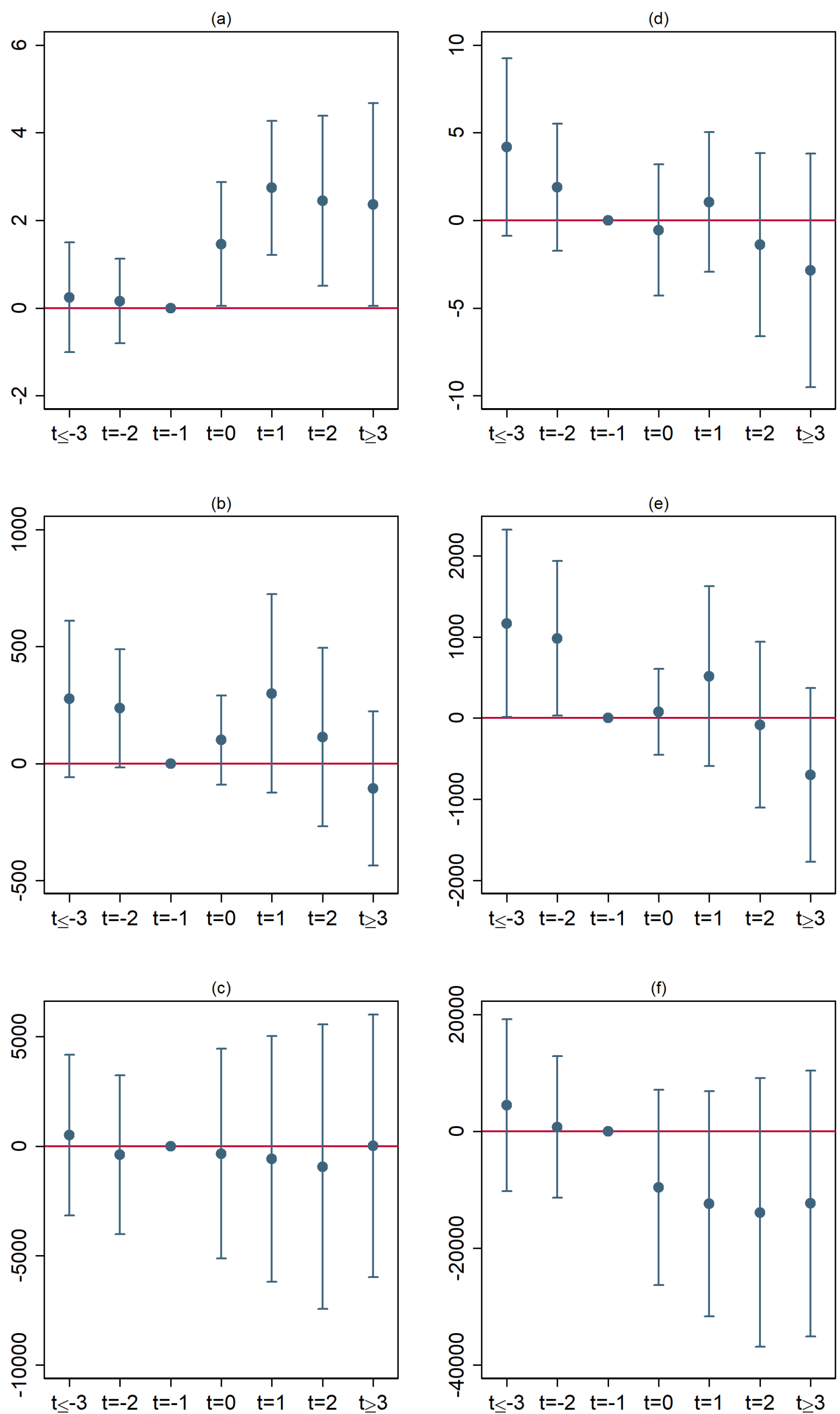

Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables: per population (1,000 people ages 19–64) (a) number of prescriptions, (b) reimbursements, (c) MMEs, and per enrollee (in 1,000s) (d) number of prescriptions, (e) reimbursements, (f) MMEs. Reimbursement is measured in 2011 dollars.

Figure A.2: Event-study results: Effects of the Medicaid expansions on hydrocodone use



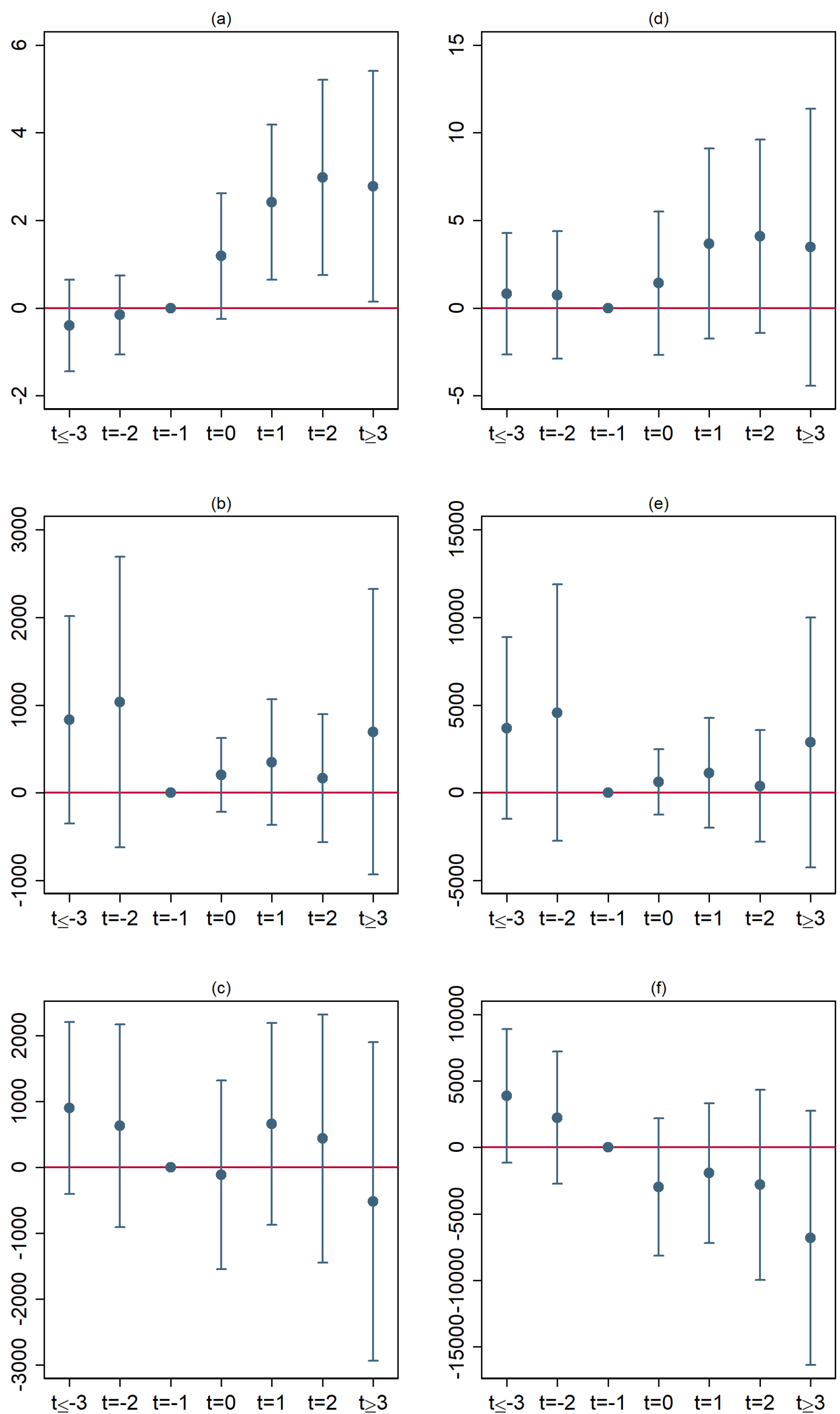

Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables: per population (1,000 people ages 19–64) (a) number of prescriptions, (b) reimbursements, (c) MMEs, and per enrollee (in 1,000s) (d) number of prescriptions, (e) reimbursements, (f) MMEs. Reimbursement is measured in 2011 dollars.

Figure A.3: Event-study results: effects of the Medicaid expansions on morphine use




Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables: per population (1,000 people ages 19–64) (a) number of prescriptions, (b) reimbursements, (c) MMEs, and per enrollee (in 1,000s) (d) number of prescriptions, (e) reimbursements, (f) MMEs. Reimbursement is measured in 2011 dollars.

Figure A.4: Event-study results: effects of the Medicaid expansions on fentanyl use




Notes: Estimates and 95% confidence intervals are results from estimating equation 1.3. *t=0* is the year of expansion, and *t=-1* is the reference year. Dependent variables: per population (1,000 people ages 19–64) (a) number of prescriptions, (b) reimbursements, (c) MMEs, and per enrollee (in 1,000s) (d) number of prescriptions, (e) reimbursements, (f) MMEs. Reimbursement is measured in 2011 dollars.

## Appendix B

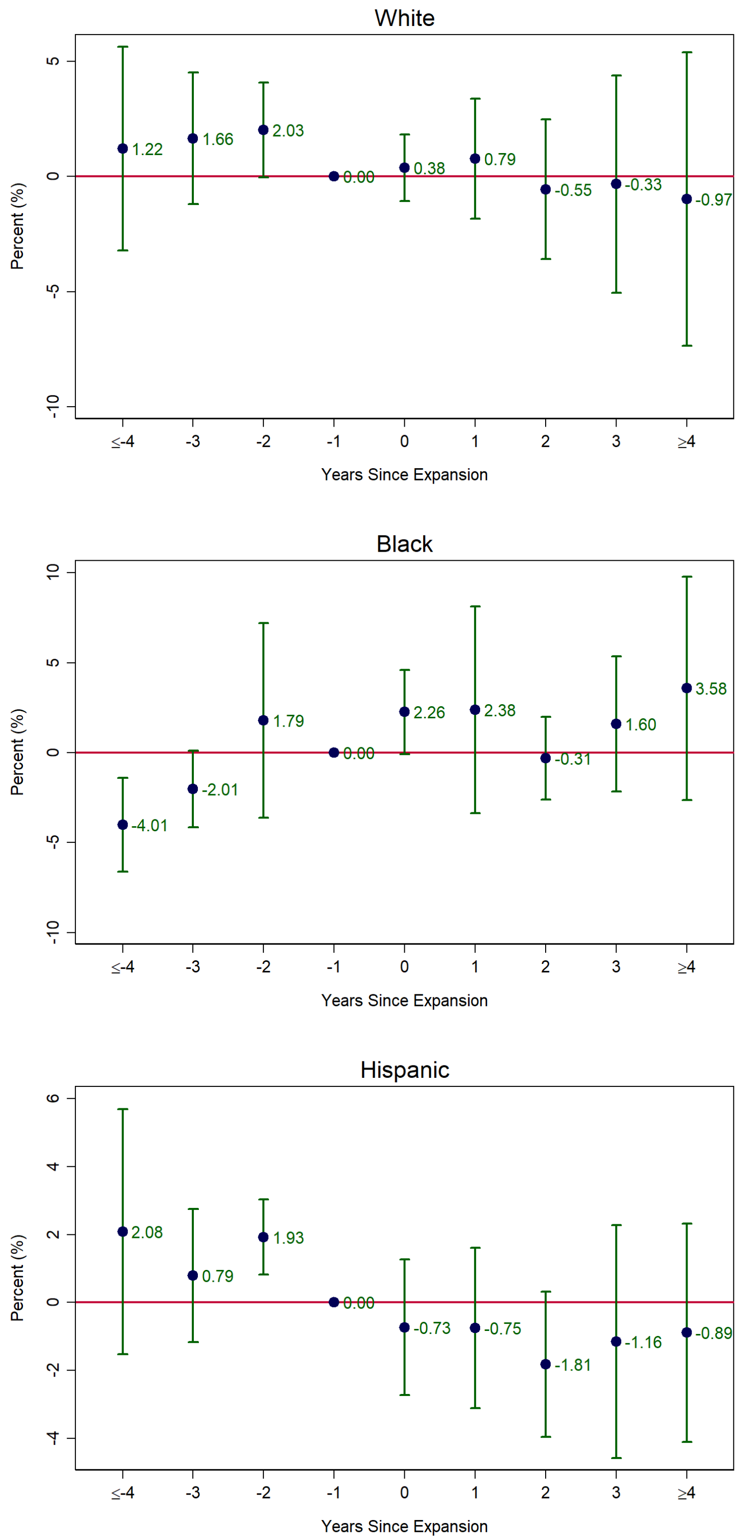
### Supplemental Material for Chapter 2

Table B.1: States' treatment periods based on the year of expansion

| | $\tau \leq$-4 | $\tau$=-3 | $\tau$=-2 | $\tau$=-1 | $\tau$=0 | $\tau$=1 | $\tau$=2 | $\tau$=3 | $\tau \geq$=4 |
|---|---|---|---|---|---|---|---|---|---|
| AZ, AR, CO, DE, HI, IL, IA, KY, MA, MD, MI, NV, NH, NM, NY, ND, OH, OR, RI, VT, WV | 2008, 2009, 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 |
| AK, IN, PA | 2008, 2009 2010, 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | |
| MT, LA | 2008, 2009, 2010, 2011, 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | | |
| CA, MN, NJ, WA | | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015, 2016 2017, 2018 |
| CT, DC | | | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014, 2015 2016, 2017 2018 |

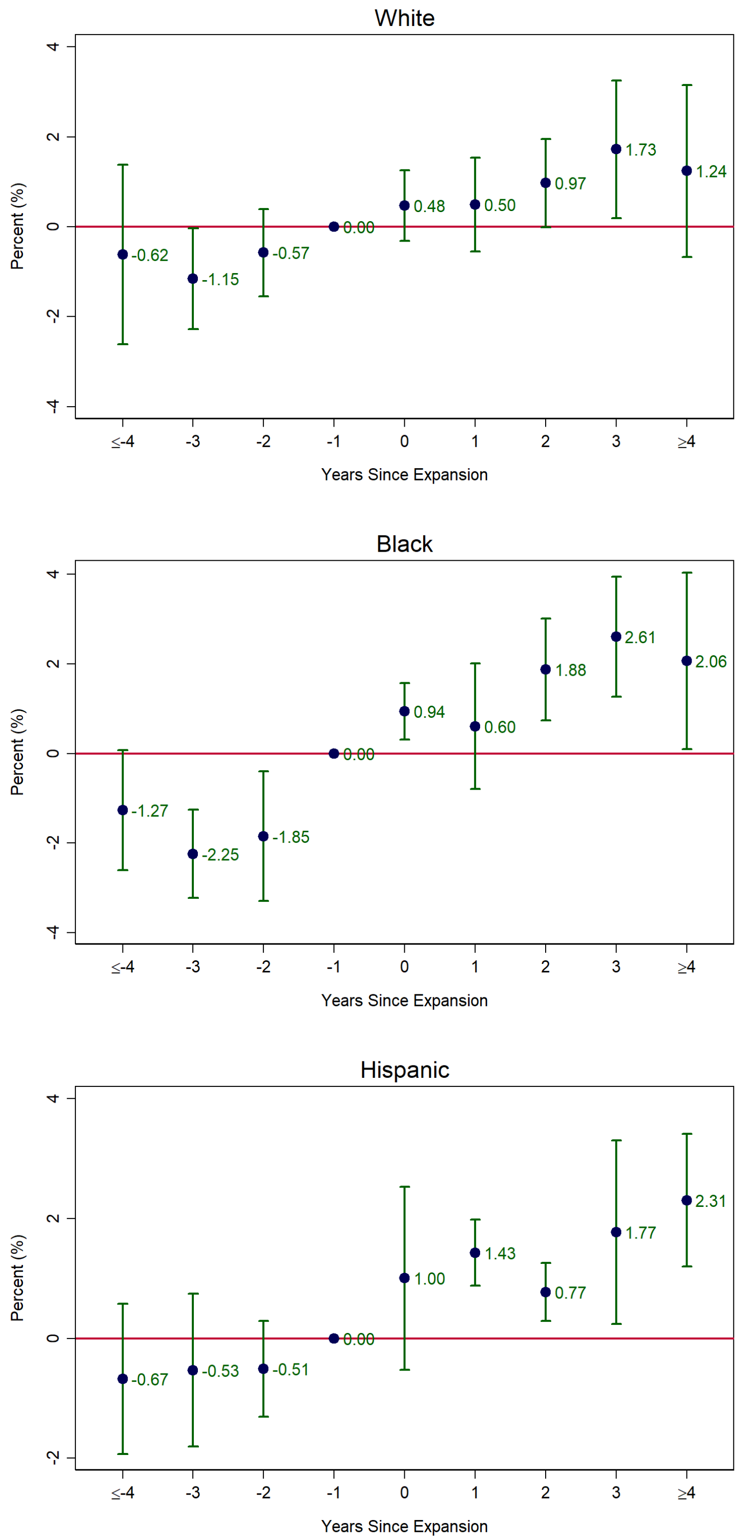
Note: $\tau = 0$ is the year of expansions.

Figure B.1: SUD treatment incompletion outcomes: dropping out of treatment



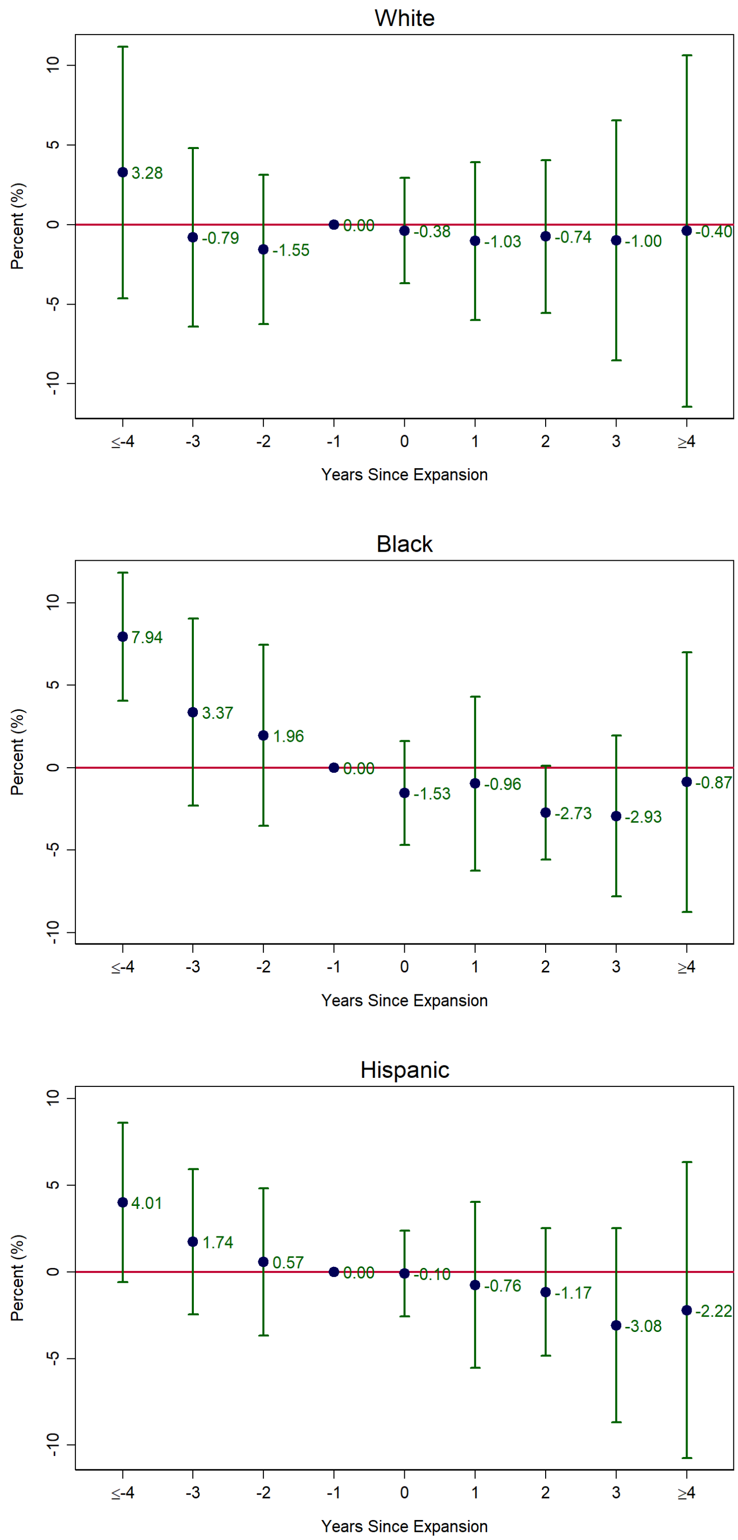

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.2: SUD treatment incompletion outcomes: administrative discharge



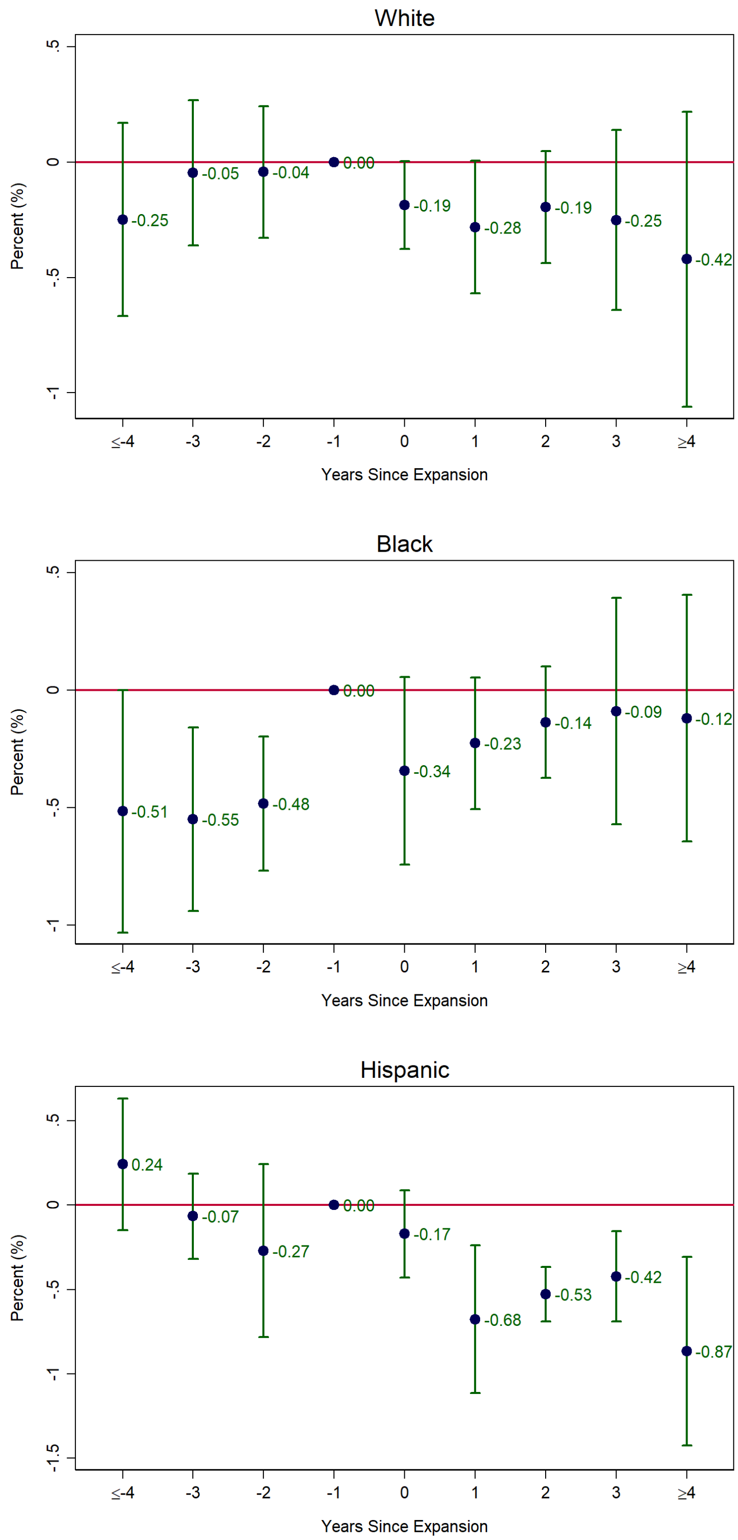

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.3: SUD treatment incompletion outcomes: transferred



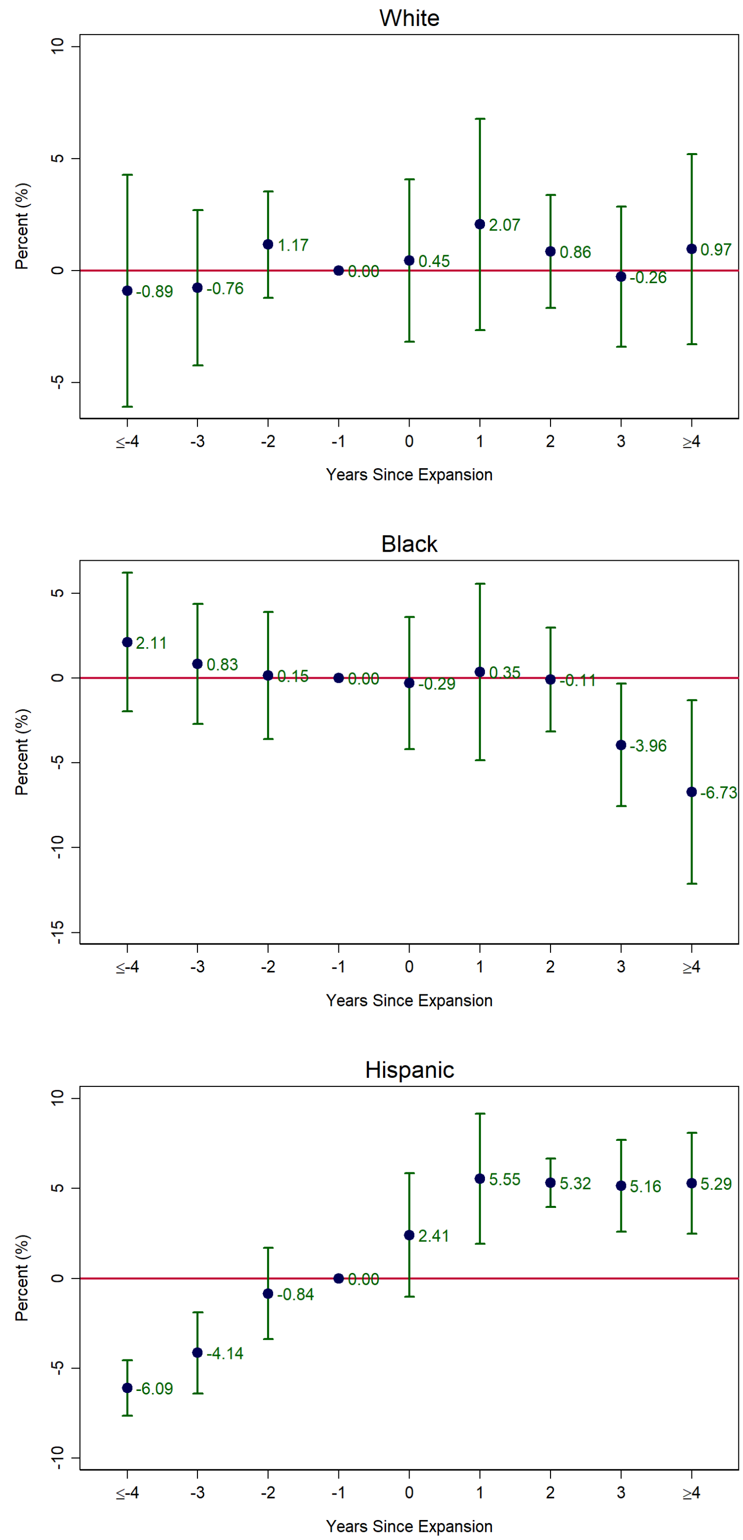

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.4: SUD treatment incompletion outcomes: incarcerated



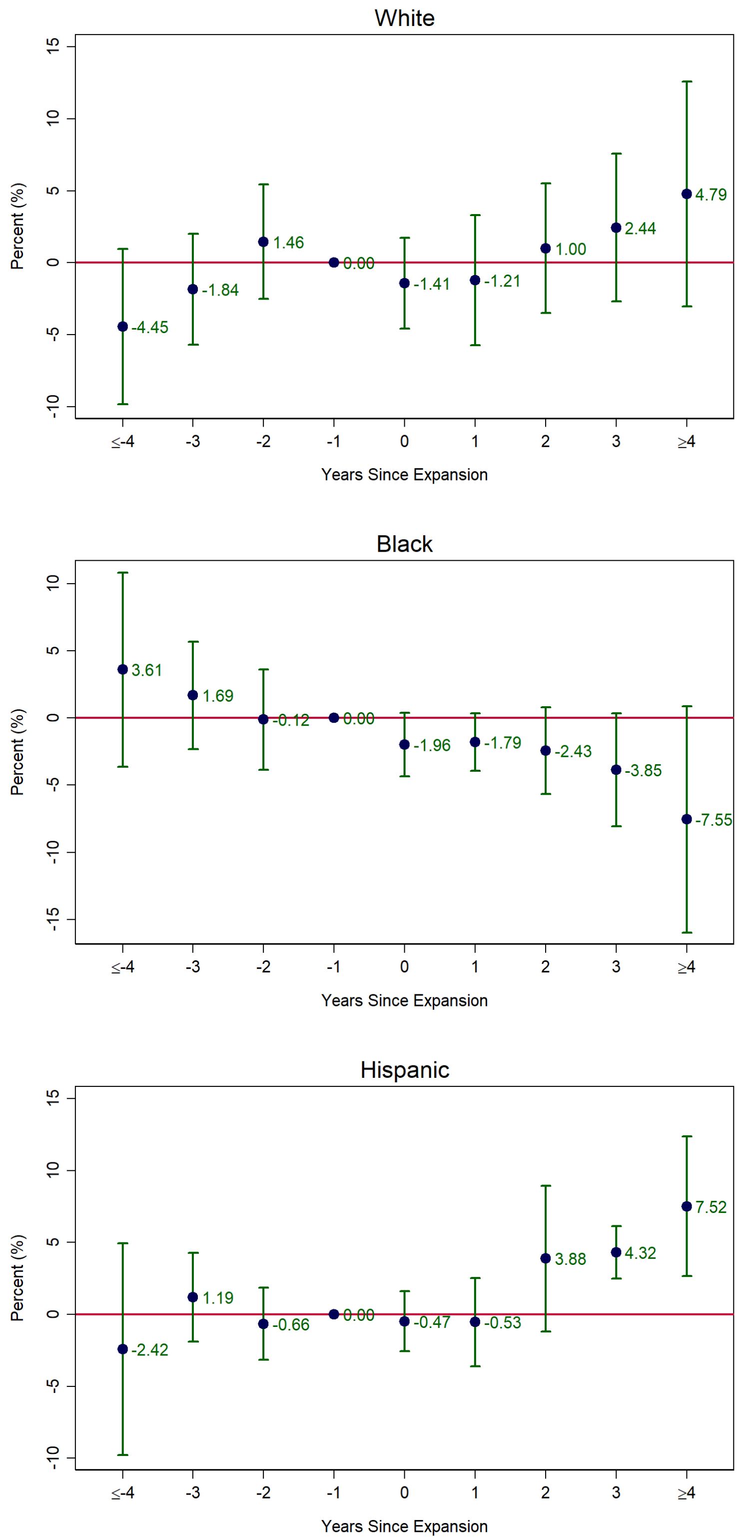

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.5: Excluding political characteristics - racial subsamples



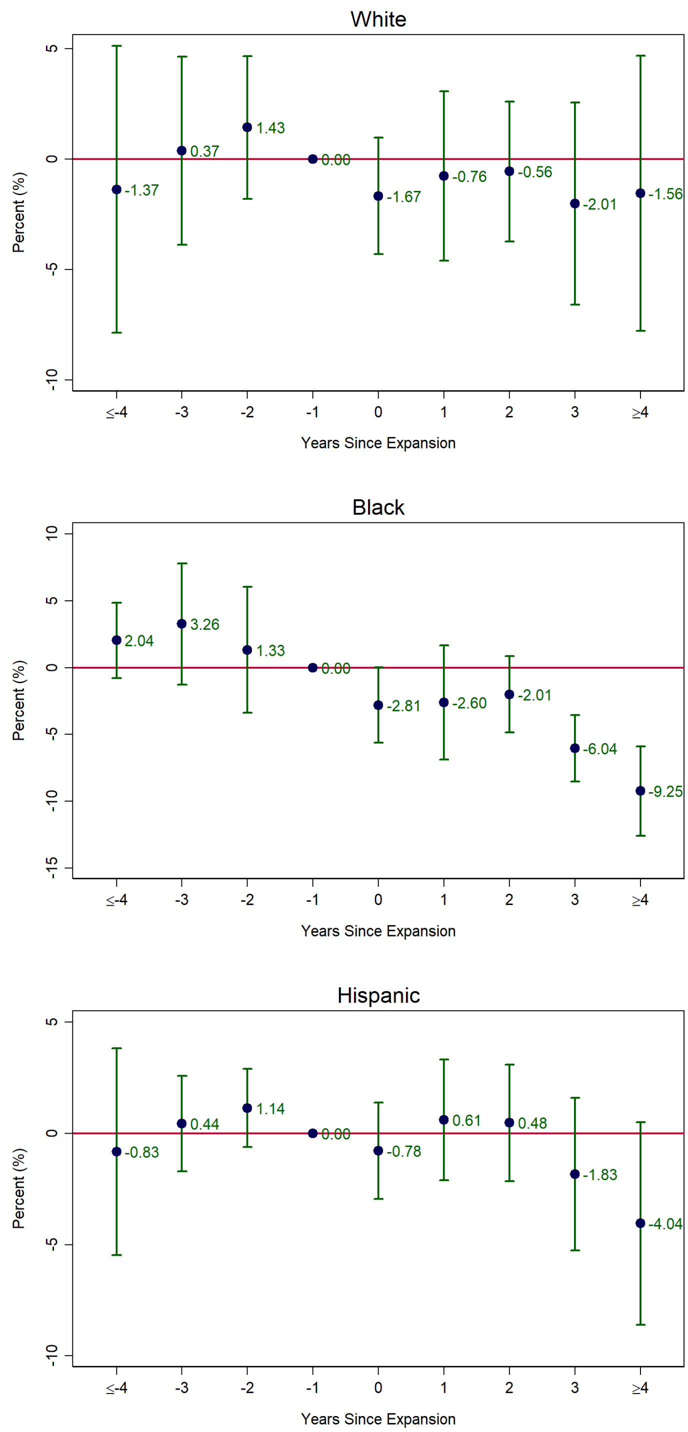

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.6: Excluding early-expansion states - racial subsamples



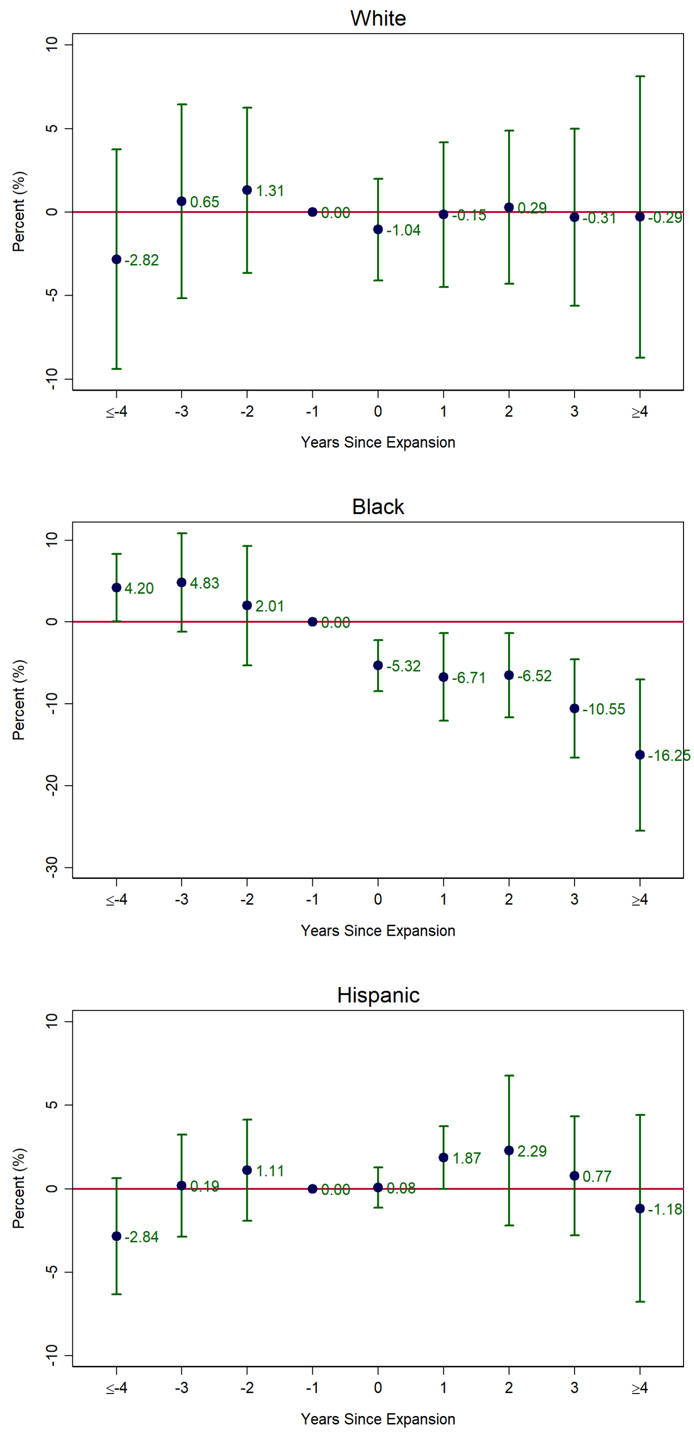

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.7: Controlling for prescription drug monitoring programs - racial sub-samples



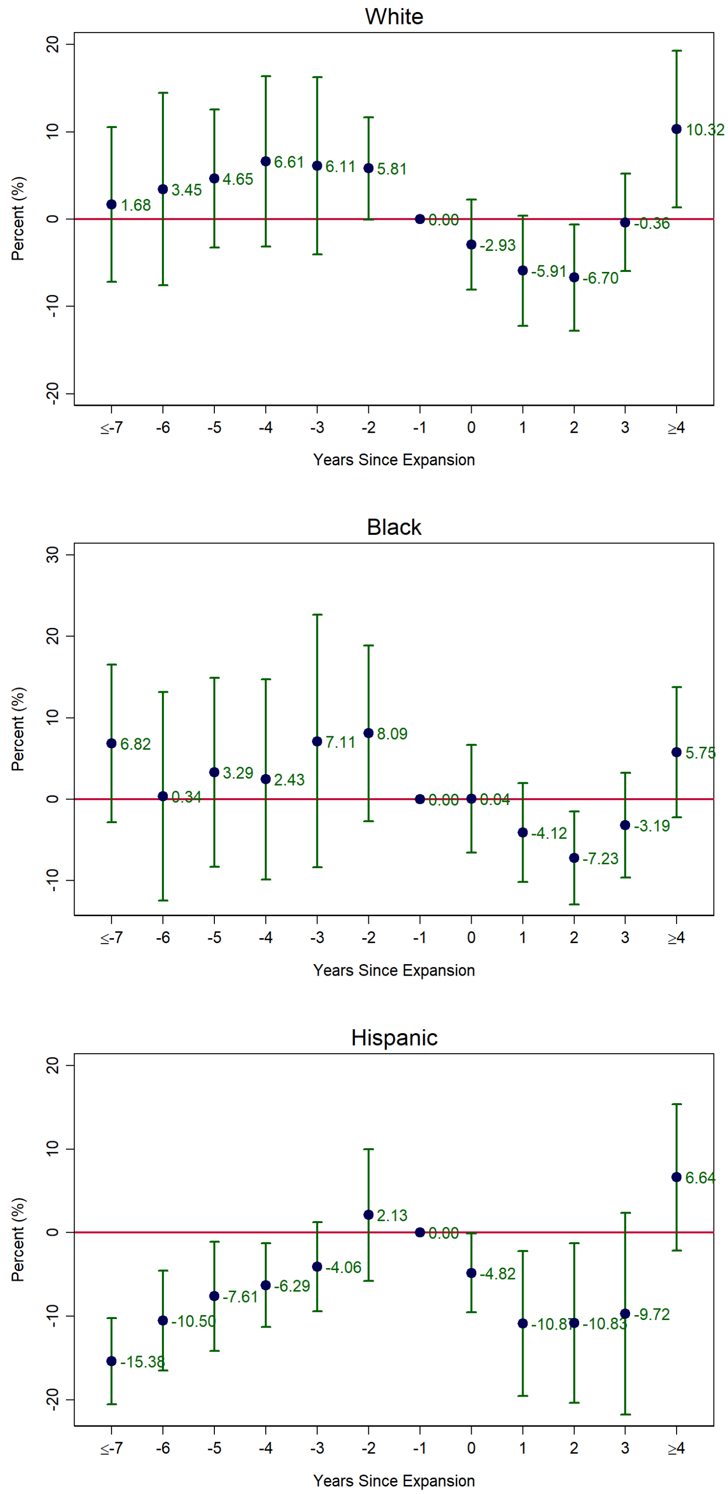

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.8: Including No. of treatment centers and time trends - racial Subsamples



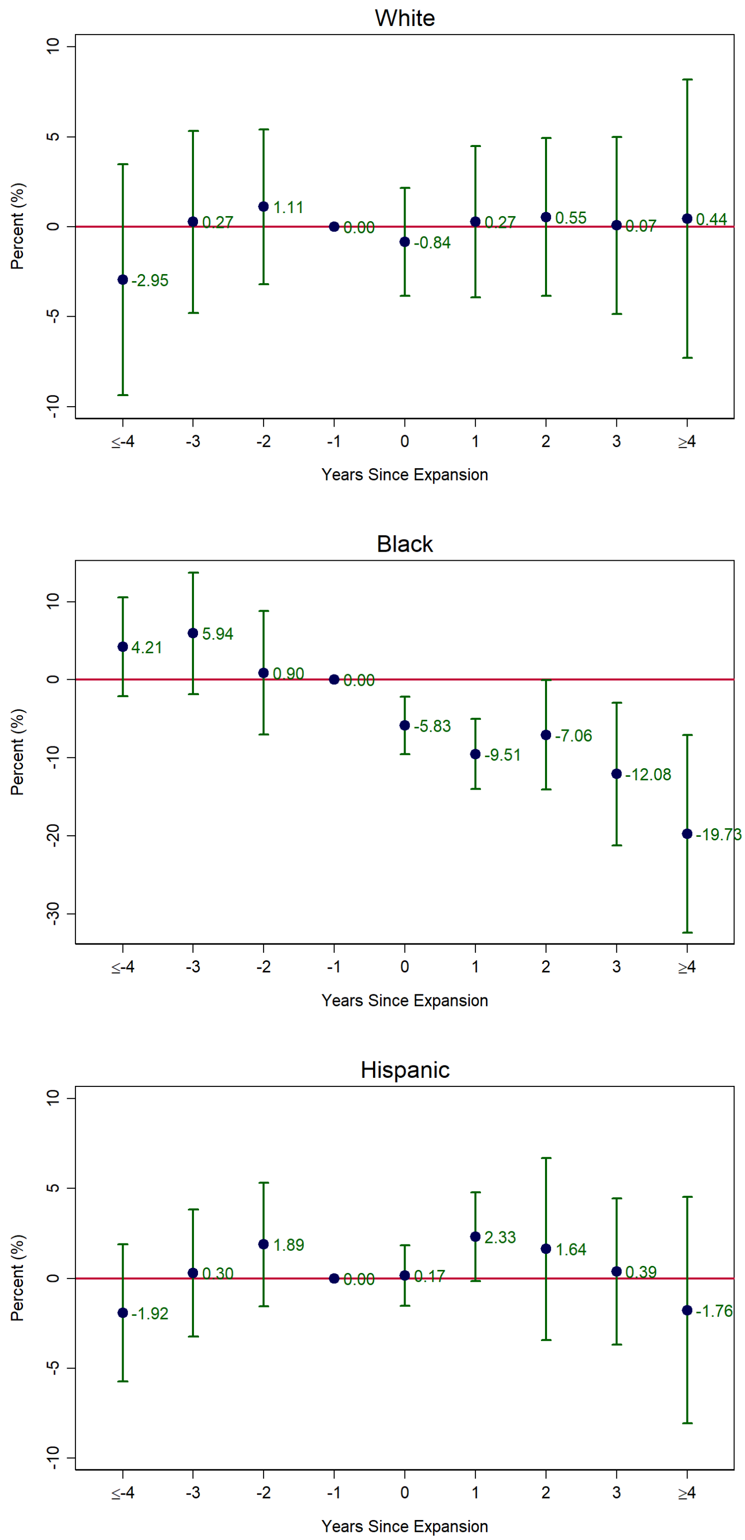

Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

Figure B.9: Balanced sample




Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state. Samples exclude: CA, CT, DE, D.C., LA, MA, MN, MT, NJ, NY, VT, WA.

Figure B.10: With racial groups' population weights

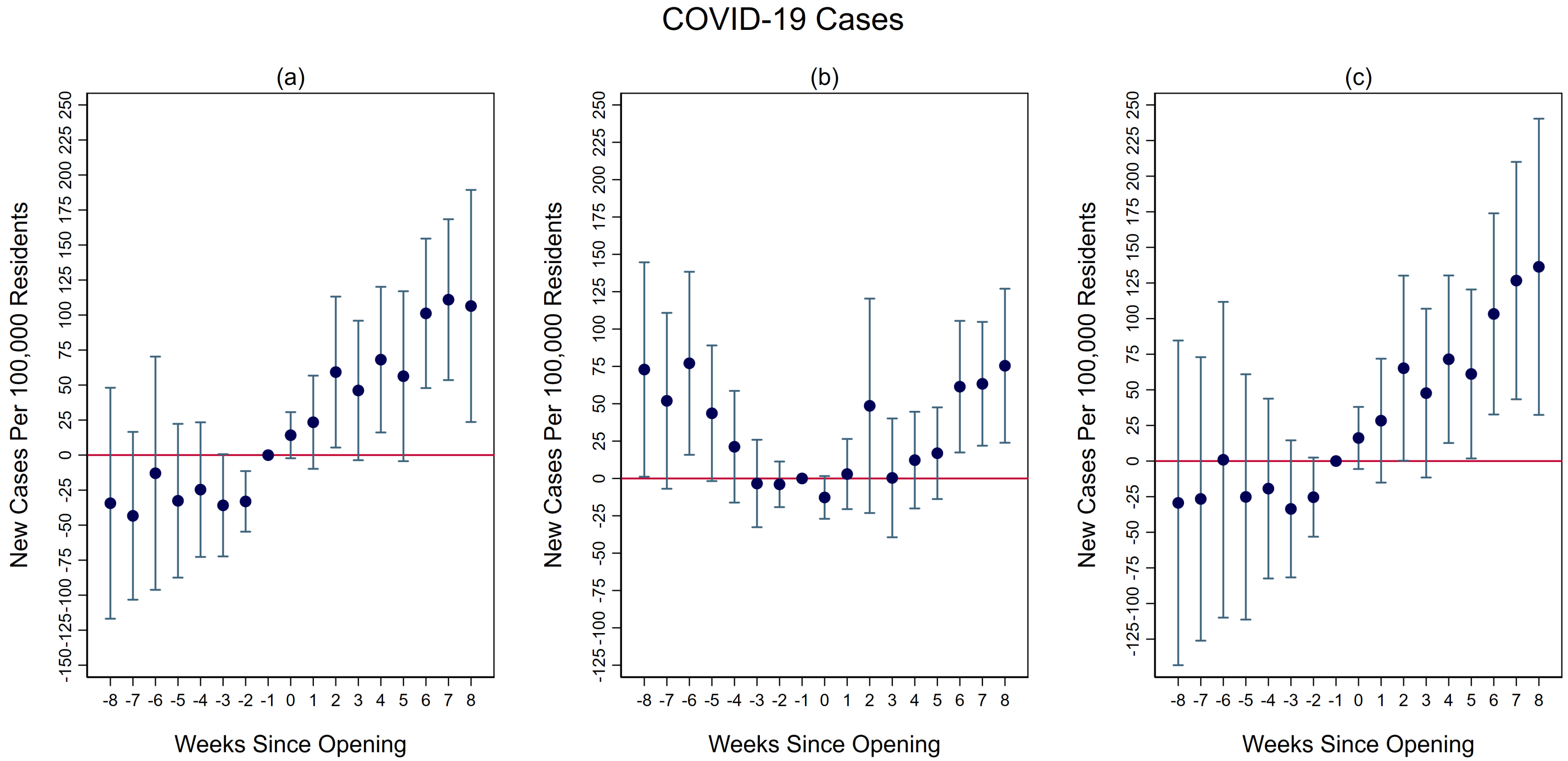



Notes: Estimates and 95% confidence intervals are results from estimating equation 2.1. *t=0* is the year of expansion, and *t=-1* is the reference year. Specifications are weighted by state's 2010 Census population. Standard errors are adjusted for heteroskedasticity and are clustered by state.

# Appendix C

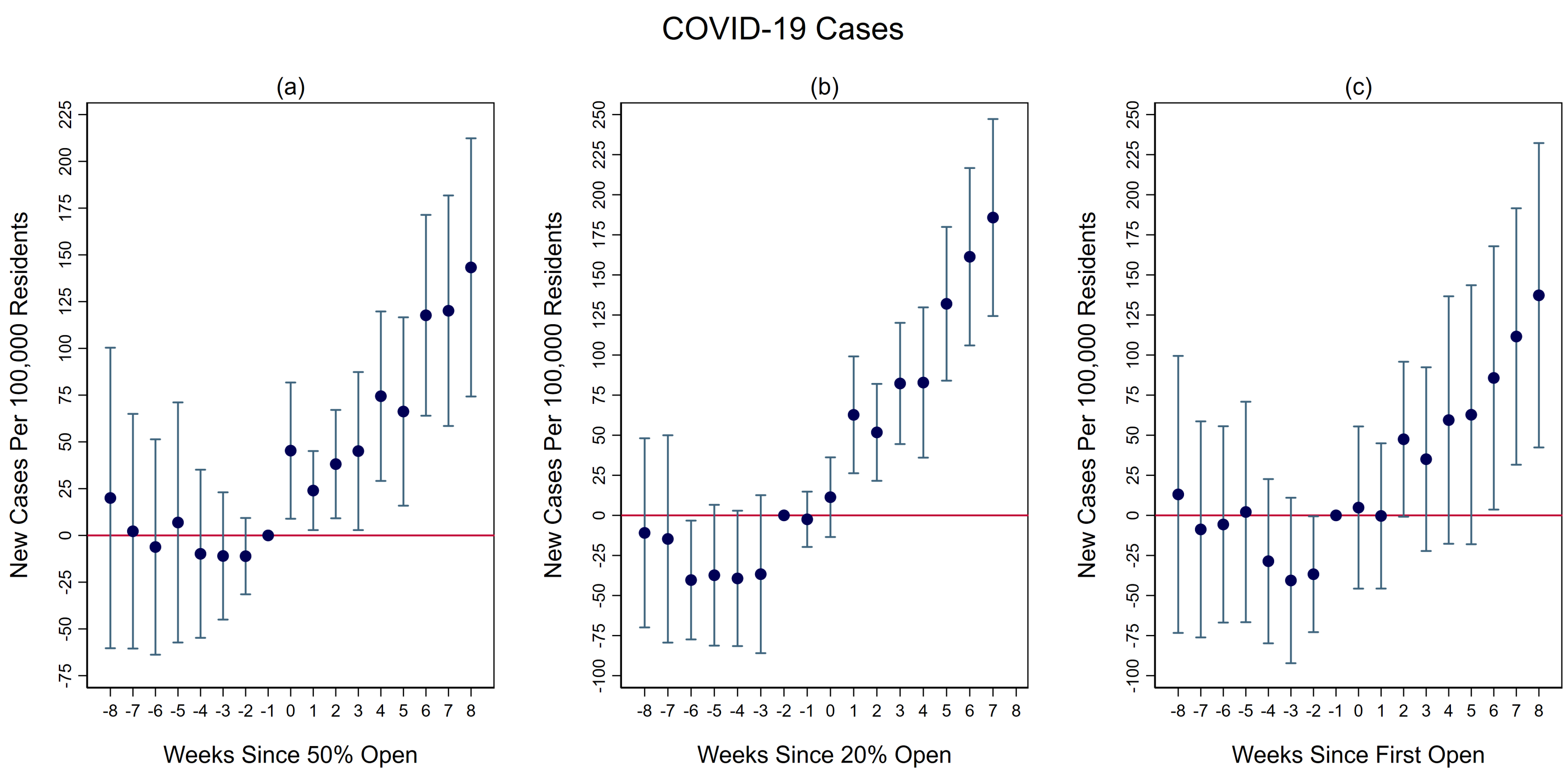
## Supplemental Material for Chapter 3

Figure C.1: Robustness checks: (a) Control for college reopenings; (b) Control for county-specific time trends; (c) Control for Trump vote share interacted with week fixed effects.



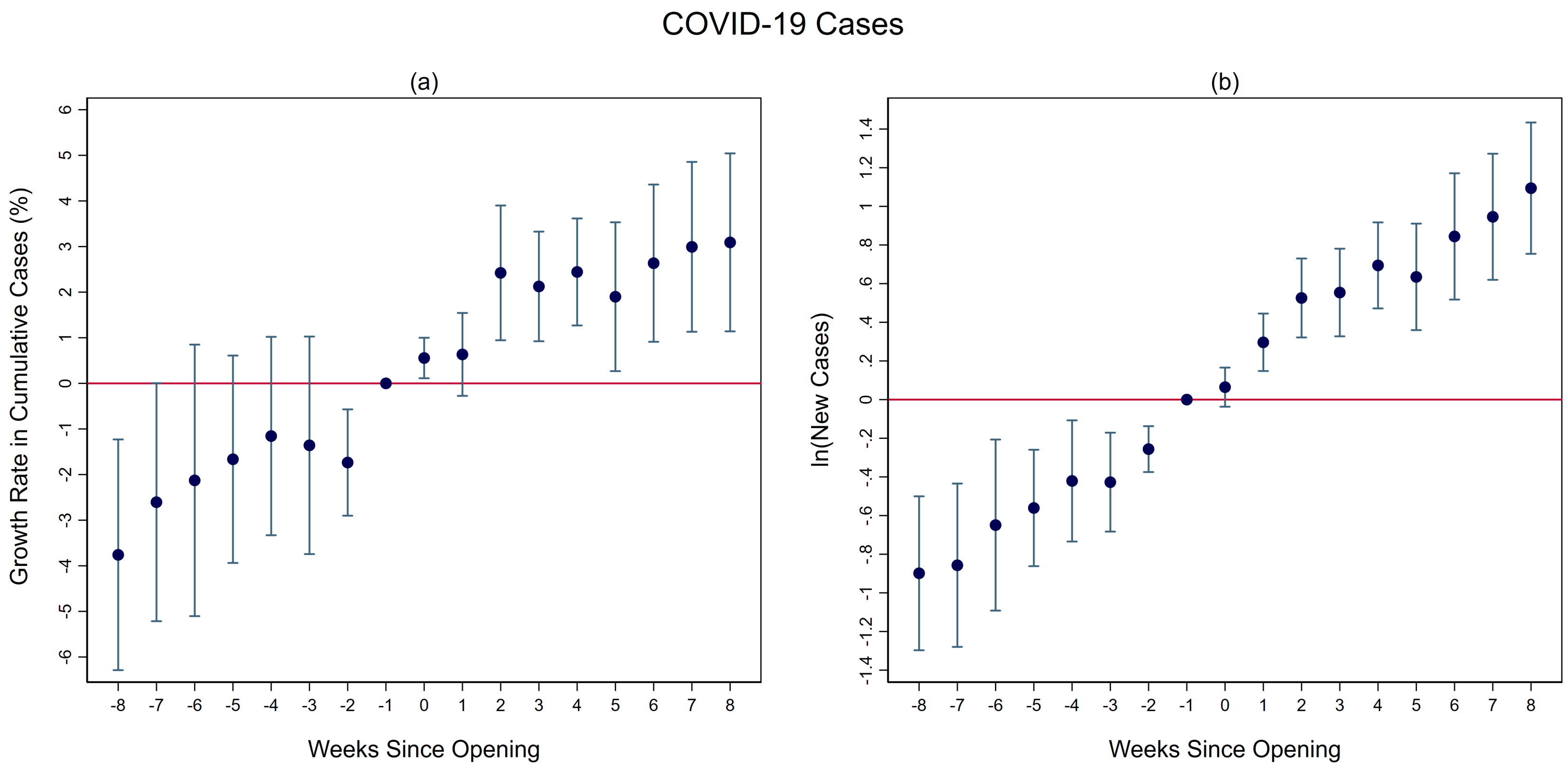

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.2: Alternative Treatment definitions for school reopenings: (a) 50% open; (b) 20% open; (c) First open



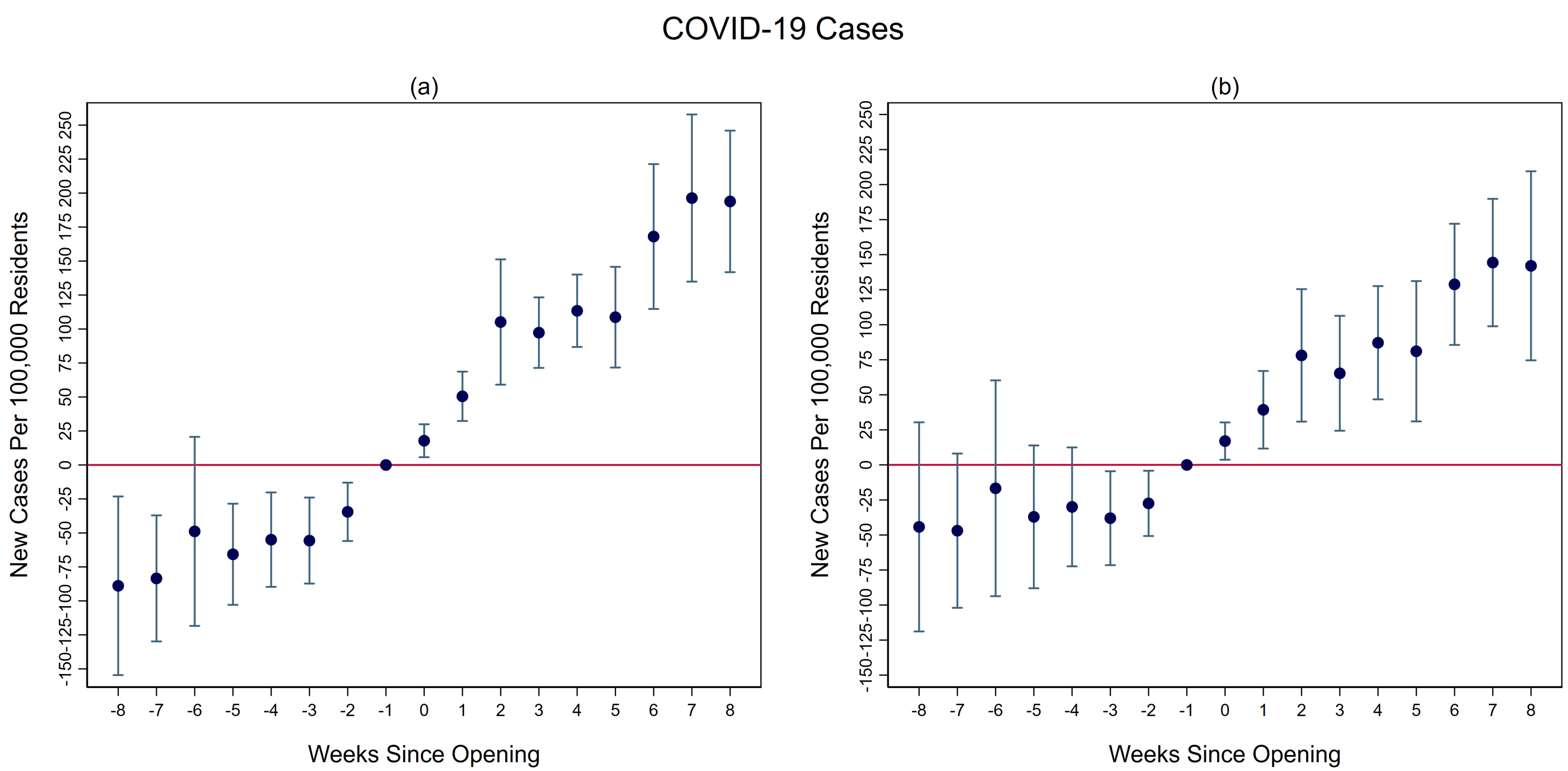

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.3: Robustness checks: (a) Outcome: cumulative cases exponential growth rate; (b) Outcome: the natural log of new case count.



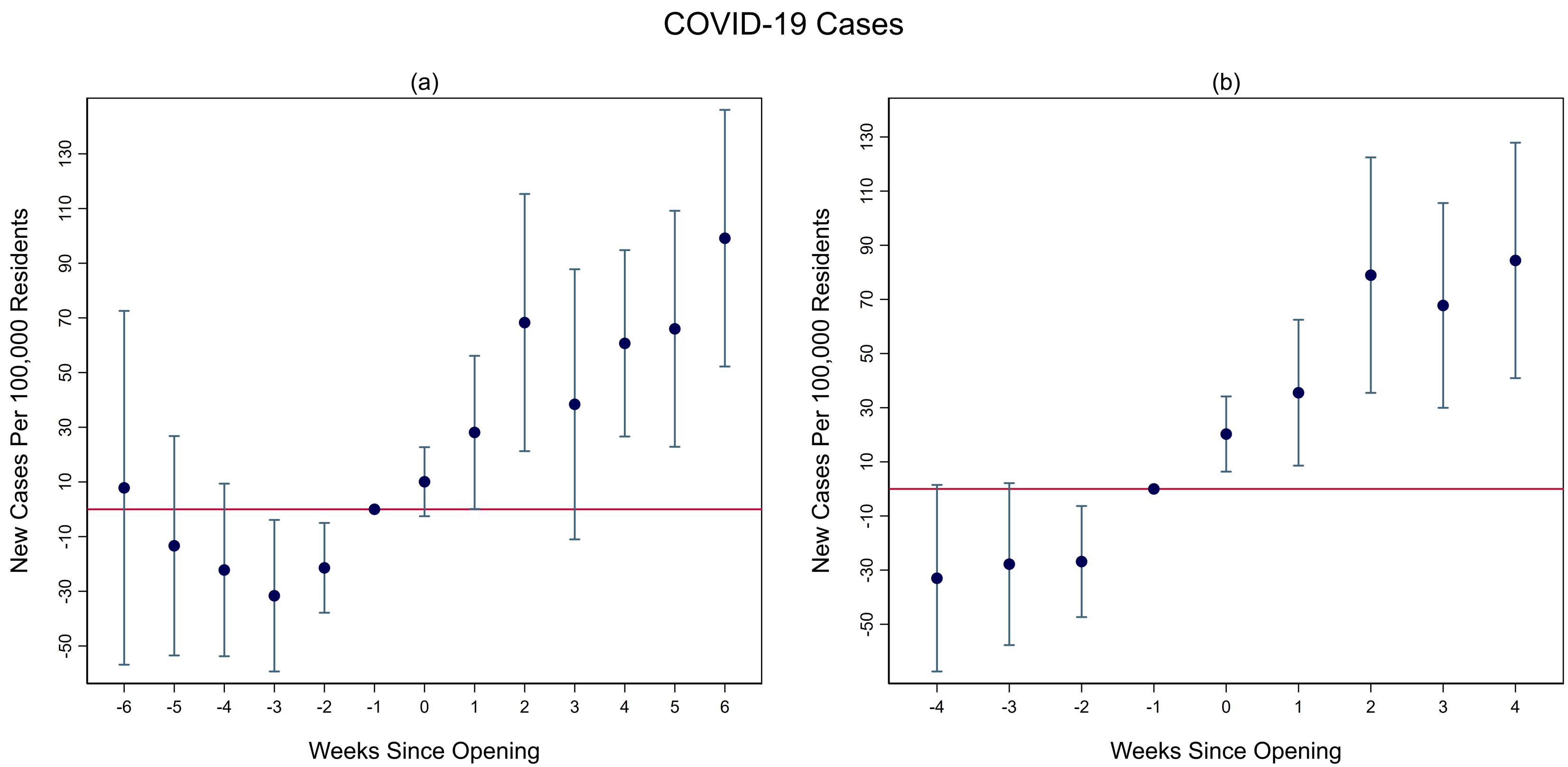

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.4: (a) Drop testing variables; (b) Control for negative tests



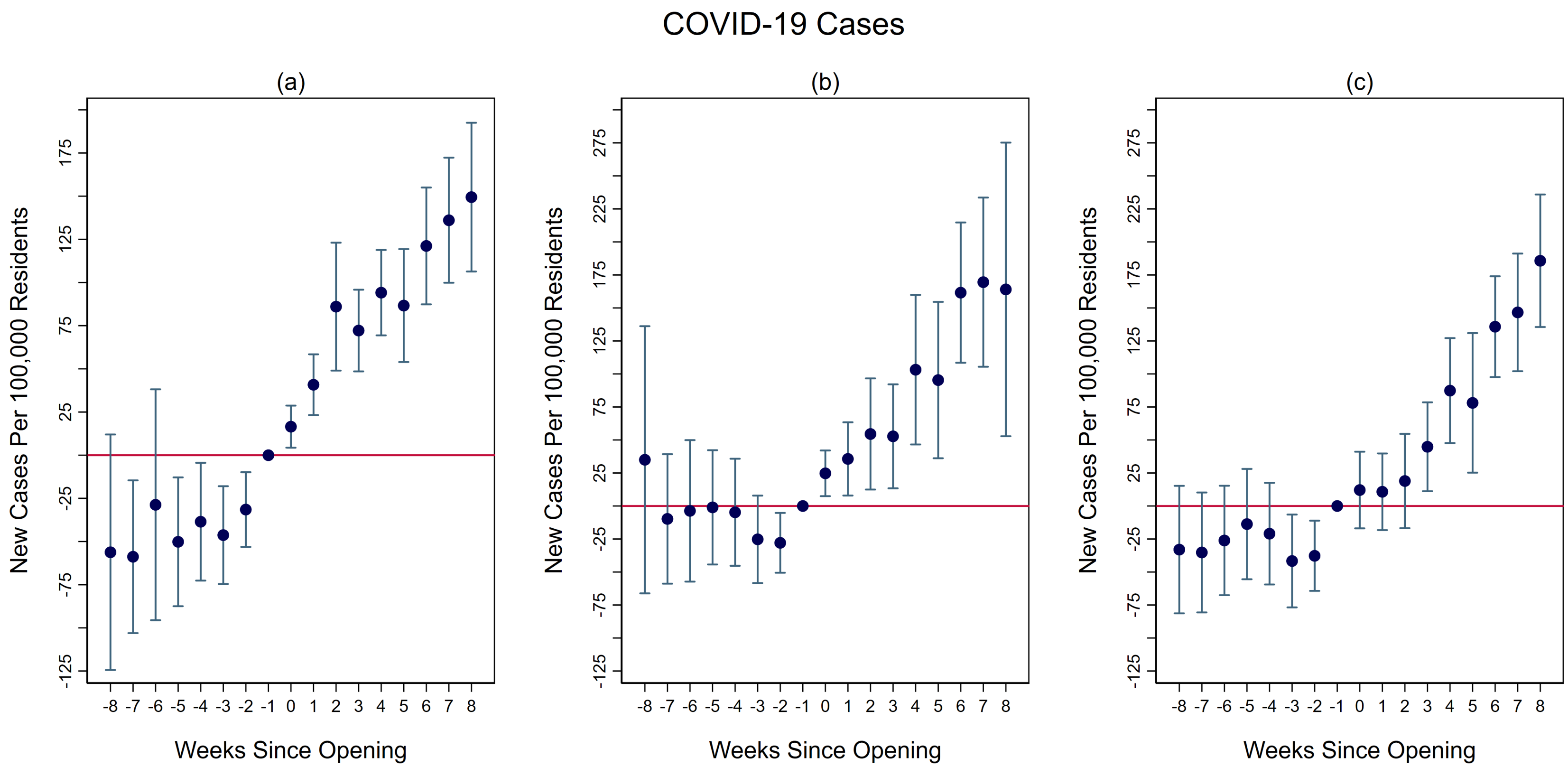

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.5: (a) 6-week event-study window; (b) 4-week event-study window.



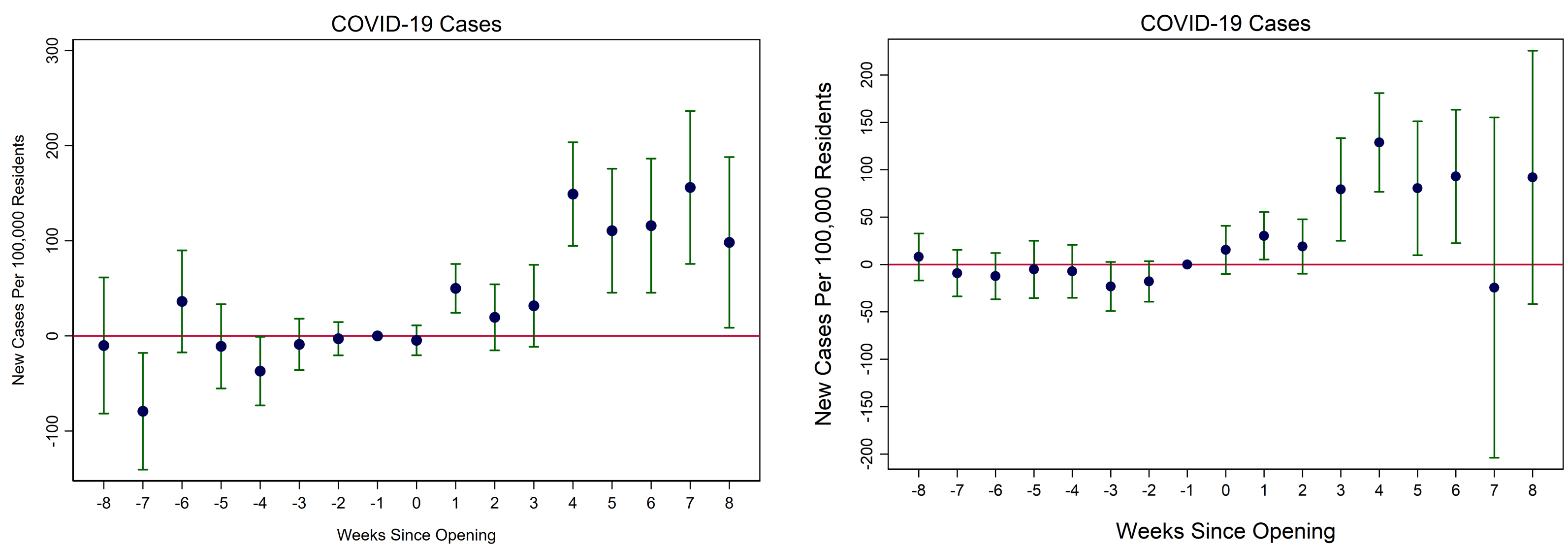

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.6: (a) Drop El Paso County; (b) Drop six counties w/ > 1 mil residents; (c) Unweighted.



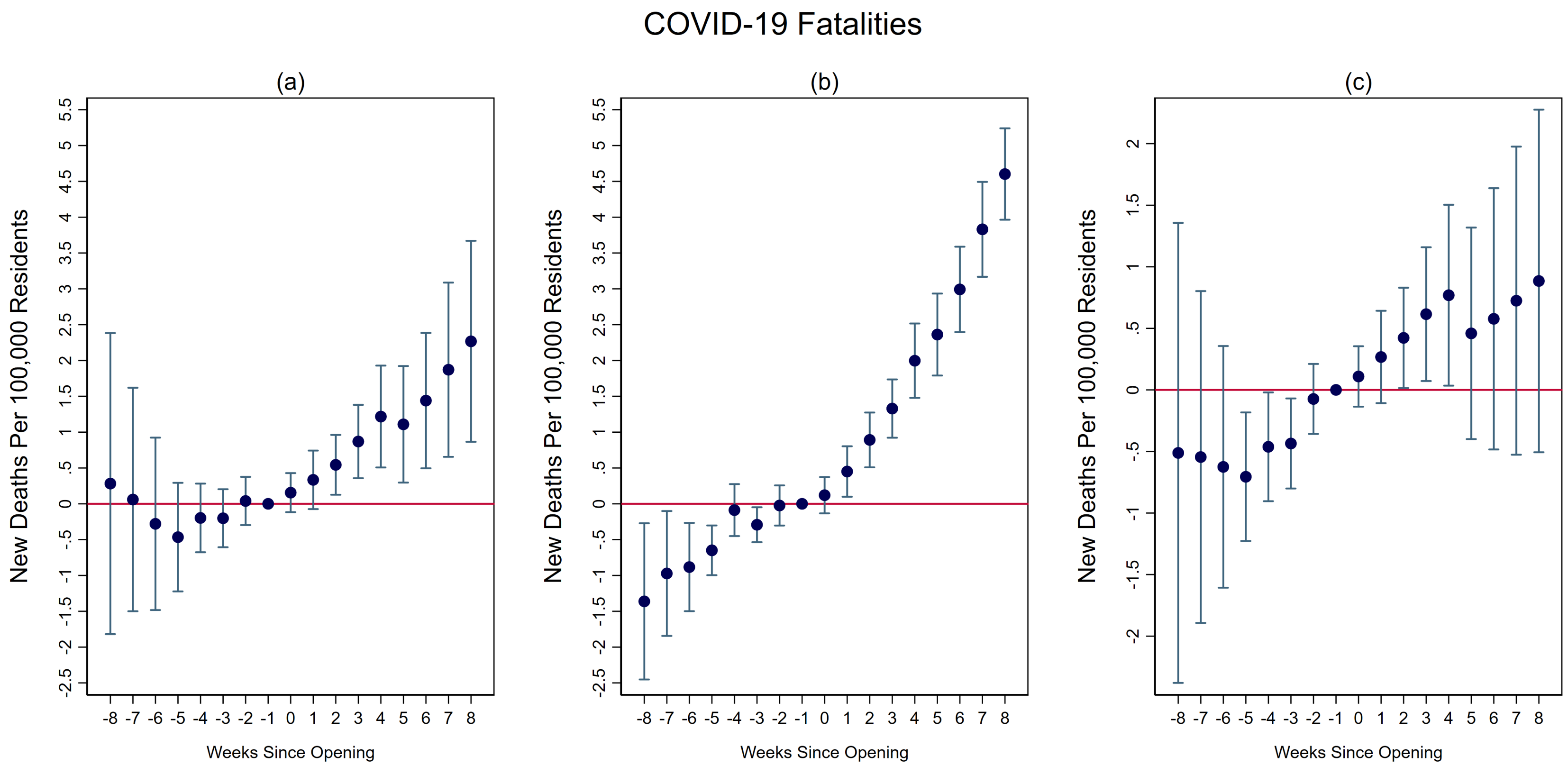

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.7: (a) Stacked regression; (b) Callaway and Sant' Anna method.



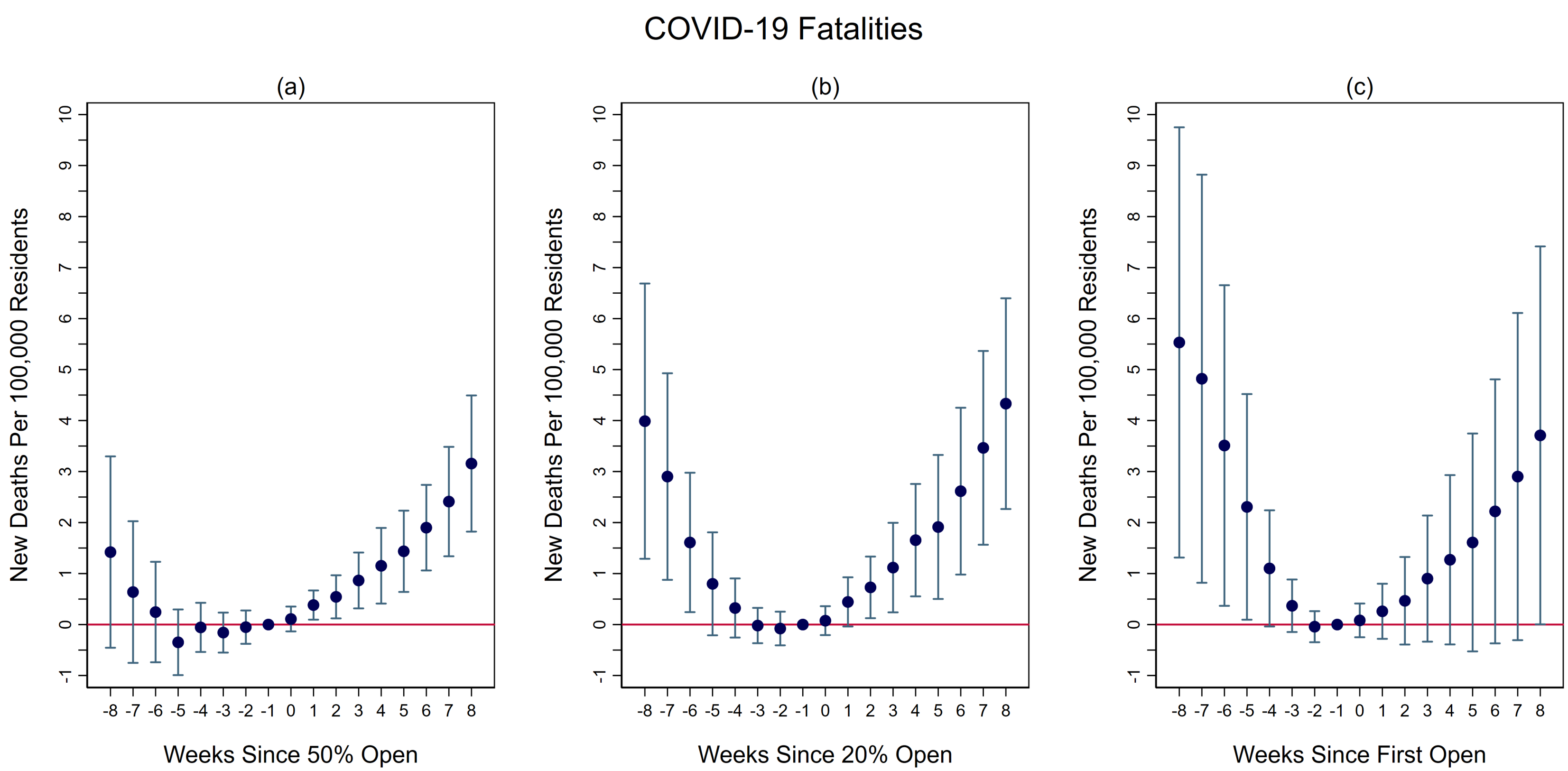

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.8: (a) Control for college reopening; (b) Control for county-specific time trends; (c) Control for Trump vote share interacted with week fixed effects.



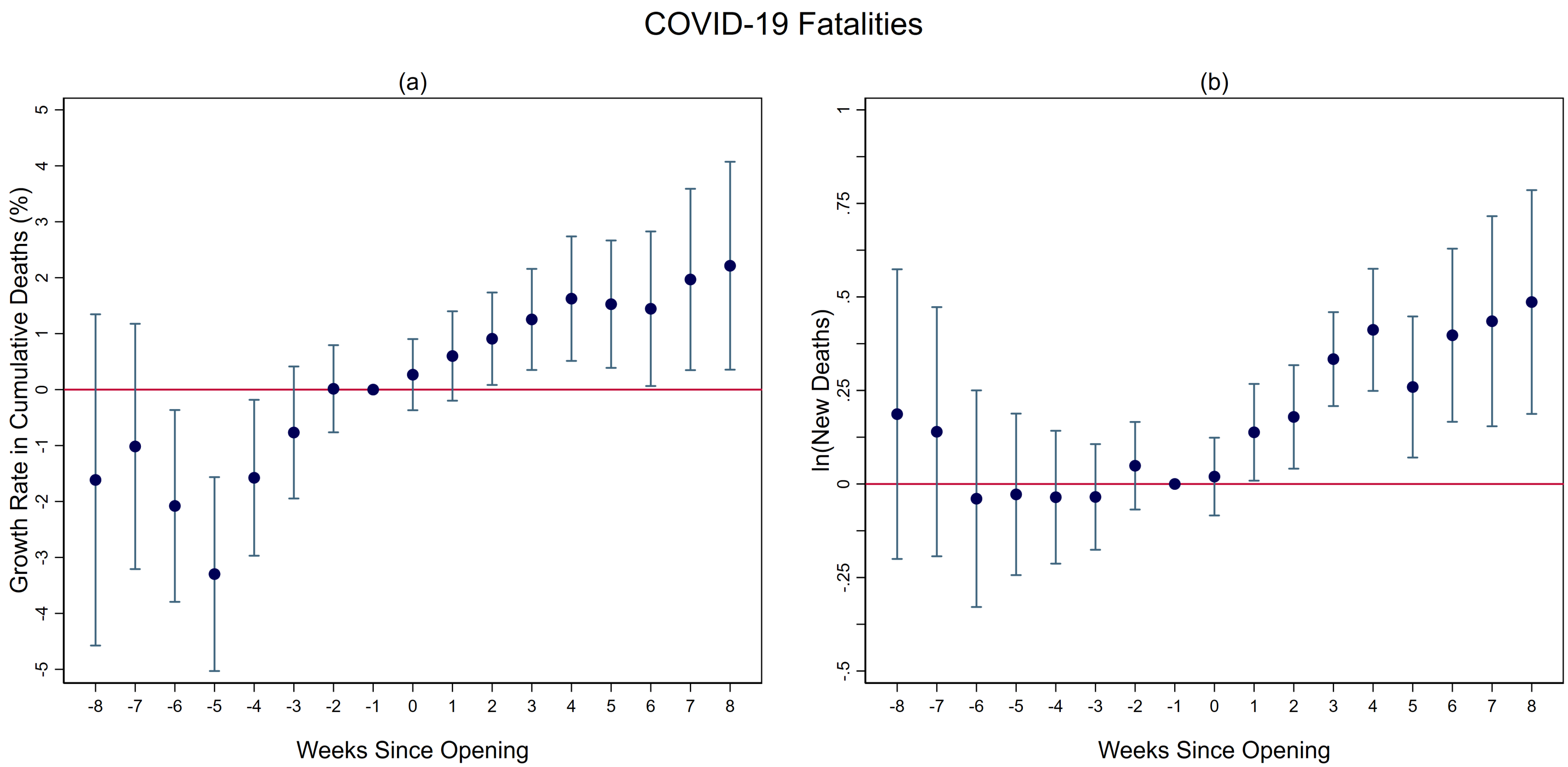

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.9: Alternative Treatment definitions for school reopenings: (a) 50% open; (b) 20% open; (c) First open



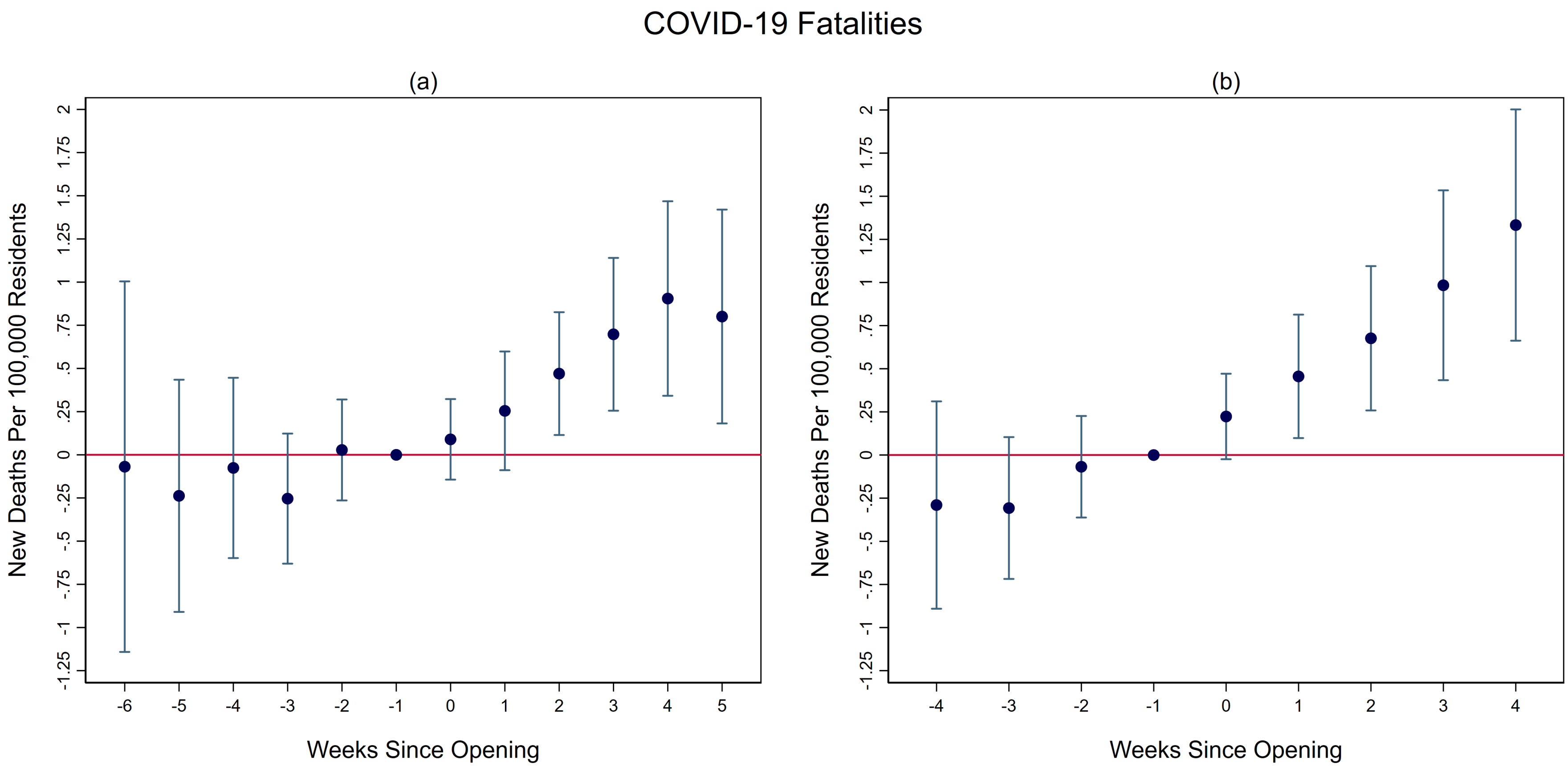

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.10: (a) Outcome: cumulative cases exponential growth rate; (b) Outcome: the natural log of new case count.



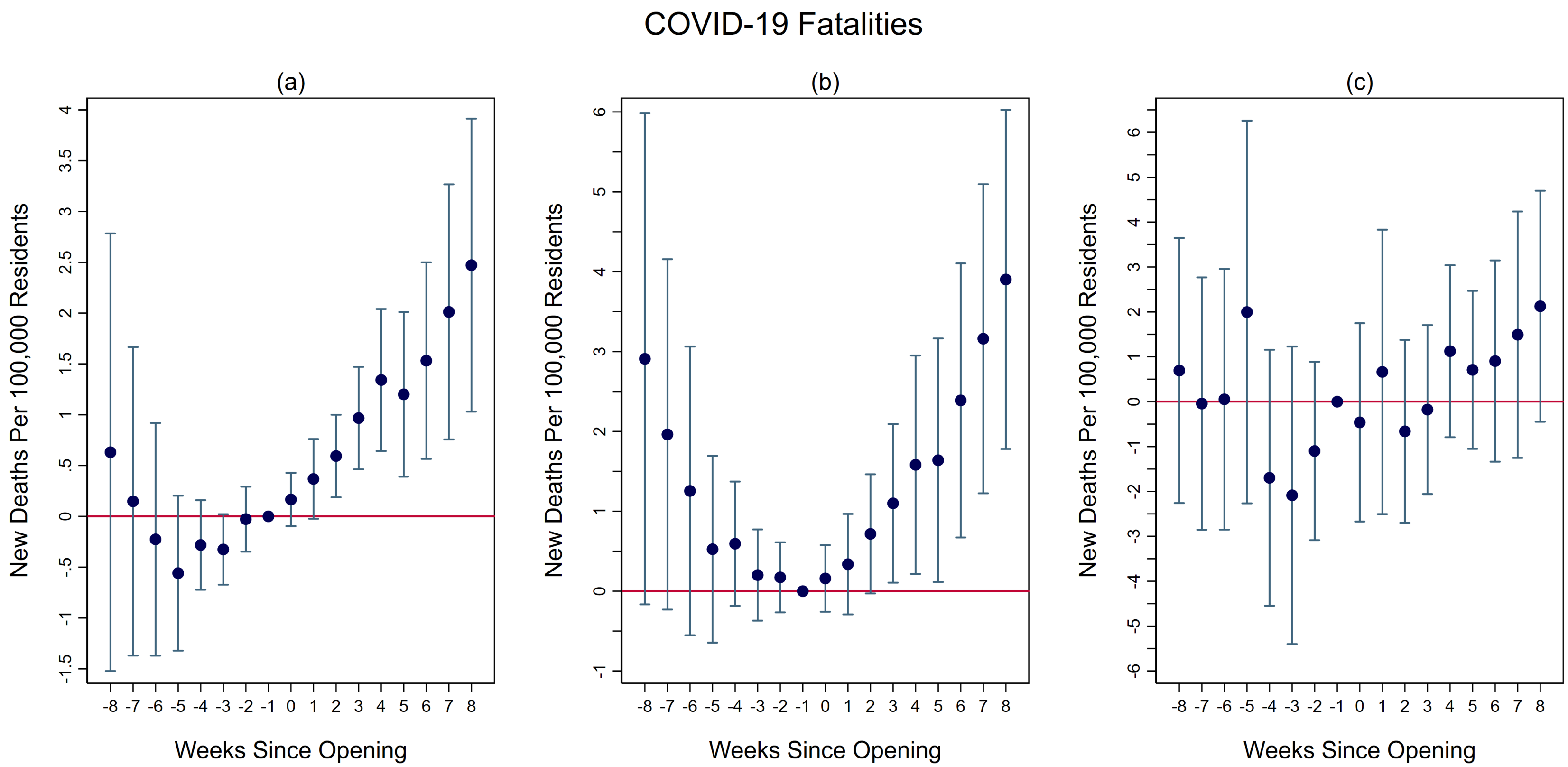

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.11: (a) 6-week event-study window; (b) 4-week event-study window.



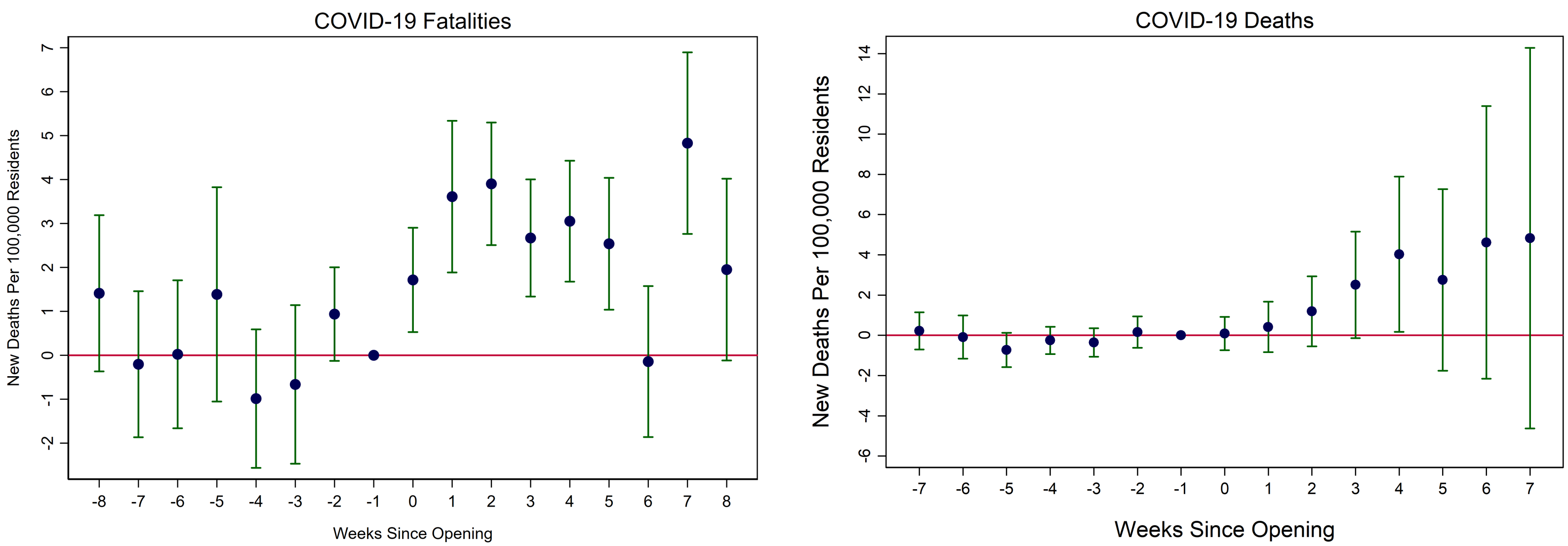

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.12: (a) Drop El Paso County; (b) Drop six counties w/ > 1 mil residents; (c) Unweighted.



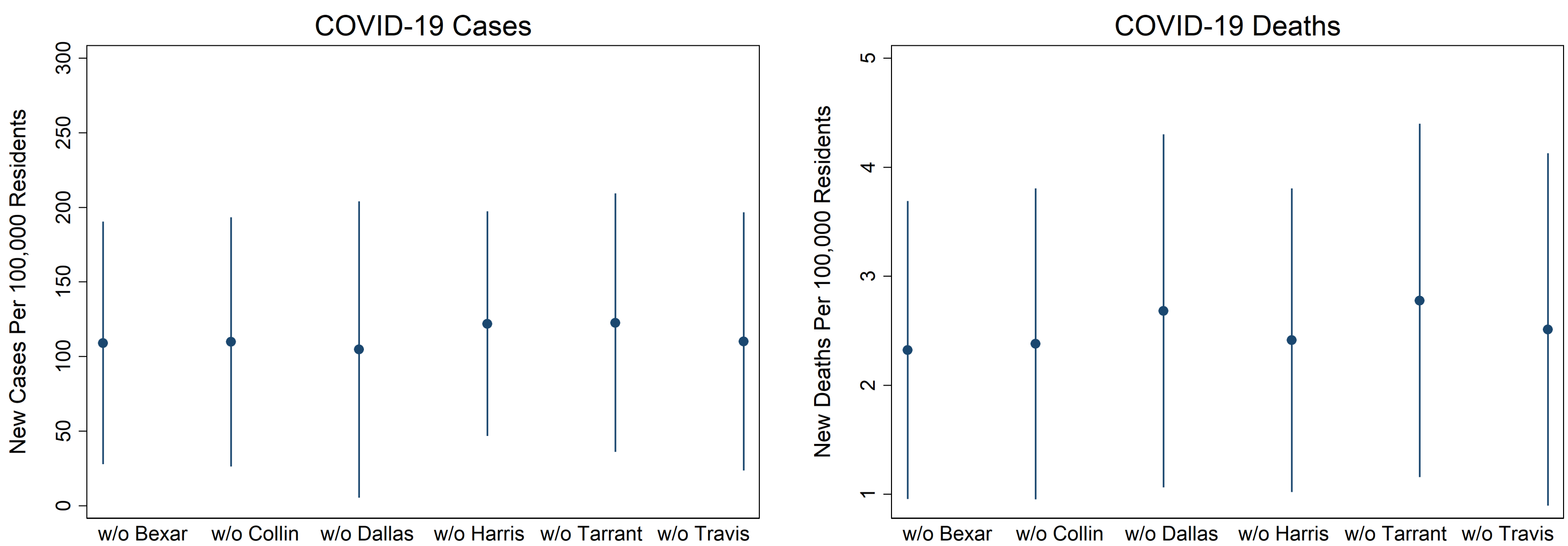

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.13: (a) Stacked regression; (b) Callaway and Sant’ Anna method.



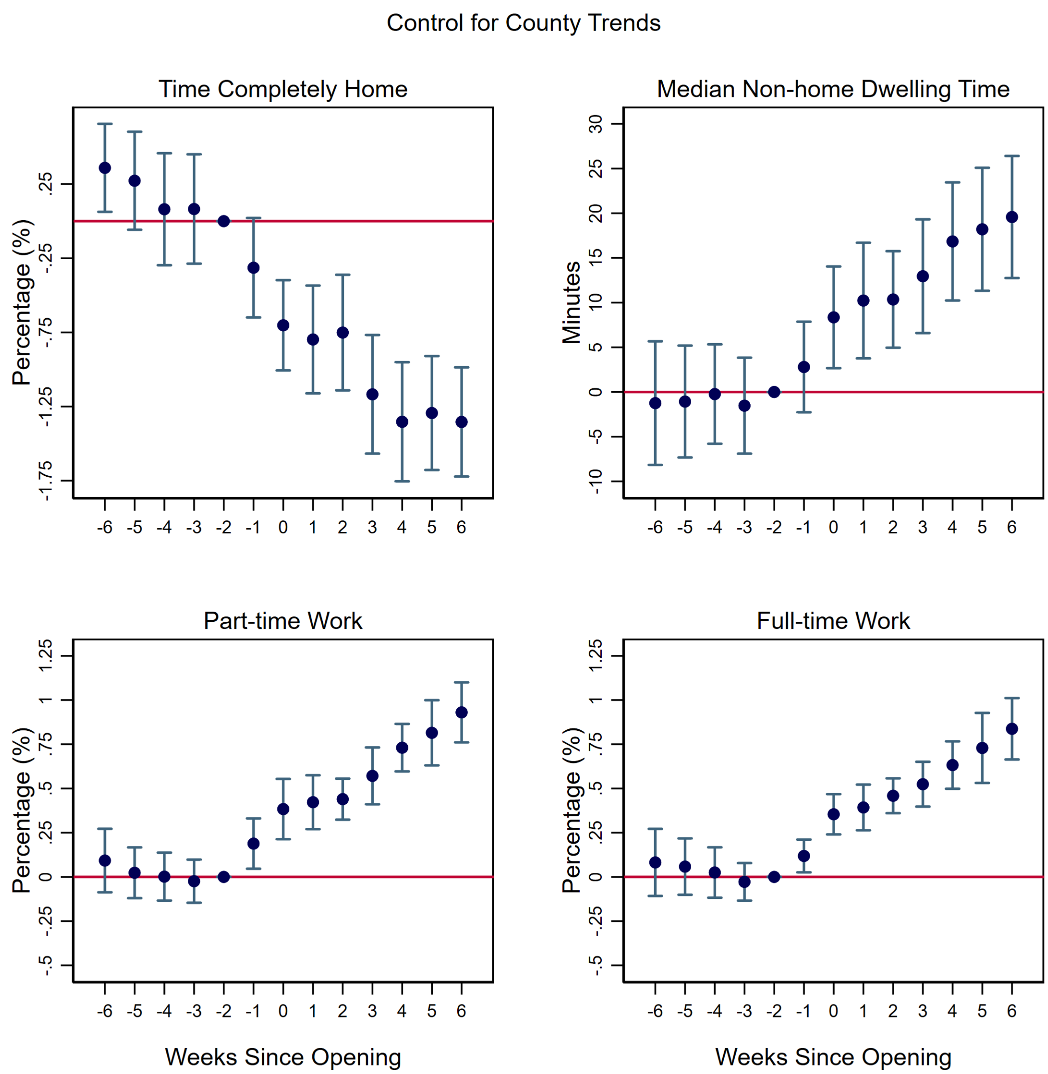

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.14: Specifications leaving-out each of the 6 largest counties in Texas one at a time – COVID-19 outcomes at week +8.



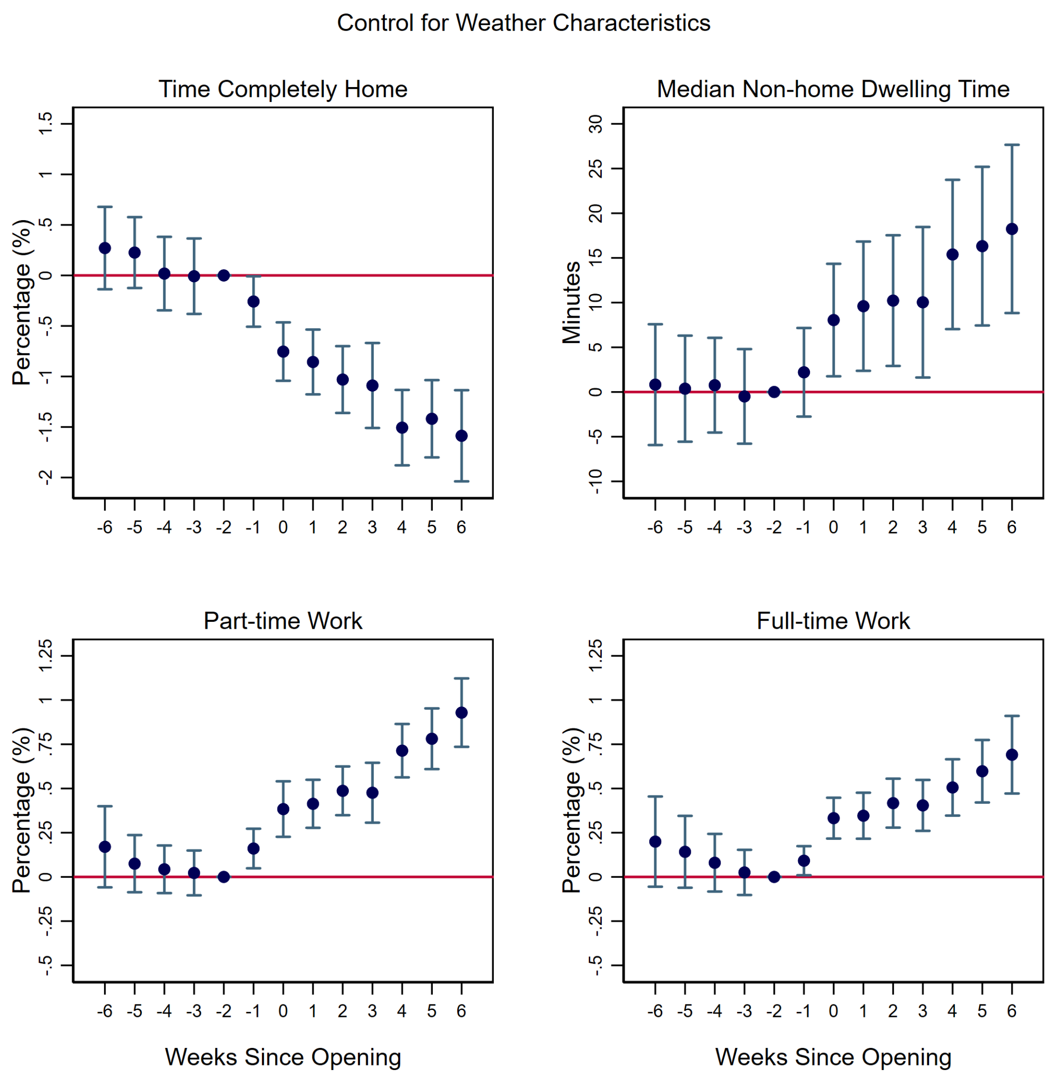

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.15: Effects of school reopening on mobility - Control for county-specific time trends (all CBGs, full-week sample)



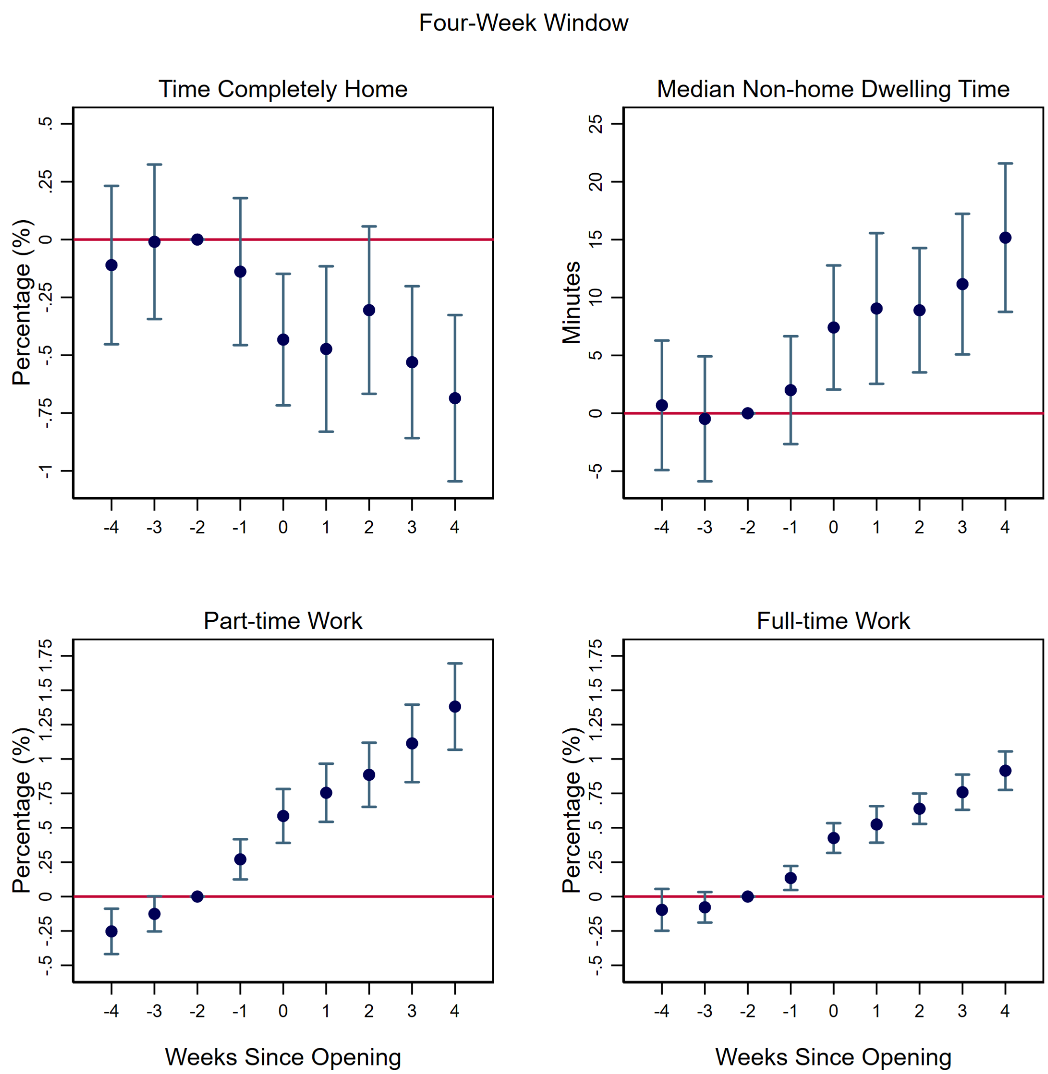

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.16: Effects of school reopening on mobility - Control for weather variables (all CBGs, full-week sample)



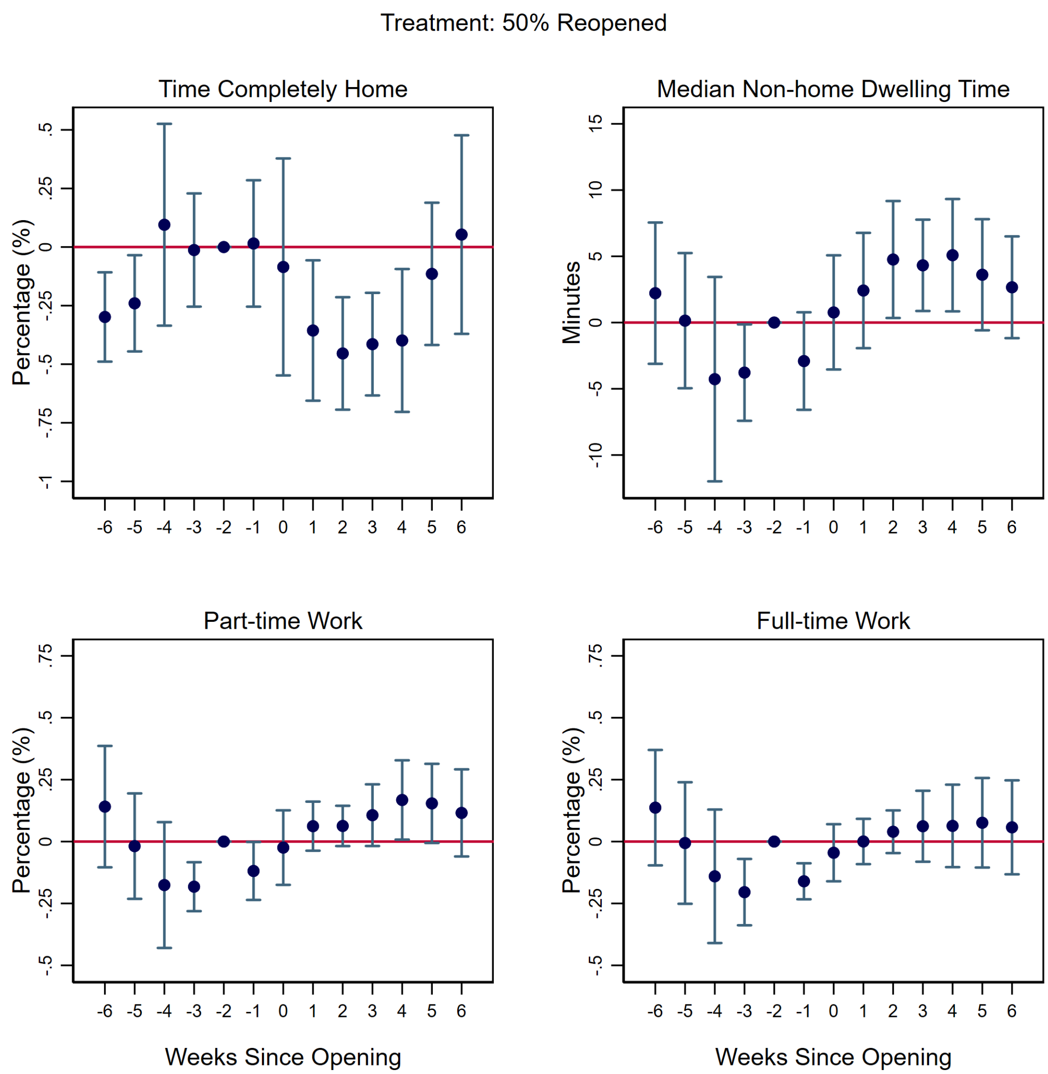

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.17: Effects of school reopening on mobility - 4-week event-study window (all CBGs, full-week sample)



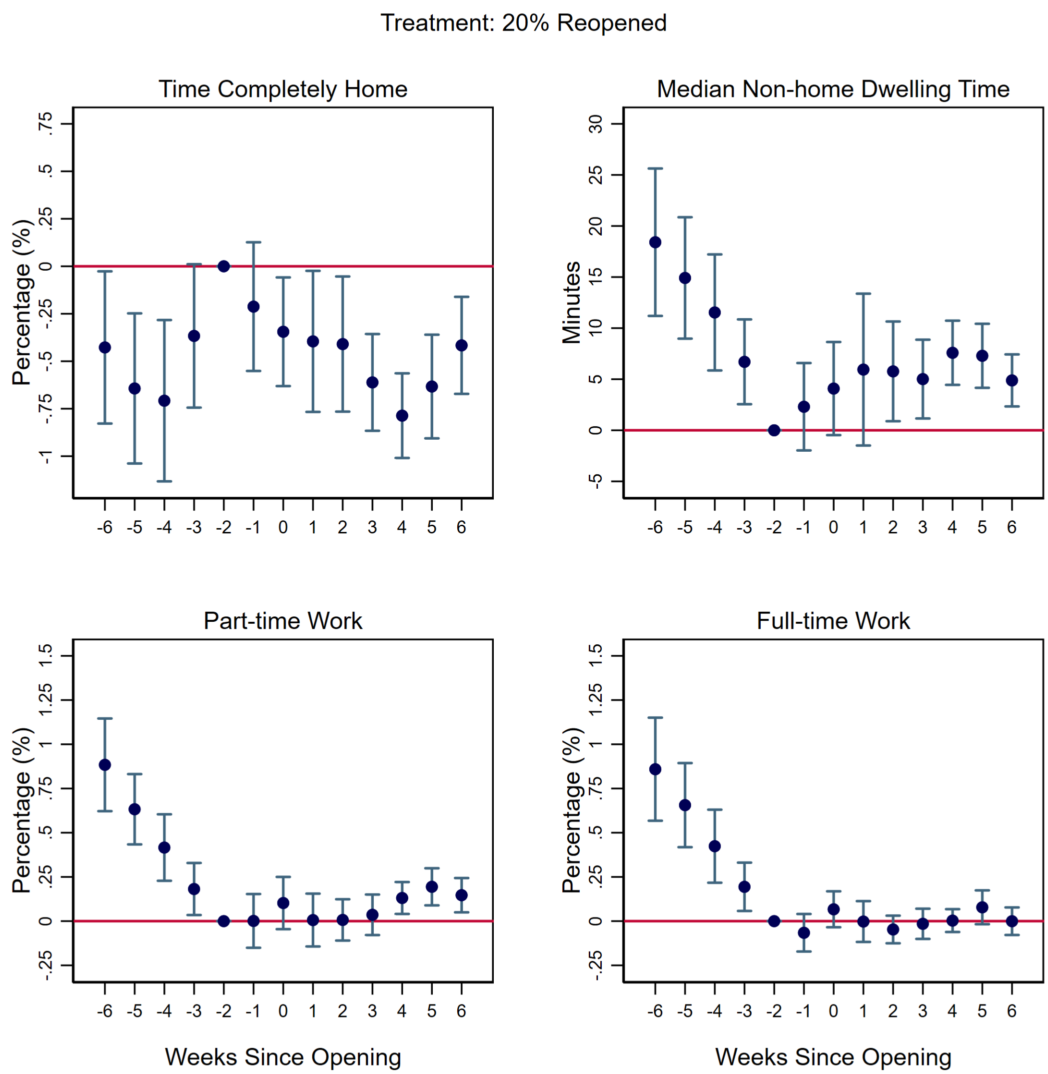

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.18: Effects of school reopening on mobility - Treatment defined when 50% of students attend reopened schools (all CBGs, full-week sample)



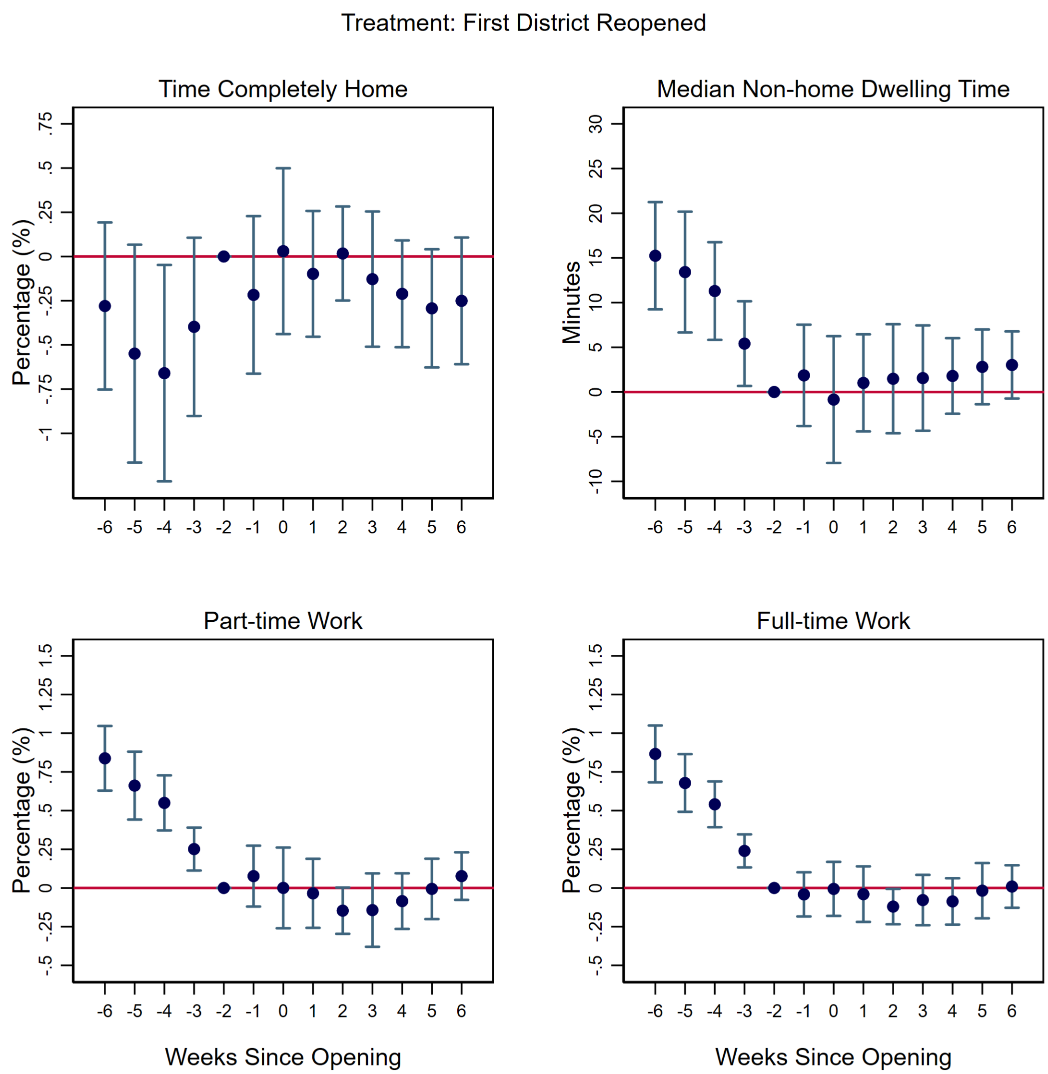

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.19: Effects of school reopening on mobility - Treatment defined when 20% of students attend reopened schools (all CBGs, full-week sample)



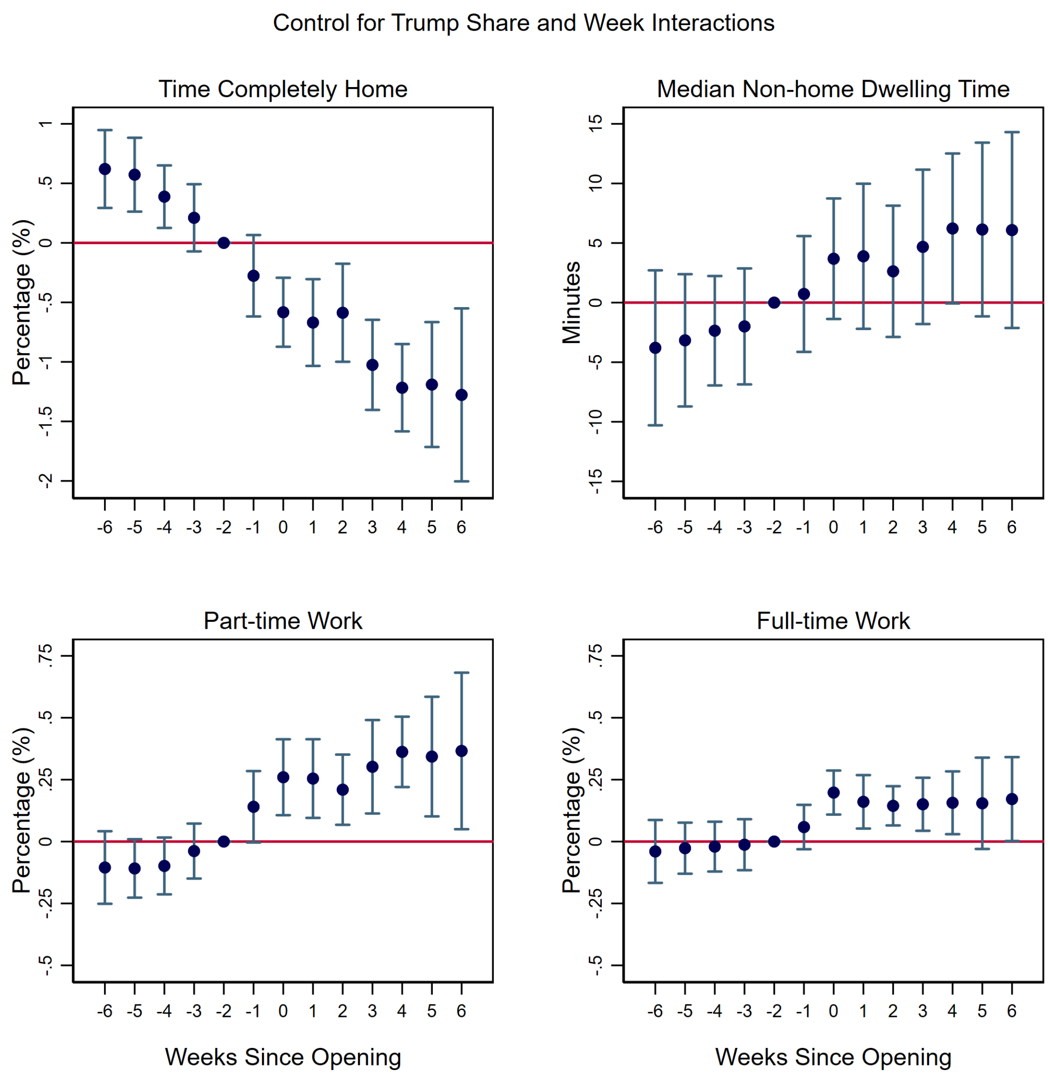

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.20: Effects of school reopening on mobility - Treatment defined when first school district reopened (all CBGs, full-week sample)



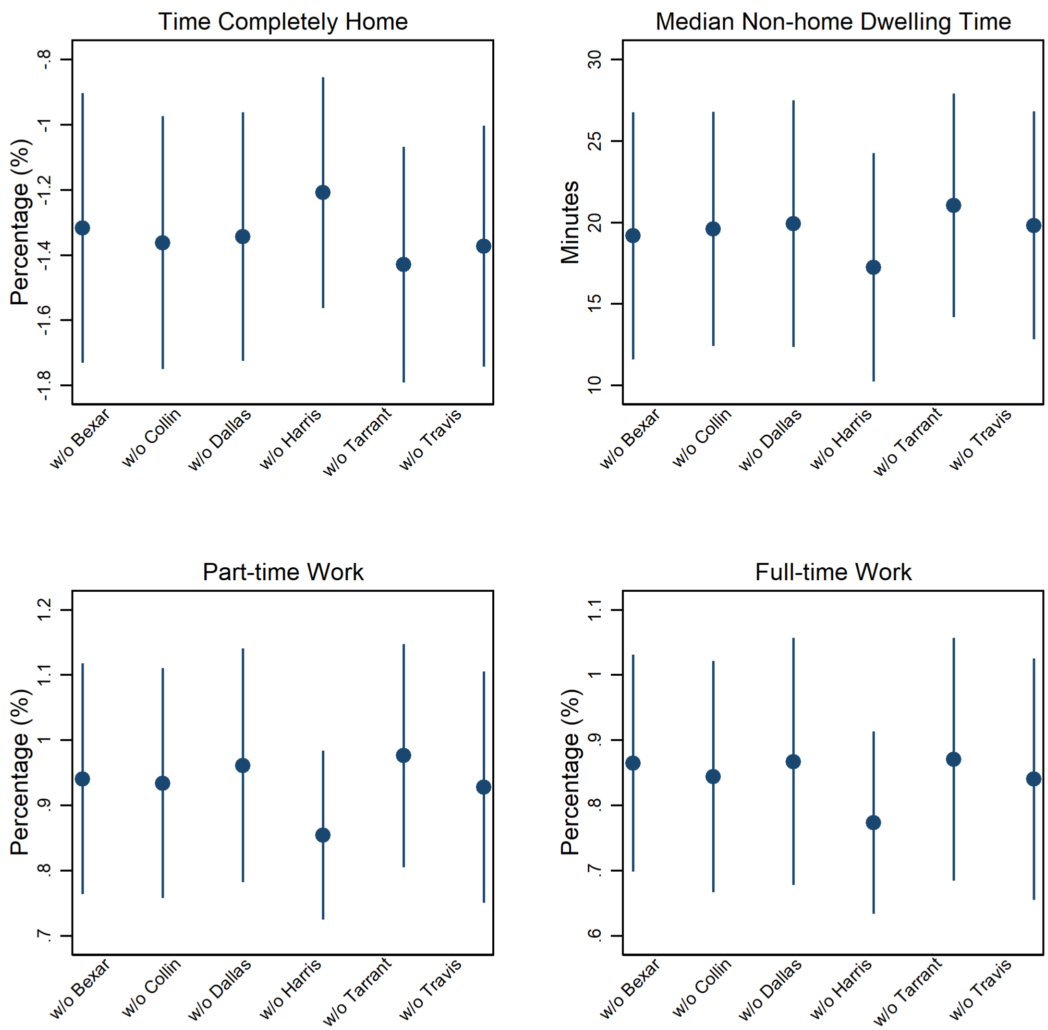

Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.21: Effects of school reopening on mobility - Control for Trump vote share interacted with week fixed effects (all CBGs, full-week sample)




Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

Figure C.22: Effects of school reopening on mobility - Control for Trump vote share interacted with week fixed effects (all CBGs, full-week sample)




Notes: Estimates and 95% confidence intervals are results from estimating equation 3.1. Each panel reports a separate dependent variable. Regressions are weighted by county population. Standard errors are robust to heteroskedasticity and clustered by county.

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### Essay 1


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**Essay 2**


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