Essays on Health Economics

by

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(Under the Direction of Josh Kinsler)

Abstract

This dissertation studies the impact of private health insurance expansions on mental and spillover outcomes. Using state level insurance expansions known as Mental Health Parity Laws (MHPLs) as an exogenous variation, I investigate the impact of increased access to mental health treatment on indirect and direct outcomes.

The first chapter studies the impact of increased access to mental health on criminality. Prior research shows that expanding access to comprehensive health insurance coverage through Medicaid reduces criminality. However, little is known about the specific effects of mental health insurance coverage on crime. I study whether increased access to mental health care, following the passage of MHPLs, affects crime. I use county-level crime data from 1994 to 2010 and exploit temporal and geographic variation in the passage of MHPLs to estimate a causal link. I implement a difference-in-differences research design using two way fixed effects models, along with recent estimators that allow for treatment effect heterogeneity. Results indicate that the passage of MHPLs reduced violent crime by 5-7% – driven by a substantial decrease in aggravated assault.

In the second chapter, I re-investigate the effects of MHPLs on mental health outcomes. I examine the impact of MHPLs on suicide rates, a proxy for mental health. Additionally, I investigate the impact of MHPLs on measures of youth mental health outcomes and investigate potential mechanisms. I use a difference in differences design, and an event study design to estimate the effects using a two way fixed effects model. In addition, I employ recent methodological advances that circumvent issues related to heterogeneous treatment effects due to treatment timing. Results indicate that there is no significant impact of MHPLs on suicide rates that is in contrast to the most recent research on impacts of MHPLs on suicide rates. The results are robust and true across specifications. Similarly, there is no impact of MHPLs on youth mental health outcomes. Finally, I investigate supply side mechanisms. I find that there is no supply side response of MHPLs on substance abuse and mental health treatment centers. This possibly explains the null impacts of MHPLs on mental health outcomes.

Index words: [Health Policy, Mental Health, Mental Health Parity Laws, Criminality, Suicide, Mental Health Outcomes, Economics of Mental Health]

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Dedication

I dedicate this dissertation to my mother Ms. Rupakala Gyawali, and all my family members.

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Contents

List of Figures

List of Tables

Chapter 1

Do Mental Health Parity Laws Reduce Crime?

1.1 Introduction

Untreated mental illness is often cited both by popular media and academic research as an important risk factor driving criminal behavior (Steadman et al., 1998; Elbogen et al., 2007; Fazel et al., 2014). Individuals with mental illness account for a significant portion of jail populations, and jails are often cited as America's largest mental health hospital.[1] A Bureau of Justice Statistics study by Bronson and Berzofsky (2017) notes that in 2011-2012, 37 percent of prisoners and 44 percent of jail inmates were diagnosed with mental health illness prior to incarceration.[2] By contrast, in 2019, only 20.6% of US adults experienced mental illness (National Alliance on Mental Health). In addition to the disproportionately large mentally ill population in jails and prisons, crime is enormously expensive to society, with estimates on its true cost ranging from US $690 billion to $3.41 trillion annually (Maurer, 2017).[3]

[1]See, for example, *The Atlantic*'s news article on June 8, 2015.

[2]Inmates are offenders confined in short-term facilities usually administered by a local law enforcement agency. This is distinct from prisoners, who are confined in long-term facilities run by a state government or federal agency, typically for incarceration of more than a year.Click here

[3]Dollar amounts reported in 2016 dollars. Heaton (2012) and Maurer (2017) have extensive reviews on costs of crime.

Motivated by an apparent link between mental health and crime, and by the high costs of crime, this study investigates the effects of expanding access to mental health treatment on crime. Beginning in the 1990s, many US states passed mental health parity laws (MHPLs) aimed at improving the overall mental health of the population at-large. These laws generally increased access to mental health care by mandating that health insurance plans provide coverage for mental illness equal to coverage for physical illness. Equal coverage includes visit limits, co-payments, annual limits, and deductibles (Robinson, 2007; Carney, 2021). Although the purpose of MHPLs was primarily to improve overall mental health, an additional benefit may have been its effect on crime. Thus, these laws provide an opportunity to examine whether increased access to mental health treatment can be a potential policy tool for crime reduction.

I estimate expanded mental health care coverage's effect on crime using county-level crime data from 1994-2010 combined with a staggered adoption of MHPLs across states. The staggered adoption allows for a difference-in-differences research design. I first implement this approach with a standard two-way fixed effects estimator. Recent work in the difference-in-differences literature identifies important weaknesses that can bias results when using this estimator. As a consequence, I also use the more recently developed estimator by Callaway and Sant'Anna (2020), which accounts for various pitfalls associated with difference-in-differences estimation when using the two-way-fixed effects model.

I find that MHPLs reduce the incidence of violent crime, with the effect driven primarily by reductions in aggravated assault. Specifically, the passage of MHPLs reduces violent crime by between 5-7%. Disaggregating by the type of violent crime, I find that MHPLs reduce aggravated assault by approximately 7%. However, MHPLs do not have a significant and robust effect on either property crime overall or on different types of property crime. Event study plots show a gradual, post-treatment drop in violent crimes and aggravated assault, with flat pre-trends. The results from Callaway and Sant'Anna (2020)'s aforementioned new method, which accounts for heterogeneity in treatment effects due to treatment timing, are consistent with the standard model. Further, I demonstrate that the estimated reductions in violent crime are robust to alternative data aggregation schemes and different transformations of the outcome variables.

Reductions in criminality associated with the passage of MHPLs suggests that there are potentially large unanticipated benefits of these policies. In a final exercise, I translate the estimated MHPL-induced crime reduction into dollars. I project that, on average, a state that passes a MHPL will have roughly 25 fewer aggravated assaults per county compared to the baseline. Using cost-of-crime estimates from McCollister et al. (2010), these reductions translate to a nationwide yearly savings of approximately $3 billion.

There are at least two possible mechanisms through which the passage of MHPLs may reduce crime. The first is access to treatment that facilitates improved behavior. Expanded mental health care access may improve an individual's ability to control their emotions and consider the likely costs of crime. Previous research finds that mental health care use increases after an MHPL's passage, particularly for those working in small firms (Harris et al., 2006; Busch and Barry, 2008). This is consistent with the fact that small-firm employees are the most likely to be affected by MHPLs, since the Employee Retirement Income Security Act of 1974 (ERISA) exempts self-insured firms from state mandates. Additionally, MHPLs may reduce substance abuse since many of the state MHPLs expand coverage for substance abuse treatment.[4] Substance abuse is often viewed as a complement to criminal behavior, suggesting that a reduction in substance abuse may lead to a decrease in criminal behavior.

The second mechanism is the potential improvement of financial health through an income effect. Reducing the costs of mental health treatment allows households that use mental health services to spend more elsewhere. And a positive income effect increases the costs of committing violent crimes, thereby reducing those crimes. Studies find that MHPLs ease the financial burden in families with children (Barry and Busch, 2007; Demchak, 2007). These studies find lower out-of-pocket spending in states that pass MHPLs compared to those that did not. And, while it is intuitive that an income effect would influence property crime more than violent crime, research shows that increases in income via emergency financial assistance result in fewer violent crime arrests (Palmer et al., 2019).

[4] Dave and Mukerjee (2011) finds that substance abuse treatment admissions increase after MHPL implementation.

This paper contributes to the growing body of literature linking health coverage and criminality. One branch of this literature studies how expanded comprehensive healthcare access affects criminal behavior. Using a difference-in-differences research design, He and Barkowski (2020) finds that Medicaid expansions lead to a significant reduction in burglary, vehicle theft, homicide, robbery, and assault, and Vogler (2020) finds a reduction in violent crimes in the Uniform Crime Reports. Jacome (2020) also studies the link between Medicaid and crime, but instead exploits sharp changes in Medicaid eligibility among adolescents. Using individual-level data from South Carolina, she finds that men who lose Medicaid are 15% more likely to be incarcerated compared to a matched comparison group.

A second segment of the literature looks at the effect of substance abuse and mental health-care treatment centers on crime. Bondurant et al. (2018) leverages variation in the opening and closing of substance abuse treatment facilities at the county level, finding that treatment centers reduce both violent and financially motivated crimes. Deza et al. (2020) considers variation across counties in the number of mental health care offices and finds that an increase in the number of offices leads to a modest reduction in crime. According to Wen et al. (2017), Health Insurance Flexibility and Accountability Waiver (HIFA-Waiver) expansions from 2001-2008 reduced robbery, aggravated assault, and larceny rates by 3%, 6-7%, and 3% respectively.[5] Their findings' primary explanation is an increase in substance use treatment.

The current study complements and extends this literature in a number of important ways. First, to my knowledge, no existing research links MHPLs and reported crimes. This is an interesting avenue to explore, as MHPLs target the type of health care coverage thought to affect criminality. Medicaid expansions, by contrast, do not target mental health coverage alone. Unique to this literature is Jacome (2020), which examines the effect of losing Medicaid coverage specifically for adolescents receiving mental health treatment. She finds that the rise in criminality associated with the loss of Medicaid is driven entirely by this population.

Second, previous work has primarily focused on populations eligible for Medicaid (Vogler, 2020; He and Barkowski, 2020). It is unclear if expanding mental health insurance coverage to other groups would

[5] HIFA are initiatives that provide states federal matching funds for expanding Medicaid to all low-income adults with family incomes of up to 200% of the Federal Poverty Limit.

also generate crime reductions. The population that is affected by MHPLs is quite different from the population covered by Medicaid, as it includes individuals (and their families) who are already employed, work in small firms, and likely have insurance coverage for physical health. Despite being a lower risk population, I still find significant reductions in violent crimes after the implementation of MHPLs.

The rest of the paper is organized as follows. Section 1.2 explains the institutional details of MHPLs. Section 1.3 provides information on the crime and policy data used in this study. Sections 1.4 and 1.5 discuss the empirical strategies and present my main findings. I describe a series of robustness checks in Section 1.6. In Section 1.7, I examine the unanticipated costs savings associated with MHPLs, before concluding the paper in Section 1.8.

1.2 Institutional Background

Between 1990 and 2010, the US federal government and individual states passed three types of legislation aimed at expanding mental health coverage. The two federal laws were the Mental Health Parity Act (MHPA) of 1996 and the Mental Health Parity and Addiction Equity Act (MHPAEA) of 2008. In addition to these, over two dozen states have mandated equal mental and physical health coverage or mental health parity. I first discuss state level MHPLs, which are my policy of interest, before discussing the federal laws.

1.2.1 State Mental Health Parity Laws

States were active in passing MHPLs from the mid-1990s to the late 2000s. In 1994, New Hampshire and Rhode Island were the first states to pass such laws; in total, 30 states have passed similar laws since then. Each state's MHPL differs in its coverage. While it is challenging to group laws, states that provide full, comprehensive coverage for a broad range of mental health illnesses are considered full parity states.

In my study, full parity states includes those that require equal physical and mental health coverage for all fully insured health plans, with no extra exemptions on firm size and costs. For example, Rhode Island is a full parity state because its mental illness coverage includes disorders listed by the Diagnostic and Statistical Manual of Mental Disorders (DSM) for all groups, HMOs, and individuals at parity.[6] The full parity sample also includes states that mandate equal coverage for all group health plans but exempt very small firms, as well as states that mandate parity coverage for state employees.[7]

States that do not meet these criteria are classified as non-parity. Nevada, to give one example, has a mandate in place from July 1st, 2000 requiring minimum benefits such that the out-of-pocket expenses for mental health care are not greater than 150% of the cost of physical health.[8] Since the mandate does not require parity of mental health coverage with physical illness, it is classified as a non-parity state for my study.

An important feature of state-level MHPLs is the exemption of self-insured firms by the Employee Retirement Income Security Act of 1974 (ERISA). If a firm self-insures (that is, it bears the risks itself, rather than contracting with an insurance company), its plan is exempt from state regulation (Andersen, 2015).[9] This means that workers in self-insured firms may not be affected by MHPLs. I discuss this in greater detail in the data section.

Other categories of laws in the literature are mandated offering, mandated-if-offered, and minimum mandated benefits. Mandated offering requires insurance plans to offer mental health coverage at parity with physical health coverage, but allows consumers to decide whether to accept the coverage. Mandated-if-offered requires equal coverage upon offer of mental health treatment. Minimum mandated benefits generally require a minimum level of mental health treatment coverage, one usually not on par with coverage for physical health treatment (per the NCSL). This paper's focus is the effects of full parity laws. By themselves, mandated offering states do not require mental health parity; therefore, I study the full parity laws, the affect of which is likely more meaningful than that of the weaker laws.

During the same period of time – 1990 to 2010 – only five states had a mandated offering statute in place: Kentucky, Florida, Utah, Arizona, and Georgia. Out of these, Kentucky passed its legislation before

[6] National Conference of State Legislatures: https://www.ncsl.org/research/health/mental-health-benefits-state-mandates.aspx

[7] Three states – Idaho, South Carolina, and North Carolina – have equal coverage for state employees.

[8] https://www.ncsl.org/research/health/mental-health-benefits-state-mandates.aspx

[9] However, MHPA and MHPAEA, both federal laws, do apply to self-insured employer-sponsored plans (Buchmueller et al., 2007).

my study period; Georgia and Arizona passed theirs in 1998; Florida's mandated offering became law in 2000; and Utah's went on the books in late 2008. I exclude these states from my main specification in order to better identify the effect of full parity relative to an absence of mental health coverage legislation. In a robustness exercise, I estimate the effect of mandated offering laws, and find no effect.[10]

My data on the years of passage for mental health parity laws comes from many sources. I first examine the previous literature on MHPLs for the year mental health parity became law in each state (Lang, 2013; Dave and Mukerjee, 2011; Popovici et al., 2017; Buchmueller et al., 2007). In the case of conflicting reports, I check the National Conference of State Legislatures (NCSL)[11], or the main state statute, for the precise timing. The NCSL is a common source for information on state level policy changes. For example, both Dave and Mukerjee (2011), Popovici et al. (2017), and Buchmueller et al. (2007) use the NCSL as a source for their definition of MHPLs.

Figure 1.1 is a representation of state MHPLs passed during my sample period. There is considerable geographic and temporal variation. Many states implemented parity legislation in 1999 and 2000, and these states will provide a key source of variation to identify the effects of MHPLs on crime.

1.2.2 Federal Parity Legislation

Studies have found the two federal laws to be symbolic policy changes, rather than substantial ones that altered access to mental health care. The 1996 MHPA went into effect in 1998 with a sunset provision originally set to expire in September of 2001 (Gitterman et al., 2001). The MHPA was eventually extended, before being replaced by the MHPAEA in 2008 (Li and Ye, 2017). While the MHPA did not require insurers to offer mental health benefits, the law mandated that mental health coverage, if offered, must have benefits equal to the annual or lifetime limits offered for physical health care (NCSL). The MHPA also required group health plans covering mental health care to offer annual and lifetime limits comparable to those for medical or surgical benefits.

[10] I discuss alternative coding in the results section.

[11] https://www.ncsl.org/research/health/mental-health-benefits-state-mandates.aspx

There were many loopholes in the MHPA. It exempted firms with fifty or fewer employees. Firms could claim an exemption if compliance with the law caused health care costs to increase by more than 1%. Firms could also circumvent the lifetime limit requirement by limiting an individual's outpatient visits and inpatient days (Buchmueller et al., 2007). These loopholes rendered the 1996 MHPA more symbolic than substantial (Peterson and Busch, 2018).

The second federal parity law, the MHPAEA of 2008, went into effect in 2010. MHPAEA preserved MHPA while adding to it. Beyond the existing benefits of equal annual and lifetime dollar limits, the new law extended benefits to other insurance features, such as deductibles and co-payments. It also extended the number of outpatient visits and inpatient days (CMS, MHPAEA). However, while MHPAEA strengthened several aspect of MHPA, it still lacked a requirement that firms include mental health treatment in their benefits packages, applying only to insurers whose inclusion of mental health coverage was voluntary.

One concern is that the two federal legislation supersede the state legislation and distort my results. However, this is unlikely as the MHPA of 1996 does not require mental health benefits to be offered, but only needs equal coverage for mental health benefit offered. In addition, research indicates that health plans circumvented the law by tightening restrictions on the number of hospital days and outpatient visits for mental health services (Barry et al., 2010). The legislation was weak and likely failed to impact mental health outcomes because of its severe restrictions.[12] The MHPAEA of 2008, while stronger lies just at the end of my sample period and does not affect my estimates.[13]

1.3 Data

In this section, I describe the various sources of variables used in this study's data, as well as their relevance.

[12] Barry et al. (2010) has more discussions on the federal legislation.
[13] Robustness checks with only data from 1994-2008 yield similar results.

1.3.1 Uniform Crime Reports

To answer my research question, I need the number of crimes committed at the sub-national (county, state) level. I use a panel of county-level crime data available at the Uniform Crime Reporting (UCR) Data Series, on the Inter-university Consortium for Political and Social Research (ICPSR) website.[14] The data is close to the population of all crimes that are reported in all US counties each year. County-level data has advantages over the state-level data set. For one thing, it allows to better control for unobserved heterogeneity across counties by accounting for sub-state level variation. It also allows better estimation of the most recent estimators that allow for heterogeneity by treatment timing. And county-level data is the smallest geographic level for which various confounding factors can be accounted. This study's data set from the National Archive for Criminal Justice Data (NACJD) is the most widely used county-level data set – nearly all researchers who use the UCR rely on it (Jacob Kaplan, 2021).

The County-Level Detailed Arrest and Offense Data of the Uniform Crime Reporting Program contains counts of reported Part I offenses (i.e., murder, rape, robbery, aggravated assault, burglary, larceny, auto theft, and arson). This long panel of crime data, from 1994-2010, corresponds with the period of passage of most state-level MHPLs. The total number of county-year observations in the sample is 52,316. The county identifier also allows me to use granular sets of fixed effects and link covariates to help in my identification. Furthermore, the UCR provides information on the size of the population covered by law enforcement agencies reporting data from a given county. I use this population variable to weigh the regression, avoiding problems that may occur when different counties are given similar weight in regressions despite having different population sizes.[15]

While the population-covered variable in the UCR data is useful for estimating representative effects, it is not without issues. Out of the total 52,316 county-year observations, there are 5,120 that report zero population. I exclude these observations from my estimation sample. There are counties where the reported population is non-zero, but the reported crime reported appears to be incomplete. The NACJD's coverage indicator provides users with a diagnostic measure of aggregated data quality in a particular

[14] https://www.icpsr.umich.edu/web/pages/NACJD/guides/ucr.html

[15] The UCR uses a data imputation method for the agencies that do not report data for all 12 months of the year.

county. I follow a sample selection procedure in which I drop those counties with coverage less than or equal to 90%.[16]

Despite these shortcomings, there are strong arguments that the Uniform Crime Reports data provides a valid and reliable measure of the Part I index crimes (Gove et al., 1985). However, to ensure that data issues do not drive these results, I use an additional source of state-level data, one aggregated and available in the FBI Crime Explorer. [17]. The data is from the Summary reporting System and contains the estimated data at the state levels. These state-level data will mitigate the shortcomings in the county-level data and reassure that the shortcomings in the UCR data are not driving my main results. And, since the county-crime data is highly right skewed, I winsorize the crime data at the 0.5 percentile to prevent outliers driving the results.

I present the summary statistics of the UCR county-level crime variables in Table 1.1. We can see that the treated states have higher violent crime rates than the never-treated for most types of crime both at the start and end of the sample period. There is also a decline in the average crime rate from 1994 to 2010 for both the states that pass MHPLs and the states that do not. This means that these states saw an overall decline in crime regardless of MHPL implementation.

1.3.2 Covariates

To complement the UCR data, I merge the time-varying county and state-level controls. The purpose of these variables is to account for any time-varying factors that might be correlated with crime rates. For example, crime rates vary significantly with gender, race, and age. If the underlying demographics of a location are changing, that location's reported crimes might also be changing.

To control for these factors, I obtain demographic data from the Survey of Epidemiology and End Results (SEER)'s US State and County Population Data by Age, Race, Sex, Hispanic origin at both the state and county level from the National Bureau of Economic Research Website.[18] I use ratio of population

[16] This sample selection procedure drops a total of 9,192 county-year observations with either a coverage indicator less than or equal to 90% or a population equal to zero.

[17] https://crime-data-explorer.fr.cloud.gov/#

[18] NBER(https://www.nber.org/research/data/survey-epidemiology-and-end-results-seer-us-state-and-county-population-data-age-race-sex-hispanic)

in the following age bins: less than 20, 20-29, 30-39, 40-49, 50-59, and 60 and above. In addition, I use the share of population in the four race categories: White, Black, Asian, and American-Indian or Pacific Islander; and share of Hispanic population. I also use the county and state gender distribution in my study.

Beyond the demographic variables, I account for the effects that local macroeconomic conditions may have on crime rates, as people who are unemployed are likely not affected by MHPLs. To determine this, I obtain county unemployment rate from the Bureau of Labor Statistics, Local Area Unemployment Statistics, and I get the county per-capita personal income from the Bureau of Economic Analysis. Levels of public health insurance coverage correlate with crime, as recent studies on Medicaid expansions and crime show (Vogler, 2020; He and Barkowski, 2020). To control for public health insurance coverage, I use the percentage covered by Medicaid and the percentage covered by Medicare in a state as a control variable. I gather data for access to public health insurance from Integrated Public Use Microdata Series (IPUMS), which is derived from the Annual Social and Economic Supplement of the Current Population Survey.

As mentioned earlier, workers in self-insured firms are not covered by state-level mandates because self-insured firms are preempted from state laws due to the Employee Retirement and Security Act (ERISA) of 1974. The share of workers in self-insured firms in a state will likely correlate negatively with the share of people affected by a state's MHPL. As a proxy for the share of workers in self-insured firms, I use the share of workers in large firms, since these are the firms most likely to self-insure. I use this variable as a control in some specifications, but also explore heterogeneity with respect to the the workers in large firms. I obtain the data on the percentage of workforce in large firms from the Census Bureau's County Business Patterns.[19]

Finally, I account for the political environment by determining the percentage of state-level House and Senate controlled by Democrats taken from Edwards et al. (2018). The political environment may be

[19]Following the literature, I define large firms as those with more than 500 workers. According to the Medical Expenditure Panel Survey (MEPS), more than 90% of firms with more than 1,000 employees are self-insured, and more than 80% of the firms with more than 500 employees are self-insured (Li and Ye, 2017). Because we do not have more precise information on the number of workers in self-insured firms, this is a good proxy for self-insured firms.

correlated with both the passage of mental health parity laws and with other laws that might influence criminality.

In Table 1.2, I present the summary statistics of the main county and state level covariates in my study. I present the county demographics and county macroeconomic variables. I also present the state public health insurance variables, the percentage of the workforce in large firms, and political variables. For most part, the demographics are similar. However, the share of people aged 40 to 49 is higher in treated states. The share of whites is higher in the untreated states both in 1994 and 2010. And the share of non Hispanic population is higher in treated states.

1.4 Empirical Strategies for Estimating Policy Effects

In this section, I describe the empirical approaches used in my analyses. I use a difference-in-differences design to analyze the impact of MHPLs on crime. I estimate the difference-in-differences estimates using a two-way-fixed-effects (TWFE) framework and the recently developed methods by Callaway and Sant'Anna (2020).

1.4.1 Difference-in-Differences

I exploit the staggered roll out of the MHPLs in the United States as a source of exogenous variation to identify the effects of MHPLs on crime. My main specifications are a TWFE difference-in-differences and an event study model. The difference-in-differences estimates yield a causal parameter to quantify the effects of MHPLs on crime, while the event study model allows me to check for pre-trends and see dynamic policy effects.

The two way fixed effects regression specification is given by:

$$Y_{sct} = \alpha_0 + \alpha_1 Post \times EverTreat_{st} + \alpha_2 X_{sct} + \gamma_t + \gamma_s + \epsilon_{sct}$$

where $Post \times EverTreat_{st}$ is a binary indicator that takes a value of 1 if state s had a mental health parity law in place in year t. It takes a value of 0 otherwise. Y_{sct} is the inverse hyperbolic sine transformation of crime in county c in state s in year t. γ_c and γ_t are county- and time-fixed effects respectively, and X_{sct} is a vector of relevant state and county level controls.[20]

I use the inverse hyperbolic sine transformation as the outcome variable as an approximation to the natural log of the total number of crimes (Bellemare and Wichman, 2020).[21] MHPLs are likely to yield constant proportional changes in crime as opposed to constant level effects, leading naturally to a log-linear model. However, there are many county-year observations with zero crimes reported. To avoid dropping zeros, I use the inverse hyperbolic sine transformation of the response variable.[22]

I first estimate a TWFE model without controls. The inclusion of county-fixed effects will capture time-invariant, county-specific unobservable characteristics. However, there could be within-county changes over time that correlate with crime and with the passage of MHPLs. To account for these concerns, I add an increasingly complex set of time-varying covariates to assess the robustness of the initial model.

I include, sequentially, the percentage of workers in large firms, county demographics, county macroeconomic variables, access to public health insurance, and political variables. Table 1.2, provides a detailed list. The only variable not listed in Table 1.2 is the census division by year fixed effects. I include these to control for seasonal and regional trends that may correlate with crime.

A key identification assumption in a difference-in-differences design is that the outcomes in the treated and control states would evolve the same way in the absence of treatment. While this cannot be tested, I check the validity of the difference-in-differences design using an event study framework discussed in more detail in the next section.

A second assumption necessary for the difference-in-differences approach to yield causal estimates is that there is no policy endogeneity – in other words, that the passage of MHPLs is not dependent on the current or anticipated levels of crime. The difference-in-differences method assumes that MHPLs were

[20] Recall that the estimation sample includes only those states passing full parity law, as treated states that had no MHPLs as controls. This allows for a straightforward comparison of treatment and control. As noted earlier, there are some states that had a mandated offering. In Section 1.6.3, I separately investigate whether mandated offering laws affect criminality.

[21] For the response variable with a mean greater than 10, the interpretation of the coefficients is similar to that of the log transformation (Bellemare and Wichman, 2020).

[22] An alternative method is to define the outcome as the crime rate. Results are qualitatively similar regardless of the response variable.

not passed because of an increase in crime. This is a plausible assumption, since political conversations around the passage of MHPLs indicate that these laws were passed not because of an anticipated increase in crime, but to improve mental health outcomes.[23]

A final concern that arises in two-way fixed effects models is due to staggered treatment timing. It is possible that, as overarching conditions change, the effect of these laws will also change. For example, the opioid epidemic may have led to a greater effect of MHPLs over time. This can generate misleading predictions of crime for future changes in mental health coverage. To address this concern, I use the (Callaway and Sant'Anna, 2020) estimator as a robustness check to my TWFE difference-in-difference estimates. I discuss this estimator further in Section 1.4.3.

1.4.2 Event Study

The difference-in-differences method delivers a single estimate of the effect MHPLs have on crime. However, it is reasonable to expect that the policy influence may expand over time, as people learn about access to mental health treatment. An event study model allows the effects of MHPLs to vary with the length of exposure. Additionally, the event study framework can illustrate whether criminality trends similarly in treatment and control states before treatment. Flat pre-treatment period effects would suggest that, in the absence of treatment, there is no difference in crime rate in the states that passed MHPLs and the states that did not pass them. A failure of parallel trends in the pre-treatment period would suggest a lack of parallel trends post-treatment – a key assumption of the difference in differences model.

The two-way fixed-effects event study design to estimate the effects of MHPLs is given by the equation:

$$Y_{sct} = \gamma_c + \gamma_t + \sum_{k=-10}^{k=10} \pi_k Parity_s * I(t - T_s = k) + \alpha_0 X_{st} + \epsilon_{st}$$

where $Parity_s$ is an indicator that the state s ever had a mental health parity law in place. Y_{sct} is the outcome variable (i.e., crime in state s, in county c in year t). γ_c, γ_t, and t denotes county-fixed effects, year-fixed effects, and year t respectively. T_s denotes the year MHPLs are passed in state s. k denotes the

[23] See Barry et al. (2010) that has a discussion on the political history of mental health parity in the US.

leads and lags after and before mental health parity is passed, respectively. The comparison year is the year before the mental health parity law is passed. X_{sct} is the set of controls discussed earlier.

1.4.3 Group Time Average Treatment Effects of Callaway and Sant'Anna (2020)

Recent developments in the difference-in-differences literature points to the pitfalls of estimating causal effects using TWFE estimators (Goodman-Bacon, 2021; Abraham and Sun, 2019; Callaway and Sant'Anna, 2020).[24] If the treatment effects are different for states treated earlier and states treated later, the TWFE estimators are poorly behaved and will give biased results.[25] When there is a staggered adoption of treatment with variation in treatment timing – as in the case of MHPLs – the two-way fixed-effects model does not give the true treatment effects. The treatment effects dynamics introduced by using early treated units as a control for later treated units makes the effects biased, and the bias increases in the absence of an untreated group.

The bias is also more prominent in settings where there are no untreated units (or a relatively small untreated group) to compare with, and relies on the groups treated earlier/later solely for comparison. My sample has 30 states that passed MHPLs in the sample period. This leaves a relatively large number of never-treated states, and so the possibility of bias due to comparisons between earlier- and later-treated groups in the TWFE model is not high.

To avoid the possible bias arising from comparisons of the newly treated units to already treated units, and as an additional robustness checks to my TWFE models, I use the recently developed estimator (Callaway and Sant'Anna, 2020). This estimator allows the use of never-treated groups as comparison groups, avoiding many pitfalls of the TWFE estimators. Below, I describe the estimator in the context of this paper.

The different years of MHPL passage gives a staggered setup of the treated states. Counties in states that implemented MHPLs in a year g is a group in this context. Callaway and Sant'Anna (2020) provides

[24] The two-way fixed effects model is the weighted average of all 2×2 estimators across treatment groups that compare timing groups with each other (Goodman-Bacon, 2021).

[25] For example, if treatment effects vary across different units, or exhibit dynamics, or change across different time periods, the treatment effects may be biased.

an estimation method to identify the group-time specific average treatment effects. These group-time average treatment effects are denoted by $ATT_{(g,t)}$, which is the average treatment effects in time t for the group that is treated at g. In this method, I am essentially estimating different treatment effects for each group of counties, where a group is defined based on MHPL passage in that county's state. Each group is only compared to the never-treated states to get the treatment effects.

Callaway and Sant'Anna (2020)'s procedure allows us to use the never-treated units or the not-yet-treated units in place of potential outcome for the treated units had they not been treated. I use the outcome of the never-treated group as a comparison group for the units treated in group g. Using the never-treated group as a control avoids the problem of using earlier treated groups as controls for earlier treated groups described in Goodman-Bacon (2021).

After getting several $ATT_{(g,t)}$, the next step is to aggregate the group-time average treatment effects using group aggregation to get treatment effects for each group. The treatment effects for each group can be aggregated further to get the estimated parameters, which can be interpreted as causal. Similarly, using the dynamic aggregation gives the event study-like parameters.

$ATT_{(g,t)}$s are the building blocks of Callaway and Sant'Anna (2020) method. Following the paper, I aggregate the $ATT_{(g,t)}$ at the the group level to get treatment effects for each group. I then aggregate group-level treatment effects, weighting further by group sizes to get the overall treatment effects. These overall treatment effects can be interpreted as causal. [26]

1.5 Results

In this section, I present the results of my analysis based on the empirical strategy described above. In Section 1.5.1, I present results of the TWFE difference-in-differences estimates. In Section 1.5.2, I present the TWFE event study estimates. Finally, in Section 1.5.3, I present the estimates from Callaway and Sant'Anna (2020).

[26] Please see (Callaway and Sant'Anna, 2020) for additional details on different aggregations types.

1.5.1 TWFE Difference-in-Differences Results

In Tables 1.3 and 1.4, I first present the results of the difference-in-differences using the TWFE model for total violent and total property crimes. I run the difference-in-differences specification with county-fixed effects, year-fixed effects, and the census division-by-year fixed effects. I present my results in seven columns, where I control progressively for relevant time-varying county- and state-level controls that could also affect criminality over time.

In my panel data, unobserved shocks in counties may be correlated, potentially giving imprecise estimates. To account for common unobserved random shocks at the state level that could lead to correlation between observations within each state, I cluster the standard errors at the state level (Cameron and Miller, 2015; Abadie et al., 2017). Moreover, I weight my regressions using the county-level population of the reporting agencies in the UCR data.

In Column (1) of Tables 1.3, and 1.4, I present a sparse model, with only the percentage of people working in large firms as control variable. I add the state-level political environment, state public health insurance coverage, county-level demographics, county-level macroeconomic variables, state-level macroeconomic variables, and state-level demographics in my models. [27] In column (7) of Tables 1.3, and 1.4, I show the results of the TWFE regression using only the state-level controls. Removing the county-level demographics does not change the estimates or the standard error. This suggests that within-county time-varying factors affect my results minimally. Given these results, and the fact that some county-level characteristics are missing, Column (7) is my preferred specification, and I present all other TWFE results based on this specification in column (7).

The results in Table 1.3 show that the effect of MHPLs on violent crime is highly significant and has an economically meaningful magnitude in all specifications. According to my preferred specification from Column (7), there is a 7% reduction in violent crimes in states that implemented MHPLs compared to states that did not implement MHPLs. The results in Table 1.4 show the effect of MHPLs on total property crimes. There is a suggestive evidence of a reduction in property crimes in Column (1) and (2) of

[27] Demographic information contains a distribution of different age groups, gender distribution, and the ratio of four race categories in a county or state.

Table 1.4. However, the results are not robust after adding state- and county-level covariates to the model. From our preferred specification in Column (7), there is no significant effect of MHPLs on property crimes.

Next, using my preferred specification, I present the effects of MHPLs on different types of disaggregated Part I violent and property crimes. In Table 1.5, I present the effect of MHPLs on different violent crimes. My results indicate that there is a 7.71% reduction in aggravated assault at the county level in states that pass MHPLs. Using the preferred specification, there is evidence that MHPLs reduce rape by around 10%. [28]

In Table 1.6, I present the results of the effect of MHPLs on different property crimes. I find that there is no significant reduction in property crime from MHPLs. The results do indicate an increase in arson due to MHPLs, with high standard errors. Arson as a proportion of total property crime is very low, with several missing values. Therefore, the effect of MHPLs on arson rate requires additional robustness checks.

Finally, I do a heterogeneity analysis by using samples of counties where the number of workers for large firms is either above or below the median. I present my analysis in Appendix Table B.3. The results suggest that counties in states with a relatively low percentage of workers in large firms show reductions in both violent and property crimes. By contrast, counties in states with a relatively high percentage of workers in in large firms do not see these reductions.

1.5.2 Event Study Results

In this section, I present the results for the event study model outlined in equation (2). Recall that the purpose of the event study is two-fold. First, it allows for dynamic treatment effects, or the evolution in the effect of MHPLs over time. Second, it allows me to examine whether the treatment and control states were trending similarly in the pre-treatment period.

In Figure 1.2, I show the event study plot for total violent and total property crimes. These figures show how the outcome evolves before and after the passage of MHPLs. The first figure shows the dynamic effect

[28] The results using an alternative set of controls are consistent with the preferred specification.

of MHPLs on the inverse hyperbolic sine of total violent crimes. The second figure shows the dynamic effect of MHPLs on the inverse hyperbolic sine of property crimes.

In the pre-period, I see flat pre-trends. For most of the pre-treatment period, the treatment effects trend at zero, with the confidence bands also capturing zero. This supports our identifying assumption that the total number of reported violent and property crimes would be similar in treated and untreated states regardless of MHPL implementation. In Appendix B.4, I present the event study results that account for heterogeneity using Callaway and Sant'Anna (2020). The event study results also show flat pre-trends.

Post-treatment, there is a significant effect of MHPLs on total violent crimes, consistent with the main results of the difference-in-differences method. There is a slow decline in the violent crime in the first year, and significant reductions after the second year post-expansion. Consistent with the results of the difference-in-differences results, there is no significant effect of MHPLs on property crimes.

In figure 1.3, I present the event study results of the four different categories of violent crimes. The effects of aggravated assaults mirror those of of total violent crimes. The figure shows flat trends in the pre-treatment period, with a substantial drop post-treatment. Like total violent crime, the effects increase in magnitude over time, with significant reductions continuing several years post-treatment. The pattern for total murder, rape, and robbery also exhibit flat pre-trends in the pre-treatment period. While there is no effect of MHPLs on murder and robbery, there is a decrease in total rape after MHPLs, which is in line with the main difference-in-differences estimates. However, the decrease is not significant post-treatment.

In Figure 1.4, I present event studies for various types of property crime. The event study figures exhibit flat pre-trends, for most part, with the treatment and control groups moving similarly pre-treatment. Like the results of the main difference-in-differences analysis, the effects post-treatment are not significantly different from zero – except for arson, which, as I mentioned earlier, was a small proportion of property crime in total.

1.5.3 Accounting for Heterogeneity: Callaway Sant'Anna Results

In this section, I discuss the results using the most recent methodological advances in the difference-in-difference literature (Callaway and Sant'Anna, 2020). These recent estimators overcome the pitfalls of the TWFE estimators.

I rely on the standard difference-in-difference parallel trends assumptions for identification. I assume that the outcomes in the treatment and control groups would be similar if there were no treatment. In the context of my study, it means that the outcome in the group of states that expanded MHPLs would have moved similarly to the outcomes in the group of states that never passed MHPLs in the absence of MHPLs. An inspection of the event study estimates derived using TWFE presented in the last section shows that the trends are similar for the treated and the control groups. Similarly, the event study type figures produced using Callaway and Sant'Anna (2020) also produces flat pre-trends, giving credibility to the difference-in-differences design.[29]

If the researcher believes that the pre-trends hold only after conditioning on co-variates, Callaway and Sant'Anna (2020) allows for using pre-treatment covariates to create propensity matches between treatment and control units. In my TWFE results, I find that there is a flat pre-trend for outcomes even without covariates. I also get flat pre-trends by using the Callaway and Sant'Anna (2020) in the event study type estimators. Therefore, I do not use covariates when I apply Callaway and Sant'Anna (2020) method. In the robustness exercise, I use pre-treatment covariates to estimate ATT using the outcomes regression estimation, and get similar results. [30]

The building blocks of the Callaway and Sant'Anna (2020) are the $ATT_{(g,t)}$s. Each $ATT_{(g,t)}$ is the average effect of participating in the treatment for units in group g at time period t.

The aggregation method also solves for heterogeneity of treatment effects that may arise due to the treatment effects being different for groups of counties that were treated at different times. In the context of staggered MHPL implementation, it is possible that, as the overarching conditions change, the effect of the law will also change. For example, the first wave of the opioid crisis started in the 1990s, just as the

[29] I present the event study-type estimates using this method in the Appendix B.4.
[30] The results are reported in the Appendix.

first round of MHPLs was passed. As conditions in states change due to the crisis, the effect of MHPLs on crime may also change, giving different treatment effects for groups.

Inference about the estimates is made using Callaway and Sant'Anna (2020)'s proposed method. I use the bootstrap method to calculate standard errors, using 1,000 iterations. To account for common unobserved random shocks at the state level that could lead to correlation between all observations within each state, I cluster the standard errors at the state level. As with the TWFE model, in order to make the treatment effects representative of the whole population and give weight to counties according to their sizes, I weigh using county-level population. The default method for calculating the standard error is the multiplier bootstrap. The standard errors also adjust for multiple hypothesis testing. I perform my estimation and inference using the *did* package in R. [31]

I present the results in Table 1.7. The Table shows the effect of MHPLs on different crimes using the baseline model with no controls. I find a significant reduction in violent crime generally and aggravated assault specifically after passage of MHPLs. The overall ATT derived by aggregation across groups suggests a 4.5% reduction in violent crimes, and a 6.01% reduction in aggravated assault after the passage of MHPLs. These effects are smaller than the TWFE estimates, but similar to the TWFE regression that shows a decrease in crime after passage. The effects are significant at the 5% level.

1.6 Robustness Checks

In this section, I perform several robustness checks to asses the validity of the main results. First, I use state-level data from the FBI's crime explorer to run state-level TWFE regressions. Second, I run several specification checks. Third and finally, I estimate the difference-in-differences estimates of the weaker type of laws using only the mandated offering states as treated, to see if there is any effect of those laws on crime rates.

[31] https://bcallaway11.github.io/did/articles/did-basics.html

1.6.1 State Level Regression

I use county-level data in my main specification because I can better account for heterogeneity across locations, both with county-fixed effects and county-level covariates. However, the county-level crime data is not perfect, for the reasons discussed in Section 1.3. Thus, in this section, I estimate the effect of MHPLs on reported crime using data from the FBI crime explorer, which gives state-level aggregate crimes. The state-level data can alleviate concerns about the county-level UCR data sets. In particular, it can help mitigate the problems of misreporting across counties and the zero values in the county level UCR data, because it is the aggregate of all crimes reported by county agencies. The literature studying crime and Medicaid also uses this data (Vogler, 2020).

The results of the state-level regression are presented in Tables B.4, B.5, B.6, and B.7 in Appendix B.3. The key findings of the state-level regression are qualitatively similar to the main county-level regressions presented in Section 1.5.1. The treatment effects for violent crimes and aggravated assault are similar in magnitude and confirm the results using the county level. For example, in Column 5 of Table B.4, we see that passage of MHPLs reduces state violent crimes by 5.5%. In Table B.6, the estimates show that the passage of MHPLs reduces aggravated assault by 7.39%. These results are in line with the main specifications using the county-level data.

1.6.2 Specification Checks

I perform several specification checks in the county-level regressions. Rather than use the inverse hyperbolic sine transformation, I implement a common alternative approach, which is to take the log of 1+crime levels to account for the missing value that become meaningless after taking the log transformation. I avoid the bias that may have crept into my estimate due to zero crimes transformed as missing values. The results are presented in Appendix B.3 Tables B.8, B.9, B.10, B.11. These results are similar to the TWFE regression using inverse hyperbolic sine transformation. There is a significant reduction in violent crime and aggravated assault after passage of MHPLs.

The robustness exercise checks discussed above confirm that my main results are insensitive to the level of aggregation, transformations of the response variable used, and a variety of combinations of state- and county-level time varying controls.

1.6.3 Policy Effects of Mandated Offering States

To see the effect of the weaker Mandated Offering laws, I estimate a difference-in-differences for the sample of states with such a law in place. While it is clear that there is a decrease in crimes after passage of MHPLs, it is pertinent to see the effect of weaker parity laws on crime rate. As mentioned earlier, only five states that passed Mandated Offering during my sample period: Kentucky, Florida, Utah, Arizona, and Georgia. Of these, Kentucky already had Mandated Offering in place at the onset of my study, and Utah implemented its law in 2008. Georgia and Arizona passed mandated offering in 1998, while Florida passed mandated offering in 2000.

The results of the mandated offering states are in Appendix Tables B.12, B.13, B.14, B.15. Unsurprisingly, the results show that mandated offering laws do not have much strength in reducing crimes.

1.7 Discussions

As discussed in Section 1.1, the literature highlights two main channels driving the reduction in violent crime due to increased access to mental health care: access to care effect, and the income effect. My results show a reduction in violent crime driven by reductions in aggravated assault. Reductions in violent crime are probably through the access to mental health care channel.

Mentally ill individuals with access to care and treatment are likely to make more rational decisions than those not in treatment. For example, Cáceda et al. (2014) finds that people with serious mental illness are affected by their illness' symptoms in addition to impairment of the ability to make healthy decisions. Mental illness can also lead to an abnormal risk assessment or reward processing, which can literally impair one's ability to decide not to commit a crime.[32] Access to mental health care can reduce this impairment

[32] To give another example, both depression and schizophrenia are linked with abnormalities related to reward processing (Cáceda et al., 2014).

and make people less likely to commit crime. Likewise, access to substance abuse treatment thanks to MHPL expansion can reduce crimes.[33]

My results show that violent crimes decrease due to the passage of MHPLs. And MHPLs also have their own primary benefits in terms of improved mental health care access and mental health treatment. However, from a policy perspective, the reduction of crime generates additional societal savings. To get a sense of these savings, I use the costs of crime given in McCollister et al. (2010). I calculate savings based of the number of crimes avoided in expansion states, using the pre-expansion means as the baseline.

Though results suggests a reduction in other types of crime, I use aggravated assault to calculate induced savings because the reduction in aggravated assaults is significant and robust in my study. I calculate the average number of county-level aggravated assaults prevented in states with MHPLs after their passage. Then, I calculate the estimated annual savings from aggravated assault potentially prevented nationally.

With the average number of aggravated assaults at 357 in expansion counties in 1994, and a 7% reduction in crime, I estimate that around 25 crimes per county are prevented in states that pass MHPLs. The cost per aggravated assault given in McCollister et al. (2010) is $107,020 in 2008 dollars. 25 crimes per treated county means an average per-county savings of $107020 \times 25 = \$2675500$ each year, just for MHPL-passage-prevented aggravated assault. There are 1,124 such counties in my sample's 30 MHPL states. Thus, the aggregated savings amount to approximately $3 billion as a result of MHPLs.

Both the estimated percentage of reduced crime and the estimated induced savings from MHPLs are similar to studies that estimate the effect of Medicaid expansion on crime. Vogler (2020) finds a 5.3% reduction in annual reported violent crime rates due to Medicaid expansion. Similarly, He and Barkowski (2020) finds a 4.43% reduction in violent crime. And Vogler (2020) estimates an annual savings of $4 billion, which is similar to my results. Welfare analysis from He and Barkowski (2020) imply that Medicaid expansions reduced welfare losses for adopting states by just under $10.5 billion per year.

[33]While not related to MHPLs, in general, access to mental health or substance abuse treatment reduces crime (Bondurant et al., 2018). With increased access to treatment, people are more likely to treat their substance use disorder, reducing criminal propensity.

As noted earlier, the marginal population affected by Medicaid expansions is not the same as the one affected by MHPL expansion. Assuming conservatively that the average percentage of workers at large firms is around 45%, the percentage of workers at small firms is likely around 55%.[34]. This would mean the population affected by MHPLs is larger than the one affected by Medicaid expansion. Considering that, on average, around 12% of population is covered by Medicaid, the increase in coverage due to Medicaid expansions would be even smaller.

My results are smaller despite affecting a larger population. Individuals covered by MHPLs are in a better place compared to those covered by Medicaid because they are likely employed and have physical health insurance to begin with. Regardless, my estimates are still large and suggest that the benefits of expanding MHPLs go beyond simply expanding access to mental health care treatment.

1.8 Conclusion

In this paper, I exploit plausible exogenous variation in mental health treatment access resulting from the the passage of Mental Health Parity Laws in order to examine whether increased mental health care access reduces crime. To answer the question, I use Uniform Crime Reports data with county-by-year observations of all crimes reported in the United States, combined with two-way fixed-effects estimators to estimate causal effects. I provide robust, policy-relevant evidence that passage of MHPLs leads to a meaningful decrease in violent crime.

I contribute to an understanding of the relationship between mental health and crime and the effects of insurance expansion on crime, and examine the plausibility of expanding insurance coverage for mental health treatment as a policy tool for controlling crime. My results have policy implications in that using parity laws to expand mental health care coverage results in less number of violent crime.

Coverage expansion via MHPLs reduces violent crimes by 7%. The effect is high for aggravated assault, which MHPLs reduce by 7.7%. This is a substantial reduction of crime and the estimates are compara-

[34] The Kaiser Family Foundation estimated that between 33 and 50 percent of U.S. employees were in self-insured plans in 2000 (Barry et al., 2010)

ble to studies that examine Medicaid expansion and crime. Additionally, lower crime rates via MHPL implementation result in an annual savings of $3 billion.

The effects are concentrated on violent crimes, rather than on property crimes. The most likely reason is that MHPLs target people who are likely employed and already have physical insurance. Studies on mental health treatment have determined that untreated mental health is a risk factor for violent crimes. The likely channel by which MHPLs reduce crime is therefore through treatment of previously unaddressed mental health illness.

1.9 Tables

Table 1.1: Summary Statistics of County Level Crime Variables

	Treated		Not Treated	
	1994	2010	1994	2010
Tot. Violent Crime (per 1000)	3.92	2.69	3.09	2.49
	(3.77)	(2.31)	(3.38)	(2.06)
Tot. Property Crime (per 1000)	32.59	22.26	27.95	20.72
	(19.77)	(12.26)	(19.30)	(11.74)
Agg. Assault (per 1000)	2.89	2.00	2.29	1.88
	(2.83)	(1.88)	(2.48)	(1.55)
Murder (per 1000)	0.06	0.03	0.04	0.03
	(0.10)	(0.05)	(0.09)	(0.06)
Rape (per 1000)	0.29	0.26	0.29	0.28
	(0.26)	(0.23)	(0.34)	(0.47)
Robberies (per 1000)	0.68	0.40	0.47	0.29
	(1.23)	(0.60)	(1.16)	(0.52)
Burglary (per 1000)	8.18	5.77	6.83	5.54
	(5.34)	(4.10)	(4.84)	(3.59)
Larceny (per 1000)	21.86	15.12	19.15	13.93
	(13.93)	(8.39)	(14.16)	(8.36)
MV Theft (per 1000)	2.29	1.22	1.74	1.09
	(2.67)	(1.09)	(2.24)	(1.18)
Arson (per 1000)	0.25	0.15	0.22	0.15
	(0.33)	(0.18)	(0.30)	(0.22)

Notes: The table shows the summary statistics of the county level crime variable from the UCR data. Standard Deviation is in the parenthesis

Table 1.2: Summary Statistics of County/State Level Controls				
	Treated		Not Treated	
	1994	2010	1994	2010
Age 0-19	0.29	0.26	0.29	0.26
	(0.03)	(0.03)	(0.03)	(0.03)
Age 20-29	0.13	0.12	0.12	0.12
	(0.03)	(0.03)	(0.03)	(0.03)
Age 30-39	0.16	0.12	0.15	0.11
	(0.02)	(0.02)	(0.02)	(0.02)
Age 40-49	0.14	0.14	0.13	0.13
	(0.01)	(0.01)	(0.01)	(0.01)
Age 50-59	0.10	0.15	0.10	0.15
	(0.01)	(0.02)	(0.01)	(0.02)
Share of male	0.49	0.50	0.49	0.50
	(0.01)	(0.02)	(0.02)	(0.02)
Share of White	0.86	0.85	0.93	0.91
	(0.16)	(0.16)	(0.12)	(0.13)
Share of Black	0.12	0.10	0.06	0.06
	(0.16)	(0.15)	(0.11)	(0.12)
American Indian Alaska Native	0.02	0.03	0.01	0.02
	(0.06)	(0.08)	(0.07)	(0.07)
Asian or Pacific Islander	0.01	0.02	0.01	0.01
	(0.02)	(0.04)	(0.01)	(0.02)
Share of Hispanic	0.04	0.07	0.08	0.11
	(0.08)	(0.11)	(0.16)	(0.17)
Unemprate	6.62	9.48	6.00	8.78
	(3.12)	(3.05)	(3.32)	(3.16)
PCI('000)	18.84	34.90	18.58	34.67
	(4.23)	(9.24)	(3.85)	(7.78)
Percent Covered By Medicare	13.04	15.31	12.90	13.92
	(2.19)	(2.18)	(1.76)	(2.20)
Percent Covered By Medicaid	11.60	14.47	11.74	15.28
	(2.89)	(2.83)	(3.16)	(3.13)
Workforce in large firms	0.46	0.48	0.47	0.50
	(0.05)	(0.05)	(0.05)	(0.04)
House Democrats	0.54	0.49	0.49	0.46
	(0.17)	(0.14)	(0.13)	(0.10)
Senate Democrats	0.57	0.52	0.49	0.42
	(0.16)	(0.15)	(0.09)	(0.08)

Notes: The table shows the summary statistics of county demographics, and macroeconomic variables. Percent covered by Medicare, percent covered by Medicaid, workers in large firms, and the political variables are state level variables. Standard deviations are in the parenthesis.

Table 1.3: Estimates of the effect of MHPLs on Violent Crimes

	1	2	3	4	5	6	7
MHPLs x Post	-0.0757***	-0.0598**	-0.0607***	-0.0585***	-0.0639***	-0.0683***	-0.0701***
	(0.0253)	(0.0244)	(0.0219)	(0.0214)	(0.0213)	(0.0200)	(0.0204)
N	38184	38184	38184	36698	36698	36698	38184
County, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Div. by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using equation (1). The dependent variable is the inverse hyperbolic sine of the total violent crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table 1.4: Estimates of the effect of MHPLs on Property Crimes

	1	2	3	4	5	6	7
MHPLs x Post	-0.0448**	-0.0280	-0.0159	-0.0107	-0.0100	-0.0138	-0.0149
	(0.0213)	(0.0169)	(0.0138)	(0.0145)	(0.0143)	(0.0139)	(0.0139)
N	38184	38184	38184	36698	36698	36698	38184
County, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Div. by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. The table presents difference-in-differences estimates using equation (1). The dependent variable is the inverse hyperbolic sine of the total property crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table 1.5: Estimates of the effect of MHPLs on Different Violent Crimes

	Agg. Assault	Murder	Rape	Robbery
MHPLs x Post	-0.0771***	0.00946	-0.102**	-0.0251
	(0.0253)	(0.0224)	(0.0476)	(0.0180)
N	38184	38184	38184	38184
County, Year FE	Yes	Yes	Yes	Yes
Census Div. by Year FE	Yes	Yes	Yes	Yes
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the inverse hyperbolic sine of the total crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table 1.6: Estimates of the effect of MHPLs on Different Property Crimes

	Burglary	Larceny	MV Theft	Arson
MHPLs x Post	-0.0126	-0.0160	-0.0242	0.130*
	(0.0180)	(0.0149)	(0.0211)	(0.0728)
N	38184	38184	38184	38184
County, Year FE	Yes	Yes	Yes	Yes
Census Div. by Year FE	Yes	Yes	Yes	Yes
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the inverse hyperbolic sine of the total crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table 1.7: Overall ATT Inverse Hyperbolic Sine of Crime

	Estimate	SE	95%CI	
Violent	-0.0444^{**}	0.0193	-0.0823	-0.0066
Agg. Assault	-0.0601^{**}	0.0233	-0.1058	-0.0145
Murder	0.035	0.0209	-0.0061	0.076
Rape	-0.0516	0.0411	-0.1322	0.0289
Robbery	0.007	0.0249	-0.0418	0.0559
Property	0.004	0.0346	-0.0638	0.0718
Burglary	0.007	0.0498	-0.0906	0.1047
Larceny	-0.0059	0.0284	-0.0614	0.0497
MV Theft	0.0459	0.0847	-0.1201	0.2118
Arson	0.1166	0.1583	-0.1936	0.4267

Notes: The table presents the ATT estimates, the SE, and the 95% confidence bands of (Callaway and Sant'Anna, 2020) estimators discussed section 1.4.3. The presented ATTs are the aggregation across group of the group time average treatment effects. The control group for estimation is the never treated group of counties. I use the default doubly robust estimation to get the estimates. No anticipation is assumed when calculating the ATT. The default uniform confidence are computed using 1000 bootstrap iterations and clustered at the state-level. Population from the UCR data is used as weight in estimation.

1.10 Figures

Figure 1.1: Passage of State Mental Health Parity Laws.

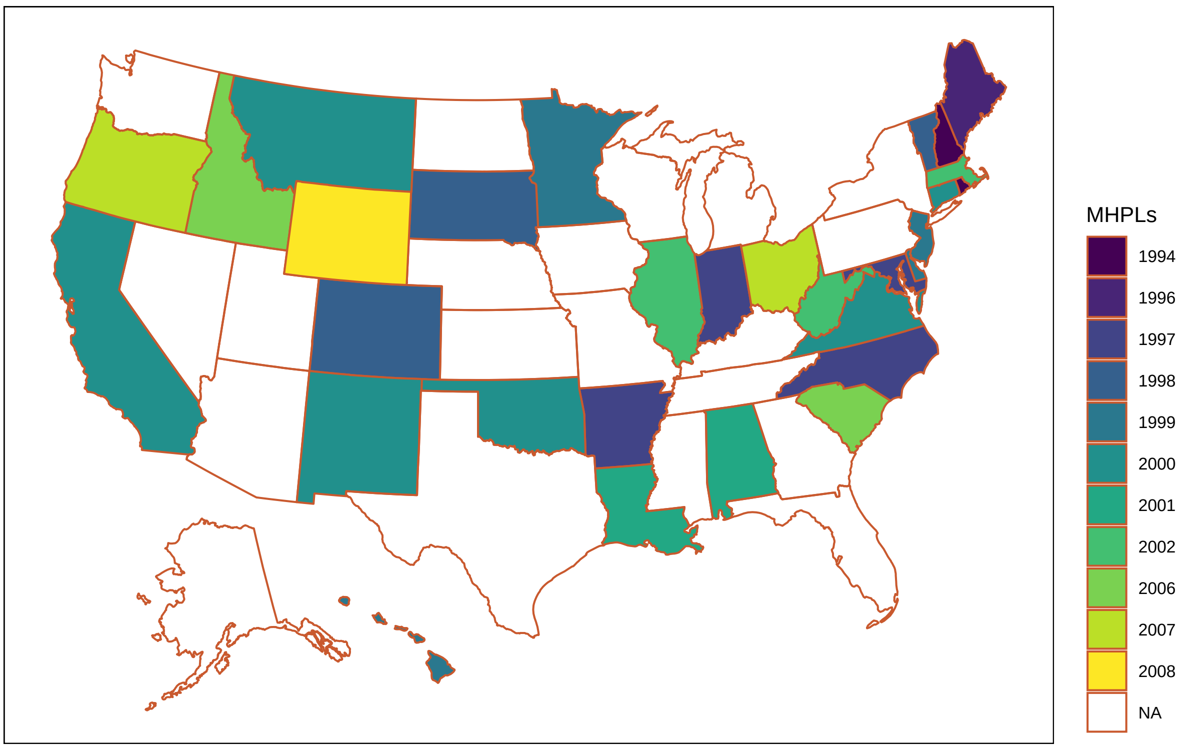
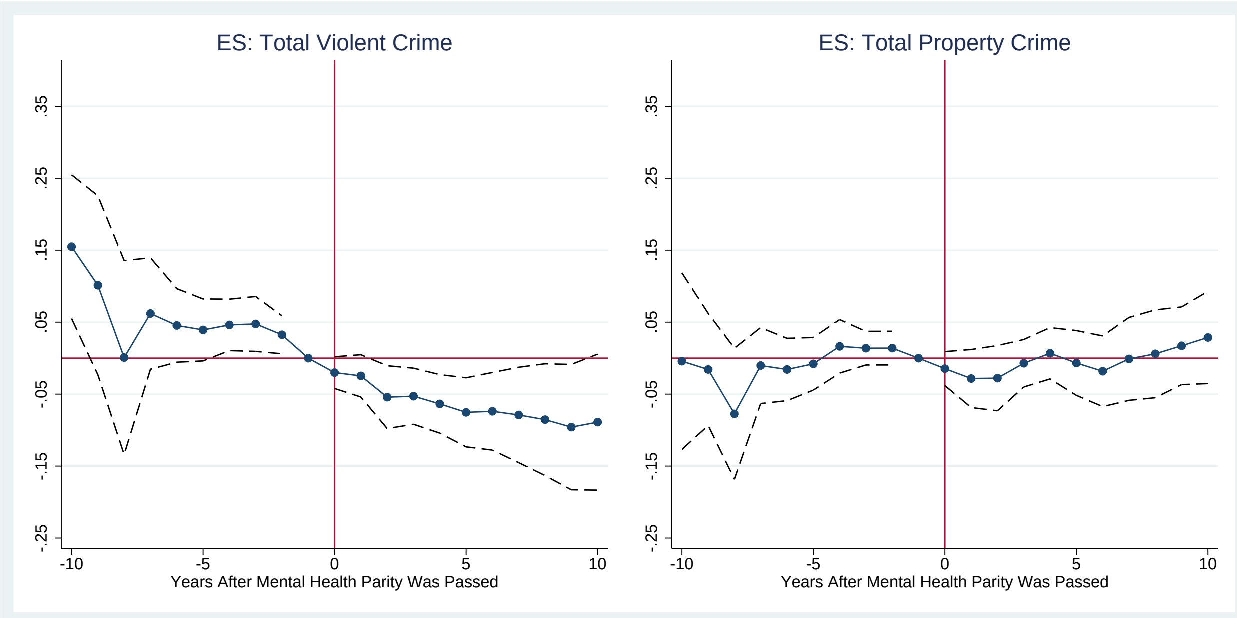
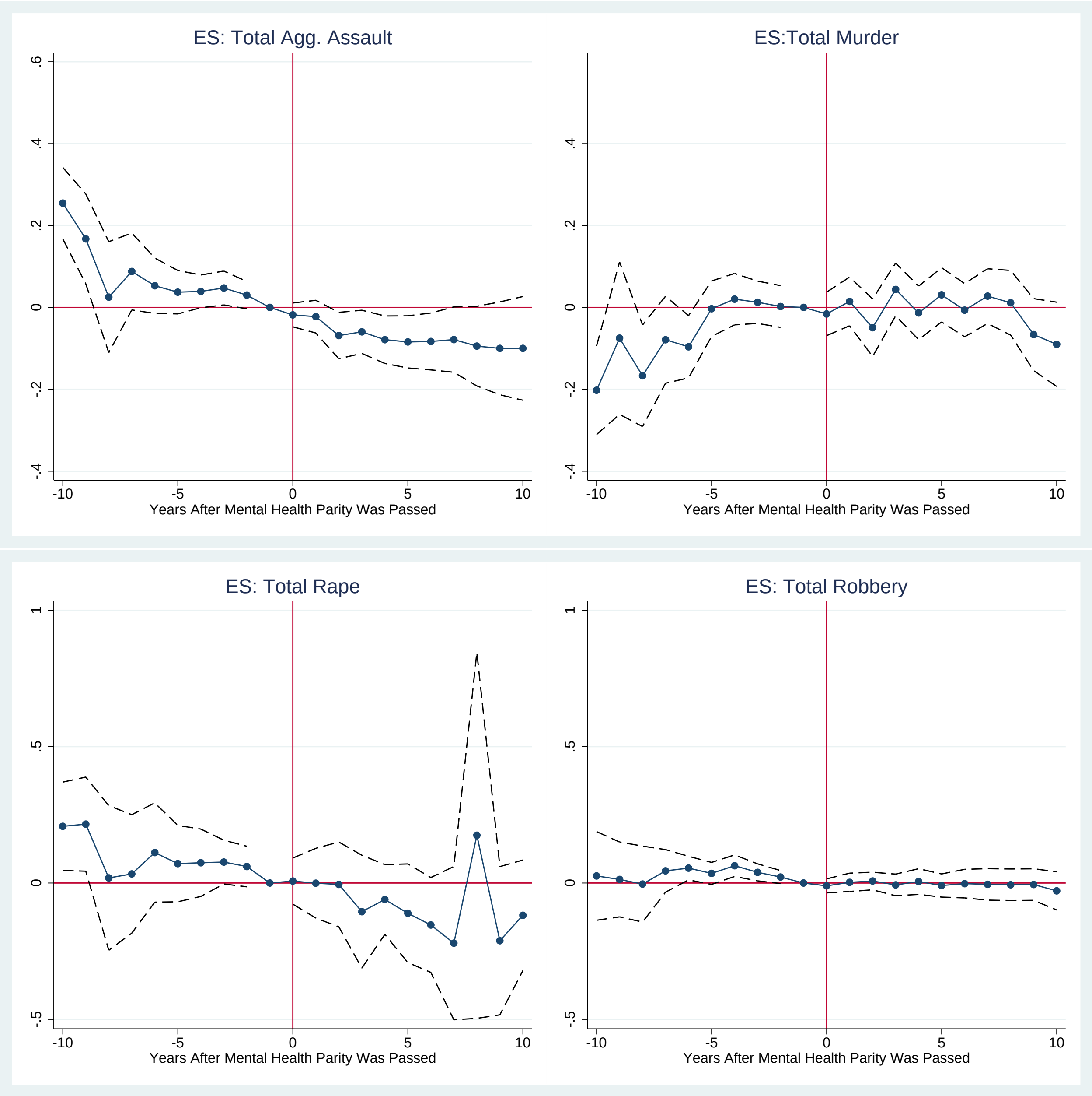


Figure 1.2: Event Study Estimates of Total Crimes



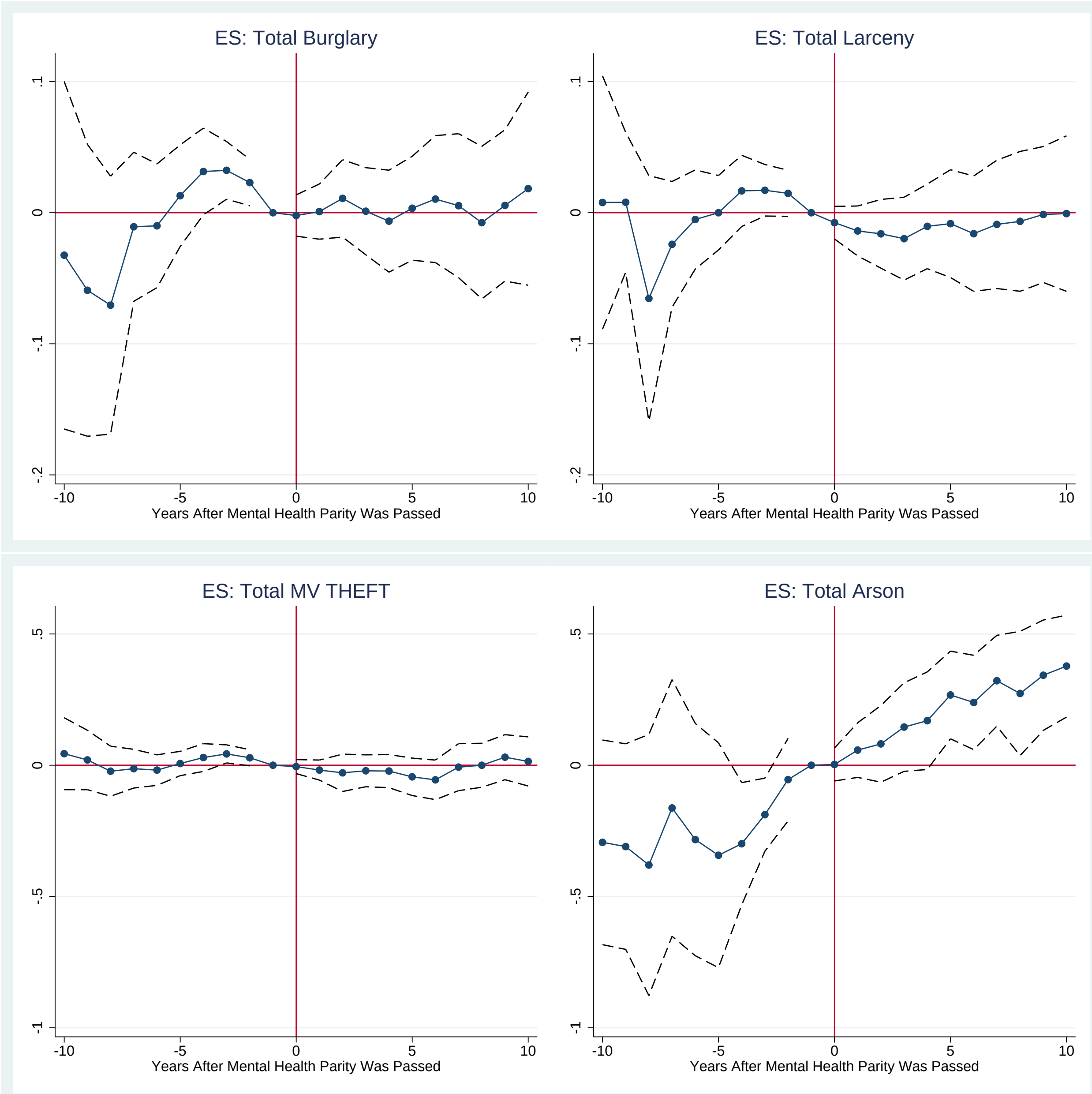
Notes: The figure plots the coefficients and the 95 percent confidence intervals of the event study design in Section 1.4.2. The dependent variable is labeled at the top of each graph. The response variable is the inverse hyperbolic sine transformation of the total crime. Standard errors are clustered at the state level. Regressions are weighted by the population variables from the UCR data. The model includes county, year, census division by year fixed effects, and controls from my preferred DID model. The vertical line represents the year of MHPL passage. The year just before passage is omitted.

Figure 1.3: Event Study Estimates of Different Violent Crimes



Notes: The figure plots the coefficients and the 95 percent confidence intervals from the event study design in Section 1.4.2. The dependent variable is labeled at the top of each graph and is the inverse hyperbolic sine transformation of the total crime. Standard errors are clustered at the state level. Regressions are weighted by the population variables from the UCR data. The model includes county, year, census division by year fixed effects, and controls from my preferred DID model. The vertical line represents the year of MHPL passage. The year just before passage is omitted.

Figure 1.4: Event Study Estimates of Different Property Crimes



Notes: The figure plots the coefficients and the 95 percent confidence intervals of the event study design in Section 1.4.2. The dependent variable is labeled at the top of each graph. The response variable is the inverse hyperbolic sine transformation of the total crime. Standard errors are clustered at the state level. Regressions are weighted by the population variables from the UCR data. The model includes county, year, census division by year fixed effects, and controls from my preferred DID model. The vertical line represents the year of MHPL passage. The year just before passage is omitted.

Chapter 2

New Evidence on the Impacts of Mental Health Parity Laws

2.1 Introduction

Historically, individuals in the US have had a difficult time accessing mental health treatment, in part because of lack of mental health coverage. Private health insurance companies typically restrict coverage for mental health treatment relative to other types of treatment (Buchmueller et al., 2007).[1] For example, there were distinctions on the benefits and coverage received by individuals for physical and mental health. The federal government passed legislation in 1998 to incentivize insurance companies to expand access, however this legislation was largely ineffective as a result of loopholes in the law. As an example, firms were free to introduce limits on visits and inpatients days for mental health treatment. Individual states responded by passing their own legislation to enhance access to mental health treatment. These mental health parity laws (MHPLs) required equal coverage for mental health services and physical health services.

[1] Many employers have been reluctant to provide equal coverage to mental health treatment in the US because of increase in costs (Gruber, 1994), (Andersen, 2015).

In this paper, I investigate the effect of mental health parity regulations on mental health outcomes and investigate potential mechanisms.

The percentage of US adults experiencing mental health illness is notable. 19.1% of adults in the US experienced Any Mental Illness (AMI) in 2018.[2] Mental health illness among young people is no better. 1 in 6 of them aged 6-17 experience serious mental illness each year.[3] Apart from being a prominent public health problem, there are other costs associated with mental illness.[4] For example, mental illness adversely affects employment and also reduces number of weeks worked and increases work absenteeism (Banerjee et al., 2017). While the purpose of MHPLs is to reduce the prevalence of mental illness and its deleterious effects, the impact of these laws is unclear. On the one hand, they could lower the cost of mental health treatment to patients, but on the other hand MHPLs could increase the costs of providing mental health coverage by increasing firm expenses. For example, Bailey and Webber (2018) show that mandates in general decrease the number of small firms but increase the number of large firms at the state level leading to firm size distortion.[5] In light of the large and growing share of US population suffering from mental health illness, and the economic costs associated with mental illness, it is imperative to understand if MHPLs are having the intended effect.[6]

Numerous papers explore the impact MHPLs have on labor market outcomes, mental health care utilization, and direct measures of mental health. Studies show an increase in mental healthcare utilization (Dave and Mukerjee, 2011; Busch and Barry, 2008; Harris et al., 2006) and an increase in employer sponsored coverage as a result of the passage of MHPLs (Li and Ye, 2017). Earlier studies on labor market outcomes like Cseh (2008)[7] find no effect on working hours, whereas later studies like Andersen (2015)[8] suggest MHPLs increases employment for all individuals with moderately distressed individuals working

[2]Adults with AMI were defined as having any mental, behavioral, or emotional disorder in the past year that met DSM-IV criteria (excluding developmental disorders and SUDs). Source: SAMSHA, 2018 National Survey on Drug Use and Health.

[3]National Alliance on Mental Illness(Mental Health by the Numbers). https://www.nami.org/mhstats

[4]Direct and indirect cost of mental health illness through lost earnings and public disability payments is estimated at 467 billion(Robertson-Preidler et al., 2020).

[5]Large firms are likely to self insure, and are not affected by state mandates because of preemption by ERISA.

[6]During the 16 year period from 2001 to 2017, the total suicide rate increased 31% from 10.7 to 14.0 per 100000. Source: https://www.nimh.nih.gov/health/statistics/suicide.shtml

[7]Time Period 1999-2004

[8]Time period from 1998-2008

more hours per week, get higher wages, and receive insurance coverage from employers. Studies on the direct impact of MHPLs on mental health outcomes are more limited. Two papers study the impact of MHPLs on suicide rates. Klick and Markowitz (2006) use state level policy and politics variables as instruments for a state having an MHPLs and find no effect of these laws on suicide rates. Lang (2013) exploits variation in the timing of MHPLs through a difference in differences approach and finds that MHPLs reduce suicides.

A review of literature indicates that there is considerable heterogeneity in the estimated effects of MHPLs depending on the time period studied. For example, studies on the effects of the MHPLs in earlier years Klick and Markowitz (2006); Cseh (2008) find no effects, whereas studies focused on later periods (Cseh, 2008; Andersen, 2015) and more states (Lang, 2013; Klick and Markowitz, 2006) tend to find significant effects. There are at least two possible explanations. One has to do with dynamic treatment effects. It is possible that it takes time for MHPLs to have an impact. A longer sample window would allow researchers to capture this. Second, it is also possible that treatment effects are heterogeneous, and this heterogeneity is correlated with the timing of adoption. In other words, MHPLs may have differing impacts in states that passed legislation early relative to states that passed legislation late. In this paper, I utilize a longer panel of suicide outcomes, additional state variation in MHPLs, and recent econometric advances to disentangle dynamic treatment effects, and treatment effect heterogeneity with the goal of better understanding the impact of MHPLs.

The most recent paper on the impact of MHPLs on mental health utilized suicide data and MHPLs passage through 2004, but significantly more years of suicide data and policy variation is currently available. Both dimensions are valuable for learning about the true impact of MHPLs. Additional time periods and more states passing MHPLs since the most recent research allows me to examine two specific things. First, the longer panel allows me to examine dynamic effects because I have more years after MHPLs were passed by states. Second, additional states passed laws in different years that allows to examine heterogeneity of treatment by treatment cohort. Novel to the difference in differences research design, three recent methodological papers (Goodman-Bacon, 2018; Abraham and Sun, 2019; Callaway and Sant'Anna, 2019) and others provide tools to analyze, and estimate causal effects in presence of heterogeneity in treatment by treatment timing. They also propose methods to overcome the negative weights problem associated with TWFE methods. These recent methods have not been used in the literature to study the impact of mental health parity laws on mental health outcomes.

Employing these methodological advances, I find that there is no significant impact of MHPLs on overall suicide deaths. The results are robust across specifications and sub-samples. Event study specifications do not show a dynamic impact of MHPLs on suicide rates over years while providing insights on the validity of parallel trends assumption. Difference in differences result do not show any significant impact of MHPLs on suicides. This result is in contrast with the result of the most recent research. For example, Lang (2013) find that there is significant impact of MHPLs on suicide rates.

It is reasonable to suspect that there is treatment effects heterogeneity by timing that is driving this contrasting results. As I have more time period, and more states passing MHPLs, I investigate for the presence of heterogeneity by timing of adoption. With the apparent heterogeneity in treatment effects seen across timing groups, I estimate the results using estimators suggested by Callaway and Sant'Anna (2019) which solves for the problems associated with treatment effect heterogeneity. The results remain robust when analyzed allowing for heterogeneity by treatment cohort. Dynamic event study plots of the group time average treatment effects estimators of Callaway and Sant'Anna (2019) satisfy pre-trends but do not show dynamic treatment effects post treatment. Average treatment effects aggregating the dynamic effects also show no significant effects of MHPLs on suicide death rates.

In addition to extending our knowledge on the impact of MHPLs on overall suicide rates, I investigate whether MHPLs reduce specific types of suicides. I investigate suicide due to guns separately. One reason is because there may be demographic differences by who owns guns and who is more likely to commit suicides by guns. Second, there are many states that consistently rank high on gun suicide rates over the sample period. Difference in differences results show that there is reduction in suicide deaths due to guns by 4.5% due to MHPLs. There is some level of dynamic impact of MHPLs on suicide due to guns. Furthermore, I investigate impacts of MHPLs on sub samples by age groups. Like the results from the full sample, there is no impact of MHPLs on overall suicide deaths of demographic subgroups aged 1-24 and 25-65.

Data from the CDC gives a grim picture on the state of mental health among the youth. For example, in 2017, 17.2 percent of youth seriously considered attempting suicide during the 12 months before the survey. The number was 13.8 percent in 2009. This underscores the importance of investigating the impact of mental health parity laws on youth suicides. The impact of MHPLs on youth mental health outcomes is not clear. To this end, I investigate the impact of MHPLs on youth mental health outcomes that has not been investigated in the literature. My results show that MHPLs do not have a significant impact on youth mental health outcomes. This results are similar to the impact of MHPLs on suicide rates for demographics aged 1-25.

Finally, I investigate the potential mechanisms through which MHPLs may impact mental health outcomes. One channel through which increased access to mental health parity laws could have affected mental health outcomes is through availability of treatment centers. One would expect treatment centers to open and provide treatment to people in response to more people seeking treatment. Supply side responses by mental health providers is a potential mechanism through which MHPLs could work. I examine the impact of these laws on substance abuse and mental health treatment centers. I find there is no impact of MHPLs on the number of substance abuse treatment centers. I do not see an increase in the number of substance abuse treatment centers due to passage of MHPLs. Supply side response possibly explains the null impact of MHPLs on suicide rates.

2.2 Institutional Background

Between 1990 and 2010, the US federal government and individual states passed three types of legislation aimed at expanding mental health coverage in the US: the federal Mental Health Parity Act (MHPA) of 1996, the Mental Health Parity and Addiction Equity Act (MHPAEA) of 2008, and state laws that mandate equal mental and physical health coverage or mental health parity laws(MHPLs). While not my policy variable of interest, I first discuss federal laws since these federal law changes occur in my sample period.

2.2.1 Mental Health Parity Act, 1996

The Mental Health Parity Act (MHPA) was passed in 1996, and went into effect in 1998 with a sunset provision that was originally set to expire in September 2001 (Gitterman et al., 2001). The MHPA was eventually extended and replaced by MHPAEA in 2008 (Li and Ye, 2017). While the MHPA did not require insurers to offer mental health benefits, the law mandated that if mental health coverage is offered, the benefits must be equal to annual or lifetime limits offered for physical health care (NCSL). It also required group health plans covering mental health care to offer annual dollar, and lifetime limits that are comparable to to those for medical or surgical benefits.

There were many loopholes in MHPA. First, it exempted firms with fifty or fewer employees. Second, firms could claim an exemption if compliance with the law causes health-care cost to increase by more than 1%. Firms could also circumvent requirement to offer equal lifetime dollar benefits by introducing limits on the number of outpatient visits and inpatient days (Buchmueller et al., 2007). These limitations made the 1998 MHPA more symbolic in nature than a substantial policy change (Peterson and Busch, 2018).

2.2.2 Mental Health Parity and Addiction Equity Act, 2008

Mental Health Parity and Addiction Equity Act (MHPAEA, 2008) was passed in 2008 and went into effect in 2010. MHPAEA preserved MHPA, and made additions to it. In addition to the existing benefits of equal annual and lifetime dollar limits, it extended benefits to other insurance features i.e., deductibles and copayments. It also extended the number of visits or days of coverage (CMS, MHPAEA). Although MHPAEA strengthened MHPA in a number of dimensions, it still did not require firms to include mental health treatment in their benefits packages. In other words, the legislation only applied to insurers that choose to include mental health in benefits packages.

2.2.3 State Mental Health Parity Laws

Finally, my policies of interest are state Mental Health Parity Laws (MHPLs). States were active in passing MHPLs since the early 1990's and continued passing them until the late 2000's. The laws differ from state to state on who and what is covered. While it is challenging to categorize laws into perfect groups, I consider full parity laws as those states that provide full comprehensive coverage to a broad range of mental health coverage. This includes states that require parity for all significant fully insured health plans with no extra exemptions on firm size and costs. This also includes states that mandate equal coverage for all group health plan but exempts very small firms. I also include state that mandates parity coverage for state employees.

I compile data on the year mental health parity laws were passed consulting many sources. First, I examine the previous literature (Lang, 2013), (Dave and Mukerjee, 2011),(Li and Ye, 2017) to obtain the year mental health parity was passed in each state. In the case of conflicting reports, I check National Conference of State Legislatures(NCSL) (or the main state statute) to obtain precise timing of mental health legislation. NCSL is a common source for information on state level policy changes. For example, (Dave and Mukerjee, 2011), (Popovici et al., 2017), and (Buchmueller et al., 2007) use NCSL as a source of their definition of MHPLs.

Table 2.1 presents the states passing mental health parity legislation over time. The states that are presented in this table are the ones that passed full parity laws. These full parity laws are those laws that provide fully comprehensive coverage to a broad range of mental health conditions. The passage of the law is concentrated in the year 1999 and 2000. With 10 states passing MHPLs, these two years provide a key source of variation in the MHPLs. The most recent research uses the sample up to 2004 (Lang, 2013). I have additional years of data and many states that pass MHPLs. Additionally, there is plenty of variation in state laws occurs when the federal legislation is fixed from 1998-2008.

Other categories of laws in the literature are mandated offering, mandated if offered, and minimum mandated benefits. Mandated offering requires insurance plans to offer mental health coverage at parity with physical health coverage, but allows consumers to accept coverage or not. Mandated if offered requires equal coverage if mental health treatment is offered. Minimum mandated benefits generally requires a minimum level of mental health treatment coverage that is generally not at par with coverage for physical health treatment. This paper's focus is on the effects of full parity laws. Previous literature, for example Lang (2013) estimated effects of parity laws compared to minimum mandated benefits in their specification. I also use alternative coding by including Mandated Offering in my specifications.

An important feature of the state-level MHPLs is that self-insured firms are exempt by ERISA. If firms self insure (firms bear the risks themselves rather than contracting with an insurance company), then the plan is exempt from state regulation under the 1974 Employee Retirement Income Security Act (ERISA) (Andersen, 2015).[9]

2.3 Data

2.3.1 Outcomes

Suicide Mortality Rate

The main outcome variable to study the impact of MHPLs is suicide rates. One reason suicide is a good measure of mental illness is because suicide is highly correlated with prevalence of mental illness and suicide is committed by people suffering from mental illness. 90% of people who die by suicide had shown symptoms of mental health conditions (Isometsä, 2001).[10] Another reason is convenience. Every suicide is recorded and is published as aggregated data in the CDC website, providing a measure of mental health for the whole population. Moreover, this measure has been used in previous studies as a proxy for mental health making it a valid candidate for a proxy of mental health.

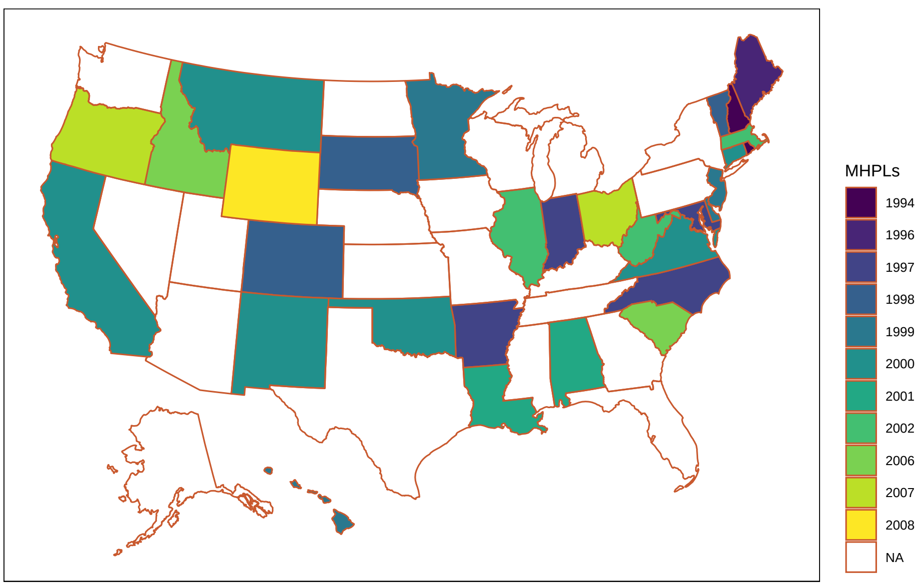
I obtain suicide death rates from the CDC-WONDER cause of death files. The data is available for public use for each state through 2017, but I limit my study from 1990 to 2010.[11] I use deaths due to all

[9] However, MHPA and MHPAEA being a federal law, applies to self-insured employer-sponsored plans (Buchmueller et al., 2007).

[10] https://www.nami.org/mhstats; https://www.ncbi.nlm.nih.gov/pubmed/11728849

[11] The Affordable Care Act extends mental health parity in 2010. Therefore, I focus on the period from 1990-2010. In addition to the baseline sample covering 1990-2010, I also consider a smaller window, 1998-2008. During this time period there are no federal law changes implying a cleaner counterfactual.

Figure 2.1: Passage of MHPLs by States



cause of suicides, including suicide due to firearms and explosives, and overall suicides. Data on deaths from 1999 and later is coded in ICD-10 Underlying Cause of Deaths, whereas data before 1999 is coded in ICD-9 Underlying Cause of Deaths.

As deaths are classified under ICD-10 codes only beginning 1999, I take Underlying Cause of Death, ICD-10 Codes X60-X84[12] for all suicide deaths for 1999-2010 and deaths due to ICD-9 codes E950-E959[13] for suicide deaths before 1999. To study if suicides by different causes are impacted differently by access to mental health care, I group them into suicides due to firearms and explosives [14], and suicides by all causes using their corresponding ICD-9 codes before 1999 and ICD-10 codes for years beginning 1999. In addition, to check for heterogeneity by age-groups, I group deaths from 0-25 years of age and deaths from 25-65 years of age.

I also obtain suicide data at the county level because of the following potential benefits. States are highly heterogeneous, and looking at the county level allows to control for heterogeneity. Second, the number of observation is more when county-level data is used. Finally, more observation gives more members per group of states treated at time t allowing us to use the group time average treatment effects of Callaway and Sant'Anna (2019) more efficiently. From the publicly available data set from CDC-Wonder, I get county level data for a balanced panel of 366 counties over 1990-2010. However, I lose the flexibility of getting deaths by different causes in my data.

YRBS

To investigate whether mental health among youth is improved following the passage of MHPLs, I use the Youth Risk Factor Surveillance System Survey (YRBS) data from CDC. My hypothesis is that gaining access to mental health treatment due to passage of parity laws would lead to fewer mental health problems as children gain access to mental health services. The YRBS survey began in 1990 and covered a

[12] ICD 10 codes X60-X64 are deaths that are coded for intentional self harm and includes purposely self in-inflicted poisoning or injury.

[13] ICD-9 codes E950 to E959 includes deaths due to suicide and self inflicted injury for years before 1999. This is the closest ICD-9 equivalent of ICD-10 death codes that I could find in CDC-Wonder.

[14] These include ICD-10 code X72-X74 and ICD-9 codes E955.

subset of US states. By 1999, most states are included in the sample. These surveys are conducted every two years. The YRBS data is representative of the state population, when the response rate is about 60%.

This individual level survey data has state identifiers, making it suitable for use in our study to analyze the impact of MHPLs on youth mental health outcomes. The number of observations in this data set from 1990-2010 is more than 500,000 whereas the number of observations from 1999-2010 is more than 385,000. Apart from being an independent outcome to study the impacts of MHPLs, measures of youth mental health will complement the main outcome on suicide deaths. The variables of interest for my study are those related to mental health of youth. Questions related to mental health of youths in YRBS include questions about suicide-ideation[15], making a plan about how they would attempt suicide [16], and number of times suicide was attempted. [17]

Data from CDC shows that status of youth mental health is getting worse. While being aware that recent data does not lie in the sample period, I use some statistics to illustrate the problem. For example, in 2017, 17.2 percent of youth seriously considered attempting suicide during the 12 months before the survey. The number was 13.8 percent in 2009. In 2017, 13.6 percent of youth made a plan about how they would commit suicide (17.1 percent for female youth). In 2009, the corresponding numbers are 10.9 percent and 13.2 percent for female. 7.4 percent of youths attempted suicide in 2017. 9.3 percent of females attempted suicides in 2017. 6.3 percent of all youths surveyed attempted suicides in 2009. 8.1 percent of females did this in 2009.[18]

2.3.2 Controls

While policy variation across states can induce heterogeneity in suicide rates across states, states differ in many other aspects that can generate variation in mental health outcomes over time. This necessitates controlling for these other factors, especially if they are correlated to MHPLs. I use several relevant time varying controls that are not captured by state fixed effects. My control variable include demographics,

[15] During the past 12 months, did you ever seriously consider attempting suicide?
[16] During the past 12 months, did you make a plan about how you would attempt suicide?
[17] During the past 12 months, how many times did you actually attempt suicide?
[18] The percentages are taken from the CDC, YRBS, Youth Online Query Tool. and https://www.cdc.gov/healthyyouth/data/yrbs/pdf/trendsreport.pdf

macroeconomic variables, state-wide policy variables that could impact our proxies of mental health, percentage of workforce in large firms to control for the fact that ERISA preempts large firms that are most likely to self insure, and measures of access to public health insurance.

Demographics include the percent of population that is black and the percentage of population in a state that live in urban areas, and the percentage of males aged 45-65. Macroeconomic indicators include per-capita income and unemployment rate from the Bureau of Labor Statistics and the Bureau of Economic Analysis.

Self insured firms are preempted from state laws as a result of Employee Retirement Income Security Act (ERISA) of 1974. Workers in self insured firms are not covered by state mental health mandates. The share of workers that work in self insured firms in a state is correlated with share of people impacted by the mental health parity law in a state. For example, even though the laws are passed in a state, people working in self insured firms will not get mental health treatment by virtue of the law. This will inflate the actual population receiving benefits due to MHPLs.

I use the percentage of the total workforce that work in firms with employment size more than 500 as a proxy for percentage of workers working in self insured firms. According to Medical Expenditure Panel Survey, more than 90% of firms with more than 1000 employees are self insured and 80% of the firms with more than 500 employees are self insured (Li and Ye, 2017). I collect the firm employment size data from the Census Bureau, Statistics of the US Business. The data set contains number of firms, employment number, and payroll information.[19] Therefore, this is a good proxy for the percentage of workers that might not be affected by MHPLs due to ERISA.

Another set of variables that I use as controls in this study is the level of access to public insurance. Access to public health insurance might be correlated to mental health status of people in a state, and may be correlated to our outcomes. Greater access to public insurance may mean better access to mental healthcare. I take the percentage of people that are covered by medicaid, and the percentage of people that are covered by medicare to control for state level access to public health insurance. Data is obtained from

[19] The data is taken from https://www.census.gov/data/tables/time-series/econ/susb/susb-historical.html and these are available as total for firms that fall in different bins according to employment size.

IPUMS and derived from Annual Social and Economic Supplement (ASEC) of the Current Population Survey. Moreover, I use state mental health expenditures, and per capita alcohol consumption from Edwards et al. (2018) to control for time varying state mental health expenditure and alcohol consumption because it could be correlated with mental health outcomes and bias our results.

There are state policies that could be correlated with suicides and are passed at the same time as MHPLs. For example, Edwards et al. (2018) find that the existence of a purchase delay in firearm reduces firearm related suicides. Especially as suicides due to guns are a significant proportion of overall suicides, I control for gun laws which are passed during the passage of MHPLs. I take data on different types of state gun-laws from Edwards et al. (2018).

2.3.3 Mechanisms: Substance Abuse and Mental Health Treatment Centers

MHPLs expand mental health coverage and thus are likely to lead to greater demand for mental health services. This increase in demand may lead to improved mental health outcomes if there is a corresponding increase in the supply of mental health services. To investigate whether there is supply side response, I use mental health treatment centers, and examine if there is an impact of MHPLs on number of substance abuse treatment centers. I expect mental health outcomes to improve if new mental health treatment centers to open and provide more service. In contrast, no improvements in mental health outcomes could be because there is no increase in mental health treatment centers even after MHPLs are passed.

The data set for examining the mechanisms for this study comes from the Census Bureau's County Business Patterns data set[20]. It is an annual series that provides sub national data on economic establishments. The data is available in the zip-code, county, and at the state level. There is information on the total number of establishments, number of employees, first quarter payroll, and annual payroll by North American Industry Classification System (NAICS) code for each sub national region.

Using the NAICS codes, I calculate the total number of establishments for each subnational level (county or state). The treatment centers I use are coded as Office of Mental Health Practitioners.[21], Office

[20] https://www.census.gov/programs-surveys/cbp/data/datasets.html
[21] 2007 NAICS Code 621330.

of Physicians(Mental Health Specialists) [22], Outpatient Care Centers [23], and Residential Mental Health and Substance abuse facilities. [24] This data is available from 1998 onward. I could not get the data before 1998 because before 1998 these institutions were coded in Standard Industrial Classification (SIC) code and SIC does not have granular information on treatment centers. Table B.1 gives a snapshot of the final compiled data.

2.3.4 Descriptive Statistics

Table 2.2 below provides descriptive statistics of the main variables used in this study. In the first column, I present summary statistics for the whole sample. In the second and third column, I present the pre and post treatment summary statistics for the treated states. In the final column, I present the summary statistics of the untreated states.

There are some differences in the summary statistics of variables in the treated states in the pre-treatment period and the variables in the untreated states. Per-capita income, per-capita mental health expenditures, and percentage covered by medicaid are different in the pre-treatment period in the treated states compared to the untreated states. Also, there is reduction in the log of suicide rate post treatment. It is 2.52 in the pre treatment and 2.47 post treatment. However, there are other important changes like changes in per-capita income, changes in per-capita real mental health expenditures, happening over time, which makes it difficult to conclude that these reductions are due to MHPLs.

The recent research investigating the impact of MHPLs on suicide death rates finds different results depending on the sample window taken in the study. One reason could be the heterogeneous treatment effects correlated to treatment timing. For example, we might anticipate heterogeneous effects if the state passing MHPLs early or late are observably different from each other.

To investigate if states passing MHPLs in different times are different in their covariates, I check summary statistics for group of states passing MHPLs early and late in 2.3. Early adopters are those states that

[22] NAICS code 621112.

[23] 2007 NAICS code 6214

[24] NAICS Code 623220.

passed MHPLs before 1999, and late are those states that passed MHPLs after 1999. I present summary statistics for those groups to compare if the states passing early or late are different in their covariates. Early adopters and late adopters can differ in their covariates prior to passing MHPLs. To see this, I compare the variables of early adopters and late adopters prior to passing MHPLs. There is some differences in covariates. For example, per-capita mental health expenditures is higher for the early treated states, fraction urban is higher in late treated states.

2.4 Empirical Strategies for Estimating Policy Effects on Suicides

In this section, I describe the empirical approaches used in my analysis. The standard approach in the most recent research has been a difference in differences analysis. To be consistent with the literature, I first analyze the impact of MHPLs taking this same approach. However, the standard differences in differences model makes a number of strong assumptions. The two way fixed effects estimator do not give causally interpretable results when some of these assumptions fail. In the subsequent sections, I estimate the treatment effects using recent advances in the literature by progressively relaxing these assumptions.

2.4.1 Difference in Differences

This study exploits the variation in adoption of mental health parity laws by states over the time period 1990-2010. At first, I use the following two way fixed effects difference in differences framework to estimate the impact of MHPLs on suicide rates. The two way fixed effects regression specification is given by:

$$Y_{st} = \alpha_0 + \alpha_1 Post \times EverTreat_{st} + \alpha_2 X_{st} + \gamma_t + \gamma_s + \epsilon_{st}$$

where, $Post \times EverTreat_{st}$ is a binary indicator that takes a value of 1 if state s had a mental health parity law in place in year t. It takes a value of 0 otherwise. Y_{st} is log of suicide death rates in state s in year t. γ_s is state fixed effects. γ_t is year fixed effects. X_{st} is a vector of relevant controls.

A key identification assumption in the difference in differences analysis is that outcomes in the treated and the non-treated units would evolve in the same way if the treated states were not treated. This assumption requires that in the absence of the treatment the difference between the treatment and the control units is constant over time. In the next section, I investigate the validity of this assumption through an event-study type analysis.

Another assumption is that there is no policy endogeneity, that is the treatment is not dependent on the outcome. This study assumes that states do not pass the mental health parity law as a result of an increase in suicide deaths. When the assumptions of difference in differences is met results can be interpreted as causal. Previous study Lang (2013) and Klick and Markowitz (2006) essentially take this approach in their study.

2.4.2 Event Study

The difference in differences method gives us a constant treatment effect of the policy change. However, it is reasonable to expect that the impact of policy may expand over time as people learn about access to mental health treatment. It is possible that the impact of MHPLs is not seen immediately after MHPLs are passed. An event study model allows the impacts of policy change be traced over time. Additionally, it also allows us to test for the parallel trends and make sure that the pre-trends are not driving the results.

In the case of mental health treatment, I expect to see the impact of gaining access to treatment several years after access to treatment has been granted rather than immediately. Additionally, the effect of the intervention may depend on the length of exposure. The event study method is an appropriate research design to trace the path of the impact of the treatment effects of access of mental health treatment due to passage of mental health parity laws.

Event study helps to get a visual idea of how the outcome is moving in absence of treatment. We can plot the difference between the treated and the control group prior to treatment and check if they are zero. This allows to see if there are existing pre-trends that are driving the results. A flat pre-treatment period effects would suggest that there are no pre-trends that could bias results.

The two way fixed effects event study design to estimate the effects of MHPLs is given by the equation:

$$Y_{st} = \gamma_s + \gamma_t + \sum_{k=-10}^{k=10} \pi_k Parity_s * I(t - T_s = k) + \alpha_0 X_{st} + \epsilon_{st}$$

where $Parity_s$ is an indicator that the state s ever had a mental health parity law in place. Y_{st} is Deaths from suicides in state s in year t. γ_s denotes state fixed effects. γ_t denotes year fixed effects. t denotes year t. T_s denotes the year MHPLs are passed in state s. k denotes the leads and lags after and before mental health parity is passed respectively. The comparison year is the year before the mental health parity law is passed.

The identifying assumptions of event study design are similar to the difference in differences model. First, in the absence of treatment the treated states would evolve similarly (parallel trends in baseline). Second there is no anticipatory behavior prior to treatment. Third, there is treatment effect homogeneity. The key difference here is that causal impact is allowed to vary with length of exposure allowing us to examine dynamic effects.

The third assumption is violated when different cohorts have different profiles of dynamic treatment effect (Abraham and Sun, 2019). In event studies heterogeneity in treatment effects could arise because cohorts could differ in their covariates and this may affect how they respond to treatment. After controlling for covariates, cohorts may still vary in their response to treatment if units select treatment timing based on treatment effects. In addition, cohorts may vary in their treatment effects due to calendar-time varying effects like macroeconomic conditions that could be different across cohorts which could result in different response to treatment (Abraham and Sun, 2019).

2.4.3 Methodological Issues in the Two Way Fixed Effects Estimators

Both the difference in differences model and event study model assume that the impact of MHPLs is homogeneous across both time of implementation and states. However, this will be violated if different treatment cohorts or states have different treatment effects.

A recent line of methodological literature in the TWFE difference in differences design point out to issues when there are multiple units and more than two time periods. These papers address issues in the results obtained from the TWFE estimators when there is variation in treatment effects due to treatment timing. They show that satisfying parallel trends is not sufficient to establish unbiased estimates when the treatment timing varies, and treatment effects evolve due to treatment timing.

Goodman-Bacon (2018) show that the two-way fixed effects estimator is the weighted average of all possible 2×2 estimators across treatment groups. Further, they show that some of the 2×2 estimators use units treated in a particular time as a treatment group and never treated units as a control group, while there are other units that use earlier treated groups, and later treated groups as a control which could be problematic. I check if treatment effect varies due to treatment timing by using different combination of early/ late/ never treated groups. I use the method advised in (Goodman-Bacon, 2018) to decompose the TWFE estimators into different 2×2.

In a TWFE regression, units whose treatment status does not change over time serve as controls for units whose treatment status change over time. When newly treated units are compared to already treated units, then it adjusts the path of outcomes for newly treated units by the path of outcomes of already treated units, which is not the potential outcomes of the untreated units. To account for variation in treatment timing, I use the recently proposed group-time average treatment effects estimator by Callaway and Sant'Anna (2019).

2.4.4 Group Time Average Treatment Effects

I employ the recent methods proposed by Callaway and Sant'Anna (2019) that allows estimation and inference on interpretable causal parameters allowing for treatment effect heterogeneity and dynamic effects. This method avoids issues of interpreting results of the two way fixed effects regressions in difference in differences set-ups that is pointed out in (Goodman-Bacon, 2018; Abraham and Sun, 2019). Callaway and Sant'Anna (2020) propose disaggregated causal parameter known as group-time average treatment effects estimators. It is the average treatment effects of group g at time t. A group is defined by the time in which units are first treated.

When there are many groups, Callaway and Sant'Anna (2020) provide steps to aggregate group-time average treatment effects into summary causal effects parameters. In addition to a single overall effect parameter, these aggregation schemes give single parameter that highlight heterogeneity along dimensions, like how average treatment effects vary due to length of exposure to treatment, how average treatment effects vary across treatment groups, and how cumulative average treatment effects evolve over calendar time. I am particularly interested in estimating the single overall treatment effects parameter along with the aggregated event study type parameter that helps me to examine how treatment effects vary due to exposure to treatment.

Callaway and Sant'Anna (2019) denote sample periods by $\mathcal{T}$ periods with a particular time period denoted by t, where t=1,...,$\mathcal{T}$. In addition, $D_{i,t}$ denotes a binary variable equal to 1 if unit i is treated in period t and equal to zero otherwise. Next, I describe the assumptions of the group-time average treatment effects parameter, estimation of the parameter, and aggregation schemes that can be used to estimate the dynamic model.

Callaway and Sant'Anna (2019) first assume that there is irreversibility of treatment. It means that no one is treated at $t = 1$, and that once a unit becomes treated they remain treated in the next period. D_{t-1}=1 implies that D_t=1. This is the staggered adoption of treatment like our case. Once a state adopts MHPLs at time t, they have MHPLs in the next period also.

Under this assumption, for all units that eventually participate in treatment, G defines the "group", they belong to. If a unit does not participate in treatment in any time period then $G = \infty$. G_g is a binary variable that is equal to 1 if a unit is first treated at time g. Also, C denotes a binary variable that is equal to 1 for units that do not participate treatment any time period. The generalized propensity score $P_{g,s}(X)$ that is the probability of being first treated at time g, conditional on pre-treatment covariates X, and on either being a member of group g (in this case $G_g = 1$) or a member of "not yet treated" group by time s (in this case $(1 - D_s)(1 - G_g) = 1$) is given by $p_{g,s}(X) = P(G_g = 1|X, G_g + (1 - D_s))$. Callaway and

Sant'Anna (2019) define the general probability of treatment as $p_g = p_{g,\mathcal{T}}(X) = P(G_g = 1|X, G_g + C)$, which is the probability of being treated in period g conditional on covariates and either being a member of a member of group g and not participating in treatment in any time period. Also they denote $\mathcal{G}$ be the support of G excluding $g = \infty$.

Under potential outcomes framework, accounting for dynamic treatment selection, (Callaway and Sant'Anna, 2019) give that the observed and potential outcomes for each unit i are related through

$$Y_{i,t} = Y_{i,t}(0) + \sum_{g=2}^{\mathcal{T}} (Y_{i,t}(g) - Y_{i,t}(0)) \cdot G_{i,g}$$

where $Y_{i,t}(0)$ denote unit $i's$ potential outcomes at time t if they remain untreated through time period $\mathcal{T}$ i.e., they do not participate in the treatment across all available periods. For $g = 2, \ldots, \mathcal{T}$, $Y_{i,t}(g)$ denotes the potential outcome that unit i would experience at time t if they were to first become treated in time period g

We only observe one potential outcomes path for each unit. For those that do not participate in the treatment in any time period, observed outcomes are the untreated outcomes for each period. For units that do participate in the treatment, observed outcomes are the unit specific potential outcomes corresponding to the particular time period when that unit adopt treatment.

Second, Callaway and Sant'Anna (2019) assume that the variables follow a random sampling. i.e $\{Y_{i,1}, Y_{i,2}, \ldots, Y_{i,\mathcal{T}}, D_{i,1}, D_{i,2}, \ldots D_{i,\mathcal{T}}\}_{i=1}^{n}$ is independent and identically distributed. The variables are independently and identically distributed. This allows all the potential outcomes to be viewed as random. This assumption does not impose restrictions between potential outcomes and treatment allocations, nor does it restrict the time series dependence of the observed random variables. This assumption imposes that each unit is randomly drawn from a large population of interest.

Callaway and Sant'Anna (2019) consider the potential outcomes framework and get the average treatment effect for units who are members of a particular group g at particular time period t is given by,

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1]$$

which are the group time average treatment effects.

$ATT(g,t)$ does not impose any restrictions on treatment effect heterogeneity across group or across time. For instance, by fixing a group g and varying t, we can examine how treatment effects evolve over time for each specific group. We can also do the same thing for different groups to check if treatment effects dynamics for some group is different than for other groups (Callaway and Sant'Anna, 2019). By aggregating this group-time average effects we can answer questions like (a) What was the average effect of participating in the treatment across all groups that participated in the treatment by time period T? (b) Are average treatment effects heterogeneous across groups? (c) How do average treatment effects vary by length of exposure to the treatment? (d) How do cumulative average treatment effects evolve over calendar time?. In my study, I am particularly interested in the dynamic treatment effects, and how the treatment effects evolve over time.

Furthermore, Callaway and Sant'Anna (2019) make limited treatment anticipation assumptions, and conditional parallel trends assumptions to identify the group time average treatment effects.

The limited treatment anticipation assumption restricts anticipation for all eventually treated groups. Formally, limited treatment assumption states that: there is a known $\delta \geq 0$ such that $\mathbb{E}[Y_t(g)X, G_g = 1]$=$\mathbb{E}[Y_t(0)|X, G_g = 1]$ almost surely for all $g \in \mathcal{G}, t \in \{1, \dots T\}$ such that $t \leq g - \delta$. When $\delta = 0$, then there is no anticipation. This is the case when treatment path is not a priori known or when units are not the ones who choose treatment. However, this model also allows for anticipation. If there is anticipation by 1 period then $\delta = 1$. Under this assumption, $ATT(g,t)$ =0 for all pre-treatment period such that $t < g - \delta$.

Next, Callaway and Sant'Anna (2019) make two alternative assumptions with respect to evolution of untreated potential outcomes. These are conditional parallel trends based on "never treated group", and conditional parallel trends assumption on "not yet treated" groups. These are two different conditional parallel trends assumptions that hold after conditioning for covariates. [25] Conditional parallel trends assumptions are more plausible than the unconditional parallel trends assumptions when participating in treatment (or adopting MHPLs) depends on these covariates.

These two alternative assumptions allow for either "never treated group", or "not yet treated group" to be taken as controls for the pre-periods. Conditional on covariates, the first assumption states that the average potential outcomes for the group first treated in period g, and the never treated group would have followed similar paths in the absence of treatment. On the other hand, the second alternative assumption states that conditional on covariates, the potential outcomes for the group first treated in period g and the not yet treated group would have followed similar paths in the absence of treatment. Using the never treated group as controls is appropriate when there is a sizable group that do not participate in treatment. Using not yet treated groups of units as control is appropriate when the never treated group of units are too small.

Finally, Callaway and Sant'Anna (2019) non-parametrically identify the group time average treatment effects under the above assumptions, which allows for heterogeneity.

While $ATT(g, t)$ can be a parameters of interest when there are relatively less number of groups, aggregating them to yield aggregated parameters can be more useful in our case as there are many groups that pass MHPLs at different g. Callaway and Sant'Anna (2019) provides ways to aggregate $ATT(g, t)$ parameters that gives a overall treatment effects parameter, and a dynamic event study type parameters. They aggregate $ATT(g, t)$ as

$$\theta = \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} w(g, t) \cdot ATT(g, t))$$

[25](Heckman et al., 1997) motivate conditional parallel trends assumptions in the context of evaluating job training programs, because the distribution of covariates like age, employment history, and education differs between who participate in job treatment, and who does not.

where $w(g,t)$ are weighing functions.[26] Aggregations can be done to answer questions like (a) How does the effect of participating in the treatment vary with length of exposure to the treatment? (b) Do groups that are treated earlier have, on average, higher/lower average treatment effects than groups that are treated later? (c) What is the cumulative average treatment effect of the policy across all groups until some particular point of time? While all other aggregations can be done, I focus on aggregations that will give me a single overall treatment effects parameter as well as aggregations related to understanding dynamic effects.

The TWFE difference in differences and the event study methodology that I described earlier in the empirical section suffers from negative weighing problems. This can cause problem because the treatment effects may be positive for each units but the TWFE estimates may result in treatment effects being negative as discussed in (Goodman-Bacon, 2018). Even when the weights are not negative, the weights on the underlying treatment effects parameters are entirely driven by the TWFE estimation strategy and are sensitive to the size of each group, the treatment timing, and the total number of periods. The negative weights problem is discussed in Goodman-Bacon (2018) for the difference in differences type, and in Abraham and Sun (2019) for the dynamic event study type. Aggregating schemes of the group time average treatment by Callaway and Sant'Anna (2019) circumvent the negative weight problems.

2.5 Results

My results are organized as follows. First, I present the two-way difference in differences results. Second, I present results of the two way fixed effects event study model. Third, I check for the presence of heterogeneity in treatment effects due to treatment timing by separately estimating models for early, never, and late adopters.[27] Finally, I present results that incorporate heterogeneity by treatment group and present aggregated and dynamic results.

[26]The weighing functions are carefully-chosen by the researcher such that θ can be used to address a well-posed empirical policy question (Callaway and Sant'Anna, 2019).

[27]I also check for heterogeneity in treatment effects due to treatment timing using 2×2 decomposition theorem of (Goodman-Bacon, 2018), and get results similar to the early/late/never.

2.5.1 Difference in Differences

I present the difference in differences results on the impact of MHPLs on suicide death rates using the full sample (1990-2010) in Table 2.4 with log of suicide death rates as the dependent variable.[28] I cluster standard errors in the regressions at the state level, and weight each regression by state population. I include state fixed effects, year fixed effects, controls for demographics, macroeconomic measures, and the percentage of workers in the state that work in large firms in a given year. Most specifications indicate that there is no significant impact of state mental health parity laws on suicides rates.

In column (1), I present the difference in differences estimates of the effect of MHPLs on total suicide rates. Although the sign is negative, I find no significant impact of MHPLs on total suicide. This is in contrast to the result by Lang (2013) but similar to the results by Klick and Markowitz (2006). In column (2), I present the same specification with a dummy for whether a state had mandated offering in place. The results are similar.

To check if there is heterogeneity in the effects of MHPLs on suicide rates by the percentage of people working in large firms, I include an interaction term in my specification and present the results in column (3). I interact MHPLs to the percentage of workforce that work in large firms. The results do not change. The coefficient term of the interaction term is very small in magnitude. Due to the timeline of the MHPLs passed by different states coincides with the passage of federal laws during our study window, it is reasonable to hypothesize that there are differential treatment effects of MHPLs depending on whether there was federal law already in place when a state passed MHPLs. To check for this, I interact the MHPLs with the the 1998 MHPA and the 2008 MHPAEA to check for differential impacts. The results are robust.

I group death rates to examine the impact of MHPLs on suicide rates due to guns. There are at least two reasons to examine this sub-sample of suicide deaths. First, it is possible that the demographics that uses guns are different, and is differently affected by the MHPLs. Second, there is some apparent regional pattern in firearm mortality rates in the United States. For example in 2018, Mississippi, Alabama, Wyoming, Missouri, Louisiana, Alaska, New Mexico, Arkansas, West Virginia and Nevada are the top

[28] I take log of suicide death rates because the death rates are more skewed towards the right.

10 states with firearm mortality. [29] Some of these states rank high in firearm mortality rates consistently. There also seems to be some pattern in suicide rates due to guns. Alabama, Nevada, Wyoming, Montana, Louisiana, Arkansas, West Virginia consistently high gun suicide rates. [30] Therefore, it is reasonable to assume that there may be heterogeneity by cause of suicide deaths. Additionally, suicide due to gun are a major chunk of suicides in many states.

I take the subgroup of suicide due to guns in each state, run the same specifications, and present the results in Table 2.5. The results are different than the results for the overall suicide deaths rates. The results show that MHPLs seem to decrease number of suicide deaths due to guns. The baseline estimates of the effects of MHPLs on suicide due to guns are in the first column. There is a significant effect of MHPLs on suicides due to guns. Suicide deaths due to guns decrease by about 4.5 percentage points in a state that passes MHPLs compared to states that do not pass MHPLs indicating suicide by guns decrease after passage of MHPLs. The other specifications are also robust to the baseline. The results in column (3) of table includes an interaction term of MHPLs with the ratio of workers working in large firms. The coefficients are larger than the baseline with no change in the significance level. Results in column (4) is also similar to results with the interaction term carrying the impact of MHPLs on suicide rates.

2.5.2 Event Study Analysis

In this section, I present the results of the two way dynamic difference in differences specification in an event study framework. The event study analysis helps us to see how our outcome evolves over time as a result of the treatment. Effects of gaining access to mental health care due to passage of MHPLs may not be seen immediately because mental health treatment usually takes time to show results. Moreover, the event study graphs helps us to visualize pre-trends in the outcome variables. We can assume that there are no pre-trends if the effects on suicide rate is not significantly different to zero.

I present two figures see the dynamic effect of mental MHPLs on suicides. The first figure 2.2 shows the dynamic effects of MHPLs on all suicide rates, the second figure 2.3 shows the dynamic effects of

[29] Source: CDC; www.cdc.gov/nchs/pressroom/sosmap/firearm_mortality/firearm.htm
[30] See graph in Appendix.

MHPLs on gun suicide rates. In all the event study plots, horizontal axis plots the time relative to the passage of MHPLs was passed showing 11 periods before and after the passage of MHPLS shown along the axis. While there are several periods both post MHPLs, I combine effects including 11 years and beyond into a single parameter and label it 11+ years after MHPLs. I do the same for time periods more than 11 years before MHPLs. The vertical axis shows the dynamic treatment effects. The reference point of the event study graph is at t=-1, which is the omitted time period in the event study equation and is used for comparison.

The first figure shows the path of the estimated coefficients for all suicides. In the pre-period, it is clear that the difference of the outcomes in states that passed mental health parity laws and states did not pass MHPLs are not significantly different from zero. In the absence of treatment, the outcomes in treatment group and the control groups are same suggesting that there are no pre-trends.

In terms of the dynamic effects, there is significant effects for the first two years after MHPLs is passed. After those two years there is no significant effects of MHPLs on suicide rates. While we see a drop in suicides rates right after MHPLs are passed, the effects are not significant for most time periods. There is no significant effects over time several years post MHPLs with the confidence intervals comfortably including the zero line. The effects though not significant are negative which is consistent with the difference in differences results.

A subgroup of suicide deaths of interest is suicide due to guns. The event study graph in figure 2.3 shows the path of the estimated coefficient of the dynamic diff-in-diff specification on log of gun suicides. In the pre-period, it is clear that the difference in outcome between the treated and the control group is not significantly different from zero. In the absence of treatment, the outcomes of the treated and the control groups are same, suggesting no pre-trends.

In terms of the dynamic effects, there is clear negative impact of MHPLs on suicide due to guns. The event study plot shows that MHPLs are indeed effective in reducing gun related suicides. A significant reduction in gun-suicide rates can be seen right after MHPLs are passed. This reduction continues for several years. The result is quite different than the impact of MHPLs on overall suicide rates. It is consistent with the results of the difference in difference analysis.

It is surprising that the suicide due to guns results do not mimic the results of over all suicides. One reason could be that there are many states where suicide due to guns are a major part of overall suicides. Another reason could be because of the demographics. It is possible that the demographics who benefit from passage of MHPLs are those who also tend to own guns. Passage of MHPLs might have reduced stigma associated with treatment of mental health among the demographics that own guns. People may have been more wiling to take help and hence reduce suicide death rates. Suicide due to guns are a large share of overall suicides. Over 50% of suicide deaths are due to firearms in 2017. 56% of suicides committed by males are due to firearms. [31] The significant effects of MHPLs is concentrated on suicide due to guns.

Results from the event study provide insights on the validity of parallel trends assumption. There is no significant impact of MHPLs on overall suicide rates many years after MHPLs are passed but significant impacts of MHPLs on suicides due to guns. However, when assumptions of the two way-fixed effects difference in differences are violated due to heterogeneous treatment effects, the results cannot be interpreted as causal. In what follows, I examine variation in effects due treatment timing and estimate the effects by relaxing the homogeneous treatment effects assumption.

2.5.3 Variation in Treatment Timing and Heterogeneous Treatment Effects

There are certain pitfalls of the two way fixed effects difference in differences that arise from treatment timing. As a first step, I check for heterogeneity in treatment effects due to timing of treatment. After checking for heterogeneity, I estimate the group-time average treatment effects of Callaway and Sant'Anna (2019) to address heterogeneity arising from treatment timing.

The full sample of states that passed MHPLs can be grouped into early and late adopting states which allow me to check for variation in treatment effects by timing groups. The estimated treatment effects in the two-way fixed effects may be biased because of heterogeneous effects due to treatment timing

[31] https://www.nimh.nih.gov/health/statistics/suicide.shtml

(Goodman-Bacon, 2018), even if the common trends assumptions hold. The separate regressions by timing groups suggest that there is indeed variation in treatment effects due to treatment timing. [32]

I do several difference in differences combinations defining early treated states as states that pass MHPLs before 1999, late as states that pass from 1999 onward. The reason for splitting the sample by 1999 is 1999 divides the whole sample of states into two approximately equal groups. In addition, one more potential reason for this timing split is the 1998 federal MHPA. States that passed MHPLs before 1999 could be different than states that passed MHPLs after 1998. The same is true for states passing after 1998. Strikingly, as seen in Table 2.6, regressions with the early/never combination has a positive coefficient. While not significantly different from zero, this exercise suggests that in the sample from 1990-2010, the early adopters, and the late adopters have opposite treatment effects. This points towards possible heterogeneity due to treatment timing. The only adopters column shows results of a regression where the sample includes only the states passing MHPLs. [33]

Moreover, Goodman-Bacon (2018)'s Bacon Decomposition plots also points to heterogeneity in treatment effects. There are a number of timing groups combinations that have a positive treatment effects as evident in the 2×2 bacon decomposition plots. The never treated vs timing groups combinations also exhibit many individual positive coefficients as evident by the 2×2 plot.[34]

The apparent heterogeneity in treatment effects due to treatment timing suggests that the two way fixed effects difference in differences model may give misleading results. Therefore, with results of the different two by two groups suggesting heterogeneous treatment effects due to treatment timing, I move forward in the analysis using recent methods that corrects heterogeneous treatment effects (Callaway and Sant'Anna, 2019).

[32] I also do the Bacon Decomposition to check the treatment effects. I present the plot in the appendix.

[33] In separate exercise, which I do not present here, I define early as states passing MHPLs before 2004 instead of 1998 and I see that the late/never combination has a positive sign in its coefficient.

[34] See appendix.

2.5.4 Addressing for Heterogeneity in Treatment Timing

To solve for possible heterogeneity, I take the county level data and use the recently proposed group-time average treatment effects proposed by Callaway and Sant'Anna (2019). I use county level data on suicide deaths.

In my data, I have states passing MHPLs from 1995-2008, with states that first receive treatment in each of the years: 1995, 1996, 1997, 1998, 1999, 2000, 2001, 2002, 2006, and 2007. As a result, there are 10 groups of counties, each of which first participate in the treatment (a group) in different year. Out of the balanced panel of 7686 counties, 3738 are never treated counties because they are in states that were never passed MHPLs. The remaining 3948 counties are distributed in these 10 groups.

The first step is to estimate the group time average treatment effects. Under the parallel trends assumption, group-time average treatment effects are identified when $g \leq t$ i.e for post treatment period t for each group(g). There are also group-time average treatment effects when $g > t$ i.e. for pre treatment periods for each group. This can be used to pre-test for parallel trends assumption. I plot these $ATT_(g,t)'s$ for each groups. I get a total of nine plots each for the timing groups. These plots are shown in Figure 2.4 and 2.5. The parallel trends assumptions seem to hold with no significant pre-trends in suicide rates before the passage of MHPLs.

Since there are a large number of plots for each timing groups. I aggregate the individual group time average treatment effects to get an aggregated group time treatment effects parameter. The aggregated group-time average treatment effects is the weighted average of all group-time average treatment effects with weights proportional to the group size.

To check for dynamic effects, alternatively, the group time average treatment effects can be aggregated into an event study plot. The aggregated plots are shown in Figures 2.6 and 2.7. Once we have this event study plot, we can check how the aggregated group-time fixed effects estimator evolves over time.

I use the default doubly robust estimators based on Sant'Anna and Zhao (2020). Doubly robust estimators are based on first step linear regression on the outcome variable and logit for the generalized propensity score. In the following section, I present results for the group-time average treatment effects.

Results for suicide all ages

The panel of plots in figure 2.4 depicts the group time average treatment for each group treated at different time period in our sample. There are a total of 10 groups of counties in states that get treated at different times. The orange points are the treatment effects pre treatment and the blue points are the treatment effects post treatment. For all groups, treatment effects before treatment show parallel trends. After treatment, for almost all groups, the treatment effects is not significantly different from zero.

There are few interesting thing in the graphs. For group 1996, there is significant and negative treatment effects two years post treatment. For group 1998, the treatment effects are positive post treatment and significant 3 years post treatment. This suggests that for counties in states that received treatment in 1998, the dynamic effects are positive. For all other groups the treatment effects are not significantly different from zero.

Results for 25-65 years

To check if the effects of MHPLs are different for the subgroup of people aged 25-65, I estimate effects for this group. I present the group time average treatment effects for the nine groups in figure 2.5. Suicide deaths from 25-65 has 9 groups instead of 10 in the previous figure. This is because, I lose data for some states due to data suppression as I trim the data down to the county level, and age group. However, the results are similar to the results for all suicides.

For all groups, the graphs do not show pre-trends. The effects are not significantly different from zero in the pre-period. Moreover, the treatment effects post treatment are not significantly different from zero for almost all the groups. Just like the results for all suicide deaths, the treatment effects post treatment is greater than zero several years after treatment for group 1998. There seems to be a significant negative effects three years post treatment for group 2002. Overall, the results point out that there has been no impact of MHPLs on any group except for a positive impact (increase in suicides) on group of counties that was treated in 1998.

Aggregated Group time Average Treatment Effects

In this section, I present the above group-time average treatment effects into a single overall effects of participating in treatment. The treatment effects shown in the two figures can be aggregated with weights proportional to group size to get a single parameter that can be interpreted as aggregated group time treatment effects.

This simple weighted combination of $ATT(g,t)$ rules out the issues due to negative weights. I present the aggregated group time average treatment effects in Table 2.7. The aggregated treatment effects do not show significant impact of MHPLs on suicide rates.

Dynamic Effects of MHPLs on Suicide Rates

One important question is whether improvements in mental health can be seen after certain years of treatment. The effects of mental health treatment may not have immediate effect and show after many years since treatment is administered. In this section, I present event-study type plots that show how treatment effects evolve over time. I aggregate group-time average treatment effects into an event study plot.

In Figure 2.6 the X-axis is the length of exposure to treatment and the Y-axis measure the average treatment effects. If the treatment effects is not different from zero post treatment, we can conclude no pre-trends. If the average treatment effects post treatment is significantly different from zero, then we can conclude that there is an impact of MHPLs on suicide deaths.

In the event study graphs in Figure 2.6 and 2.7 that account for heterogeneity, I do not see any dynamic impact of MHPLs on all suicides. Every time period post treatment, there is no impact of MHPLs on suicide rates. This is robust to the two way specification and different than the results in Lang (2013).

In Figure 2.7, I present the dynamic effects of MHPLs on suicide death rates for subgroup of 25-65 years. There is no impact of MHPLs on suicide rates of this demographic group as well.

2.6 Results: YRBS

2.6.1 Difference in Differences

Finally, I investigate the impact of MHPLs on youths, taking data from the YRBS. I take use data from 1999 to 2010 because before 1999, there are many missing states and most states have data available from 1999. My variables of interest are those related to suicide ideation, making a plan about suicide, and suicide attempt. This demographics is often ignored in studies related to MHPLs. My interest in studying the impact of MHPLs on youth outcomes is because if MHPLs increased access to mental health treatment services, then we should see improvements in mental health of youths.

I use the two-way fixed effects difference in differences design, and an event study model towards this end. Table 2.8 presents the estimated effects of MHPLs on youth mental health variables. Surprisingly, and inconsistent with the suicide deaths results for demographic groups aged 1-25, there is a positive but insignificant impact of MHPLs on suicide ideation. One explanation for the inconsistency is that suicide ideation does not result in suicide.

A measure that correlates more with suicides and that is comparable with suicide deaths examined earlier would be number of attempted suicides. The results are presented in the third column of 2.8. Consistent with the results for suicide deaths, there is no impact of MHPLs on number of attempted suicides. This means that youth mental health did not improve significantly even after passage of MHPLs.

2.6.2 Event Study

I present event-study specifications using the two way fixed effects specification in 2.8 through 2.10 for measures of youth mental health.

In figure 7, I present event study plots of the effects of MHPLs on whether youth in the sample seriously considered attempting suicide. The pre-treatment period indicates that there is no pre-trends. The dynamic effects are not significant. In figure 8, I present the impacts of MHPLs on whether youth in the YRBS made a plan to do suicide. While there are no pre-trends, the effects also seem to be zero.

Finally, figure 9 plots the dynamic effects of MHPLs on suicide attempts. The results show no significant effects of MHPLs on attempted suicides.

The results from the YRBS data is consistent with our main analysis showing that there is no improvement in youth mental health outcomes due to MHPLs.

2.6.3 Sub sample Analysis: Heterogeneity by Age Group

I return back to the suicide data, and do a subsample analysis to check for heterogeneity in the effects by age group. The results are presented in Table 2.9. To examine if effects would differ by age groups, I break the whole sample into deaths aged 1-25 and 25-65. It is possible that people in different age groups have a different impact of MHPLs. For example, people in age groups 25-65 are more likely to be employed and more likely to get employer sponsored insurance. Whereas, people above 65 are more likely to have public insurance. People below age 26 are likely to be under a parents health insurance. In addition, prevalence of mental health disease may be different in different age groups. In the table below, I present the difference in differences results by age group.

The results in Table 2.9 indicate that there is no significant effects of MHPLs on suicides of people in different age groups. Consistent with the baseline difference in differences, I do not see an impact of MHPLs on suicide rates of either age groups. Compared to the baseline difference in differences results, the coefficients for the age group 25-65 are larger. However, there is no significant effects of MHPLs on suicides across both groups.

2.7 Mechanisms

In order to shed light on the potential mechanisms, I investigate whether passage of MHPLs lead to a change in number of mental health and substance abuse treatment centers (SATCs). Using data from the county business patterns from 1998 onward, and using a difference in differences framework, I estimate the link between MHPLs and number of substance abuse treatment centers. There could be a supply side channel that is hindering treatment access leading to no improvement in mental health outcome despite MHPLs being passed. The Table 2.10 presents the results.

The results in Table 2.10 suggest that there is no supply side improvement due to MHPLs. The dependent variable is the log of the per-capita number of substance abuse treatment centers in a state in a given year. The number of mental health and substance abuse treatment facilities are not positively associated with the passage MHPLs. The number of SATCs do not increase due to mental health parity laws. Even though MHPLs are passed there are no SATCs opening up, thus leading null effects of the MHPLs. The first column of the table is the result of the result without controls. The second column shows the results with the controls.

2.8 Conclusion

Number of states passed mental health parity laws in 1990's and 2000's to increase access to mental health. This study leverages increase in access to mental health services due to state parity laws as a natural experiment to study the impact of state level health mandates on health outcomes. Taking a longer time period and a larger sample of states compared to previous research, this study contributes to understanding of the impact of state level MHPLs on mental health outcomes. This study contributes to the bigger picture of understanding the impact of access to general health care due to state level policies.

In contrast to the most recent research, MHPLs do not seem to decrease overall suicide rates in a state. This is consistent across subgroups. This is not true for gun suicides. MHPLs seem to decrease number of gun suicides In general, results do not show MHPLs improve mental health outcomes across various demographic groups. There is no dynamic impact of MHPLs on suicides.

2.9 Tables

Table 2.1: States and Mental Health Parity Laws

Year Law was Passed	States	No of States
1995	NH, RI	2
1996	ME	1
1997	AR,IN,MD,NC	4
1998	CO,SD,VT	3
1999	DE,HI,MN,NJ	4
2000	CA,CT,MT,NM,OK,VA	6
2001	AL, LA	2
2002	IL,MA,WV	3
2006	ID,SC	2
2007	OH,OR	2
2008	WY	1
Total		30

Table 2.2: Summary Statistics

	(1)		(2)		(3)		(4)	
	All		Pre		Post		Untreated	
	mean	sd	mean	sd	mean	sd	mean	sd
Mental Health Parity	0.31	0.46	0.00	0.00	1.00	0.00	0.00	0.00
Log of Suicide Crude Death Rate	2.52	0.26	2.52	0.27	2.47	0.28	2.55	0.23
Log of Gun Crude Death Rate	1.91	0.48	1.96	0.49	1.75	0.55	2.00	0.35
Fraction Black	0.09	0.09	0.10	0.10	0.08	0.09	0.09	0.09
Fraction Urban	0.72	0.15	0.69	0.15	0.72	0.18	0.73	0.12
Percent of Workers in Large Firms	0.44	0.11	0.44	0.07	0.42	0.14	0.46	0.11
SATC per 100000 in a state	17.13	8.16	15.01	4.47	19.35	9.52	15.09	6.36
Unemployment rate	5.45	1.77	5.60	1.45	5.27	1.96	5.49	1.83
Ratio of Males aged 45-65	28.83	3.59	26.49	2.60	31.65	2.56	28.36	3.46
Per-capita income (1000s)	38.21	6.51	35.39	5.94	41.91	6.84	37.41	5.24
Per capita real mental health expenditures	71.28	39.94	53.40	17.29	89.64	43.97	70.14	42.30
Purchase Delay	0.49	0.50	0.52	0.50	0.44	0.50	0.52	0.50
longwait	0.15	0.36	0.23	0.42	0.24	0.43	0.02	0.14
Log per-capita alcohol consumption	0.82	0.19	0.81	0.16	0.86	0.19	0.79	0.22
Percent Covered By Medicare	13.60	2.47	13.07	2.07	14.53	2.22	13.25	2.71
Percent Covered By Medicaid	11.08	3.60	10.21	2.97	11.66	3.82	11.25	3.73
Observations	1050		306		324		420	

Table 2.3: Summary Statistics Early, Late adopters.

	Early		Late	
	mean	sd	mean	sd
Mental Health Parity	0.00	0.00	0.00	0.00
Log of Suicide Crude Death Rate	2.54	0.19	2.52	0.29
Log of Gun Crude Death Rate	2.03	0.33	1.95	0.53
Fraction Black	0.08	0.09	0.10	0.10
Fraction Urban	0.60	0.17	0.72	0.13
Ratio of Workforce Working in Large Firms	0.43	0.06	0.44	0.07
SATC per 100000 in a state	.	.	15.01	4.47
Unemployment rate	5.39	1.39	5.66	1.46
Ratio of Males aged 45-65	25.14	1.34	26.88	2.74
Per-capita income (1000s)	33.58	3.86	35.91	6.32
Per capita real mental health expenditures	48.31	11.60	54.86	18.36
purchasedelay	0.59	0.50	0.50	0.50
longwait	0.26	0.44	0.22	0.41
Log per-capita alcohol consumption	0.83	0.21	0.81	0.14
Percent Covered By Medicare	12.91	2.19	13.12	2.03
Percent Covered By Medicaid	9.63	2.75	10.38	3.02
Observations	68		238	

This table compares early adopters and late adopters prior to MHPLs passing at all. Early is defined before 1999.

Table 2.4: Difference in Differences Estimates of Effects of MHPLs on Log of Suicide Rates

	(1)	(2)	(3)	(4)
MHPLs=1	-0.0185	-0.0198	-0.0170	0.00313
	(0.0185)	(0.0189)	(0.0192)	(0.0219)
Emp. Ratio	0.252*	0.300**	0.295**	0.299*
	(0.149)	(0.149)	(0.144)	(0.150)
Mandated Offering=1		-0.0127	-0.0125	-0.0134
		(0.0178)	(0.0179)	(0.0179)
MHPLs=1 × Emp. Ratio			-0.00638	
			(0.0305)	
MHPLs=1 × MHPA'98=1				-0.0255
				(0.0249)
MHPLs=1 × MHPAEA'08=1				0.00548
				(0.0133)
Constant	1.809***	1.798***	1.800***	1.791***
	(0.236)	(0.233)	(0.232)	(0.234)
nobs	1050	1050	1050	1050
R-sq	0.950	0.950	0.950	0.950
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses

SE clustered at the state level. Regressions weighted by population.

Emp. Ratio is the ratio of total workforce working in large firms

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Difference in Differences Estimates of Effects of MHPLs on Log of Gun Suicide Rates

	(1)	(2)	(3)	(4)
MHPLs=1	-0.0447**	-0.0448**	-0.0538**	0.00783
	(0.0218)	(0.0215)	(0.0258)	(0.0196)
Emp. Ratio	0.0919	0.0979	0.114	0.0775
	(0.172)	(0.207)	(0.216)	(0.211)
Mandated Offering=1		-0.00160	-0.00217	-0.00312
		(0.0213)	(0.0212)	(0.0217)
MHPLs=1 × Emp. Ratio			0.0201	
			(0.0409)	
MHPLs=1 × MHPA'98=1				-0.0502**
				(0.0239)
MHPLs=1 × MHPAEA'08=1				-0.0197
				(0.0192)
Constant	1.892***	1.891***	1.884***	1.871***
	(0.267)	(0.267)	(0.267)	(0.273)
nobs	1050	1050	1050	1050
R-sq	0.977	0.977	0.977	0.977
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses

SE clustered at the state level. Regressions weighted by population.

Emp. Ratio is the ratio of total workforce working in large firms

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Early, Late, Never, Always Combinations 1990-2010(Early defined at 1998)

	Early/Never	Late/Never	Only adopters	All Obs
Mental Health Parity=1	0.0168	-0.0269	-0.0284	-0.0185
	(0.0221)	(0.0213)	(0.0209)	(0.0185)
nobs	630	840	630	1050
R-sq	0.956	0.955	0.938	0.950
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Standard errors in parentheses

SE clustered at the state. Regressions weighted by population.

Early defined as states passing prior 1998, late after 1998.

Untreated are those that never had MHPLs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Aggregated Group Time Average Treatment Effects

	Overall Suicide	Suicides (25-65)
Aggregated Group Time ATE	-0.0150	-0.0142
	(0.0149)	(0.0186)

Table 2.8: Difference in Differences Estimates Youth Mental Health (YRBS) on Mental Health Parity Laws

	Seriously Cons. Attempt Sui.	Made Plan Abt Sui.	Attempted Sui.
Mental Health Parity=1	0.0156**	0.00641*	0.00774
	(0.00709)	(0.00364)	(0.00543)
nobs	375044	350305	328967
R-sq	0.00362	0.00311	0.00262
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Standard errors in parentheses

SE clustered at the state, Regressions weighted by population

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Subsample Analysis: Heterogeneity by Age

	Age(1-24)	Age(25-65)
Mental Health Parity=1	-0.0122	-0.0201
	(0.0205)	(0.0209)
nobs	1033	1050
R-sq	0.878	0.932
State FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses

SE clustered at the state level. Regressions weighted by population

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Exploring Mechanism: Log of Per-capita SATC on MHP

	(1)	(2)
Mental Health Parity=1	-0.0129	-0.0177
	(0.0374)	(0.0332)
nobs	559	559
R-sq	0.967	0.971
State FE	Yes	Yes
Year FE	Yes	Yes
Controls	No	Yes

Standard errors in parentheses

SE clustered at the state level. Regressions weighted by population.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.10 Figures

Figure 2.2: Event Study All Suicides

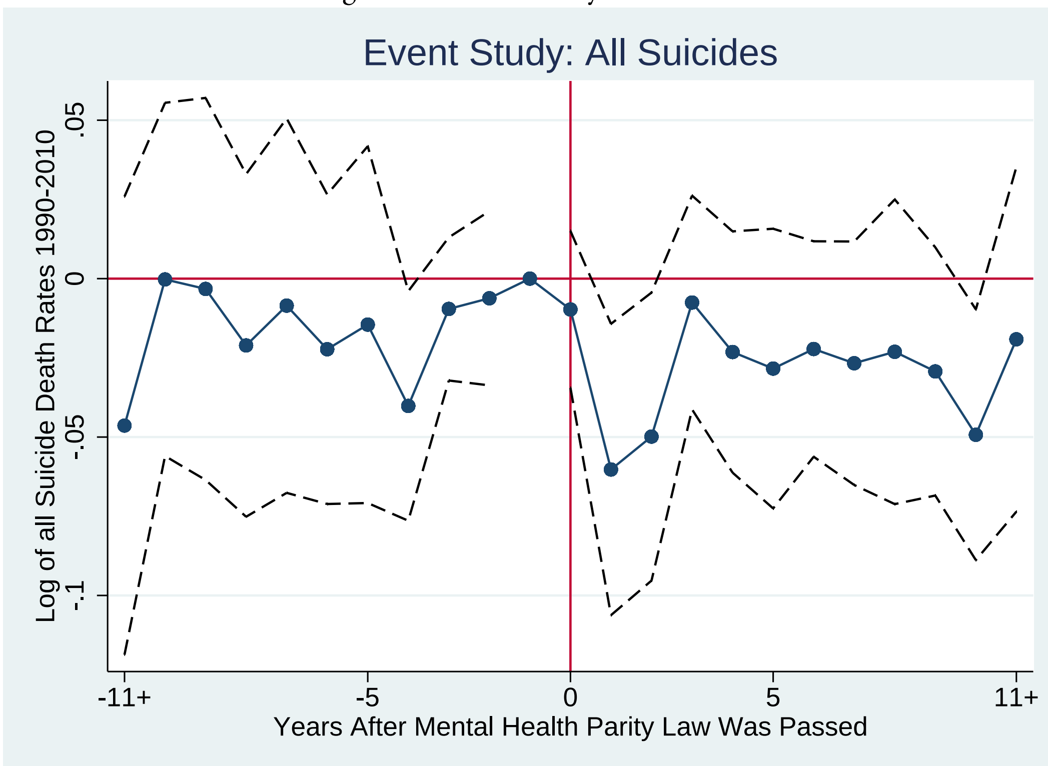


Figure 2.3: Event Study: Gun Suicides

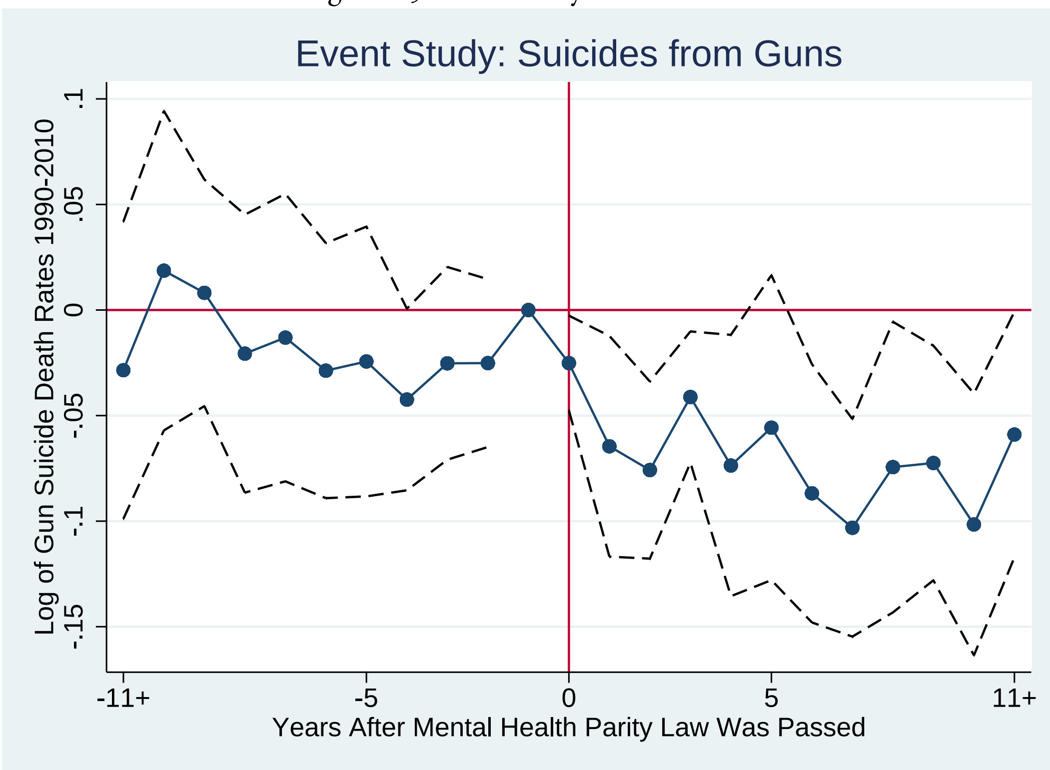


Figure 2.4: Group Time Average Treatment Effects for Overall Suicides

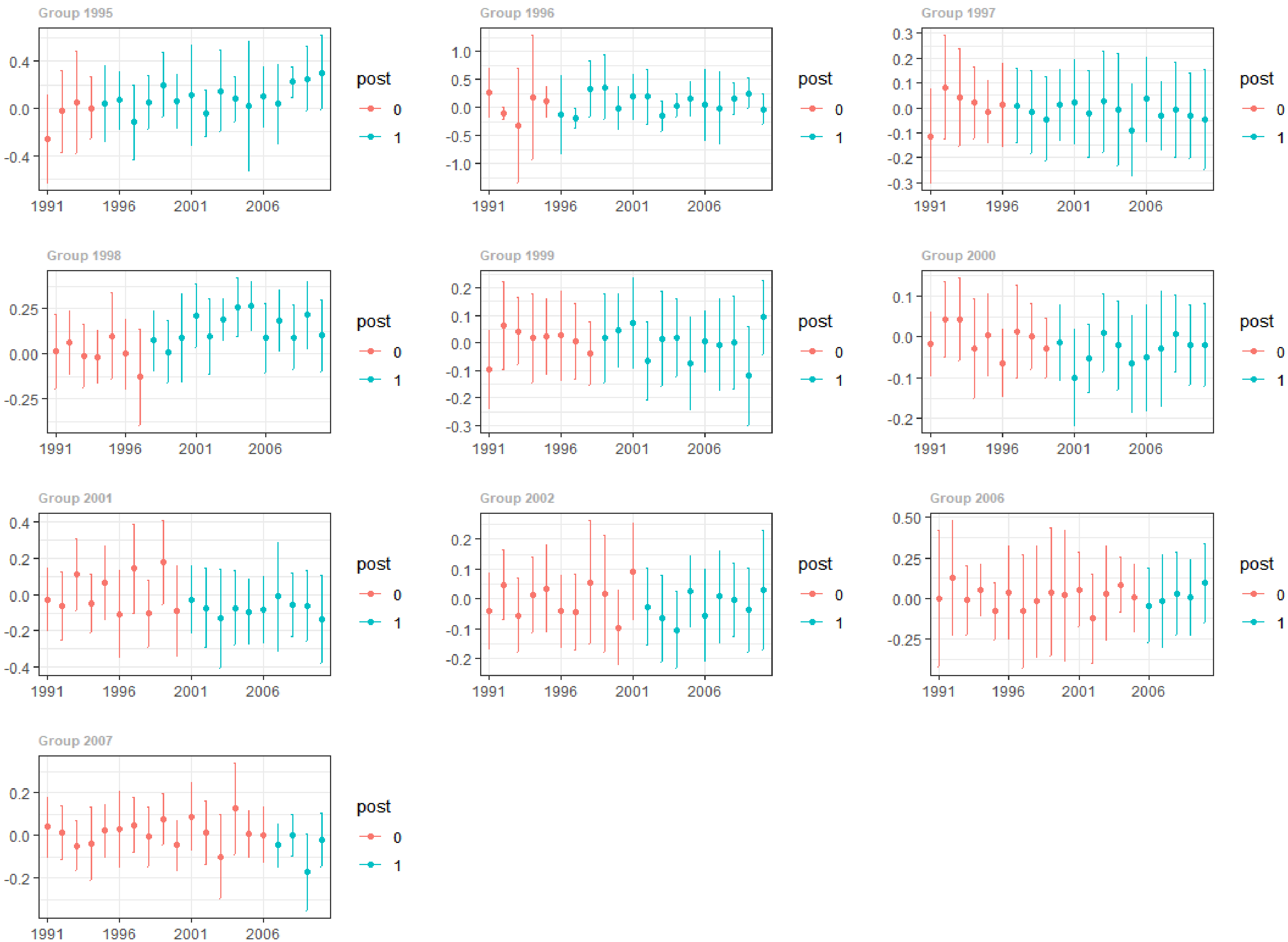


Figure 2.5: Group Time Average Treatment Effects for Suicides 25-65 Years

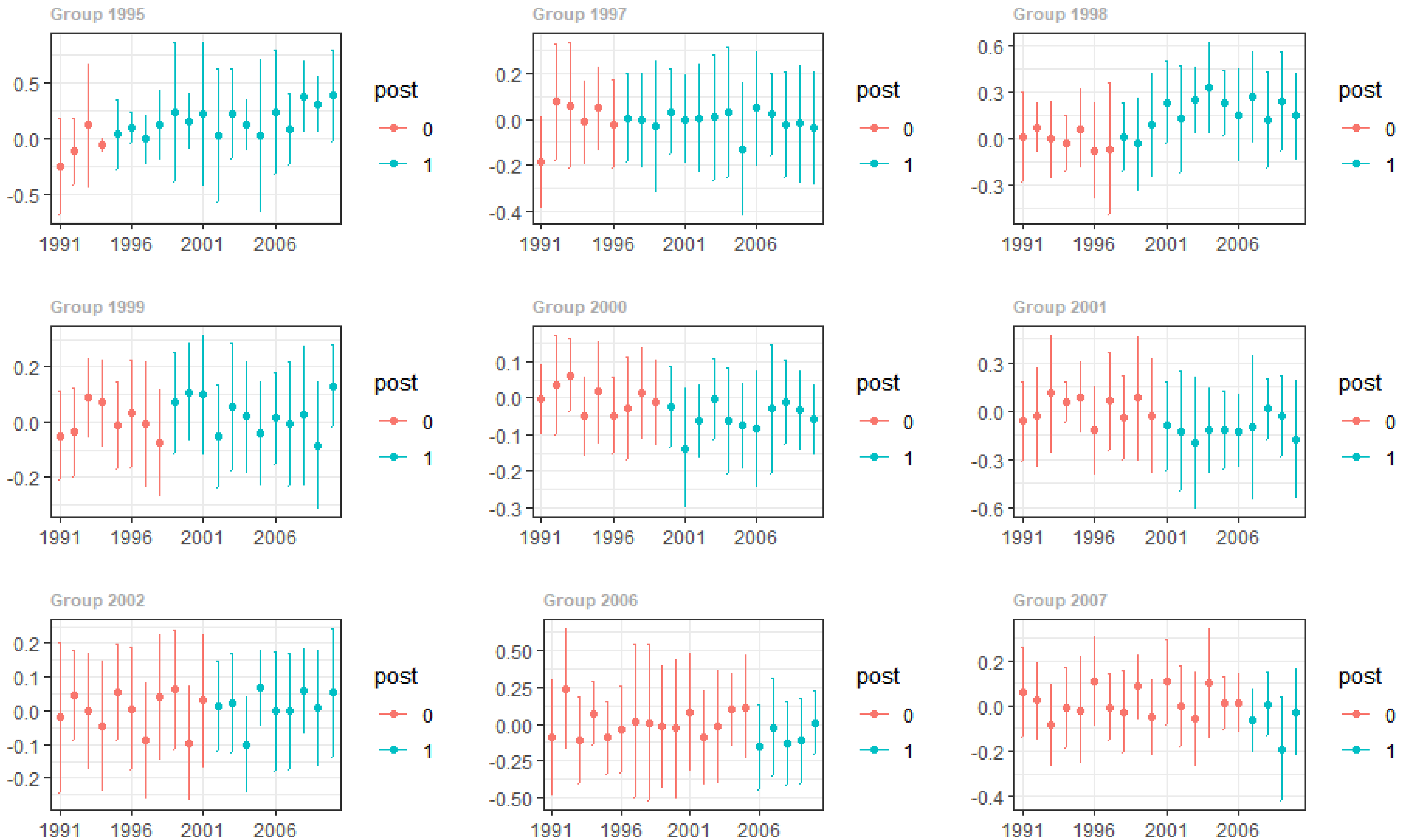


Figure 2.6: Dynamic Effects of MHPLs on All Suicides

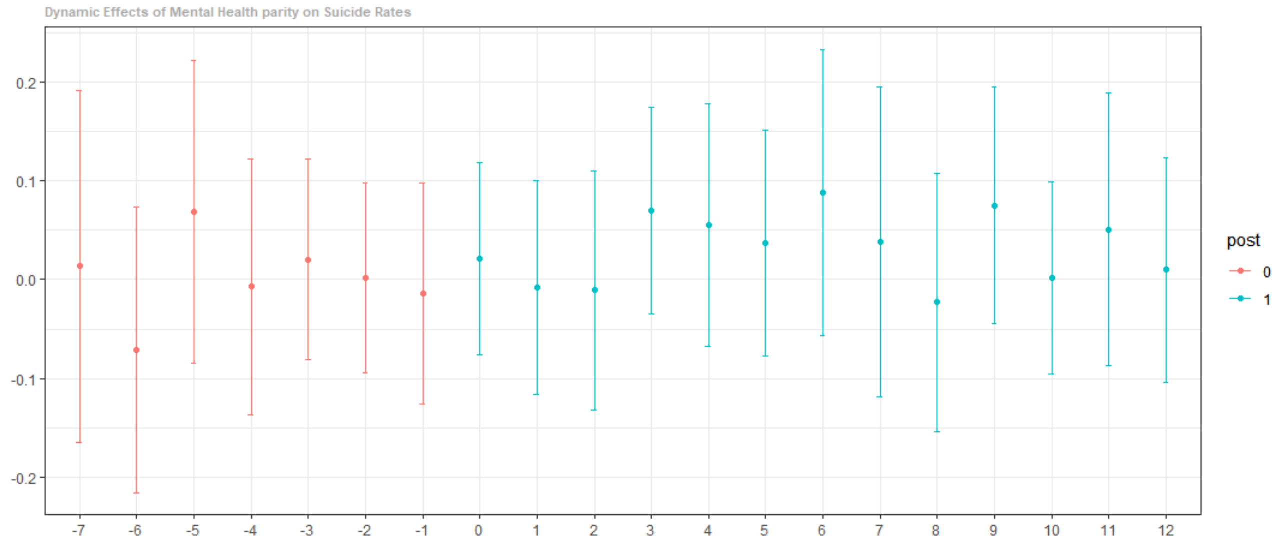


Figure 2.7: Dynamic Effects of MHPLs on Suicides 25-65 Years

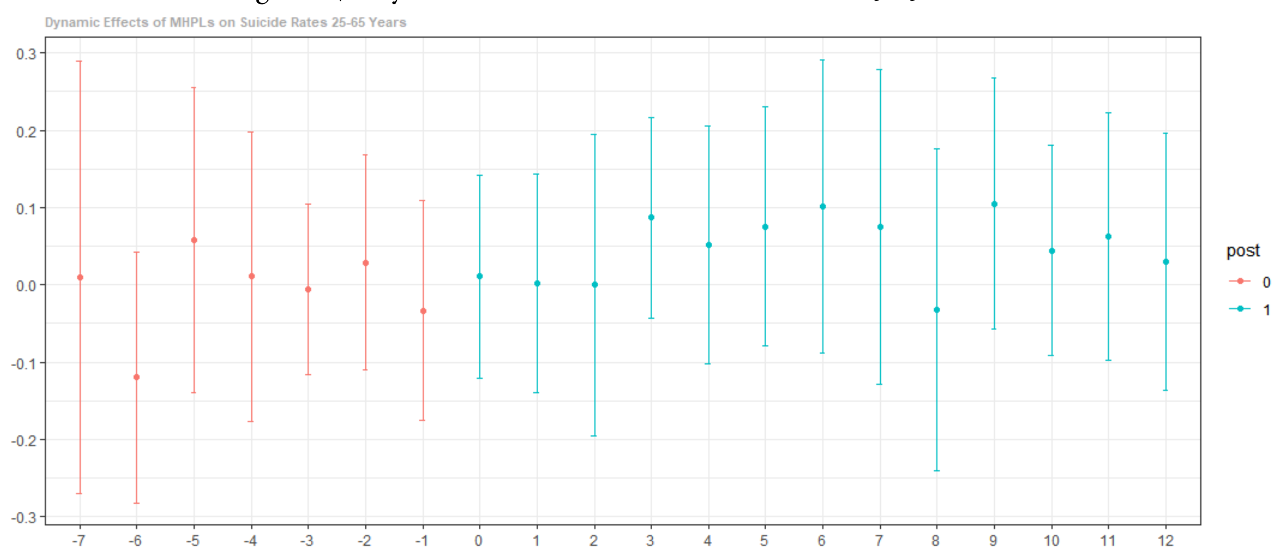


Figure 2.8: Dynamic Effects on Youth Suicide Ideation

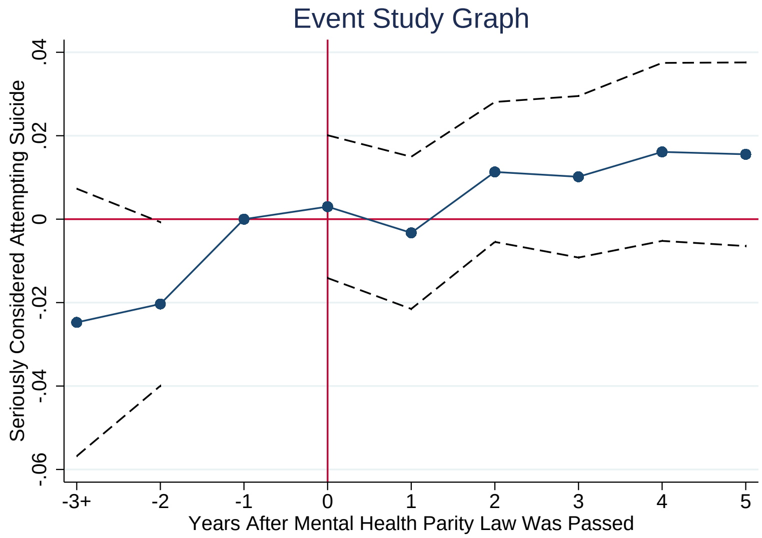


Figure 2.9: Dynamic Effects on Plan to do Suicide

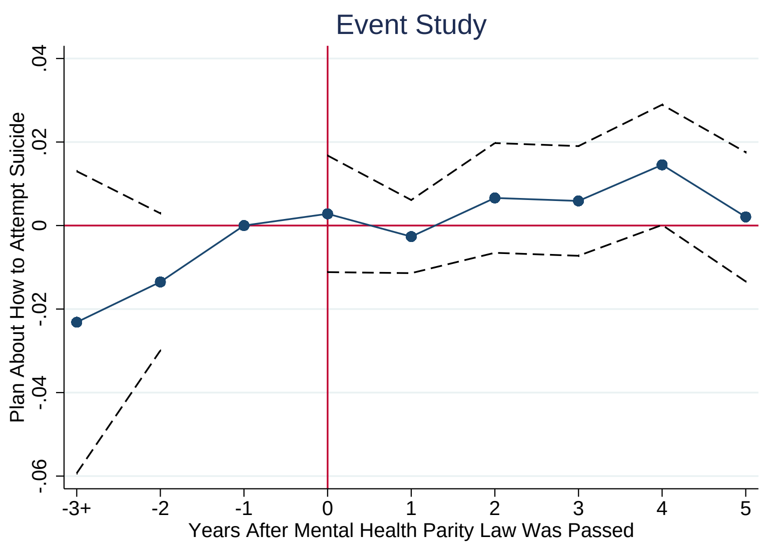
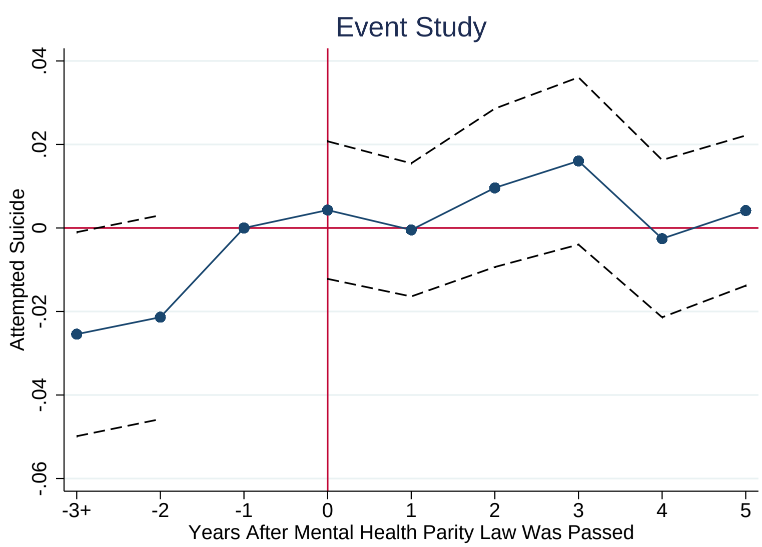


Figure 2.10: Dynamic Effects on Attempted Suicide



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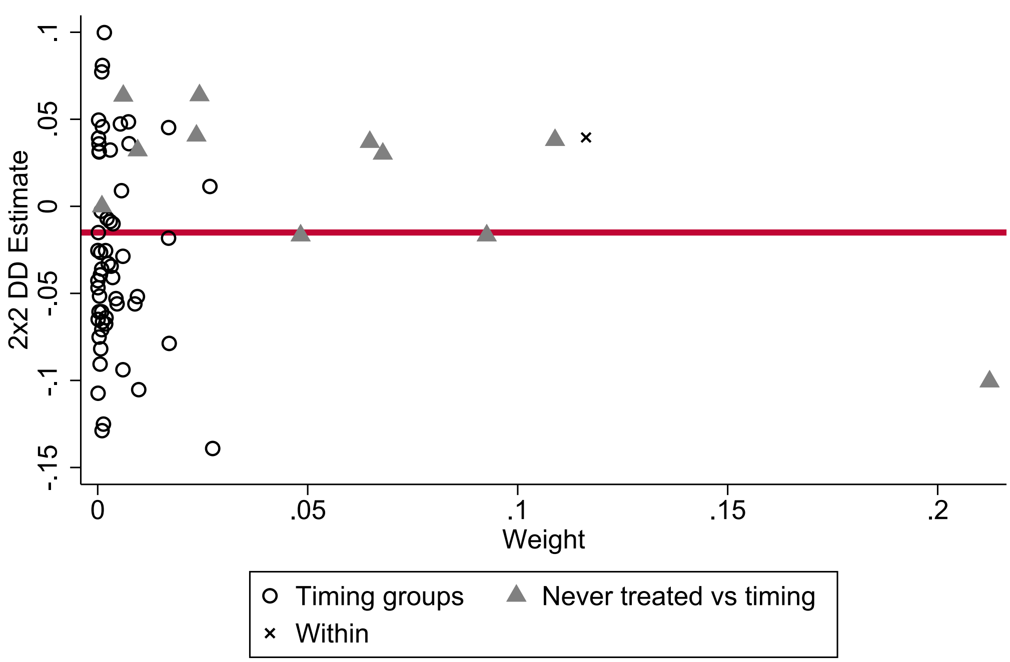
Appendix A

A.1 Goodman-Bacon Decomposition

Figure A.1: Goodman-Bacon decomposition



Overall DD Estimate = -.0150315
Within component = .03957309 (weight = .11627744)

A.2 Difference in Differences(1998-2008)

Table A.1: Difference in Differences Estimates of Log of Suicide Rates on Mental Health Parity Laws

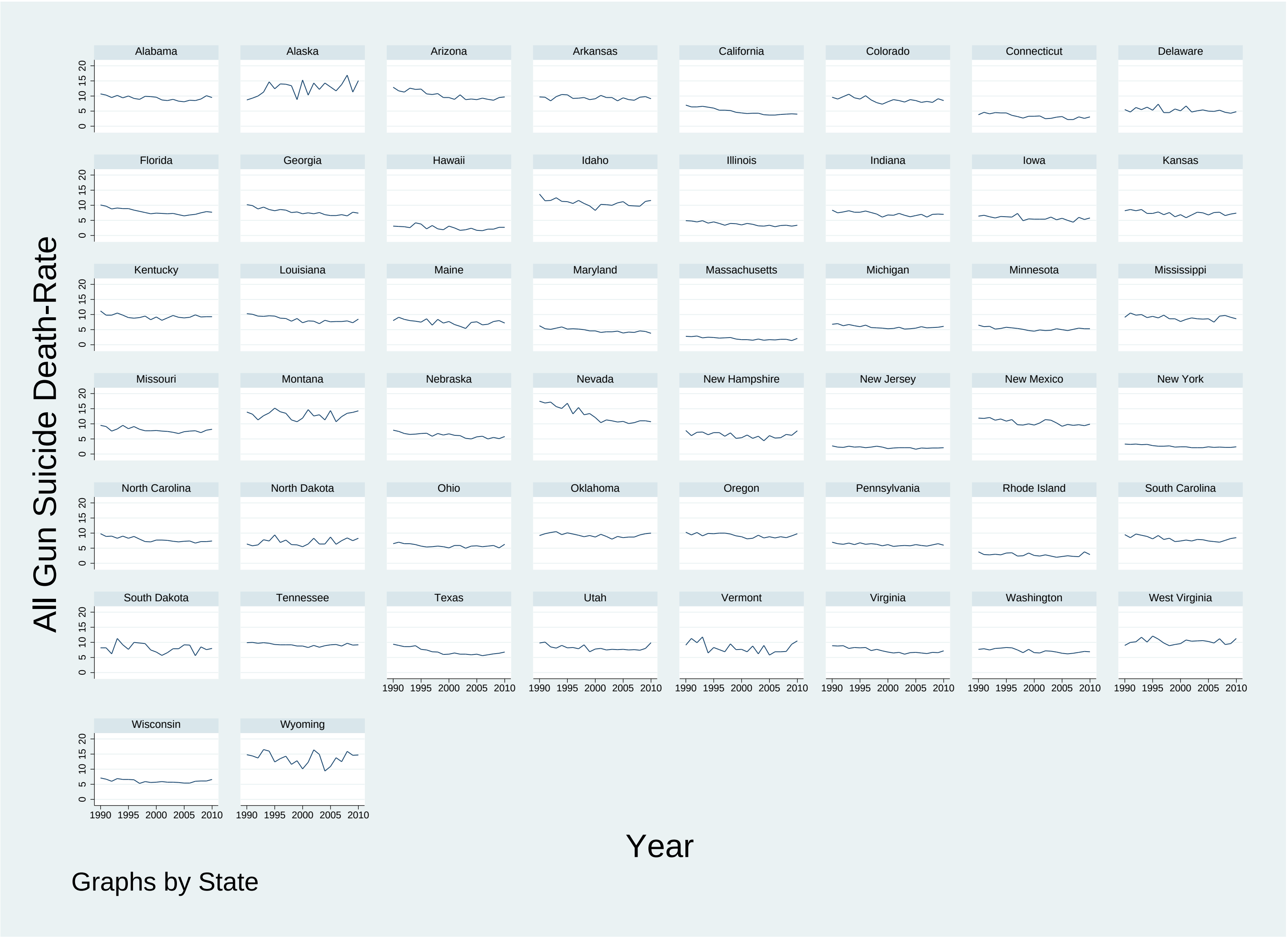
	All Sui	Gun Sui	Sui Not Guns
Mental Health Parity=1	-0.0269*	-0.0622***	-0.0135
	(0.0144)	(0.0189)	(0.0191)
nobs	550	550	550
R-sq	0.966	0.982	0.905
F-Stat	.	.	.
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Standard errors in parentheses

SE clustered at the state, Regressions weighted by population

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.2: Gun Suicide Deaths Over Years by State



Appendix B

B.1 Snapshot of Available Data

Table B.1: Snapshot of Available Data

Data Name	Variables	Years Available	Source	Geographic Identifiers
Mental Health Parity Laws	State and Years when MHP laws were Passed	1990-2010	NCSL, Previous Literature	State
Mental Health Outcomes	Suicide Mortality	1990-2010	CDC-Wonder MCOD	State, County
Youth MH Outcomes	Think about committing suicide Attempted suicide, etc	1999-2010	YRBS	State
Demographics	Percent of population that is black, lives in urban area	1990-2010	The Census Bureau, SEER, and (Edwards et al., 2018)	State
Macroeconomic Indicators	Per Capita Income, Unemployment Rate	1990-2010	BLS, BEA	State, County
Workforce in large firms	Percentage of workers that work in large firms[1]	1990-2010	The Census Bureau	State, County
Access to Public Health Insurance	Percent of population covered by medicaid, medicare per-capita state mental health expenditure	1990-2010	IPUMS-USA,(Edwards et al., 2018)	State
Other controls	State wise gun Laws, per capita alcohol consumption	1990-2010	Edwards et al. (2018), NIAA	State
Treatment Centers	Number of MH, and sub. abuse treatment centers	1999-2010	Census Bureau County Business Patterns	State, County

Notes: Suicide mortality data available for almost 350 counties from 1999-2010. The number of counties with data and balanced panel is less if I take time period from 1990-2010. YRBS is individual level data with state identifiers. For YRBS data is available every two years for almost all states beginning 1999. However, some states have missing data before 1999. For Workforce in large firms, I define large firms as firms with more than 500 employee.

B.2 Passage of MHPLs

Table B.2 below presents the states that passed mental health parity legislation during my study period. The states in this table passed full parity laws.

Table B.2: States and Mental Health Parity Laws

Year Law was Passed	States	No of States
1994	NH, RI	2
1996	ME	1
1997	AR,IN,MD,NC	4
1998	CO,SD,VT	3
1999	DE,HI,MN,NJ	4
2000	CA,CT,MT,NM,OK,VA	6
2001	AL, LA	2
2002	IL,MA,WV	3
2006	ID,SC	2
2007	OH,OR	2
2008	WY	1
Total		30

B.3 Robustness and Specification Checks

Table B.3: Heterogeniety by Level of Workers in Large Firms

	Violent		Property	
	Low	High	Low	High
MHPLs	-0.0799**	0.0269	-0.0469***	-0.0121
	(0.0315)	(0.0268)	(0.0170)	(0.0262)
N	19203	20183	20072	20524
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1) by the level of the ratio of workers in large firms. High refers to the sample that has ratio above the median level of workers in large firms, and low refers below median. The dependent variable is the inverse hyperbolic sine of the total crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.4: Estimates of the Effect of MHPLs on Log of State Level Violent Crimes

	1	2	3	4	5
MHPLs x Post	-0.102***	-0.0995***	-0.0824***	-0.0784***	-0.0557**
	(0.0344)	(0.0329)	(0.0285)	(0.0291)	(0.0223)
N	764	764	764	764	764
Workers in Large Firms		Yes	Yes	Yes	Yes
Population	Yes		Yes	Yes	Yes
State Political			Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes
State Macro				Yes	Yes
State Demo					Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the log of state level violent crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.5: Estimates of the effect of MHPLs on Log of State Level Total Property Crimes

	1	2	3	4	5
MHPLs x Post	-0.0488**	-0.0484**	-0.0296	-0.0221	-0.0179
	(0.0217)	(0.0216)	(0.0203)	(0.0191)	(0.0136)
N	764	764	764	764	764
Workers in Large Firms		Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes
State Macro				Yes	Yes
State Demo					Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the log of state level property crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.6: Estimates of the effect of MHPLs on Log of State Level Crimes

	Agg. Assault	Homicide	Rape	Robbery
MHPLs x Post	-0.0743**	0.0486	-0.0386	-0.00848
	(0.0287)	(0.0394)	(0.0239)	(0.0259)
N	764	764	764	764
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the log of state level crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.7: Estimates of the Effect of MHPLs on Log of State Level Crimes

	Burglary	Larceny	MV Theft
MHPLs x Post	-0.0236	-0.0177	-0.0151
	(0.0181)	(0.0155)	(0.0364)
N	764	764	764
Workers in Large Firms	Yes	Yes	Yes
Population	Yes	Yes	Yes
State Political	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes
State Macro	Yes	Yes	Yes
State Demo	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference-in-differences estimates using preferred specification of equation (1). The dependent variable is the log of state level property crimes. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.8: DID of Log of Violent Crimes Plus 1

	1	2	3	4	5	6	7
MHPLs	-0.0752***	-0.0596**	-0.0601***	-0.0580***	-0.0633***	-0.0673***	-0.0688***
	(0.0249)	(0.0241)	(0.0214)	(0.0210)	(0.0209)	(0.0195)	(0.0198)
N	38184	38184	38184	36698	36698	36698	38184
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1). The dependent variable is the log(total violent crimes+1). Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.9: DID of Log of Property Crimes Plus 1

	1	2	3	4	5	6	7
MHPLs	-0.0448**	-0.0280	-0.0160	-0.0107	-0.0100	-0.0137	-0.0149
	(0.0213)	(0.0170)	(0.0138)	(0.0145)	(0.0143)	(0.0139)	(0.0139)
N	38184	38184	38184	36698	36698	36698	38184
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1). The dependent variable is the log(total property crimes+1). Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid Coverage, and Medicare Coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.10: DID of Log of Different Violent Crimes Plus 1

	Agg. Assault	Murder	Rape	Robbery
MHPLs	-0.0758***	0.00726	-0.0888**	-0.0248
	(0.0246)	(0.0194)	(0.0422)	(0.0170)
N	38184	38184	38184	38184
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1). The dependent variable is the log(total crimes+1). Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.11: DID of Log of Different Property Crimes Plus 1

	Burglary	Larceny	MV Theft	Arson
MHPLs	-0.0125	-0.0160	-0.0245	0.120*
	(0.0180)	(0.0148)	(0.0209)	(0.0645)
N	38184	38184	38184	38184
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1). The dependent variable is the log(property crimes+1). Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. County demographics include four race categories. State public health insurance includes Medicaid Coverage, and Medicare Coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.12: DID of Inverse Hyperbolic Sine Violent Crimes(Mandated Offering)

	1	2	3	4	5	6	7
Mandated Offering	-0.0463	-0.0309	-0.0658	-0.0544	-0.0579	0.0175	0.00884
	(0.0394)	(0.0260)	(0.0388)	(0.0375)	(0.0463)	(0.0412)	(0.0427)
N	22218	22218	22218	21474	21474	21474	22218
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1) on Mandated Offering Laws. The dependent variable is the inverse hyperbolic sine of violent crime. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.13: DID of Inverse Hyperbolic Sine Property Crimes(Mandated Offering)

	1	2	3	4	5	6	7
Mandated Offering	-0.0333	-0.0179	-0.0187	-0.0399	-0.0339	-0.0178	-0.0234
	(0.0275)	(0.0140)	(0.0191)	(0.0294)	(0.0310)	(0.0260)	(0.0262)
N	23066	23066	23066	21965	21965	21965	23066
Workers in Large Firms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population		Yes	Yes	Yes	Yes	Yes	Yes
State Political			Yes	Yes	Yes	Yes	Yes
State Pub. HlthIns.			Yes	Yes	Yes	Yes	Yes
County Demo				Yes	Yes	Yes	
County Macro				Yes	Yes	Yes	
State Macro					Yes	Yes	Yes
State Demo						Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1) on the inverse hyperbolic sine on Mandated Offering Laws. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.14: DID of Inverse Hyperbolic Sine Violent Crimes(Mandated Offering)

	Agg. Assault	Murder	Rape	Robbery
Mandated Offering	-0.0117	0.0219	0.0193	0.0527**
	(0.0553)	(0.0375)	(0.0316)	(0.0239)
N	23066	23066	23066	23066
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1) of the inverse hyperbolic sine of different violent crimes on Mandated Offering Laws. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

Table B.15: DID of Inverse Hyperbolic Sine Different Property Crimes(Mandated Offering)

	Burglary	Larceny	MV Theft	Arson
Mandated Offering	-0.0296	0.00313	-0.0708	-0.732***
	(0.0278)	(0.0232)	(0.0475)	(0.201)
N	23066	23066	23066	23066
Workers in Large Firms	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes
State Political	Yes	Yes	Yes	Yes
State Pub. HlthIns.	Yes	Yes	Yes	Yes
State Macro	Yes	Yes	Yes	Yes
State Demo	Yes	Yes	Yes	Yes

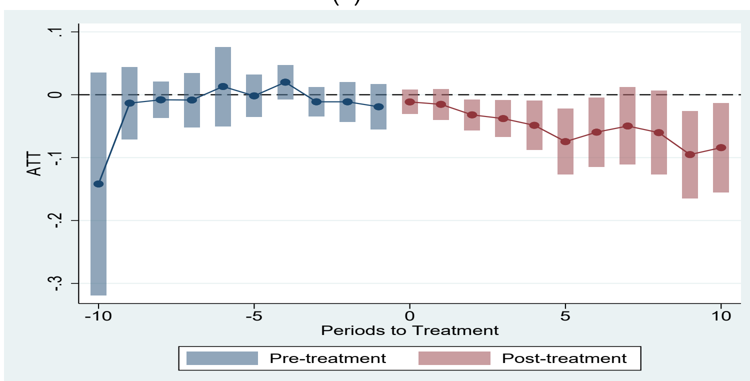
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table presents difference in differences estimates using equation (1) of the inverse hyperbolic sine of different property crimes on Mandated Offering Laws. Each regression includes county fixed effects, year fixed effects, and census division by year fixed effects. State public health insurance includes Medicaid coverage, and Medicare coverage. Standard errors are clustered at the state level. Regressions are weighted by the county population from the UCR.

B.4 Event Study Accounting for Heterogeneity

In this section, I present the event study type estimates from Callaway and Sant'Anna (2020). I use the stata *csdid* command by Rios-Avila et al. (2021) to estimate the dynamic treatment effects.

Figure B.1: Event Study Type Estimates from Callaway and Sant'Anna (2020)

(a) Violent Crimes



(b)Property Crimes

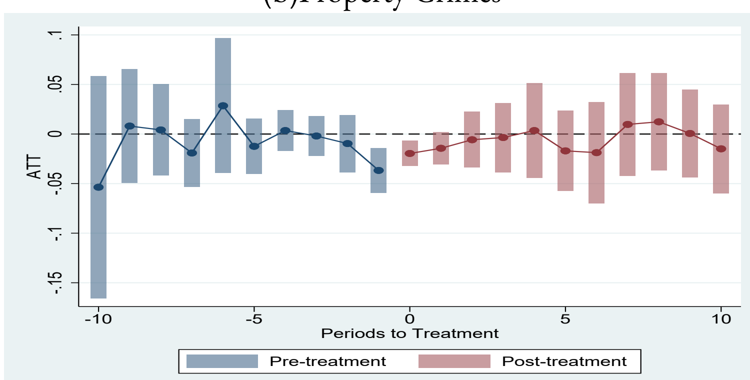
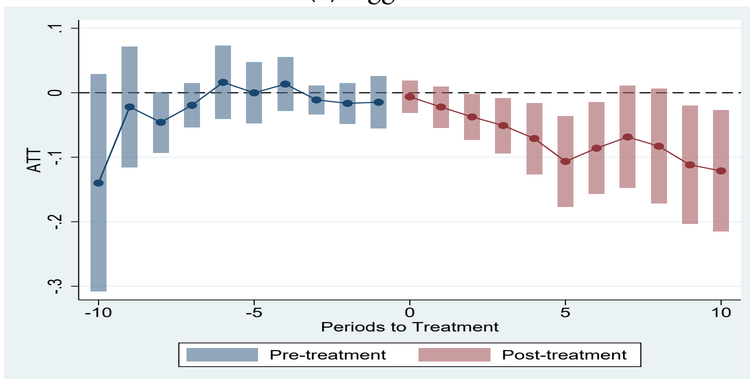
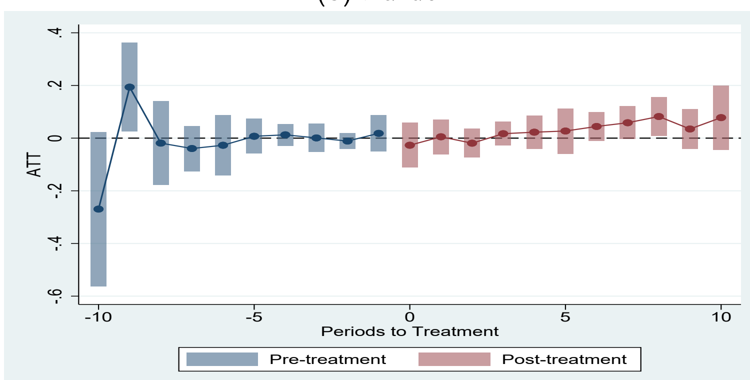


Figure B.2: Event Study Type Estimates from Callaway and Sant'Anna (2020)

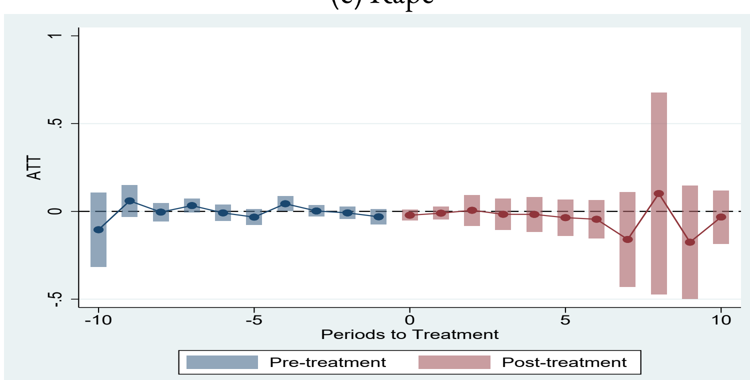
(a) Aggravated Assault



(b)Murder



(c) Rape



(d)Robbery

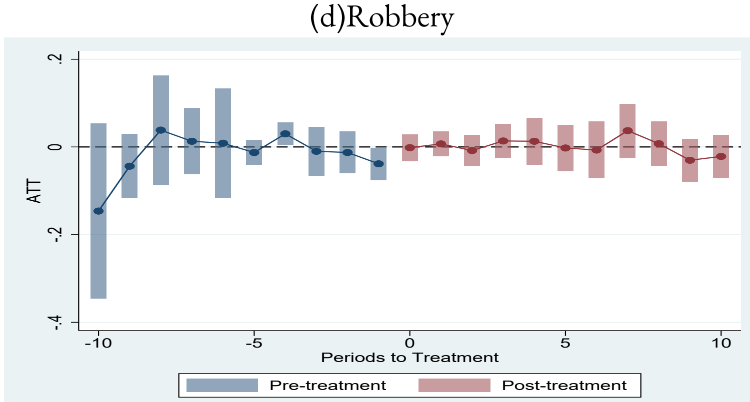
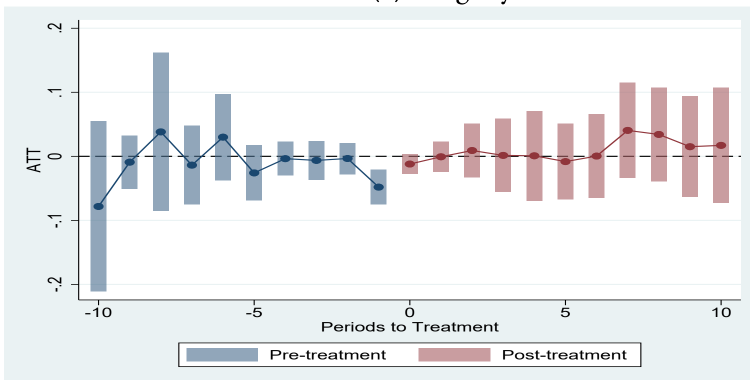
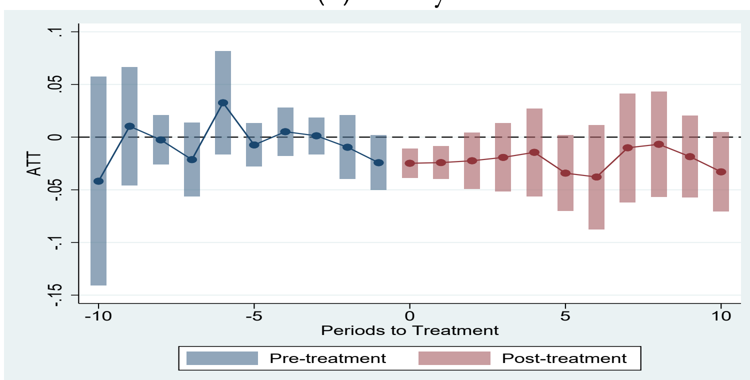


Figure B.3: Event Study Type Estimates from Callaway and Sant'Anna (2020)

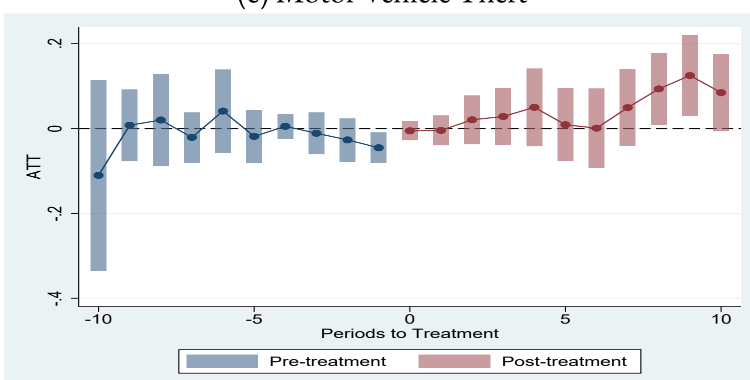
(a) Burglary



(b)Larceny



(c) Motor Vehicle Theft



(d)Arson