Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility*

Elisa Jácome†

Stanford University

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This paper explores whether access to mental healthcare can reduce criminal activity. Specifically, I study the effect of losing insurance coverage on low-income men's likelihood of incarceration using administrative data from South Carolina that has been linked across six state government agencies. Leveraging a discrete break in Medicaid coverage at age 19 and a difference-in-differences strategy, I find that men who lose access to Medicaid eligibility are 15% more likely to be incarcerated in the subsequent two years relative to a matched comparison group. The effects are concentrated among men with mental health histories, suggesting that losing access to mental healthcare plays an important role in explaining the observed rise in crime. By their 21st birthdays, men with mental health histories who lost Medicaid coverage are 22% more likely to have been incarcerated than the comparison group. Cost-benefit analyses show that expanding Medicaid eligibility to low-income young men is a cost-effective policy for reducing crime, especially relative to traditional approaches like increasing the severity of criminal sanctions. These findings have important implications for the design of criminal justice policies if low-income young men are more deterred from participating in illegal activity through the provision of healthcare than through harsher punishments.

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†Stanford Institute for Economic Policy Research, Stanford University. Email: ejacome@stanford.edu.

> In the 1950s and 1960s intellectual discussions of crime were dominated by the opinion that criminal behavior was caused by mental illness [...] I explored instead the theoretical and empirical implications of the assumption that criminal behavior is rational [...] Rationality implied that some individuals become criminals because of the financial rewards from crime compared to legal work, taking account of the likelihood of apprehension and conviction, and the severity of punishment.
>
> Gary Becker, 1992

1 Introduction

For the past fifty years, policymakers and academics in the U.S. have debated the root determinants of criminal behavior. In the 1960s, one dominant strain of thought argued that criminal behavior stemmed from mental illness, which prompted contemporary psychiatrists to advocate for replacing the "unscientific" criminal justice system with a more therapeutic approach (Menninger 1968). An alternative perspective, put forth by economist Gary Becker, posited that offenders are rational and can thus be deterred from committing crime by either improving the alternatives to criminal activity or raising the expected costs of crime (Becker 1968).

In the decades that followed Becker's pioneering work, crime rates rose in many cities across the country and policymakers embraced the punitive implications of Becker's model (Donohue 2007). The severity of punishments increased at both the state and federal level, contributing to rapid growth in the incarcerated population (Raphael & Stoll 2013b). Today, incarceration is a common occurrence for low-income and minority men.[1] Recent research has documented the adverse effects of incarceration—including increased barriers to employment and greater reliance on public assistance—in addition to the onerous fiscal costs (see e.g., Dobbie et al. 2018, Kearney et al. 2014, Mueller-Smith 2015). These high economic and social costs have called into question the cost effectiveness of the modern criminal justice system and have forced policymakers to consider alternative policies for deterring criminal behavior.

Contemporary debates about the the determinants of criminal activity have overlooked the disproportionate representation of mentally ill individuals in the criminal justice population. On any given day, over one million people with mental illness are in jail, prison, probation, or parole (Frank & McGuire 2010). Figure 1 plots the cumulative likelihood of incarceration for low-income

[1] Black men are more likely to have prison records than bachelor's degrees, and Black high school dropouts are more likely to be imprisoned than employed (Pettit & Western 2004, Western & Pettit 2010).

men with and without prior mental health diagnoses using the primary data source in this paper. Low-income men *with* a mental health history were almost 3 times more likely to have been incarcerated by age 24 than men *without* a mental health history. The prevalence of mental illness among incarcerated individuals raises the natural question: is increasing access to health services a cost-effective way to reduce criminal activity?

This paper revisits the role of mental healthcare in helping reduce criminal behavior. One motivation for returning to the decades-old conversation surrounding mental illness and crime is the significant scientific progress that has been made in the past fifty years in understanding and treating mental illness (Frank & Glied 2006, Kendler 2019). Moreover, the lack of data linkages between state agencies in the U.S. has been a perennial obstacle to assessing the efficacy of mental healthcare in deterring crime. However, this paper is able to employ rich administrative data from South Carolina that links individual-level records across various government agencies, thereby allowing me to identify men with mental health histories and measure any contact they have with the criminal justice system.

To estimate causal effects, I leverage a discrete break in public health insurance eligibility and study the effect of losing coverage on low-income men's likelihood of incarceration. South Carolina, like many other states in the U.S. South, provides free health coverage to low-income children via the Medicaid program, but it does not provide most childless adults with access to public health insurance. Individuals who are enrolled in Medicaid and who are utilizing its services throughout adolescence therefore lose coverage on their 19th birthdays.

Specifically, I employ a matched difference-in-differences approach, in which I study the evolution of outcomes for men who were impacted by the termination in eligibility. To control for age trends in crime, these "treated" men are compared to otherwise similar low-income men who were likely eligible, but not enrolled in Medicaid prior to their 19th birthdays, and who were therefore less affected by the loss in eligibility. The assumption underlying this approach is that, in the absence of the Medicaid loss, treated men would have trended similarly to the comparison group in their propensity to commit crime. I provide support for this assumption by showing that the two groups were trending similarly prior to age 19, and only began to diverge when treated men lost access to Medicaid.

I find that treated men who lose access to Medicaid coverage are 15 percent more likely to be incarcerated in the following two years. These baseline results suggest a strong, positive

relationship between Medicaid disenrollment and criminal activity among low-income young men. Importantly, the rich nature of the data allows me to split the sample by men's mental health histories, and I find that the effects are entirely driven by men with mental illness. By their 21st birthdays, treated men with mental health histories are 22 percent more likely to have ever been incarcerated relative to men in the matched comparison group. I find increases in violent, drug, and property crimes, suggesting that losing access to health coverage impacts all types of criminal involvement. Finally, I find that the effects are particularly pronounced for men who were using behavioral health services right before their 19th birthdays and for men who relied on Medicaid for access to mental health medications. These findings reaffirm the notion that losing access to mental health services plays an important role in explaining the observed rise in criminal activity.

In the last part of the paper, I use the estimates quantifying the effect of Medicaid eligibility on criminal activity to conduct a series of cost-benefit analyses. First, I show that the benefits of providing low-income young men with Medicaid eligibility—in terms of reduced fiscal and social costs—outweigh the program costs. Next, I compare the cost effectiveness of providing Medicaid eligibility to that of longer punishments, which has been a favored crime-reduction policy for the past fifty years. To make this comparison, I first replicate the approach of prior studies and show that low-income adolescents in my sample are relatively undeterred from engaging in criminal behavior when faced with harsher criminal sanctions (i.e., upon reaching the age of criminal majority) (Hjalmarsson 2009, Lee & McCrary 2017). Using these estimates of deterrence, I show that if the goal is to deter young adults from engaging in crime, then providing Medicaid eligibility is significantly more cost effective than increasing sentence lengths. These results suggest that policymakers should consider improving access to healthcare as an approach for reducing crime and lowering criminal justice expenditures.

The data and empirical approach used in this study are advantageous for several reasons. First, I use administrative data that links individual-level records across six state government agencies, so that I can follow the same individual across datasets and over time. Importantly, this dataset includes information on an individual's enrollment spells in the Medicaid program as well as detailed information on all Medicaid insurance claims, which allows me to identify individuals with diagnosed mental illness. The dataset also includes records from three state law-enforcement agencies, thereby allowing me to measure any contact that an individual has with the relatively fragmented criminal justice system (i.e., juvenile detentions as well as adult arrests and incarcerations). Furthermore, I leverage exogenous variation in Medicaid eligibility at

the individual level to study the dynamic evolution of outcomes of affected individuals (relative to similar individuals in close geographic proximity who are less affected by the policy change). This study therefore does not rely on cross-state policy variation or individual enrollment choices that may be correlated with other state-level or individual-level changes, respectively.

This paper contributes to a recent and growing literature in economics studying the effect of mental health and mental health services on various outcomes, including criminal activity (Anderson et al. 2015, Bondurant et al. 2018, Busch et al. 2014, Chatterji & Meara 2010, Deza et al. 2020, Fletcher & Wolfe 2009, Heller et al. 2017, Marcotte & Markowitz 2011, Teplin et al. 2002).[2] By leveraging a sudden loss of Medicaid coverage, this paper adds to our understanding of the causal relationship between mental health service provision and contact with the criminal justice system among low-income young adults. Moreover, this paper argues that criminal involvement is a function of health, similar to a number of papers that study the effect of developmental health—via changes in lead exposure—on criminal behavior (Aizer & Currie 2019, Billings & Schnepel 2018, Feigenbaum & Muller 2016, Reyes 2007).

Finally, by studying changes in access to Medicaid, this paper contributes to a recent literature studying the effects of health insurance expansions on public safety (Aslim et al. 2019, He & Barkowski 2020, Vogler 2017, Wen et al. 2017).[3] Unlike most of these studies, which rely on aggregate crime statistics in order to measure changes in public safety, this paper uses individual-level records, allowing me to identify and study offender traits (e.g., mental health history, first-time offenders). More broadly, this paper contributes to a growing literature quantifying the social returns to Medicaid (see e.g., Arenberg et al. 2020, Boudreaux et al. 2016, Brown et al. 2020, Goodman-Bacon 2016).[4] This study is different from previous papers in two main ways. First, it focuses on the *immediate*, rather than the long-term, effects of Medicaid eligibility. The findings

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[2] Numerous other studies consider the relationship between mental health and human capital, including Cowan & Hao (2020), Cuellar & Dave (2016), Currie & Stabile (2006), Currie & Stabile (2007), and Fletcher & Wolfe (2008). Finally, other related papers include Biasi et al. (2019), Bütikofer et al. (2020), Chatterji et al. (2011), Chorniy & Kitashima (2016), Cuddy & Currie (2020), Cuellar & Markowitz (2006), Fletcher (2013), Ludwig et al. (2009), Maclean et al. (2019), and Ridley et al. (2020), which study the relationship between mental health and labor market outcomes, risky sexual behaviors, overall health, and mortality.

[3] By estimating the effect of health coverage on an individual's likelihood of engaging in criminal activity, this paper is also related to an expansive literature studying deterrence and desistance from crime. For detailed reviews of this literature, I refer the reader to Chalfin & McCrary (2017) and Doleac (2020).

[4] For a detailed review of the effect of Medicaid on various outcomes and populations, I refer the reader to Currie & Duque (2019).

can therefore help quantify the short-term returns to increasing Medicaid access, which are likely of interest to policymakers weighing the costs and benefits of expanding public insurance coverage. The findings of this paper may also help improve our understanding of the mechanisms underlying the documented long-term effects of Medicaid access. Second, whereas previous studies typically focus on historical Medicaid expansions for children, this study focuses on the provision of Medicaid eligibility to modern cohorts of adolescents and young adults. Because young adults are relatively less likely to be insured—and are thus the group who stands most to gain from modern health insurance expansions—understanding the returns to this investment is of policy relevance. In that regard, this paper complements the findings from the Oregon Health Insurance Experiment, by focusing on a younger and more racially diverse population as well as considering additional outcomes (Baicker et al. 2014, Finkelstein et al. 2012).[5]

The remainder of the paper is organized as follows. Section 2 provides a brief discussion about the relationship between mental illness and criminal activity. In Section 3, I describe the data and the sample. Sections 4 and 5 outline the research design and discuss the estimation strategy. Section 6 presents the main results and corresponding robustness checks, and Section 7 explores heterogeneous effects. Sections 8 and 9 conduct a series of cost-benefit analyses. Section 10 concludes.

2 Mental Health & Criminal Activity

2.1 Historical Background: Differing Views on Policy Responses to Crime

As crime rates began rising in the United States in the 1960s, contemporary observers debated the extent to which mental illness causes crime, and consequently, the degree to which the criminal justice system should be replaced with alternative, more therapeutic approaches (Murphy 1969). One prevalent perspective was that the penal system punished criminal symptoms instead of curing criminal causes. Dr. Karl Menninger, a well-respected psychiatrist at the time, published a book titled "The Crime of Punishment," arguing that "psychiatrists cannot understand why the legal profession continues to lend its support to such a system after the scientific discoveries of the past century have become common knowledge" (Menninger 1968). Individuals who held this view

[5] The population that participated in the Oregon experiment was 83% white and on average 41 years old. The samples in this study are typically 70% Black and their outcomes are measured before age 21.

advocated for reforms or alternatives to the penal system, such as providing judges with psychiatric reports prior to sentencing, or establishing "community safety centers" tasked with identifying and treating offenders and would-be offenders.[6]

At the same time, economist Gary Becker published his seminal work on the economics of crime, providing an alternative perspective for understanding and addressing criminal behavior. His framework argued that criminal offenders make a rational calculation, weighing the associated costs and benefits when deciding whether to commit a crime (Becker 1968). The implications of his model were that society could deter offenders from committing crimes by either making punishments more severe or more certain (e.g., via longer prison sentences or more police officers) or by raising the opportunity cost of crime (e.g., via improved employment opportunities or schooling).

For the remainder of the 20th century, policymakers reduced Becker's framework to its punitive implications and used it as an intellectual justification for adopting harsher criminal sanctions (Donohue 2007). Policymakers at all levels of government increased the length of punishments as well as the likelihood of sending convicted offenders to prison, two policies which contributed to a nearly fivefold increase in the incarceration rate (Pfaff 2017, Neal & Rick 2016, Raphael & Stoll 2013b).[7] By 2010, roughly 2.3 million individuals were incarcerated in local jails or in state or federal prisons (Glaze 2011).

Policymakers' reliance on harsher punishments also coincided in timing with the closure of state mental hospitals and a shift away from inpatient mental health treatment.[8] Recent research suggests that around five percent of incarcerated individuals with mental illness in the 1980s–2000s would have been institutionalized in state mental hospitals, rather than in prisons, in prior decades (Raphael & Stoll 2013a).

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[6] See also "A Psychiatrist Views Crime." TIME Magazine, vol. 92, no. 23, Dec. 1968, pp. 117—120.

[7] Republicans and Democrats both contributed to this increased support for and reliance on punitive policies. Prior to the passage of the Violent Crime Control and Law Enforcement Act of 1994, First Lady Hillary Clinton argued "We need more police, we need more and tougher prison sentences for repeat offenders. The 'three-strikes-and-you're-out' for violent offenders has to be part of the plan. We need more prisons to keep violent offenders for as long as it takes to keep them off the streets."

[8] In 1963, President Kennedy signed into law the "Community Mental Health Act," which aimed to transfer mental health treatment from state hospitals to community-based facilities. In addition to this act, several other policies accelerated deinstitutionalization, including the introduction of medications, the implementation of the Medicaid and Medicare programs with particular funding schemes, and a U.S. Supreme Court decision limiting the reasons for which an individual could be involuntarily committed (Raphael & Stoll 2013a).

2.2 Prevalence of Mental Illness Among Criminal Justice Populations Today

Today, the relationship between mental illness and criminal behavior is significantly more well-established (see Frank & McGuire 2010 for a detailed review).[9] Individuals with mental illness are significantly over-represented in prisons and jails: 37 percent of prison inmates and 44 percent of jail inmates have been diagnosed with a mental disorder prior to incarceration (Bronson & Berzofsky 2017). Accordingly, the criminal justice system spends a significant share of its resources housing and treating individuals with mental illness, especially given this population's higher recidivism rates, longer sentences, and more expensive medical needs (Osher et al. 2012).[10]

The persistent relationship between mental illness and criminal involvement raises the question of whether improved access to behavioral health services can reduce the likelihood that mentally ill individuals commit crime. It is worth noting that when policymakers and academics were discussing the relationship between mental illness and crime in the 1960s, healthcare may not have been an effective way to reduce criminal behavior. However, in the decades that have transpired, significant scientific progress has been made in understanding and treating mental illness, including important developments and improvements in psychotropic drugs (e.g., antidepressants, mood stabilizers) as well as alternative modes of psychotherapy (e.g., cognitive behavioral therapy) (see e.g., Frank & Glied 2006, Hofmann et al. 2012, Kendler 2019, Lieberman & First 2018, Marder & Cannon 2019, Park & Zarate Jr. 2019). Acknowledging this progress, this paper revisits the potential role that mental healthcare can play in reducing criminal activity.

2.3 Role of Healthcare in Affecting Criminal Propensity

There are multiple channels through which losing access to health insurance can affect an individual's criminal behavior. This study focuses on the mental health channel: for many individuals,

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[9] It is important to distinguish here that even though a significant portion of criminal offenders have mental health histories, it is not the case that most mentally ill individuals commit crimes.

[10] Advocates, researchers, and media outlets have noted that jails and prisons have become the country's largest mental health hospitals. See for example, *The Atlantic*'s piece "America's Largest Mental Hospital Is a Jail" or NPR's segment "Nation's Jails Struggle With Mentally Ill Prisoners."

losing Medicaid eligibility means losing access to mental health treatments or medications.[11,12] This loss of access to mental healthcare could result in increased criminal behavior for various reasons.

First, individuals who lose insurance coverage might begin to find criminal activities more appealing. For example, individuals who lose access to medications might begin to self-medicate via higher use of illicit drugs (Khantzian 1987, Khantzian 1997). Indeed, Busch et al. (2014) finds that following a regulatory policy that decreased antidepressant prescriptions, adolescents with depression were more likely to use illegal drugs. Loss of mental healthcare could also disrupt an individual's human capital formation or labor market productivity, thereby making criminal alternatives more attractive. Biasi et al. (2019) finds that increased access to lithium—a psychiatric medication primarily used to treat mood or depressive disorders—improved the career trajectories of individuals suffering from bipolar disorder. In addition, Currie & Stabile (2006) and Currie & Stabile (2007) find that mental health conditions can have deleterious effects on educational attainment, so to the extent that access to public health insurance can minimize these effects, then losing access could hinder an individual's academic achievement.[13]

Finally, individuals who lose access to behavioral health services might be more prone to making errors in judgment or decision-making, and thus be more likely to engage in criminal behavior. For example, Heller et al. (2017) finds that low-income adolescents participating in cognitive behavioral therapy (CBT) programming during the school year were significantly less likely to be arrested for both violent and non-violent offenses, but that these effects did not persist after the program ended. Furthermore, health insurance coverage provides individuals with access to resources (e.g., social workers, community-based services) that could help de-escalate mental

[11] Medicaid is the largest payer for behavioral health services in the United States, covering both inpatient and outpatient services. In 2009, the program accounted for 26% of nationwide behavioral health spending. Moreover, behavioral health services are a significant component in health spending for children and adolescents. In 2011, 20% of enrolled children ages 7–20 had a behavioral health diagnosis and those individuals accounted for 50% of Medicaid spending for that age group (MACPAC 2015).

[12] Beyond this mental health channel, loss of insurance can also affect an individual's likelihood of committing crimes via changes in expected medical costs. Previous studies have found that access to Medicaid reduces out-of-pocket medical spending, thereby freeing up additional resources for the household (see e.g., Gallagher et al. 2019, Gross & Notowidigdo 2011, Hu et al. 2018).

[13] Individuals who lose access to insurance could have their education disrupted if (1) diminished physical or mental well-being prevents them from successfully investing in their human capital, or (2) they are forced to seek employment, rather than invest in education, in order to obtain insurance (a situation which is more likely to arise in the U.S. where health insurance access is often tied to employment).

health crises or treat substance abuse, thereby preventing future criminal involvement (Bondurant et al. 2018).[14]

3 Data and Sample

This paper studies the effect of health insurance on crime in the state of South Carolina. South Carolina is relatively poorer than other states in the U.S. and it also has low levels of health insurance coverage among non-elderly adults.[15]

The data source used in this study is individual-level administrative data from various state agencies, which have been linked by South Carolina's Revenue and Fiscal Affairs (RFA) Office. Linked datasets like this one are common in Scandinavian countries, but are relatively rare in the United States. The dataset not only contains detailed information about an individual's enrollment in government-run programs like Medicaid and SNAP, but it also contains rich information about an individual's contact with the criminal justice system, educational achievement, and fertility. RFA linked individual-level data from six state government agencies for this study, so that I can identify the same person across datatsets and time using an individual identifier.

3.1 Sample

The primary sample used in this study is a disproportionately low-income group of individuals born between 1990 and 1999. One can think of this sample as representing the residents of the poorest half of neighborhoods in South Carolina.[16] Specifically, an individual is included in the sample if he or she ever attended a high school among the poorest half of high schools in the state. The dataset provides information on 210,443 individuals starting at age ten. For more details on the sample and the variable construction, I refer the reader to Appendix B.

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[14] Because police are often the first to respond to psychiatric crisis calls, individuals experiencing mental health crises also face an increased likelihood of interacting with law enforcement officials (Lamb et al. 2002).

[15] South Carolina's poverty rate is 15% and its median household income is $51,015, compared to a nationwide poverty rate and median household income of 11% and $60,293, respectively (U.S. Census Bureau 2020). In 2018, 18% of individuals ages 19–25 were uninsured and South Carolina ranked 7th in the country in the overall share of uninsured non-elderly adults (SHADAC 2020).

[16] Between the ages of 10 and 18, 72% and 66% of the individuals in this sample were ever enrolled in Medicaid and in SNAP, respectively. In terms of demographics, 60% of individuals in the sample are classified as Black and 4% are Hispanic (relative to state-level averages of 27% and 6%, respectively [U.S. Census Bureau 2020]).

3.2 Medicaid Claims

Detailed information on an individual's Medicaid enrollment spells as well as insurance claims comes from data provided by South Carolina's Department of Health and Human Services. The recipient file includes information on demographic characteristics as well as the dates of enrollment spells. The remaining data files contain insurance claim information from all pharmacy, office, and hospital visits, including all diagnoses, billing codes, and pharmacologic-therapeutic drug classifications.

The insurance claim data allow me to classify visits and prescriptions as mental healthcare. In particular, diagnoses are classified as mental health diagnoses if any of the corresponding codes fall into the mental, behavioral, and neurodevelopmental disorders category. Drugs are identified as mental health medications if their drug classification corresponds to psychiatric medications (e.g., antidepressants, benzodiazepines). A claim is then considered a "mental health claim" if it includes a mental health diagnosis or if it prescribes a mental health medication.

3.3 Data on Criminal Behavior

To measure crime-related outcomes, I use records from the South Carolina Law Enforcement Division (SLED), the Department of Corrections (DOC), and the Department of Juvenile Justice (DJJ). Data from SLED provide information on all arrests in the state as well as information detailing whether the individual was taken into custody in an adult correctional facility. Data from the DOC provide details on incarceration spells in a state prison, including the dates of admission and release as well as the inmate's most serious offense. Finally, data from DJJ contains information on all contact between juveniles and the juvenile justice system. It is worth noting that in South Carolina, individuals are legally treated as adults on their 17th birthday.

3.4 Other Data Sources

I augment these sources with educational records from the Department of Education. These data include information on an individual's district and school attended as well as standardized test scores. I also make use of data from the Department of Social Services. The records from this agency provide information on enrollment in the Supplemental Nutrition Assistance Program (SNAP) and in the Temporary Assistance for Needy Families (TANF) program. Finally, I make use of death certificate records from the Department of Health and Environmental Control.

4 Empirical Strategy

Children ages 0–18 with household incomes up to 208 percent of the federal poverty level (FPL) are insured via the Children's Health Insurance Program (CHIP), which is operated through the Medicaid program in South Carolina (SCDHHS 2020b). Upon reaching 19 years of age, residents of South Carolina are no longer eligible for CHIP and childless individuals have limited access to Medicaid services.[17,18]

This section describes the approach I use to estimate the reduced-form impact of losing access to Medicaid coverage on men's likelihood of incarceration. I first introduce the research design—a matched difference-in-differences framework—and then describe the matching procedure as well as the characteristics of the matched sample.

4.1 Research Design

The main outcome of interest is men's likelihood incarceration, which peaks in prevalence in men's late teens and early twenties, and is particularly common for men in low-income communities (Freeman 1999, Lofstrom & Raphael 2016). I measure this outcome by combining data from the South Carolina Law Enforcement Division—which identifies individuals who were detained in an adult correctional facility after being arrested—as well as data from the Department of Corrections, which tracks incarceration spells in a state prison. This outcome therefore measures the likelihood that an individual is incarcerated in any adult correctional facility.

In order to measure the impact of losing Medicaid eligibility on men's likelihood of incarceration, I employ a difference-in-differences design. Men who were likely to be enrolled in Medicaid before their 19th birthday serve as the treated individuals in the analysis (for the remainder of the paper, I will refer to this group as the "high-enrollment" group). Individuals who were enrolled in Medicaid earlier in adolescence, but who were less likely be enrolled prior to their 19th birthday serve as the comparison group (henceforth, the "low-enrollment" group). Importantly, because all

[17] Adults who are eligible for Medicaid services in South Carolina include low-income pregnant women (up to 199% of the FPL), parents with dependent children (up to 67% of the FPL), children formerly in foster care (up to age 26), and disabled individuals (SCDHHS 2020a, KFF 2019).

[18] Because I do not see any other health insurance data besides Medicaid claims, I cannot know with certainty whether individuals who lose access to Medicaid become uninsured or transition to private insurance. However, I assume that the share of this population that becomes uninsured is relatively high: in 2010, among individuals ages 19–25 living at or below 138% of the FPL, 43% were uninsured and 41% were covered by private insurance (SHADAC 2020).

treated individuals lose Medicaid coverage at the same point in the life cycle, it is important to compare their outcomes to those of a comparison group in order to estimate and account for age trends in crime.[19] It is not possible to discern from the data why the low-enrollment group was enrolled in Medicaid earlier, but not later in adolescence. However, possible explanations include large hurdles to enrolling (e.g., long waits at the county office) or no longer wanting or needing access to Medicaid services. Regardless of the reason for not being enrolled, the comparison group helps me estimate counterfactual outcome paths for the treated individuals and thus allows me to disentangle the effect of the insurance loss from pure age effects.

Specifically, I assign men into the high- and low-enrollment groups based on their enrollment in Medicaid between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$. I then follow the natural evolution of each group's outcomes for three and a half years: the first year and a half serves as the study's pre-period (ages 17.5–19) and the latter two years, which begin on the individual's 19th birthday, serve as the study's post-period (ages 19–21). Figure 2 offers a graphical timeline of the approach.[20]

4.2 Matching Procedure

To ensure that the low-enrollment individuals serve as a suitable group for estimating the high-enrollment group's counterfactual outcome paths, I implement a matching procedure that guarantees balance along observable characteristics. To be eligible for inclusion in this procedure, individuals must have been enrolled in Medicaid at some point between the ages of 10 and 18. To avoid including individuals who might have left the state, I also restrict the sample to individuals who were present in any of the data sources between ages 15–18. I then drop individuals who passed away or were incarcerated before the study's pre-period (which starts at age $17\frac{1}{2}$) since these outcomes could mechanically determine the assignment of an individual into the treatment or control group. Finally, the baseline sample focuses on individuals born between 1990 and 1993. There are 22,063 eligible treatment and 8,980 eligible control men, which is roughly two-thirds of

[19] Moreover, a recent literature highlights a number of issues that come up when relying on the timing of treatment to identify the effect of a policy (Abraham & Sun 2018, Borusyak & Jaravel 2017, Goodman-Bacon 2018).

[20] I assign individuals into the treatment and control groups a year before the study pre-period in order to guarantee that the assignment is uncorrelated with the outcomes in the pre-period. For example, men who are incarcerated before age 19 might be less likely to be enrolled in Medicaid. Assigning individuals to a group before the start of the study pre-period therefore allows me to follow the natural evolution of outcomes both before and after their 19th birthdays. I check the sensitivity of the main results to this choice in Section 6.3.

the men in the low-income sample born in these cohorts.[21]

I then match each treated person to all "counterfactual" individuals using a parsimonious set of characteristics. Treatment men are matched to all control men based on year of birth, race (measured as Black or non-Black), school district (measured as the last school district attended prior to age 19), and mental health history prior to age 16 (i.e., measured as having a Medicaid mental health claim between the ages of 10 and 15). This matching procedure—and the corresponding weighting scheme described below—is similar to the approach used in Smith et al. (2019). I intentionally avoid matching on outcome variables.

Matching on these characteristics assists in constructing a comparison group that would plausibly exhibit similar trends to the treated individuals in the absence of the Medicaid loss. Importantly, Figure 1 illustrates that men with prior mental health histories are on significantly different criminal trajectories than men without any diagnosed mental disorders. Matching on mental health history before age 16, when both treated and control individuals were enrolled in Medicaid, is therefore useful for matching treated individuals to counterfactual men with similar criminal propensities (as opposed to explicitly matching on criminal histories, which would entail matching on an outcome variable). I illustrate the importance of including mental health history as a matching characteristic in Section 6.3.

4.3 Characteristics of Matched Sample

Table 1 reports summary statistics for the full sample and various subsamples. All characteristics are measured before an individual's 19th birthday (i.e., between the ages of 10 and 18). On average, individuals in the matched sample (column 3) are more likely to be Black and more likely to have a mental health history than individuals in the full low-income sample (column 1). Importantly, men in the matched sample are also observably different from the group of individuals who were ever enrolled in Medicaid (column 2). In particular, treated men are more likely to have a mental health history and slightly more likely to have had prior contact with the criminal justice system. These differences in observable characteristics prior to age 19 reinforce the importance of implementing the matching procedure in order to ensure that the low-enrollment group serves as a suitable comparison group for estimating the counterfactual outcome paths of high-enrollment

[21] Almost all individuals who are not included in the matching procedure are excluded because they were not enrolled in Medicaid between the ages of 10 and 18.

individuals.[22]

Appendix Table A2 shows the distribution of diagnoses for the full sample as well as for treated men. This table highlights that hyperkinetic syndrome of childhood (i.e., attention-deficit/hyperactivity disorder [ADHD]), developmental delay, and disturbances of conduct are among the most common diagnoses in this sample. Importantly, I also note that the shares in this table do not sum to 100 because most teens have more than one diagnosis in their medical records (more than 60 percent of treated men with mental health histories have at least two different diagnoses in their claims). Finally, Appendix Table A3 shows that individuals utilizing behavioral health services most often file claims for psychotropic drugs as well as case management services.

The matching procedure guarantees that there is balance between the high- and low-enrollment groups in terms of race, cohorts, geographic location, and mental health history. However, there still might be concern that the two groups significantly differ along unobservable characteristics and would thus plausibly exhibit different trends in criminal propensity. Appendix Figure A1 plots the raw means of standardized test scores as well as juvenile justice referrals for felony offenses for the two groups earlier in adolescence. These figures show that the treated and control men in the matched sample were relatively similar to each other in terms of their educational achievement and criminal propensity, both in levels and in trends, throughout their adolescence.

4.4 Loss of Medicaid Eligibility in Matched Sample

To confirm that high-enrollment men are indeed affected by the loss of Medicaid eligibility at age 19, I use the Medicaid enrollment and claims data to plot the share of men enrolled in the program and filing claims. The first two panels of Figure 3 show that treated men experience a decline in Medicaid enrollment and in their likelihood of filing claims after their 19th birthday. It is worth noting that because individuals are assigned to the treatment and control groups prior to age $17\frac{1}{2}$, the share of control men enrolled and filing claims in the pre-period is not mechanically zero; however, men in this low-enrollment group are significantly less likely to be affected by the loss of Medicaid eligibility at age 19.

Among treated men, the average share enrolled drops from 78 percent in the pre-period to 14

[22] Table A1 compares means for the matching candidates (i.e., the individuals who met the criteria for being eligible for the matching procedure) as well as the successfully matched individuals. One takeaway from this table is that around 97% of eligible treated individuals were successfully matched to at least one control individual.

percent in the post-period, thereby confirming the presence of a large "first stage." Appendix Figure A2 shows that the high-enrollment men did not experience comparable drops when considering enrollment in other public assistance programs (i.e., SNAP and TANF).

The second two panels plot the raw means separately by mental health history, showing that both groups of treated men experience a decline in Medicaid enrollment. Indeed, both groups of treated men are more than 30 percentage points less likely to file Medicaid claims in the post-period. Finally, I note that the drop in enrollment among men with mental health histories is smaller in magnitude than the analogous drop for men without a mental health history, which would bias me against finding an effect for the former group.

5 Estimating the Effect of Losing Medicaid on Criminal Behavior

5.1 Baseline Specification

To estimate the impact of the loss in eligibility on criminal activity, I compare the average outcomes of high-enrollment men to those of the comparison group around their 19th birthdays. The regression analogue of this comparison is a fully dynamic matched difference-in-differences regression of the following form:

$$Y_{it} = \sum_{\tau=-6}^{\tau=8} \Big[\beta_\tau (\text{Treat}_i \times \gamma_\tau) + \theta_\tau \gamma_\tau \Big] + \mu \text{Treat}_i + \delta_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is an outcome variable for individual i at time t, and γ_τ is the quarter relative to an individual's 19th birthday. The pre-period and post-period are six and eight quarters, respectively. Treat_i is an indicator variable equal to 1 for high-enrollment men (i.e., those enrolled in Medicaid at ages 16.5–17.5), δ_t are calendar time fixed effects, and ε_{it} is an error term. To ensure balance between the treatment and control groups in the regression, I weight each control observation by one over the number of control units matched to the corresponding treated individual. All standard errors are clustered at the individual level.

The coefficients of interest are β_τ, estimating the treat-control differences in outcome Y at event time τ. Because many of the outcomes are relatively rare occurrences, all outcome variables are multiplied by 100. I omit the quarter before a person's 19th birthday, so that each β_τ coefficient measures the differences in outcome Y relative to the difference that occurred at time $\tau = -1$.

When presenting the results, I typically report the post-period average of the β_τ coefficients

(i.e., $\frac{1}{8}\sum_{\tau=1}^{8}\beta_\tau$), or in other words, the average treatment effect in the post period. When considering cumulative variables (e.g., individual i has *ever* been incarcerated), I instead report the last coefficient β_8, which measures the treat-control difference in outcome Y by age 21. Finally, when interpreting the magnitudes of the estimates, I re-scale the average treatment effect in the post-period by the control group's average outcome Y in the post-period. When using cumulative variables, I re-scale β_8 by the comparison group's average Y in the final period of the post-period.[23]

5.2 Identification Assumption

I interpret the β_τ coefficients as the causal effect of losing Medicaid eligibility on outcome Y. Identification of the causal effect comes from differences in outcomes between high-enrollment and low-enrollment men over time. Even though the loss of Medicaid eligibility occurs at different calendar times for each treated individual, the presence of the comparison group ensures that the effects are estimated using the differences between treated and control units (as opposed to using the differences in timing to identify the treatment effect, like in an event-study approach). Accordingly, the identifying assumption in this empirical strategy is that high-enrollment men would have trended similarly to low-enrollment men in the absence of the Medicaid loss.

One concern with this identifying assumption might be that men in the treatment and control groups are fundamentally different and thus likely to have differing outcome paths. For example, if treated men are relatively lower-income than the comparison group, then we might expect treated men to be more likely to engage in criminal activity regardless of the loss in Medicaid eligibility. Matching on observable characteristics, like race and geographic location, helps alleviate this concern.[24] Moreover, even if the two groups differ in their overall levels of outcome Y, the identifying assumption relies on the two groups *trending* similarly over time in their criminal propensity. If the two groups are trending similarly and only begin to diverge when treated individuals lose access to Medicaid, then these patterns suggest that the control individuals are a suitable comparison group for estimating the counterfactual post-period outcomes of the treated men in the absence of the insurance loss. In practice, I plot the raw data and estimate non-parametric

[23] Because criminal propensity tends to rise with time in this age range, re-scaling the estimated effect by the control group's average in the pre-period would yield larger effects. I therefore choose the more-conservative approach and re-scale the effect by the post-period average. This choice is also motivated by the identifying assumption: the comparison group is tracing out the counterfactual outcome paths of the treated units, so I re-scale the estimates by the average counterfactual outcomes.

[24] The robustness checks in Section 6.3 also addresses this concern by matching on school rather than district.

specifications in order to corroborate the plausibility of the parallel trends assumption.

Finally, one other threat to the identifying assumption could be that treatment status is correlated with other shocks that occur at the same time as the loss in Medicaid eligibility, thereby confounding the estimated effects. Two factors mitigate this concern: treated and control individuals turn 19 at similar times (by construction) and all individuals have their 19th birthdays at different points in calendar time. For unobserved shocks to confound the estimates, it would have to be the case that these shocks only affect high-enrollment men—and not observably similar individuals living in close proximity—and that the timing of these shocks coincided with these individuals' 19th birthdays. The differences in the timing of birthdays as well as the matching procedure therefore make it unlikely that the estimated effects are driven by treated individuals experiencing unobserved shocks unrelated to their loss in Medicaid eligibility.

5.3 Heterogeneity

Given the established connection between mental illness and criminal activity, the effect of the Medicaid loss might be particularly salient for individuals with a mental health history. Indeed, among low-income men in this sample who served an incarceration spell in state prison before age 21, 79 percent of them had been diagnosed with a mental disorder during adolescence. When studying criminal outcomes, I therefore begin by showing the main results for the full sample, but I then estimate the effects separately by mental health history. Specifically, I estimate a regression of the following form:

$$Y_{it} = \sum_{\tau=-6}^{\tau=8} \Big[\alpha_\tau(\text{Treat}_i \times \text{Hist}_i \times \gamma_\tau) + \nu_\tau(\text{Hist}_i \times \gamma_\tau) + \beta_\tau(\text{Treat}_i \times \gamma_\tau) + \theta_\tau \gamma_\tau \Big] + \text{Treat}_i + \delta_t + \varepsilon_{it} \tag{2}$$

where Hist_i is a variable indicating whether individual i had a mental health history prior to age 16 (i.e., one of the characteristics used in the matching procedure). This specification is similar to equation (1) in that high-enrollment men are still being compared to the matched comparison group over time. However, this specification allows treated individuals with mental health histories to have differing treatment effects—captured by α_τ—than treated individuals without mental health histories. When reporting results, I report the post-period average of the α_τ coefficients (or α_8 for cumulative variables), which measures whether treated individuals *with* a mental health history had a different average treatment effect than individuals *without* a mental health history. In that

regard, this regression is equivalent to running a triple-differences specification.[25]

6 Main Results for Criminal Behavior

6.1 Full Sample

I first consider an individual's likelihood of being incarcerated in a given quarter. Panel (a) of Figure 4 plots the raw means using the full matched sample, and panel (b) plots the estimates from equation (1).[26] These figures confirm that treated men were trending similarly to the men in the comparison group prior to age 19, thereby providing support for the parallel trends assumption. It is also worth highlighting that the treatment group's likelihood of incarceration resembled that of the comparison group's not only in its trending behavior, but also in its levels during the entire pre-period. The estimates from panel (b) show that treated men were 15 percent more likely to be incarcerated in any given quarter of the post-period than the men in the comparison group. The results from this and the following subsection are summarized in Table 2.

In an effort to understand whether the estimates are driven by the same or different individuals being incarcerated over time, I then use a cumulative variable, which captures the extent to which *new* individuals are being incarcerated.[27] Panels (c) and (d) once again show that treatment and control individuals were trending similarly prior to age 19, and began to significantly diverge after age 19. By their 21st birthdays, men in the high-enrollment group who lost access to Medicaid were 2 percentage points (or 18 percent) more likely to have been incarcerated than men in the matched comparison group.

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[25] I could have also split the sample by mental health history and run two separate double-difference regressions using equation (1). The benefits of using equation (2) are that I can more easily test whether the differences are statistically different from each other and I can use the full sample to estimate the calendar time fixed effects.

[26] Analogous plots of the raw means and treat-control differences using alternative measures of criminal behavior are displayed in Appendix Figures A3 and A4, and summarized in Appendix Table A4.

[27] The use of a cumulative variable is also motivated by the imperfect information on an offender's full incarceration spell. Specifically, unlike the DOC data, the SLED data does not contain information on when an individual was released from custody. By failing to capture an offender's detainment status, the results from panels (a) and (b) are likely underestimates of an individual's likelihood of being incarcerated in any given quarter. For example, it may be the case that an individual is still in custody in subsequent quarters after being arrested and detained, but the specification will not be able to distinguish between this scenario and one in which the individual has been released and does not re-offend.

6.2 Heterogeneity by Mental Health History

To see whether this rise in criminal activity is driven by men with mental illness, I split the sample into two groups based on men's mental health history prior to age 16. Figure 5 displays the results. Panel (b) plots the treat-control differences for men without a mental health history (44 percent of treated men, in orange) as well as the the treat-control differences for men with a mental health history (56 percent of treated men, in blue). These results suggest that the increase in crime is driven entirely by men with a mental health history. Treated men without mental health histories continue to trend similarly to their corresponding comparison group throughout the post period.[28]

I then use the dependent variable measuring men's cumulative likelihood of incarceration. The results in panels (c) and (d) show that the share of treated men who have ever been incarcerated begins diverging from the analogous share of the comparison group starting at age 19. By their 21st birthdays, treated men with a mental health history are 3 percentage points (or 22 percent) more likely to have ever been detained in an adult correctional facility. These results highlight that the observed rise in crime is not driven by a small group of men being detained for long periods of time or consistently recidivating. Instead, the termination of Medicaid eligibility results in new individuals being incarcerated.

A back-of-the-envelope calculation suggests that in the absence of the Medicaid loss, around 420 fewer men born in these cohorts would have been incarcerated before age 21. Because this sample only represents roughly half of the state's population, this number is almost certainly an undercount of the number of men in these cohorts who would not have been incarcerated had their Medicaid eligibility not expired on their 19th birthdays.

Finally, I can use these reduced-form estimates to estimate the implied effect of Medicaid enrollment on men's likelihood of incarceration. Table 3 displays the results, showing that Medicaid enrollment decreases low-income men's likelihood of incarceration by around 25 percent.[29] Among

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[28] Appendix Figure A5 plots these same treat-control differences by race, indicating that the higher likelihood of incarceration is more pronounced for Black men (who represent 70% of high-enrollment men with a mental health history).

[29] I obtain these estimates via two-stage least squares, estimating the equation:

$$Y_{it} = \beta_0 \text{Enrolled}_{i\tau} + \beta_1 \text{Post}_{\tau} + \beta_2 \text{Treat}_i + \delta_t + \varepsilon_{it} \tag{3}$$

where Post_{τ} is any of the eight quarters after a person's 19th birthday, and $\text{Treat}_i \times \text{Post}_{\tau}$ is an instrumental variable for Medicaid enrollment. The Wald estimate (i.e., β_0) is the ratio of the reduced-form and the first-stage estimates.

men with mental illness, Medicaid enrollment decreases their likelihood of incarceration by 2 percentage points, a 40 percent reduction. To put these magnitudes into perspective, Lochner & Moretti (2004) find that one extra year of schooling reduces men's likelihood of incarceration by approximately 10 percent. These findings thus suggest that providing health coverage to individuals with mental illness could be one way to significantly reduce their criminal involvement.

6.3 Robustness of Main Results

6.3.1 Robustness to Clustering and Matching

In this section, I consider the robustness of the main results displayed in Figure 5 to various other choices I could have made when constructing the baseline sample and implementing the preferred specification. Appendix Table A5 reports the results from this subsection. The first row reproduces the main results using the baseline sample. In the second and third rows, I cluster the standard errors at different levels: at the match level and at the match and individual level, respectively. The statistical significance of the main results is preserved.

I then test the sensitivity of the estimates to the matching procedure and implicit weighting scheme. To verify the parallel trends assumption in these robustness checks, Appendix Figure A6 displays the graphical results. First, instead of matching each treated unit to all possible counterfactual control units, I force each treated unit to only have one randomly chosen counterfactual observation (fourth row of Appendix Table A5). In the fifth row, I return to the baseline matching procedure, but I drop control units that get disproportionate weight in the regression.[30] In the next row of the table, I alter the timing of assignment into the treatment and control groups, so that individuals are assigned to a group based on their Medicaid enrollment during the study's pre-period (as opposed to a year before the study's pre-period). Finally, instead of using an individual's school district as a matching characteristic, I use an individual's school (measured as the last school attended before age 19) so that high-enrollment individuals are even more likely to resemble the low-enrollment group. This change reduces the number of matched treated units, but it does not significantly alter the main findings (seventh row).

[30] As shown in Table A1, there are more eligible treated units than there are eligible control units. The matching procedure gives greater weight to eligible control individuals that resemble the treated units along observable characteristics. However, this procedure could result in certain control units getting a disproportionate amount of weight in the regression. To address this possibility, I calculate how much total weight each control unit is given in the baseline sample, and drop the control units whose weight is in the top one percent of that weight distribution. I then reconstruct weights for the remaining usable control units so that there is balance within the regression.

Next, I consider the robustness of the main results to alternative ways of constructing the comparison group. First, I abstain from any matching and re-weighting, and instead run the specification with all eligible treated and control men.[31] The main results by mental health history (eighth row) are very similar to those using the baseline matched sample. Finally, instead of relying on coarsened exact matching to construct a suitable comparison group, I instead re-weight the eligible control men using the DFL re-weighting approach (DiNardo et al. 1995, Fortin et al. 2011). I estimate two potential weights using this method: one based on the baseline set of matching characteristics and one that replaces school district with school. I find that the results are robust to this alternative approach for re-weighting the eligible control individuals.

As a final way to test the robustness of the main result using the matched difference-in-differences approach, I conduct a "placebo" check, in which I shift the entire empirical approach back one year and estimate men's likelihood of incarceration around age 18, rather than around age 19.[32] If the main findings are driven by the loss of Medicaid at age 19, then I should not find an immediate increase in treated men's criminal propensities after their 18th birthday when there was not a break in Medicaid eligibility. Figure A8 displays the results from this exercise, which suggest that the immediate increase in the cumulative likelihood of incarceration among treated men with mental health histories is indeed driven by this group's loss of Medicaid eligibility at age 19.

6.3.2 Alternative Empirical Strategy: Regression Discontinuity

As an alternative way to check the robustness of the main result—that men with mental health histories who lose Medicaid eligibility at age 19 are more likely to be incarcerated in the following two years—I abstract away from assigning men to treatment and control groups, and instead utilize a regression discontinuity approach, similar to that in Card et al. (2009) and Lee & McCrary (2017). Specifically, I compare arrest probabilities before and after individuals' 19th birthdays to estimate

[31] Appendix Figure A7 plots the raw means for all eligible treated and control men. Panels (a) and (b) highlight that without any matching or re-weighting, the comparison group in the full sample does not exhibit parallel trends to the high-enrollment men. Once mental health history is taken into account, however, the control men in each group do exhibit similar trends to the corresponding treated men in the pre-period. These latter two figures thus illustrate the importance of including mental health history as a matching characteristic if the goal is to find control men who can serve as a suitable comparison group for tracing out the counterfactual outcome paths of treated men.

[32] I assign individuals to treatment and control groups based on their Medicaid enrollment at ages 15.5–16.5 (instead of 16.5–17.5). I then implement the matching procedure and estimate the treat-control differences around men's 18th birthdays. I run the same specification as equation (2) with six and eight quarters in the pre- and post-period, respectively, but only report the coefficients for the first four quarters of the post-period in order to assess whether treated individuals are more likely to commit crimes between their 18th and 19th birthdays.

the effect of losing Medicaid coverage on criminal propensity.[33] The analysis is based on the following reduced-form regression-discontinuity model:

$$Y_{it} = f(a_i) + \beta_1 \text{Post19}_t + \beta_2[\text{Post19}_t \times f(a_i)] + \varepsilon_{it} \tag{4}$$

where Y_{it} represents a criminal outcome for individual i at time t, a_i represents the individual's age (measured in months around his 19th birthday), $f(\cdot)$ is a flexible quadratic polynomial, and Post19_t is an indicator variable for months after an individual's 19th birthday. I also interact the Post19_t indicator with $f(\cdot)$ to allow the slope of the crime-age profile to vary after an individual's 19th birthday. The parameter of interest is β_1, which captures the causal effect of losing Medicaid coverage at age 19 on an individual's criminal propensity. The underlying assumption in this approach is that apart from Medicaid disenrollment, no other factors that influence criminal involvement change discontinuously at age 19.

Appendix Figures A9 and A10 display the results for various subgroups, showing that men's likelihood of being arrested increases upon reaching age 19. Importantly, panels (c) and (d) show that these discontinuities are particularly salient for men with mental health histories, and especially for men who were using Medicaid's behavioral health services right before their 19th birthdays. These findings suggest that men with mental health histories were 0.25 percentage points (or roughly 10 percent) more likely to be arrested upon reaching their 19th birthdays.

7 Heterogeneity in Offenses and in Behavioral Healthcare Utilization

The results up to this point suggest that increases in criminal activity are driven by men with mental health histories. In this section, I study whether the rise in crime was driven by any particular offense type or by any specific subgroup within the group of high-enrollment men with mental health histories.

7.1 Offense Types

In order to gain insight into why men with mental health histories are more likely to be incarcerated, I study whether these men are more likely to be incarcerated for any particular types

[33] Because this empirical approach focuses on the change in criminal propensity that occurs immediately upon reaching age 19, I only consider criminal outcomes measured in the SLED arrest records (as opposed to using incarceration records from the Department of Corrections, which might reflect longer-term judicial decisions).

of crimes. In particular, I classify the offenses that result in incarceration into (1) violent crimes (e.g., murder, assault, robbery), (2) drug- and alcohol-related crimes, (3) property crimes (e.g., burglary, larceny, motor vehicle theft), and (4) miscellaneous offenses (e.g., weapons offenses, parole or probation violations).

Figure 6 and Table 4 display the results, showing that treated men with mental illness are more likely to be incarcerated for violent, drug-related, and property offenses relative to their matched comparison group. The largest effect comes from drug-related offenses (50 percent increase), suggesting that self-medication might be a particularly relevant channel through which the loss of insurance results in increased criminal activity. However, Appendix Figure A11 shows that the rise in drug-related offenses comes from increases in both possession-related and distribution-related charges (66 percent and 54 percent increases, respectively), thus indicating that at least part of the rise in criminal activity is likely economically motivated. Overall, these findings suggest that there is not a single channel through which loss of access to insurance results in increased criminal involvement.

7.2 Differences by Behavioral Healthcare Utilization

Because losing insurance coverage means losing access to mental health treatment or medications, we would expect the treatment effects to be larger in magnitude for men who were using behavioral health services right before turning 19. To test this hypothesis, I focus on men with mental health histories and split this group based on how recently each treated individual was filing claims for Medicaid's behavioral health services. Specifically, I designate treated men with mental health histories as recent beneficiaries if they had a mental health claim in the year and a half before turning 19 (i.e., the study pre-period). Among men with mental health histories, 37 percent of them filed a behavioral health claim during this period. Within this group, 56 percent were filing claims for mental health medications, 8 percent for alcohol and substance abuse treatment services, and 38 percent for psychotherapy or case management services.

In practice, I maintain the same matched control individuals for each treated person, and run triple-difference specifications—similar to equation (2)—in order to estimate whether the treat-control differences for men who were using behavioral health services *more* recently are statistically different from the treat-control differences of individuals using services *less* recently. If the loss of mental health services is an important factor driving the increase in criminal activity, then I should find a positive and statistically significant average treatment effect for more-recent

beneficiaries.

Figure 7 shows that men who were using behavioral health services right before their 19th birthdays (denoted by the purple circles) are indeed more likely to be incarcerated after losing access to Medicaid, when compared to men with mental health histories who were using these services *less* recently (gray markers). Overall, men with mental health histories who were using behavioral health services are roughly 10 percent more likely to be incarcerated than less-recent users of behavioral health services. Table 5 and Appendix Figure A12 show that this difference is mainly driven by more-recent beneficiaries committing more property crimes after losing Medicaid coverage, suggesting that these crimes were likely economically motivated.

Finally, it is worth noting that around half of these men were relying on Medicaid for access to psychotropic drugs, suggesting that losing access to mental health medications might be an important channel through which losing insurance coverage results in increased criminal activity. To consider the specific role of psychotropic drugs, I instead split the sample of treated men with mental health histories based on mental health medication usage. In particular, I split the sample into those who filed claims for mental health drugs between the ages of 14–18 (36 percent of treated men with mental health histories) and those who did not file such claims during these ages.[34]

Appendix Figure A13 shows that in every quarter after their 19th birthdays, the men who had previously filed claims for psychotropic drugs were significantly more likely to be incarcerated than men with mental health histories who did not rely on Medicaid for access to mental health medications. Appendix Figure A14 classifies incarcerations by offense type and shows that men relying on Medicaid for medications were more likely to commit both violent and property offenses than men who did not file claims for psychotropic drugs. If the self-medication channel were driving the observed increase in crime, we might have expected to find a larger treatment effect for drug-related crimes among men who relied on Medicaid for psychotropic drugs; the lack of a difference in treatment effects between the two groups in this figure therefore suggests that the self-medication channel is not driving the results.

[34] There is significant overlap between men relying on Medicaid for mental health medications and those who were using behavioral health services right before their 19th birthdays: 69% of men who filed claims for psychotropic drugs between the ages of 14 and 18 also filed a mental health claim in the six quarters before their 19th birthday.

7.3 Differences by Mental Health Diagnoses

Next, I use the diagnoses codes in the Medicaid claims to study which subgroups of men are more likely to contribute to the rise in crime. I first divide treated men with mental health histories into two groups, motivated by the groupings in the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV). Specifically, I divide men into those with a diagnosed personality disorder (which includes intellectual disabilities and represents 18 percent of men with mental illness) and those without a diagnosed personality disorder. Appendix Figure A15 shows that both groups are contributing to the rise in crime.[35]

To further explore which subgroups might be driving the results, Appendix Figure A16 reproduces the main result in Figure 5 for men with a mental health history, dropping one diagnosis at a time (i.e., dropping all men ever diagnosed with a particular disorder). The results in this figure indicate that the main result is most sensitive to dropping men who have ever been diagnosed with conduct disorders, disturbances of emotions (e.g., oppositional defiant disorder), non-dependent drug abuse, and depressive disorders. These results thus indicate that men ever diagnosed with these disorders are those who are most likely to commit crime after losing access to Medicaid eligibility.[36] In other words, these findings suggest that Medicaid access was able to suppress these men's criminal propensities and thus was serving as as important safety net for men with these diagnoses.

Lastly, noting the importance of substance abuse in driving the main results, I split the treated men with mental illness into two groups based on whether they have ever been diagnosed with alcohol or drug dependence or abuse (28 percent of men with mental health histories). I then consider the effect of the Medicaid loss on drug-related offenses. Appendix Figure A17 shows that men with mental health histories who have been diagnosed with substance abuse are more likely to be incarcerated for drug-related offenses after losing Medicaid eligibility than men with mental illness who were never diagnosed with a substance abuse disorder.

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[35] Men ever diagnosed with intellectual disabilities make up 90% of the group with personality disorders. However, it is worth noting that many individuals in this group have also been diagnosed with other disorders (e.g., 27% and 45% have been diagnosed with conduct and emotional disorders, respectively).

[36] It is important to note that many of these disorders are co-occurring. For example, 24% and 40% of men with conduct and emotional disorders have been diagnosed with substance abuse at some point before age 19, respectively. Because of the co-occurrence of diagnoses and the prevalence of hyperkinetic syndrome of childhood (i.e., ADHD) in this sample, panel (u) in this figure drops men who have this disorder as their only diagnosis.

8 Cost Effectiveness of Providing Medicaid Eligibility

The results up to this point suggest that providing low-income young men with access to Medicaid services would decrease their criminal involvement. In this section, I put the causal estimates into context by considering the cost effectiveness of providing Medicaid coverage. In particular, I compare the cost of providing insurance access with the associated benefits, which include lower social costs from fewer victimizations as well as reduced fiscal and social costs from fewer incarcerations.[37] In this section, I summarize the approach and estimates, but I refer the reader to Appendix C for a more-detailed discussion.

8.1 Costs of Expanding Medicaid Eligibility

First, I calculate the cost of providing a cohort of low-income young men in South Carolina with Medicaid eligibility until their 21st birthdays—as opposed to until their 19th birthdays—and conclude that this cost would amount to roughly $15 million. This cost is a function of the estimated take-up of the program (calculated based on these men's enrollment patterns prior to age 19) as well as the average annual spending per full-benefit enrollee in South Carolina among children ages 0–18 (Young et al. 2015). Because the income eligibility thresholds are being held constant in this scenario, crowding out of private insurance coverage is less of a concern.

8.2 Benefits of Expanding Medicaid Eligibility

To estimate the social benefits accrued as a result of fewer criminal victimizations, I first summarize the effect of the Medicaid loss on public safety using the following regression equation:

$$Y_{it} = \beta_0(\text{Treat}_i \times \text{Post}_\tau) + \beta_1 \text{Post}_\tau + \beta_2 \text{Treat}_i + \delta_t + \varepsilon_{it} \tag{5}$$

This specification is almost identical to equation (1), except that it measures Medicaid's effect on serious arrests (as opposed to on a person's likelihood of being incarcerated). The coefficient β_0 estimates the extent to which treatment and control individuals differ in their arrest propensity in the post-period relative to the pre-period. The results are reported in Table 6.

[37] I focus exclusively on men in this analysis because providing women with Medicaid eligibility could result in additional associated benefits that I would not be taking into account (for example, in terms of educational attainment or earnings). As an example, prior research has shown that expanded access to family planning services through Medicaid have reduced teen birth rates (Kearney & Levine 2015).

Using these estimates and taking a relatively conservative approach, I find that the reduced social costs of crime are roughly $22 million. Specifically, I use the estimates from Table 6 to estimate the number of "excess" violent, property, and drug arrests that occurred as a result of the termination in Medicaid eligibility. I then use arrest-to-victimization ratios from Heckman et al. (2010) as well as the average cost per crime using estimates from Cohen & Piquero (2009) and Miller et al. (1996) to calculate the reduction in the total social costs of victimization. Appendix C discusses the sensitivity of these estimates to the average cost assigned to certain offenses (i.e., murder and drug-related offenses).

Next, I calculate the benefits from fewer incarcerations, and estimate that providing Medicaid eligibility would result in $2.4 million and $5.1 million lower fiscal and social costs, respectively. To estimate the fiscal costs, I calculate the cost of incarcerating men in South Carolina using the average sentence served for various offenses as well as the daily cost per inmate. Finally, I use the estimates from Mueller-Smith (2015) to calculate the social costs generated by prison spells in terms of economic impact (i.e., reduced employment, greater reliance on public assistance) and post-release criminal behavior.

After comparing the costs of Medicaid provision with the potential benefits generated from reduced criminal activity, I conclude that providing insurance coverage to low-income young men is a cost-effective way to reduce crime. Using moderately conservative estimates, the findings of this paper suggest that for every dollar spent on insuring low-income young men via Medicaid, society recoups $2 in social and fiscal costs. Table 7 summarizes the estimates from this analysis, showing that even in the most conservative approach, the estimated benefits of Medicaid provision outweigh the costs.

8.3 Marginal Value of Public Funds

Finally, I consider an alternative cost-benefit framework that is conducive to welfare analysis. Specifically, I calculate the marginal value of public funds (MVPF), which estimates the ratio of society's willingness to pay for the expansion of Medicaid eligibility to the net cost to the government of implementing this policy (Finkelstein & Hendren 2020). Similar to the cost-benefit exercise above, I construct an upper and a lower bound for the ratio. I find that the MVPF of expanding Medicaid eligibility for two years is between 1.77 and 14.96. The value of the ratio is sensitive to the average cost assigned to crime and to assumptions about which party bears the economic incidence of uncompensated care (Finkelstein et al. 2019).

One advantage of calculating this ratio is that it can be compared to the MVPF of other policy changes, thereby shedding light on its relative cost effectiveness. The lower bound of the MVPF is close to the MVPFs for policies targeting adults, and the upper bound is closer to the ratios found for direct investments in low-income children's health and education (Hendren & Sprung-Keyser 2020). Given the age range and socioeconomic status of this population, it seems reasonable to think that this policy's MVPF would likely fall between these two categories. Overall, though, the findings from this exercise confirm that $1 of spending on this policy delivers more than $1 in benefits to its beneficiaries.

9 Comparing Medicaid Provision to Longer Punishments

In this section, I compare the cost effectiveness of providing low-income young men with Medicaid eligibility with the cost effectiveness of harsher criminal sanctions, which have been a favored approach for reducing crime for the past fifty years. To make this comparison, I need to calculate the extent to which sentence lengths would need to increase in order to achieve the same reduction in crime as providing Medicaid eligibility. This elasticity can be defined as:

$$\varepsilon_{c,f} = \frac{\%\Delta\text{Crime}}{\%\Delta\text{Punishment}}$$

Following prior studies in the literature, I use the sample of low-income men in South Carolina to estimate the degree to which adolescents are deterred from engaging in criminal activity upon reaching the age of criminal majority (Hjalmarsson 2009, Lee & McCrary 2017). In South Carolina, when individuals are charged with a crime before their 17th birthday, their case is typically handled by the Department of Juvenile Justice. If the offense is committed on or after their 17th birthday, the case is handled by the adult criminal justice system, which typically sentences individuals to significantly longer prison spells.

9.1 Empirical Approach for Estimating Deterrence

In order to estimate the deterrence effect of harsher sanctions, I compare men's likelihood of committing a felony before and after their 17th birthdays using data from both the Department of Juvenile Justice and the South Carolina Law Enforcement Division. I estimate this likelihood for all of the men in the low-income sample for whom I have an available birth month (i.e., ever enrolled in Medicaid) and who had not committed a felony prior to age 16 (66 percent of the full

sample, $N = 72,939$).[38] Any estimated discontinuity at age 17 captures the change in the likelihood of committing a first felony offense upon reaching the age of criminal majority. For more details on the sample and variable construction, I refer the reader to Appendix D.

I follow the approach in Lee & McCrary (2017) and calculate the number of individuals arrested for a felony in a given month as a share of those who are still at risk of committing a felony. In practice, I construct an unbalanced panel of individuals to calculate the hazard of a felony arrest in a given month, for the 12 months before and after an individual's 17th birthday. I then summarize these averages and the corresponding discontinuity with the following logit specification:

$$
\begin{aligned}
P(Y_{it} \mid D_t, X_t) &= F(\alpha X_t' + D_t \theta) \\
\text{where} \quad X_t' &= (1, (t-t_0), (t-t_0)^2, (t-t_0)^3) \\
\text{and} \quad F(z) &= \frac{\exp(z)}{1+\exp(z)}
\end{aligned}
\tag{6}
$$

The outcome Y_{it} is an indicator variable for a felony arrest for person i in time t, and t_0 is the month of the individual's 17th birthday. The indicator variable D_t is equal to 1 if $t \geq t_0$ and 0 otherwise.

The parameter θ represents the discontinuous change in the log-odds of committing a felony offense upon reaching the age of criminal majority. In order to interpret θ as the deterrence effect of harsher sanctions, the main assumption is that other determinants of criminal propensity were not changing discontinuously at age 17. Unlike Lee & McCrary (2017), the age of criminal majority in South Carolina coincides with the age at which individuals can legally drop out of school. After presenting the main results, I discuss this potential confounder and provide evidence showing that the minimum dropout age is most likely not confounding the estimate of θ.

9.2 No Evidence of Deterrence

Figure 8 shows the hazard rates of a felony arrest around age 17, suggesting that low-income adolescents are equally likely to commit felony offenses upon reaching the age of criminal majority. The circles show the share arrested for a felony in that month as a share of individuals who had

[38] There are three advantages to studying this group of adolescents using these data. First, the combination of juvenile and adult arrest records circumvents the fact that crime is under-reported at relatively high rates for juveniles in adult arrest records (Arora 2019). Second, individuals are selected into the full sample based on their public school enrollment, as opposed to on their past criminal involvement. Finally, because these individuals have not yet committed any felony offenses, the estimates are less likely to be confounded by any determinants of recidivism.

not yet been arrested for a felony. The solid lines plot predicted probabilities of arrest based on estimates using equation (6). The estimated discontinuity θ is small and statistically insignificant, showing little indication of a systemic drop in felony arrests upon reaching age 17.[39] The bottom panel of this figure displays the same deterrence estimates after splitting the sample by an individual's mental health history. The findings are suggestive of teens with mental health histories being even less deterred by harsher punishments than teens without mental health histories.

I then use these estimates to calculate the corresponding elasticity of criminal behavior to sentence lengths (summarized in Table 8). Specifically, I use the discontinuity estimate from the full sample and the smallest negative effect consistent with the confidence intervals to conclude that at most, there was a 14 percent decline in the probability of a felony arrest (after dividing the most negative marginal effect estimate by the age-16 average). With respect to the percent change in sentence lengths, calculations detailed in Appendix D suggest that the expected sentence length rose by approximately 1,550 percent (i.e., from 96 days in juvenile detention to 4.5 years in adult prison). These estimates therefore imply an elasticity of -0.009, which is very close to Lee & McCrary (2017)'s reduced-form elasticity of -0.007. If I use the average time served in state prison (2.3 years), then the corresponding elasticity is -0.018.

9.3 Minimum Dropout Age is not Confounding Deterrence Estimates

Because the age at which adolescents can legally drop out of school coincides with the age of criminal majority, there might be concern that the deterrence estimate is combining the negative effect of the harsher punishments with the effect of adolescents dropping out of school upon reaching age 17. In principle, one might think that dropping out of school has a positive effect on the likelihood that an adolescent engages in crime, primarily because schools "incapacitate" would-be offenders (see e.g., Anderson 2014, Berthelon & Kruger 2011, Fischer & Argyle 2018).[40]

To test for the presence of this "incapacitation" effect in this setting, I first estimate men's likelihood of being arrested for a felony during each month of the calendar year. Appendix Figure

[39] Appendix Figure A18 shows the likelihood of a felony arrest around age 17 using the approach outlined in Section 6.3.2 (i.e., using shares instead of hazard rates). The statistically insignificant estimate from this figure confirms that individuals are equally likely to commit a felony offense upon reaching the age of criminal majority.

[40] Increased educational attainment has also been shown to reduce an individual's likelihood of incarceration and arrest in the long run (Lochner & Moretti 2004). Cook & Kang (2016) also find that individuals who drop out of high school are more likely to commit crimes in the subsequent years. Interestingly, the authors find that the increase in adult crime is concentrated at age 19 (and not at ages 17 or 18 immediately after dropout).

A19 displays the graphical results and Appendix Table A6 summarizes the estimates, showing that adolescents in South Carolina are *not* arrested more often during summer months for felony offenses. If anything, these teens are more likely to be arrested for felonies during the school year. These estimates thus indicate that at least for individuals in this sample, schools do not play a significant incapacitation role.

Second, I leverage the seasonality of dropout: Appendix Figure A20 shows that teens are significantly more likely to drop out of school in the beginning of the school year. Individuals born in the first half of the year are therefore less likely to drop out immediately after turning 17 than individuals born in the second half of the school year (Appendix Figure A21). If dropout is contaminating the estimates of deterrence, then we would expect the estimate of θ for teens born in the second half of the calendar year to be more confounded (i.e., less negative) than the estimate for individuals born earlier in the year. Appendix Figure A22 shows that this pattern is not present in the data, providing another piece of evidence against the idea that the minimum dropout age is contaminating the deterrence estimates.

Finally, I utilize the fact that the share of students dropping out of school is declining with time in this sample in order to gauge the extent to which the deterrence estimate is confounded by dropout. In other words, if dropout is contaminating the estimate and the share of students dropping out is declining with time, then we should see θ become less confounded (i.e., more negative) with time as well. The first panel of Appendix Figure A23 illustrates that the share of students dropping out decreases with time: the share of teens enrolled in school at age 17 is 12 percentage points higher for the 1997–1999 birth cohorts than for the 1990–1992 birth cohorts. The remaining panels illustrate that despite this large decline in the likelihood of dropout, the deterrence estimates do not fall monotonically with time, remaining both close to zero and statistically insignificant.

Overall, the results from these three exercises suggest that the deterrence estimates are not confounded by students dropping out of school and committing more crime upon reaching age 17. However, I still use individuals born in the 1997–1999 birth cohorts (i.e., the group that is least likely to drop out of school) to calculate the most conservative elasticity available in this sample. Using the most negative effect consistent with the confidence intervals, I estimate an elasticity of crime to sentence lengths of -0.019 (or -0.037 using the average sentence served).[41]

[41] Note that the most negative effect consistent with the confidence intervals here will also be extra negative because of the lower levels of precision stemming from only using three birth cohorts, as opposed to the full sample.

9.4 Cost-Comparison for 10% Crime Reduction

I conclude this section by using the estimated elasticities of crime with respect to sentence lengths to compare the cost effectiveness of two crime-reduction approaches: providing Medicaid eligibility and increasing sentence lengths. The estimates in Table 6 imply that providing Medicaid eligibility to low-income young men would reduce crime by 10 percent. I thus compare the cost of each policy for achieving this same reduction in crime.[42] I summarize the estimates here, but refer the reader to Appendix E for a more-detailed discussion.[43]

First, I find that the total cost of reducing crime by 10 percent via Medicaid totals $77 million. This cost includes the total cost of providing Medicaid coverage to low-income young men (which I calculated and discussed in Section 8 and totals $15 million) as well as the cost of incarcerating men who were *not* deterred from committing crime for the current length of sentences (totaling $20.6 million). Two other components are included in this figure: the fiscal costs of recidivism ($4.6 million) as well as the social costs of victimizations ($36.6 million) arising from the fact that a share of these men will likely re-offend after serving the relatively shorter sentences.

Alternatively, I find that the total cost of reducing crime by 10 percent through longer sentence lengths totals $109.6 million, which is roughly 45 percent higher than the cost of Medicaid provision. In order to calculate this cost, I use the preferred estimate of the elasticity of crime with respect to sentence lengths ($\varepsilon_{c,f} = -0.018$), which implies that sentences would need to be increased by roughly 570 percent in order to achieve a 10 percent reduction in crime. In order for these two policies to have the same overall cost, $\varepsilon_{c,f}$ would need to be roughly two times higher (i.e., -0.032). Overall, this comparison allows me to conclude that low-income young men would need to be significantly more responsive to changes in expected sentence lengths in order for harsher punishments to be as cost effective as healthcare provision.

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[42] The estimate in column 1 of Table 6 suggests that among men born between 1990–1993, there were roughly 403 excess serious arrests (21,455 $\times$ 0.00235 $\times$ 8 quarters). I then use the raw data to calculate that there were 3,918 serious arrests among 19- and 20-year olds born in these cohorts, which implies that serious crime would have been 10 percent lower if Medicaid eligibility had not suddenly expired at age 19.

[43] Appendix E also compares the cost of healthcare provision to the cost of hiring more police officers. I find that hiring police officers is potentially more cost effective as a crime-reduction approach, but the comparison is sensitive to the social cost assigned to violent crime and to the degree to which hiring police officers reduces violent crime among individuals in older age groups. I also note that one important caveat to this finding is that the comparison does not account for the social costs of policing.

10 Discussion & Conclusion

Motivated by the prevalence of mental illness among the criminal justice population, this paper studies the potential for mental healthcare to serve as a crime-reduction policy. To estimate the causal effect of health insurance on crime, I leverage a discrete break in Medicaid coverage that occurs on an individual's 19th birthday and employ a matched difference-in-differences research design. Importantly, I use rich administrative data linked across various state agencies in South Carolina, which allows me to identify individuals with mental health histories and measure any contact these men have with the criminal justice system.

I find that men who lost access to Medicaid eligibility on their 19th birthdays are more likely to be incarcerated in the subsequent two years than otherwise comparable low-income men who were likely eligible, but not enrolled in Medicaid. I show that these effects are driven entirely by men with mental health histories, and I find increases in violent, drug, and property crimes, suggesting that losing access to mental healthcare impacts all types of criminal involvement. Moreover, using detailed information from insurance claims, I find that the effects are particularly pronounced for men who were using behavioral health services right before the loss in eligibility and for men who relied on Medicaid for access to mental health medications.

The findings of this study offer a number of takeaways and policy implications. First, this paper studies a subpopulation of offenders—low-income young men with mental health histories—for whom access to healthcare serves as an effective deterrence mechanism. The cost effectiveness of Medicaid provision thus suggests that policymakers might consider improving access to healthcare as one of the tools in their arsenal for reducing crime and decreasing criminal justice expenditures. Moreover, to the extent that mental healthcare improves an individual's decision making, then it may be the case that providing mental healthcare might make traditional, incentive-based strategies for deterring crime more effective.

Second, the increase in criminal activity that follows the sudden termination of Medicaid coverage illustrates the social costs of having a fragmented social safety net. The results of this study should thus inform the future design of public insurance eligibility rules and of social insurance programs more broadly. Finally, this study quantifies the social returns of healthcare provision for low-income young adults, a group that is relatively likely to be uninsured. Policymakers should incorporate these findings into their valuations of the Medicaid program and when weighing the costs and benefits of expanding health insurance access to this population.

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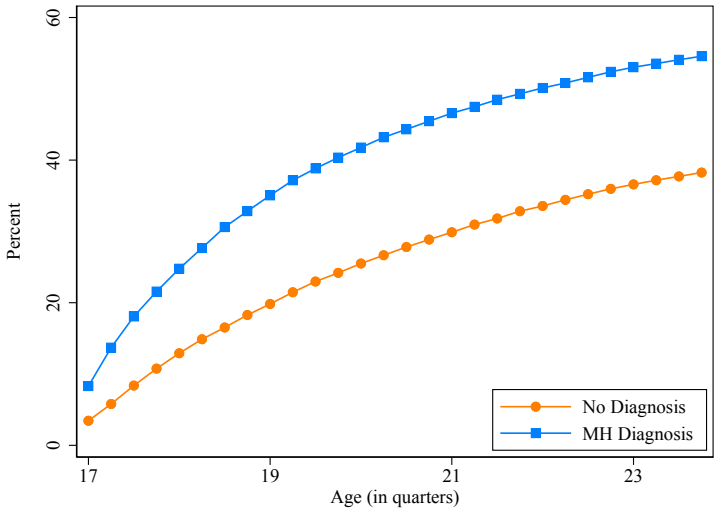
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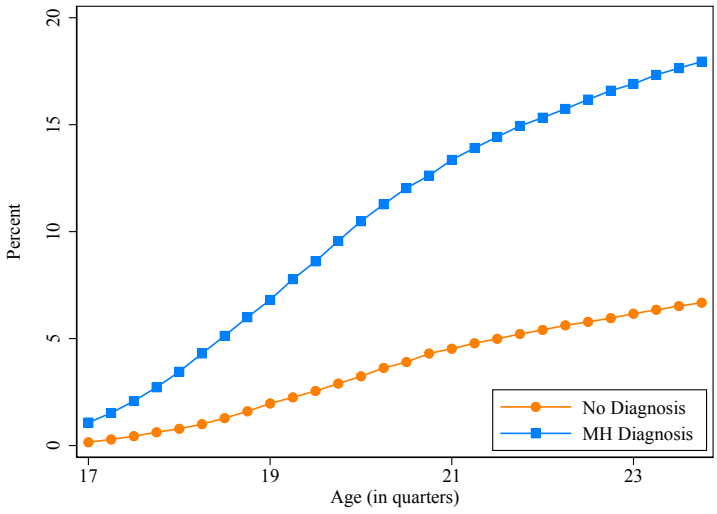
Figures and Tables

Figure 1: Criminal Activity, by Mental Health Diagnosis Earlier in Adolescence

(a) Ever Arrested

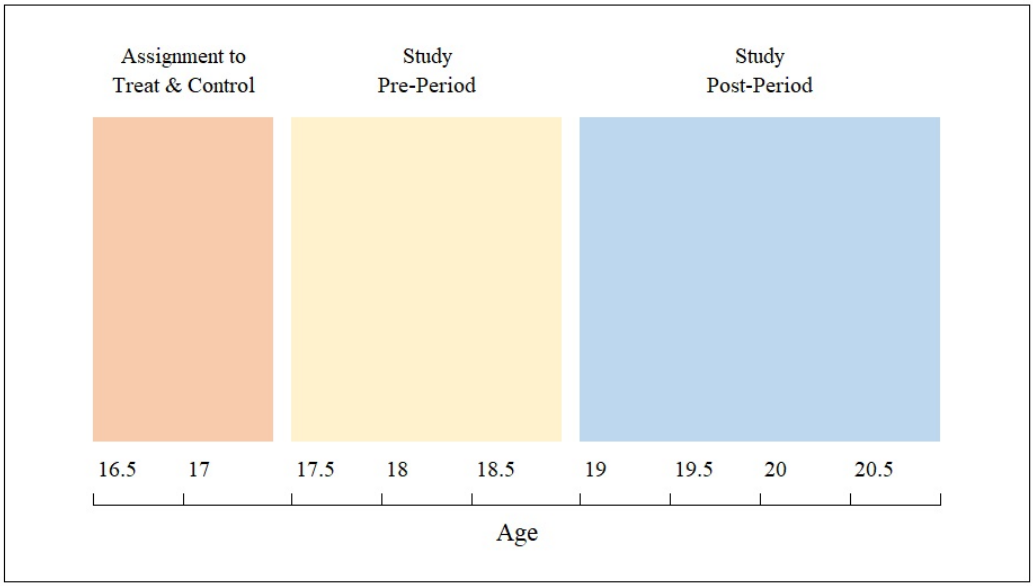


(b) Ever Incarcerated



NOTE: These figures plot the share of each group that has ever been arrested or incarcerated at a given age in South Carolina. "Mental health diagnosis" refers to having a Medicaid claim with a mental health diagnosis before the age of 17. The sample consists of male individuals in the 1990–1993 birth cohorts who were enrolled in Medicaid at some point between ages 10–18. "Arrested" refers to having an arrest in the South Carolina Law Enforcement Division data. "Incarcerated" refers to either having an arrest that resulted in being detained in an adult correctional facility in the Law Enforcement Division data or having an incarceration spell in the Department of Corrections data.

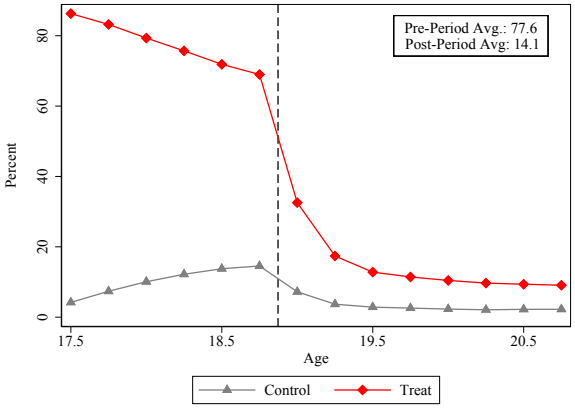
Figure 2: Timeline of Empirical Strategy



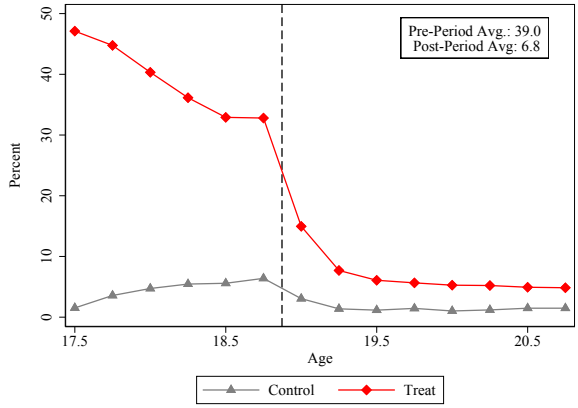
NOTE: Individuals are assigned to the treatment and control groups based on their enrollment in the Medicaid program between ages $16\frac{1}{2}$ and $17\frac{1}{2}$ (orange). Their outcomes are then allowed to evolve naturally in the study pre-period (yellow) and post-period (blue).

Figure 3: Share of Matched Treated and Control Individuals
Enrolled in Medicaid and Filing Claims (Raw Means)

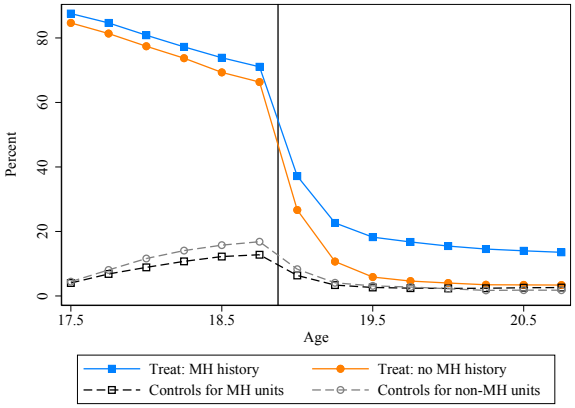
(a) Enrolled



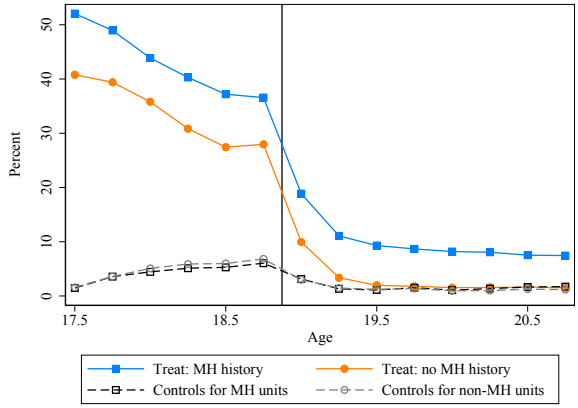
(b) Filing any claims



(c) Enrolled



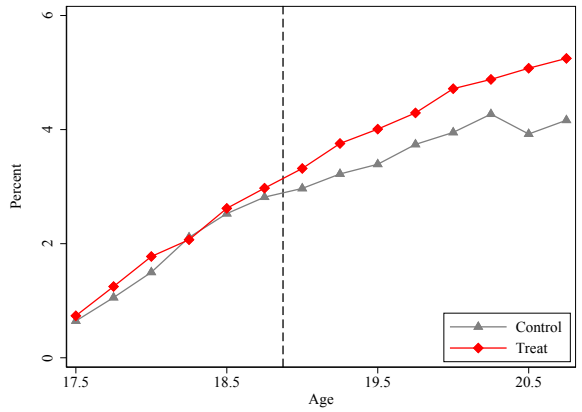
(d) Filing any claims



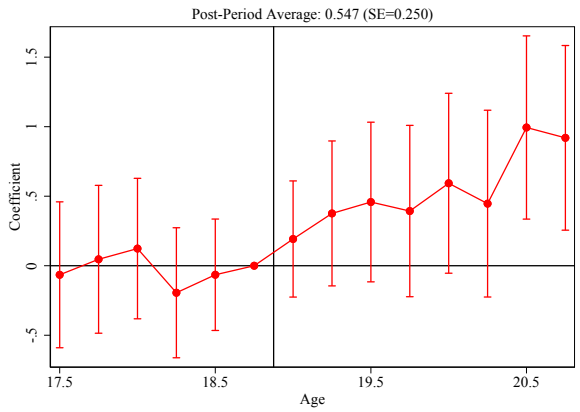
NOTE: These graphs use the sample of matched treated and control men to plot the raw averages of Medicaid enrollment and of filing any Medicaid claims in a given quarter. The first two panels consider the full sample. The averages reported in each figure correspond to the pre- and post-period shares of treated individuals. The second two panels divide the sample by mental health history (prior to age 16).

Figure 4: Likelihood of Incarceration in Full Sample
(Raw Means & Treat-Control Differences)

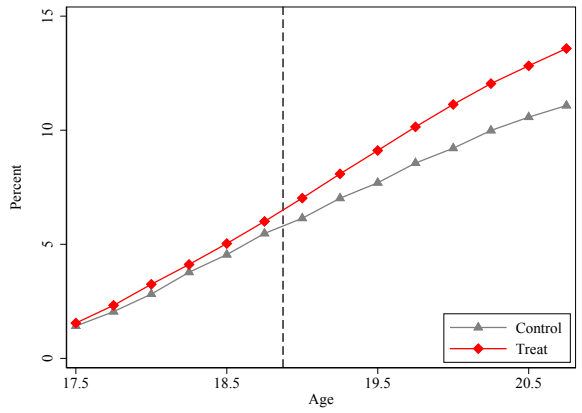
(a) Incarcerated that quarter



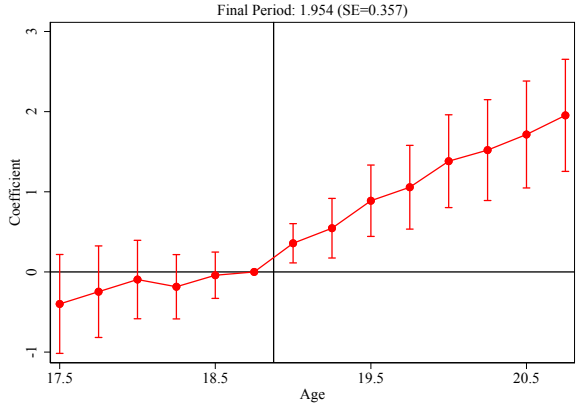
(b) Incarcerated that quarter



(c) Ever incarcerated



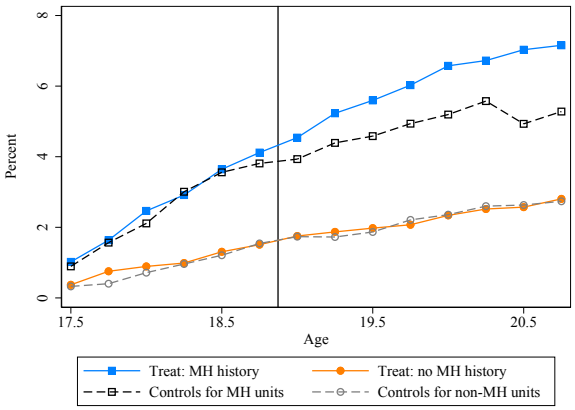
(d) Ever incarcerated



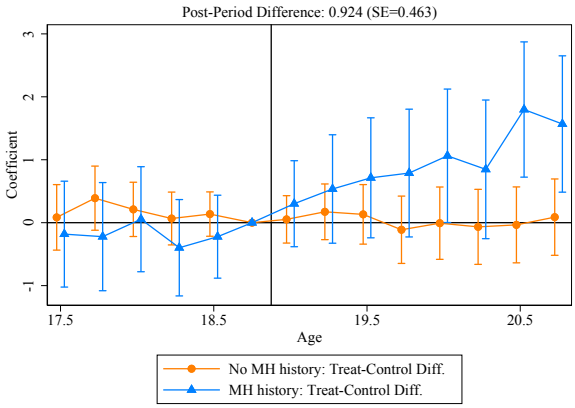
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the raw means of the matched sample. Panels (b) and (d) plot the treat-control differences estimated using equation (1). "Post-Period Average" and "Final Period" refer to the average of the post-period β_τ coefficients or to β_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 5: Likelihood of Incarceration, by Mental Health History
(Raw Means & Treat-Control Differences)

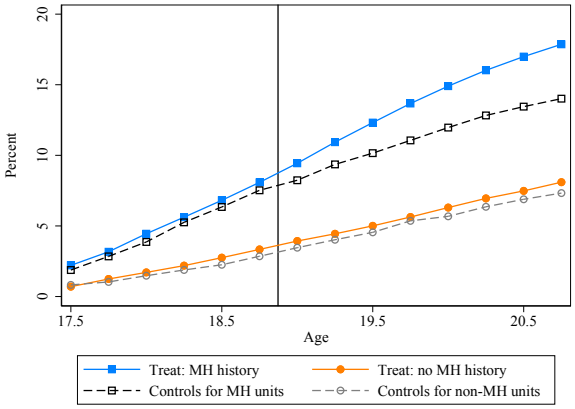
(a) Incarcerated that quarter



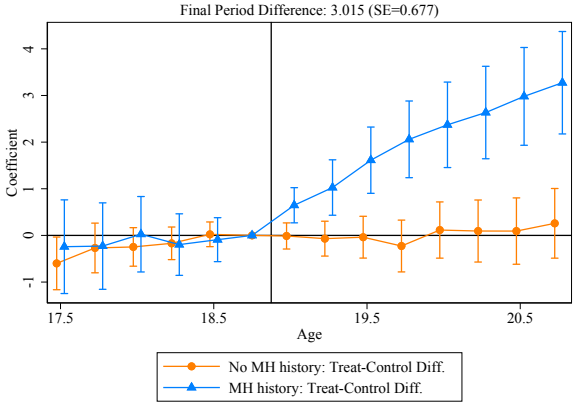
(b) Incarcerated that quarter



(c) Ever incarcerated



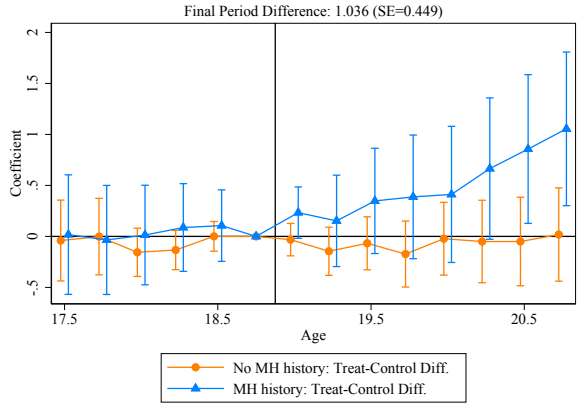
(d) Ever incarcerated



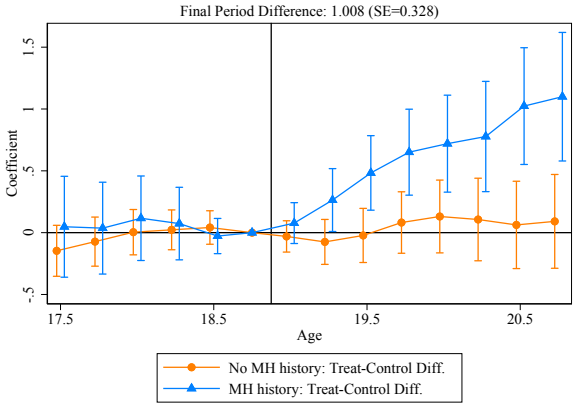
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the raw means of the matched sample separately by mental health history. Panels (b) and (d) plot the treat-control differences estimated using equation (2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 6: Likelihood of Ever Being Incarcerated, by Mental Health History and Offense Type

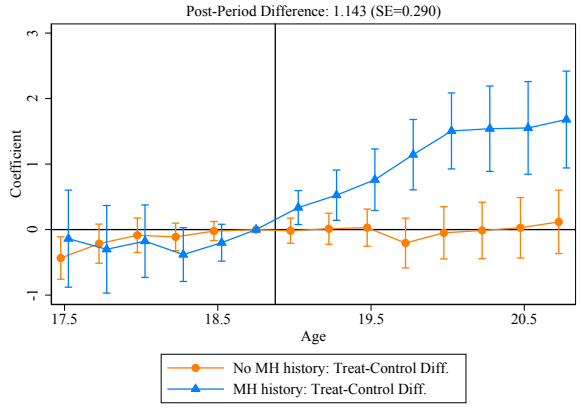
(a) Violent Offenses



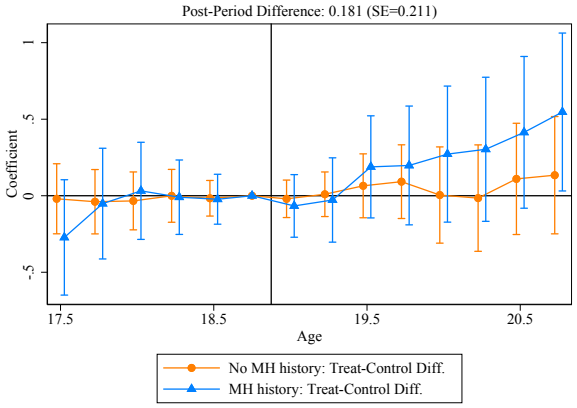
(b) Drug-Related Offenses



(c) Property Offenses



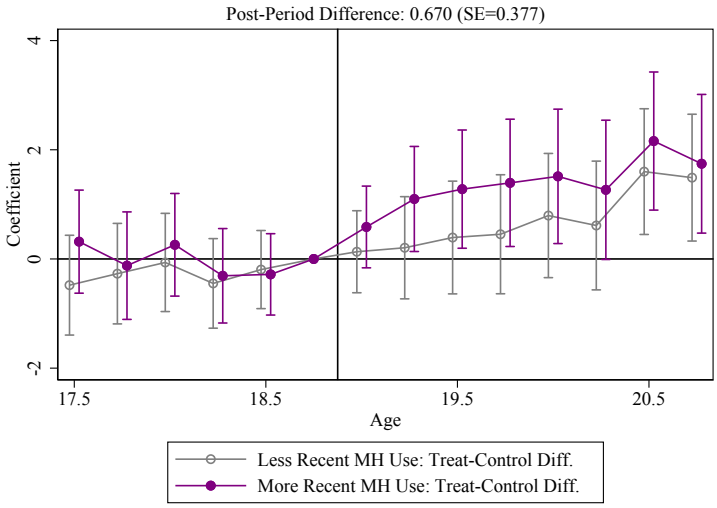
(d) Miscellaneous Offenses



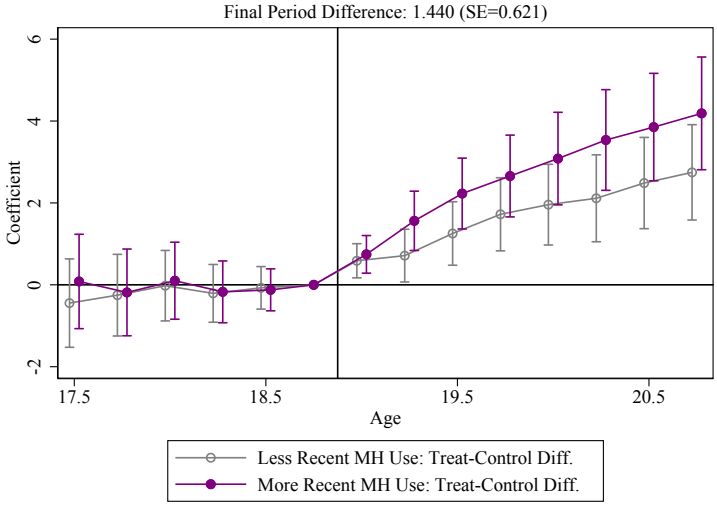
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures plot the treat-control differences estimated using equation (2). "Final Period" refers to the α_8 estimate in that specification. Standard errors are clustered at the individual level.

Figure 7: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization

(a) Incarcerated that quarter



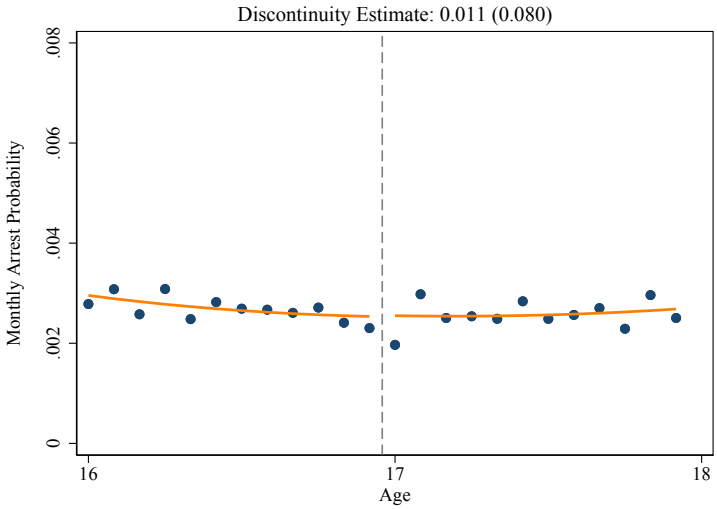
(b) Ever incarcerated



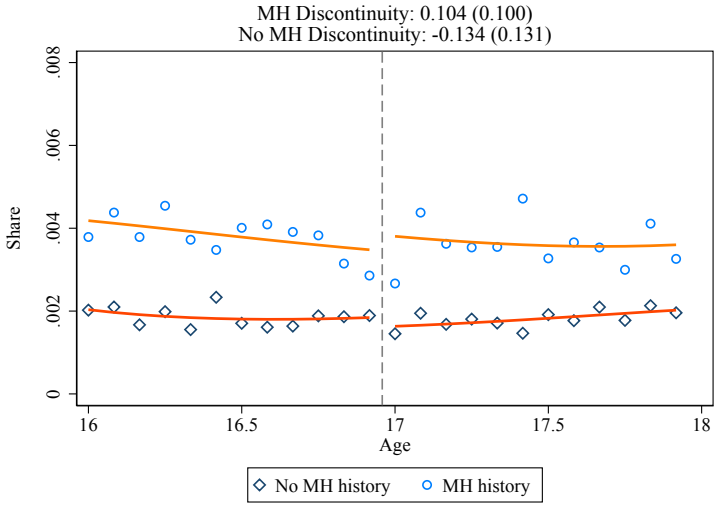
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who were recently using mental health services (in purple) and for men who were less recently using mental health services (in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. Recent usage is defined as filing a mental health claim during the study pre-period. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 8: Felony Propensity Estimates Around the Age of Criminal Majority

(a) Full Sample



(b) By Mental Health History



NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The bottom panel splits the sample based on an individual's mental health history prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Table 1: Summary Statistics of Full and Matched Sample

	(1) All men	(2) Medicaid men	(3) Treated men	(4) Treated men with mental health histories
Black	0.61	0.70	0.72	0.70
Medicaid (ever)	0.67	1.00	1.00	1.00
Medicaid (years)	4.49	6.69	7.80	8.32
Mental health claim (pre 16)	0.34	0.51	0.56	1.00
Juvenile justice referral (ever)	0.30	0.38	0.41	0.52
Arrests (ever)	0.20	0.25	0.27	0.33
SNAP (ever)	0.63	0.84	0.89	0.89
SNAP (years)	3.61	5.02	5.75	5.97
Observations	46,990	31,533	21,455	12,046

NOTE: All characteristics are computed between the ages of 10 and 18 (with the exception of having a mental health claim, which is measured between the ages of 10 and 15). "Ever" refers to the individual ever being enrolled or experiencing that outcome. "Years" refers to the average number of years that an individual was enrolled in Medicaid or SNAP. Columns 1 reports means for the full low-income sample born between 1990 and 1993. Column 2 reports means for all individuals in these cohorts who were enrolled in Medicaid at some point between ages 10 and 18. Columns 3 reports means for all treated individuals (i.e., individuals enrolled in Medicaid at ages 16.5–17.5). Column 4 reports means for treated individuals with a mental health history. See Table A1 for summary statistics for eligible candidates for the matching procedure.

Table 2: Effect of Medicaid Loss on Men's Likelihood of Incarceration

	Full Sample		By Mental Health History	
	Incarcerated	Ever Incarcerated	Incarcerated	Ever Incarcerated
Estimated Effect	0.547**	1.954***	0.924**	3.015***
	(0.250)	(0.357)	(0.463)	(0.677)
Control Average	3.70	11.08	4.85	14.01
Observations	425,348	425,348	425,348	425,348

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. For the full sample, the estimated effects in columns 1 and 2 correspond to the post-period average of the β_τ coefficients and to the β_8 coefficient, respectively, estimated using equation (1). For the mental health sample, the estimated effects in columns 3 and 4 correspond to the post-period average of the α_τ coefficients and to the α_8 coefficient, respectively, estimated using equation (2). Standard errors are clustered at the individual level. For the non-cumulative variables, "Post-period Control Average" refers to the post-period average for the matched control group (for the full sample in column 1 and just for those with a mental health history in column 3). For the cumulative variables, this statistic reports the value measured in the last quarter of the post period (age 20, quarter 4) for the full sample in column 1 and just for those with a mental health history in column 3.

Table 3: 2SLS Effect of Medicaid Enrollment on Men's Likelihood of Incarceration

	Full Sample	Mental Health History
Effect of Medicaid on Incarceration	-1.013*** (0.371)	-2.066*** (0.631)
Post-period Average	4.06	5.48
Observations	425,348	212,450

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports the two-stage least squares coefficient using equation (3), in which Medicaid enrollment is instrumented with a $\text{Treat}_i \times \text{Post}_\tau$ indicator variable. The first column considers the full matched sample, whereas the second column focuses on individuals with mental health histories prior to age 16. "Post-period Average" refers to the average post-period incarceration rate of the full sample in column 1 and to the average post-period incarceration rate of men with a mental health history in column 2. Standard errors are clustered at the individual level.

Table 4: Effect of Medicaid Loss on Men's Likelihood of Ever Being Incarcerated, by Mental Health History & Offense Type

	Violent	Drugs	Property	Misc.
Estimated Effect	1.036** (0.449)	1.008*** (0.328)	1.562*** (0.450)	0.413 (0.327)
Control Average	4.18	2.01	5.14	2.03
Observations	425,348	425,348	425,348	425,348

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The estimated effects correspond to the α_8 coefficient in equation (2), analogous to the estimates presented in Figured 6. "Misc." refers to miscellaneous offenses. "Post-Period Avg." refers to the average incarceration rate of men with mental health histories in the matched comparison group, measured in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level.

Table 5: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization

	Incarcerated	Ever Incarcerated				
	All	All	Violent	Drugs	Property	Misc.
Estimated Effect	0.670*	1.440**	0.075	0.077	0.976**	-0.210
	(0.377)	(0.621)	(0.398)	(0.320)	(0.443)	(0.276)
Post-Period Avg.	6.37	17.83	6.77	3.63	8.76	3.43
Treated Men	12,046	12,046	12,046	12,046	12,046	12,046

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table only considers treated men with mental health histories (and their corresponding matched control units) and splits the group based on treated men's behavioral healtchcare utilization in the study pre-period. The estimated effect in column 1 corresponds to the post-period average of the α_τ coefficients using equation (2). The estimated effects in columns 2–6 correspond to the α_8 coefficient using equation (2). "Misc." refers to miscellaneous offenses. For column 1, the post-period average reports the average incarceration rate in the post-period for treated men with a mental health history who did not use behavioral health services in the pre-period. For the cumulative variables, this statistic reports the value for this same group measured in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level.

Table 6: Effects of Medicaid Enrollment on Public Safety

	(1) All	(2) Violent	(3) Non-Violent	(4) Drugs	(5) Property	(6) Miscellaneous
Treat × Post	0.235***	0.081*	0.154**	0.094***	0.087*	0.032
	(0.075)	(0.045)	(0.063)	(0.032)	(0.050)	(0.031)
Control Average	0.98	0.32	0.67	0.18	0.40	0.18
Scaled Effect	0.24	0.26	0.23	0.52	0.22	0.18

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers the full sample and measures the effect of the loss in Medicaid eligibility on serious crime, as measured by arrests in the South Carolina Law Enforcement Division data that end with an individual being taken into custody. This table reports the Treat × Post coefficient from equation (5). "Control Average" refers to the post-period average of the control group. "Scaled Effect" is the quotient of the reduced-form estimate and the control group's post-period average. "Non-violent" refers to all drug-related, property, and miscellaneous offenses. Standard errors are clustered at the individual level.

Table 7: Summary of Costs: Providing Medicaid & Reduced Criminal Activity (In Millions of $2010)

	Estimated Cost		
	Most conservative	Moderately conservative	Least conservative
Costs			
Total program costs	$18.2	$14.6	$12.5
Benefits			
Victimization costs	$16.4	$22.3	$28.3
Fiscal costs	$2.1	$2.4	$2.6
Social costs	$4.3	$5.1	$5.8
Total	$22.8	$29.8	$36.8

NOTE: This table reports the estimates from the cost-benefit analysis in Section 8. "Most conservative" refers to the scenario in which I deliberately bias the estimates toward understating the benefits and overstating the costs. "Least conservative" refers to the scenario in which I estimate the most generous benefits and the least expensive costs. "Victimization costs" refer to the reduced social costs from fewer victimizations. "Fiscal costs" and "social costs" refer to the reduced fiscal and social costs from incarcerating fewer individuals. For a full derivation of these costs, I refer the reader to Appendix C.

Table 8: Summary of (Most Negative) Estimated Elasticities of Crime to Sentence Lengths

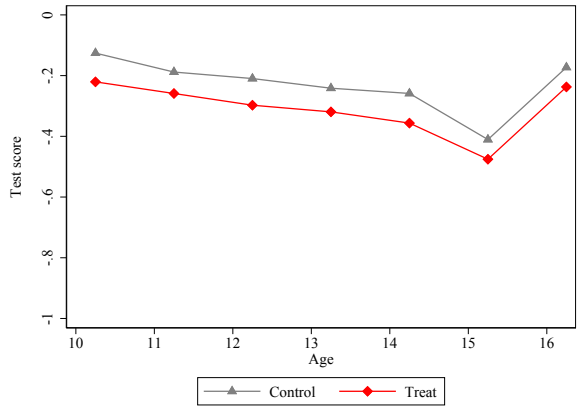
	Full Sample		1997–1999 Cohorts	
	Sentence Length	Sentence Served	Sentence Length	Sentence Served
Elasticity	-0.009	-0.018	-0.019	-0.037

NOTE: This table reports the elasticities computed in Section 9, using the most negative deterrence effect consistent with the confidence intervals. The first two columns use the full sample ($N = 72,939$), whereas the third and fourth columns focus on the three youngest birth cohorts ($N = 23,194$). Columns 1 and 3 use the average length of prison sentences (4.5 years) to calculate the elasticity. Columns 2 and 4 use the average time served in state prison (2.3 years) to estimate the elasticity. For a full derivation of these estimates, I refer the reader to Appendix D.

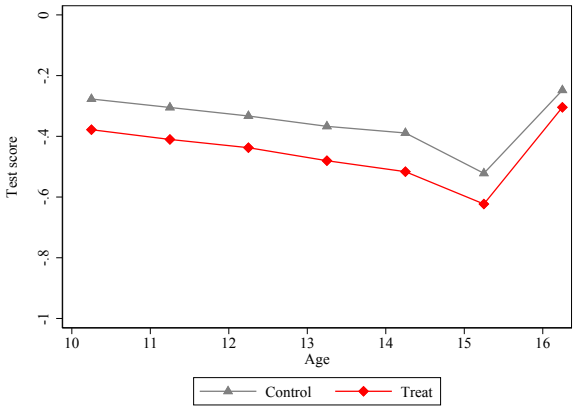
A Appendix Figures and Tables

Figure A1: Standardized Test Scores and Juvenile Justice Felony Referrals (Raw Means) For Full Sample

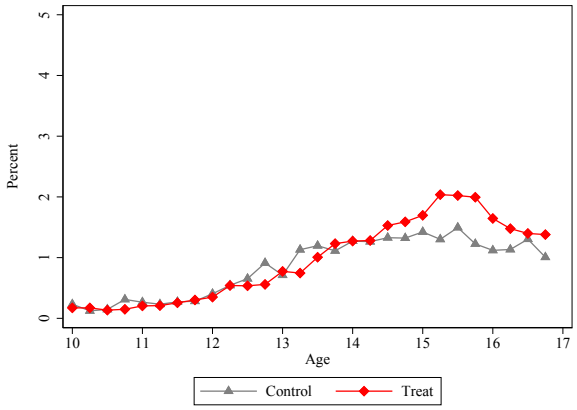
(a) Math score



(b) ELA score



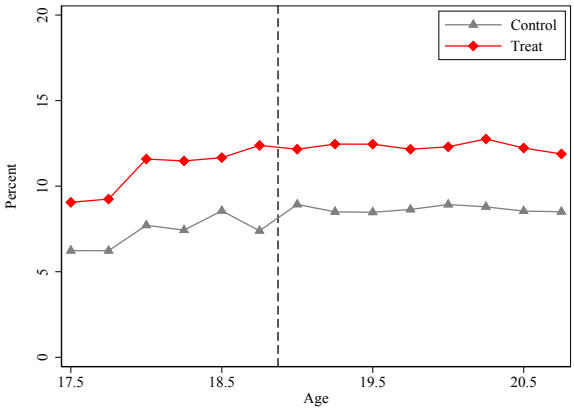
(c) DJJ referral for felony offense



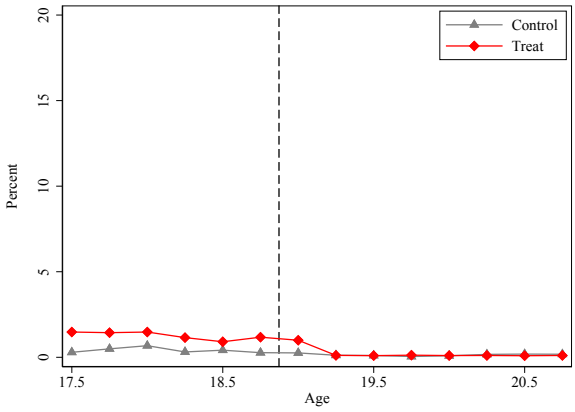
NOTE: These graphs plot the raw means of each outcome in a given year (or quarter in the third panel) for the full sample of matched treated and control men. Data on test scores comes from South Carolina's Department of Education. "ELA score" refers to test scores on English Language Arts standardized exams. Data on referrals to the juvenile justice system for felony offenses come from South Carolina's Department of Juvenile Justice.

Figure A2: Enrollment in Public Assistance, Full Sample (Raw Means)

(a) SNAP enrollment



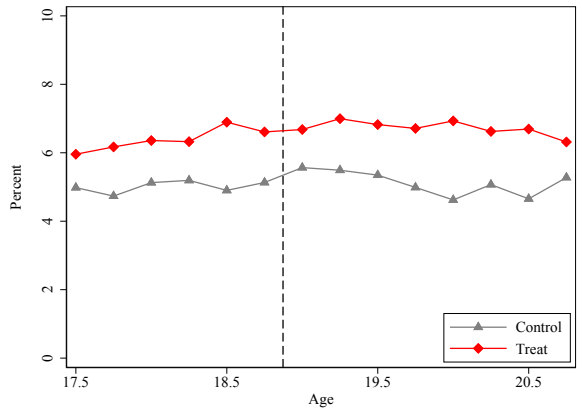
(b) TANF enrollment



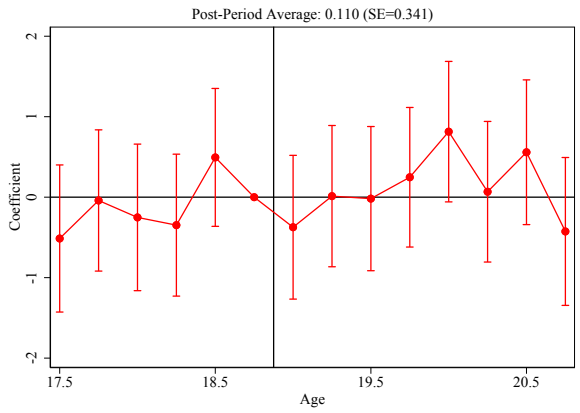
NOTE: These graphs plot the raw means of each outcome in a given quarter for the full sample of matched treated and control men. Data on program enrollment comes from South Carolina's Department Social Services. "SNAP" refers to the Supplemental Nutrition Assistance Program and "TANF" refers to the Temporary Assistance for Needy Families program.

Figure A3: Criminal Activity For Full Sample (Raw Means & Treat-Control Differences)

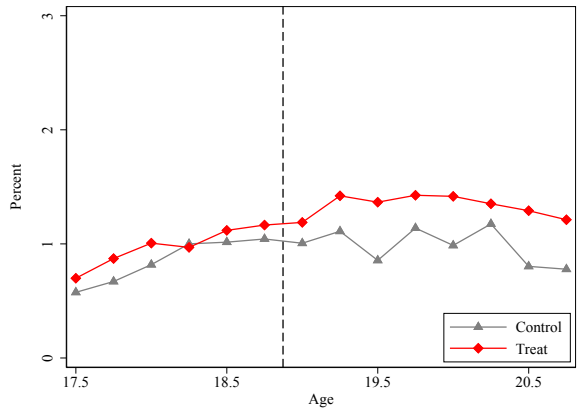
(a) All Arrests



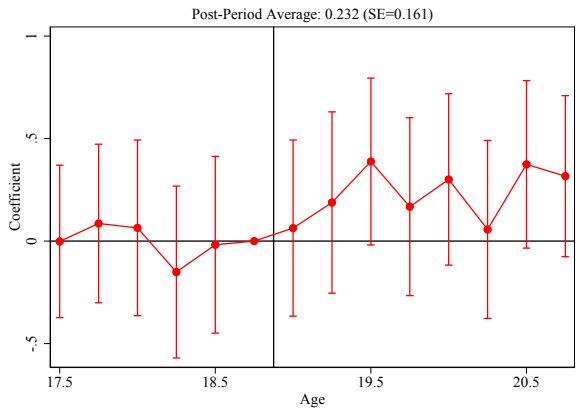
(b) All Arrests



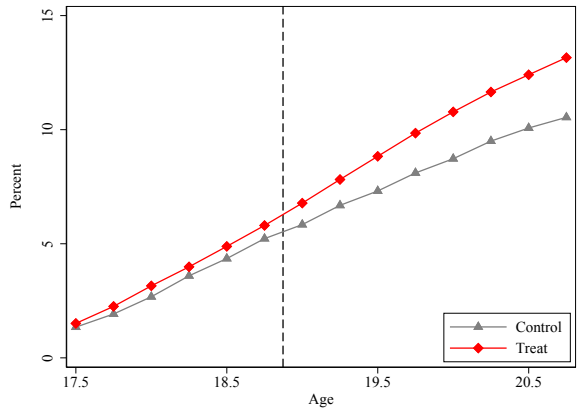
(c) Custody Arrests



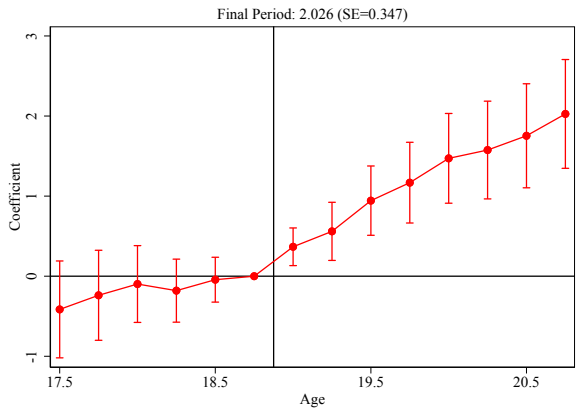
(d) Custody Arrests



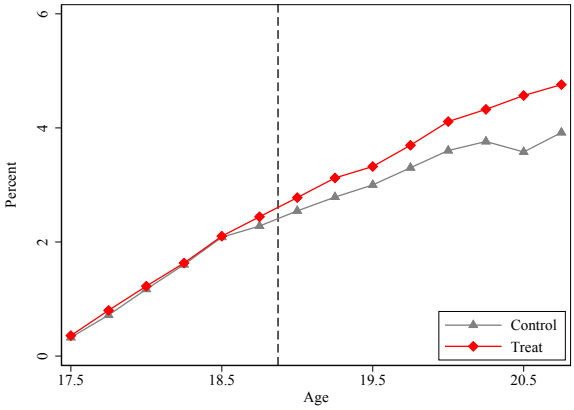
(e) Ever Taken into Custody



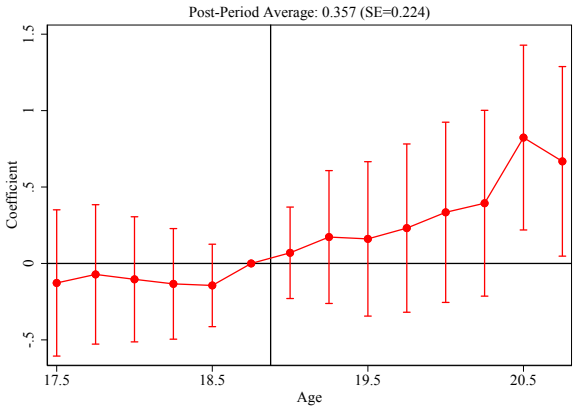
(f) Ever Taken into Custody



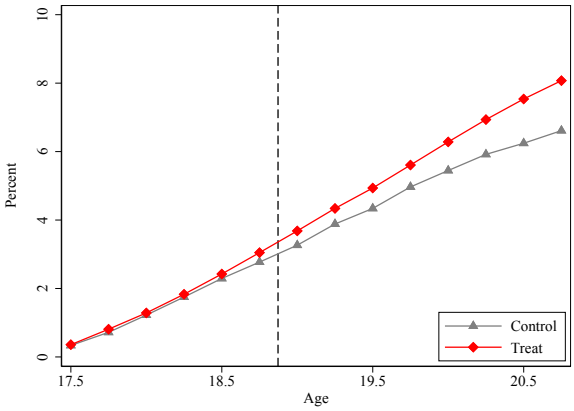
(g) Incarcerated in State Prison



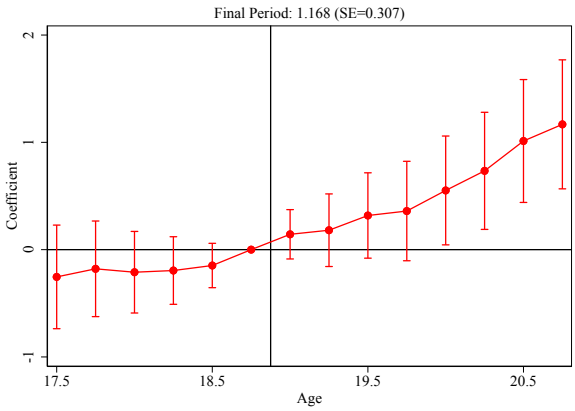
(h) Incarcerated in State Prison



(i) Ever Incarcerated, State Prison



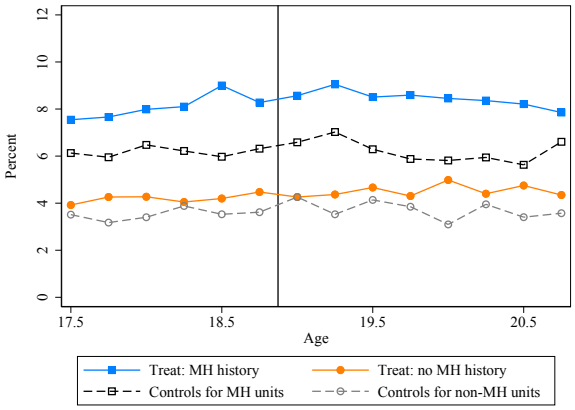
(j) Ever Incarcerated, State Prison



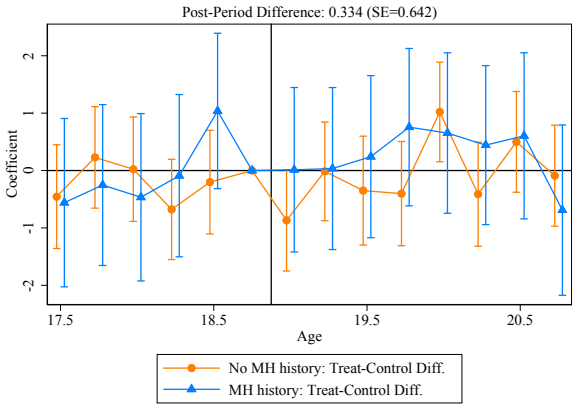
NOTE: Data on arrests and incarcerations come from the Law Enforcement Division and the Department of Corrections, respectively. The graphs on the left plot the raw means of each criminal outcome in a given quarter, and the graphs on the right plot the corresponding treat-control differences using equation (1) for the full sample. "Post-Period Average" and "Final Period" refer to the post-period average of the β_τ coefficients or to β_8, respectively. "Custody arrests" refers to arrests in which an individual was arrested and taken into custody. Standard errors are clustered at the individual level.

Figure A4: Criminal Activity, by Mental Health History
(Raw Means & Treat-Control Differences)

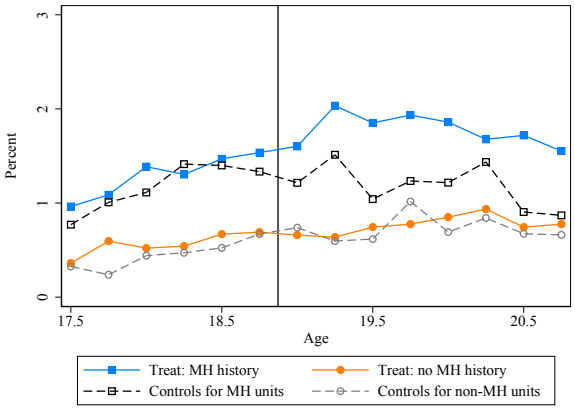
(a) All Arrests



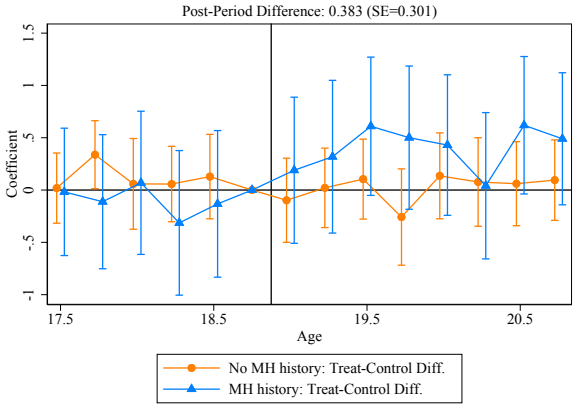
(b) All Arrests



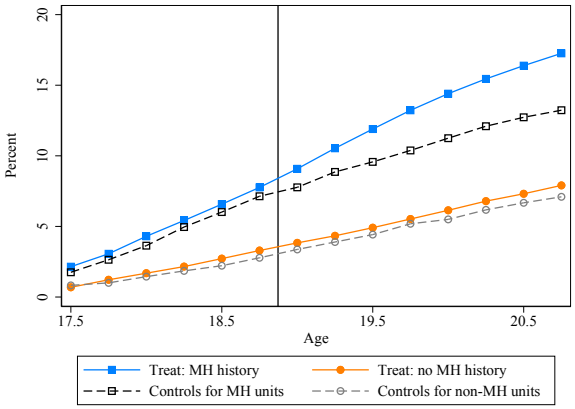
(c) Custody Arrests



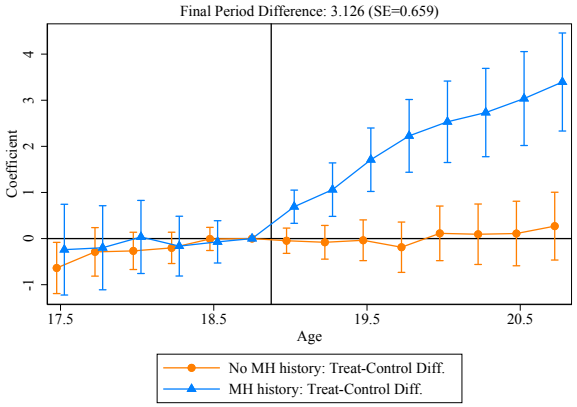
(d) Custody Arrests



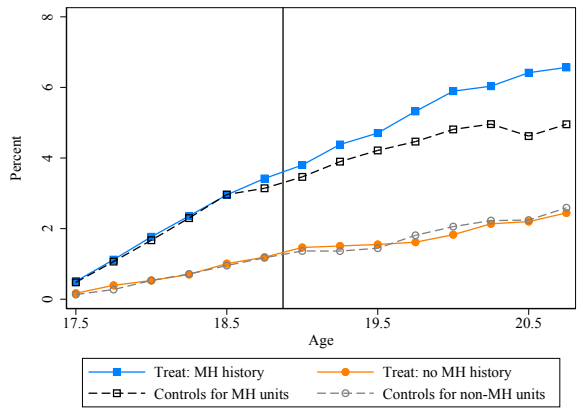
(e) Ever Taken into Custody



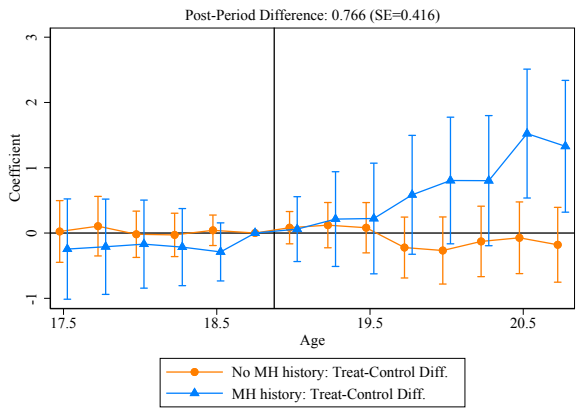
(f) Ever Taken into Custody



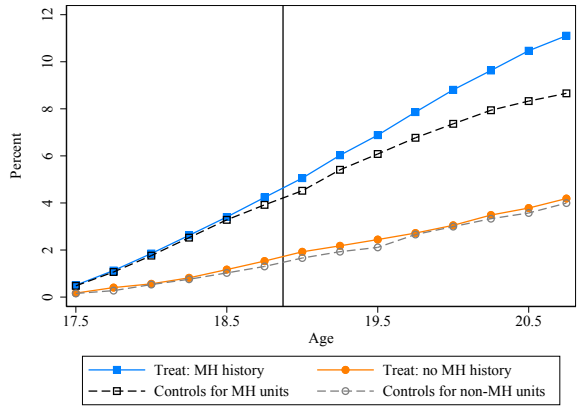
(g) Incarcerated in State Prison



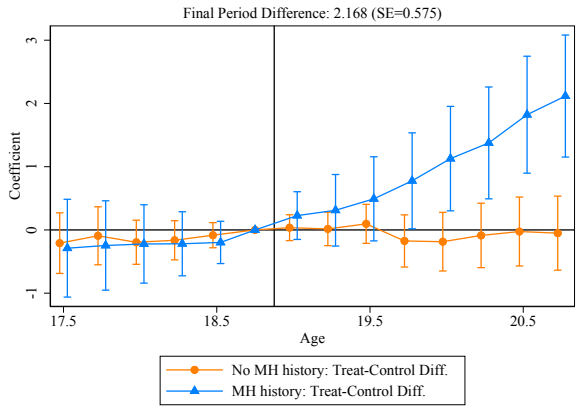
(h) Incarcerated in State Prison



(i) Ever Incarcerated, State Prison



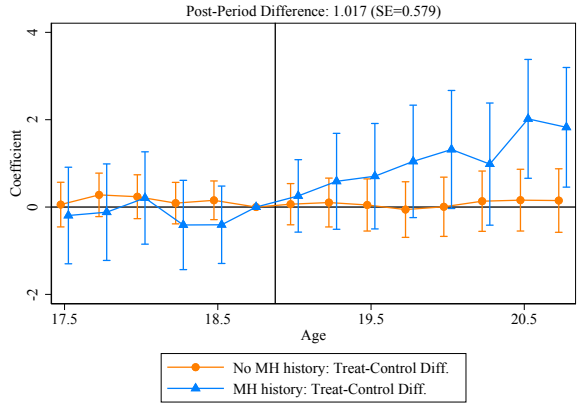
(j) Ever Incarcerated, State Prison



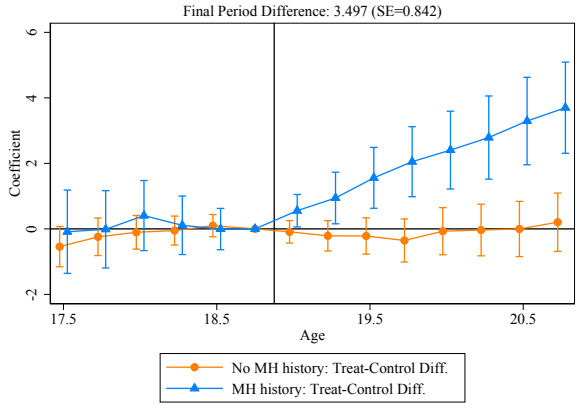
NOTE: Data on arrests and incarcerations come from the Law Enforcement Division and the Department of Corrections, respectively. The graphs on the left plot the raw means of each criminal outcome in a given quarter, and the graphs on the right plot the corresponding treat-control differences using equation (2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Custody arrests" refers to arrests in which an individual was arrested and taken into custody. Standard errors are clustered at the individual level.

Figure A5: Likelihood of Incarceration, by Mental Health History and Race

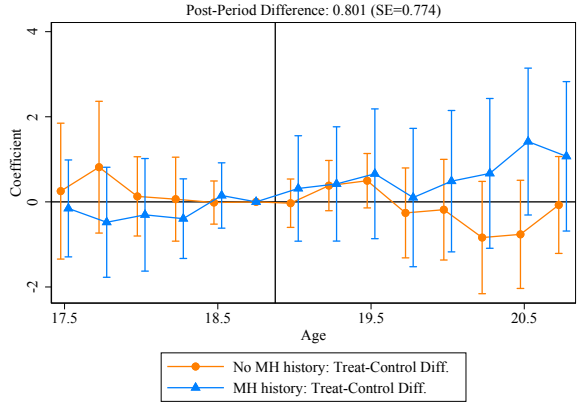
(a) Black: Incarcerated that quarter



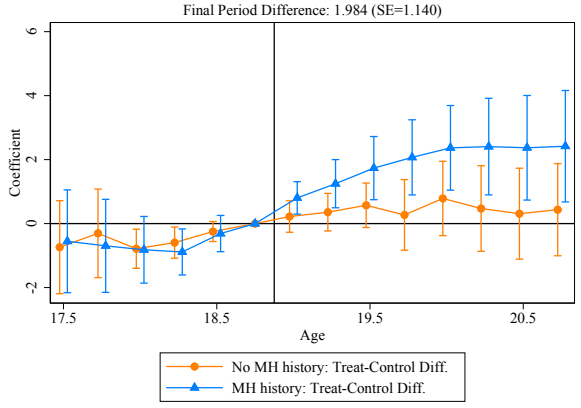
(b) Black: Ever incarcerated



(c) White: Incarcerated that quarter



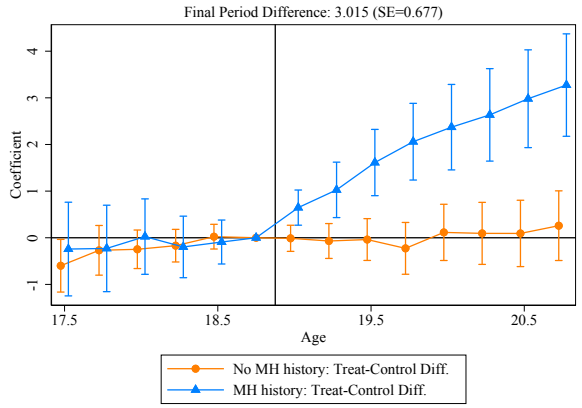
(d) White: Ever incarcerated



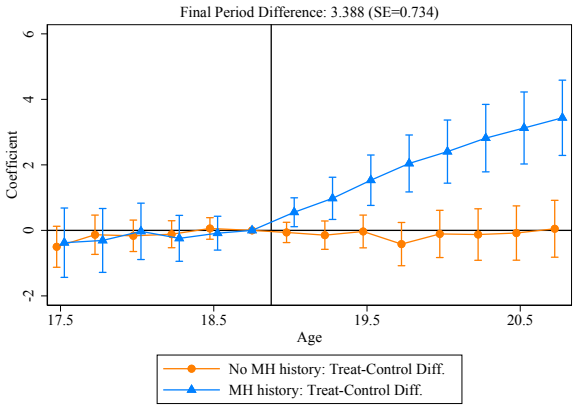
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the treat-control differences of the likelihood of being incarcerated that quarter. Panels (b) and (d) plot the treat-control differences in the cumulative likelihood of having ever been incarcerated. "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively, using equation (2). "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure A6: Robustness of Main Result to Sample and Matching Choices
(Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)

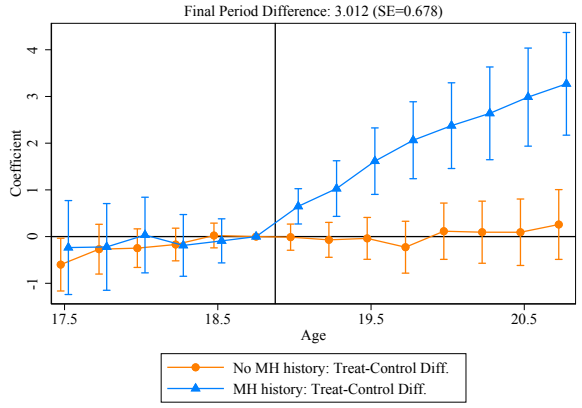
(a) Baseline



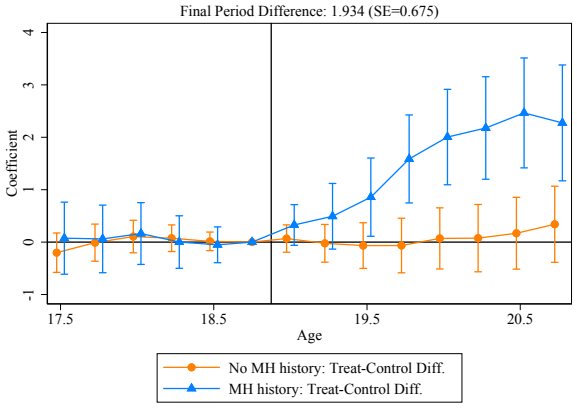
(b) Only one match per treated



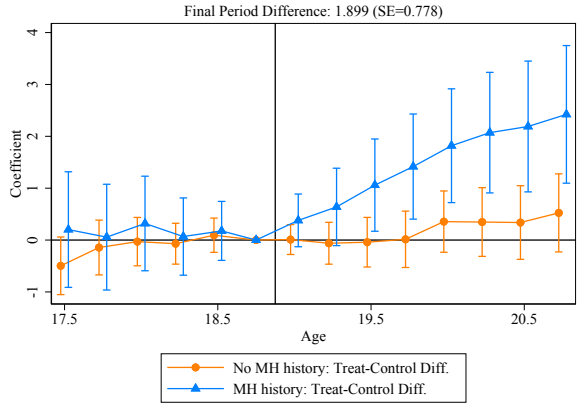
(c) Down-weight outliers



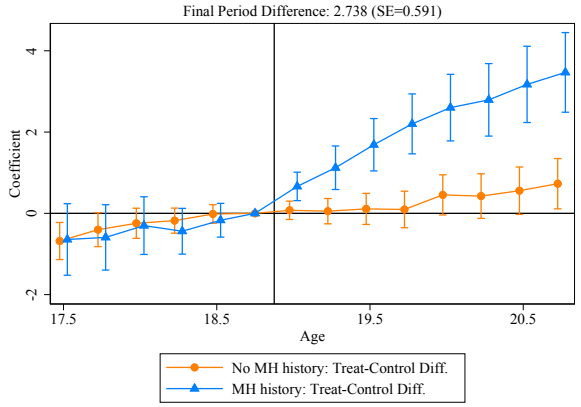
(d) Pre-period assignment



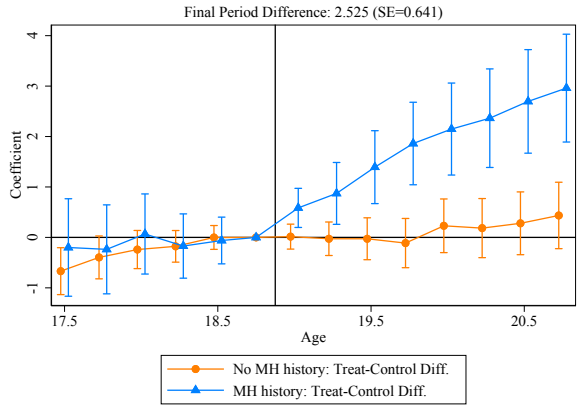
(e) Match on school



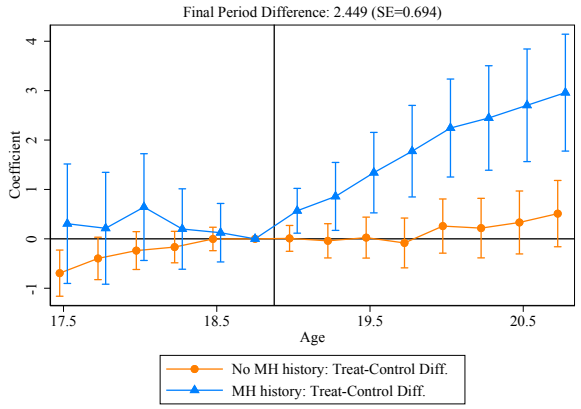
(f) No matching



(g) DFL: same matching characteristics



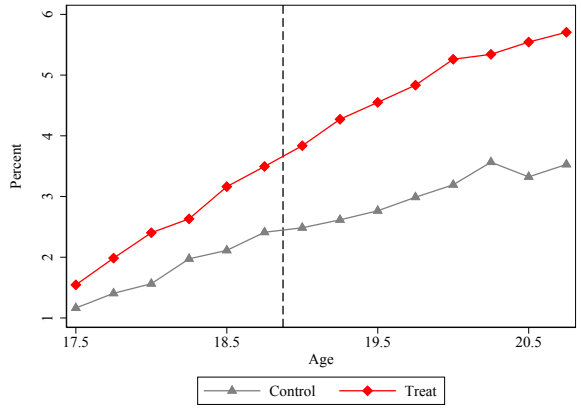
(h) DFL: matching on school



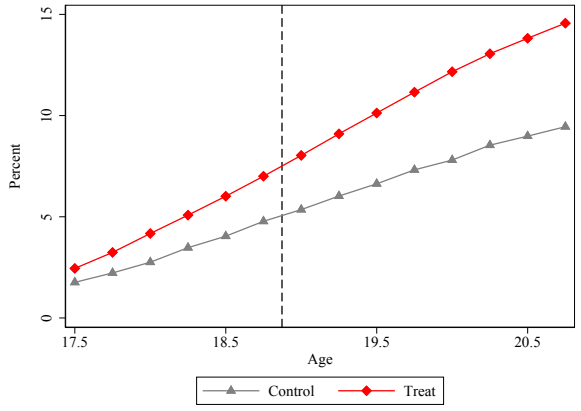
NOTE: The dependent variable is a measure of whether an individual was ever arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panel (a) plots the baseline result from Figure 5. Panel (b) randomly assigns one control individual to each treated individual. Panel (c) drops control observations with disproportionate weight in the regression. Panel (d) assigns individuals to the treatment and control period based on their Medicaid enrollment in the study pre-period. Panel (e) matches treatment and control men using school attended instead of district. Panel (f) runs the specification for all eligible treated and control individuals (i.e., with no matching). Panels (g) and (h) use the DFL re-weighting approach (instead of the coarsened matching procedure) to re-weight the eligible control individuals. Panel (g) re-weights men based on race, year of birth, school district, and mental health history, and panel (h) replaces district with school. "Final Period" refers to the α_8 coefficient using equation (2). Standard errors are clustered at the individual level.

Figure A7: Raw Means for All Eligible Treat & Control Individuals (No Matching)

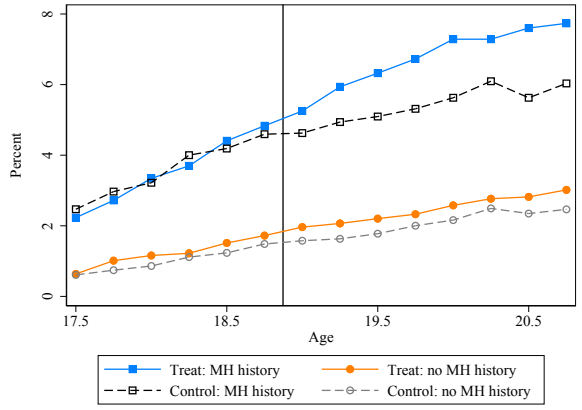
(a) Incarcerated that quarter



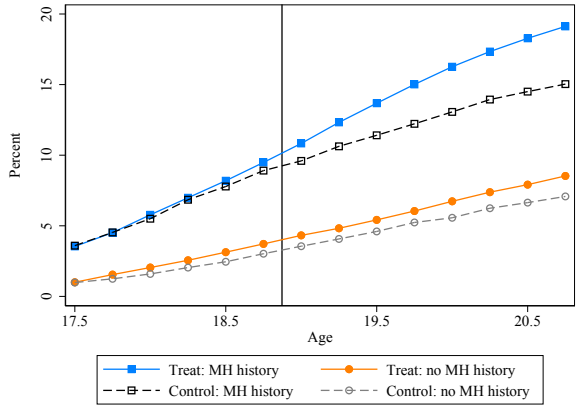
(b) Ever incarcerated



(c) Incarcerated that quarter



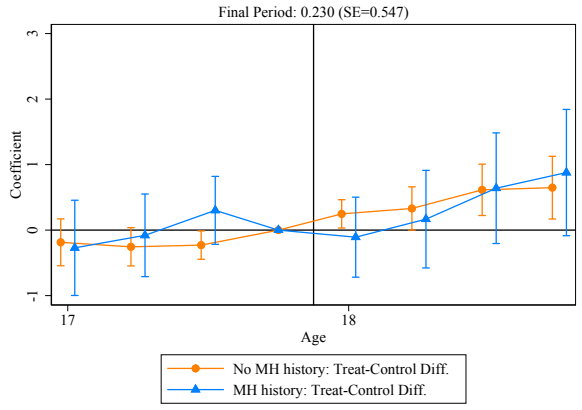
(d) Ever incarcerated



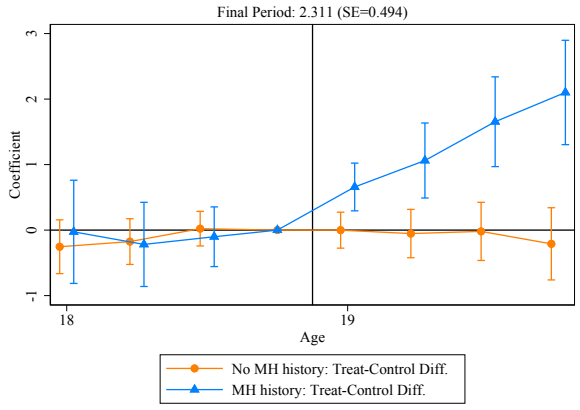
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (b) plots the raw means for all treatment and control individuals, and panels (c) and (d) separate these groups by men's mental health history prior to age 16.

Figure A8: Placebo Check: Moving Treatment Age to 18
(Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)

(a) Age 18



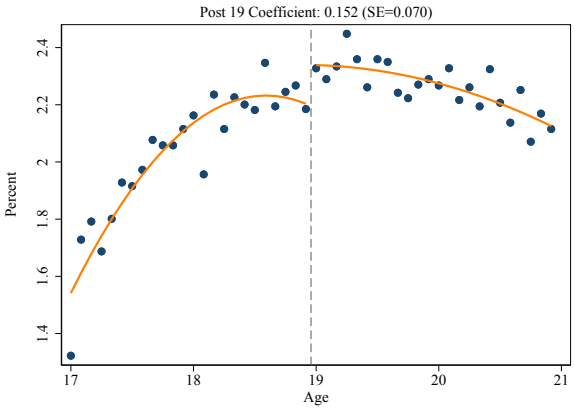
(b) Age 19



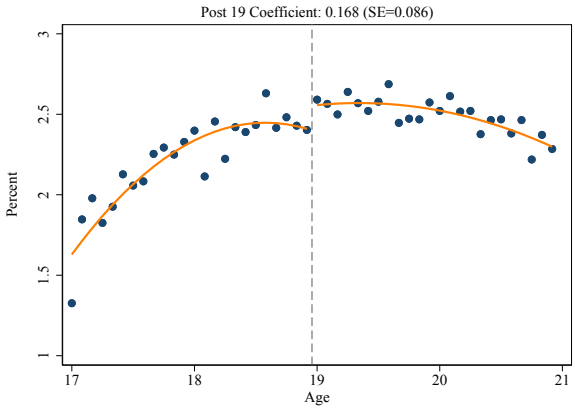
NOTE: The dependent variable is a measure of whether an individual was ever arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panel (a) presents the results from the placebo check (i.e., measuring the likelihood of incarceration around age 18). Panel (b) presents the main results from Figure 5 zoomed in around ages 18 and 19. "Final Period" refers to the α_8 coefficient using equation (2). Standard errors are clustered at the individual level.

Figure A9: Regression Discontinuity: Arrest Probability Around Age 19

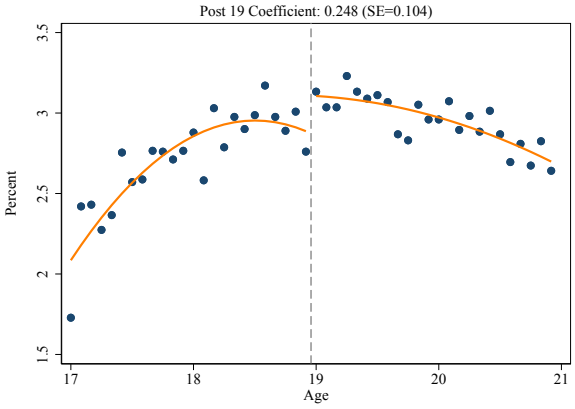
(a) Enrolled in Medicaid, Ages 10–18



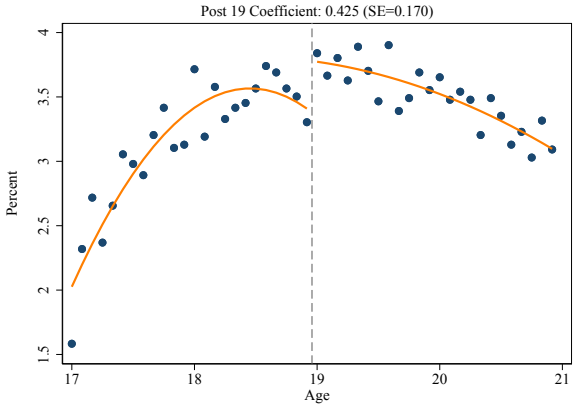
(b) Enrolled in Medicaid, Ages 17–18



(c) Mental Health History, Ages 10–18



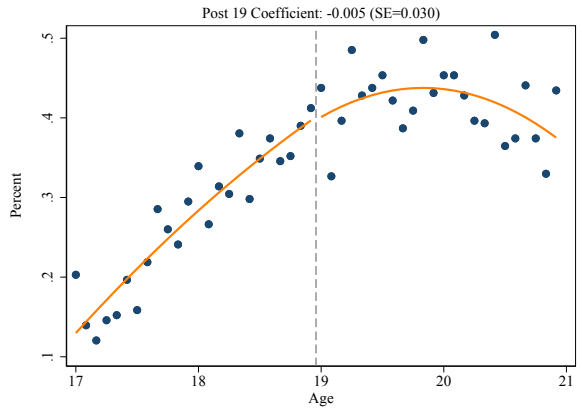
(d) Using Behavioral Health Services, Ages 17–18



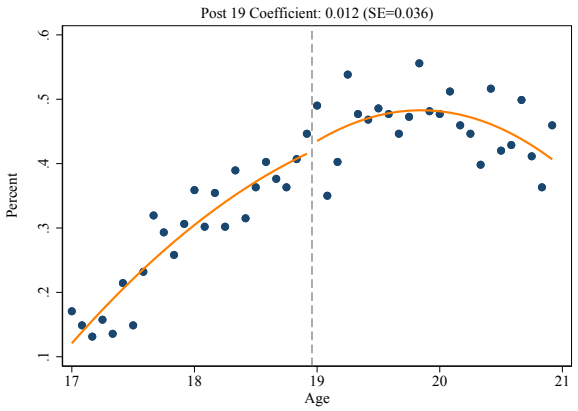
NOTE: This figure plots men's probability of being arrested for each month around their 19th birthday, using arrest data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (4). The estimate reported above each figure reports the discontinuity estimate from this equation and its standard errors (clustering at the individual level). Panels (a) and (b) consider all men enrolled in Medicaid between ages 10–18 and 17–18, respectively. Panel (c) considers all individuals with a Medicaid mental health claim between ages 10 and 18 (inclusive), whereas panel (d) only considers individuals with a mental health claim right before the loss in eligibility (i.e., at ages 17 or 18).

Figure A10: Regression Discontinuity: Probability of Being Taken into Custody Around Age 19

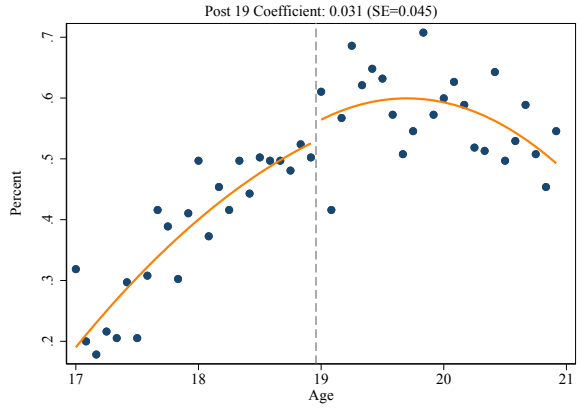
(a) Enrolled in Medicaid, Ages 10–18



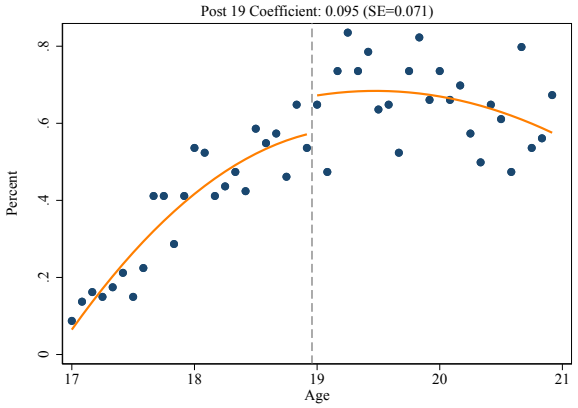
(b) Enrolled in Medicaid, Ages 17–18



(c) Mental Health History, Ages 10–18



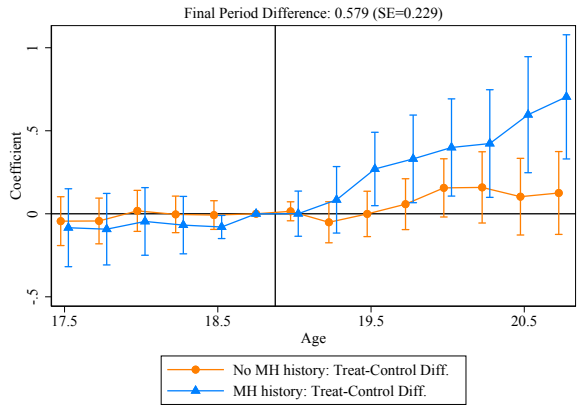
(d) Using Behavioral Health Services, Ages 17–18



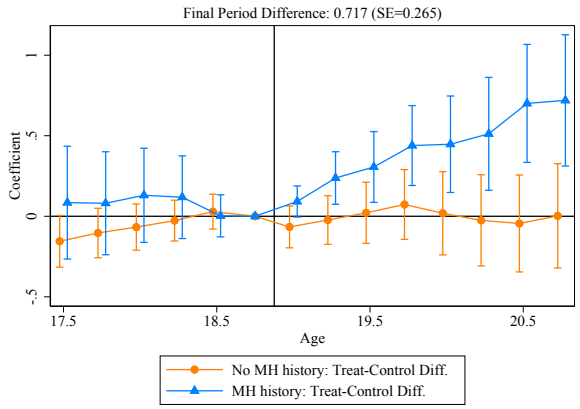
NOTE: This figure plots men's probability of being arrested and taken into custody for each month around their 19th birthday, using arrest data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (4). The estimate reported above each figure reports the discontinuity estimate from this equation and its standard errors (clustering at the individual level). Panels (a) and (b) consider all men enrolled in Medicaid between ages 10–18 and 17–18, respectively. Panel (c) considers all individuals with a Medicaid mental health claim between ages 10 and 18 (inclusive), whereas panel (d) only considers individuals with a mental health claim right before the loss in eligibility (i.e., at ages 17 or 18).

Figure A11: Likelihood of Ever Being Incarcerated for Drug-Related Offense, by Mental Health History and Offense Type

(a) Possession-Related Charge



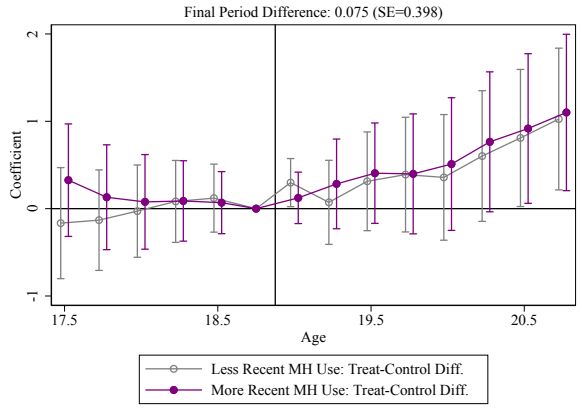
(b) Distribution-Related Charge



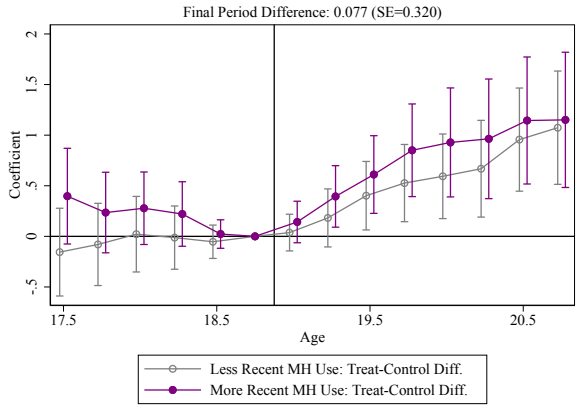
NOTE: The dependent variable is a measure of whether an individual has ever been arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data for a drug-related offense. These figures plot the treat-control differences estimated using equation (2). "Final Period" refers to α_8 in that equation. Standard errors are clustered at the individual level.

Figure A12: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization & Offense Type

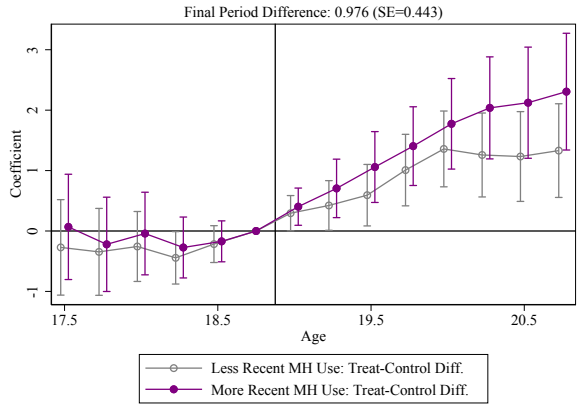
(a) Violent Offenses



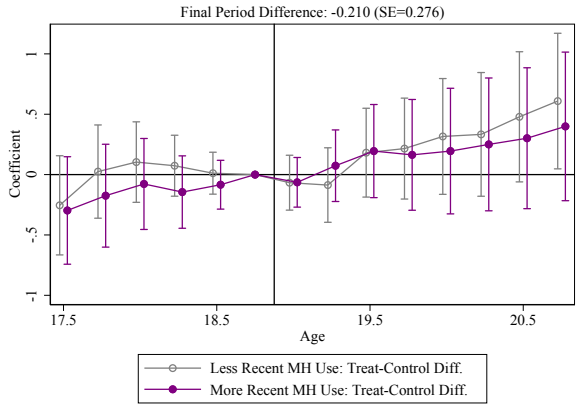
(b) Drug-Related Offenses



(c) Property Offenses



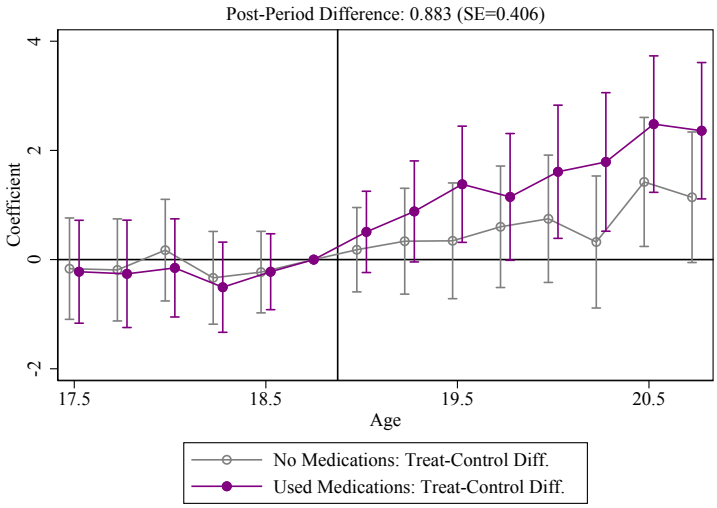
(d) Miscellaneous Offenses



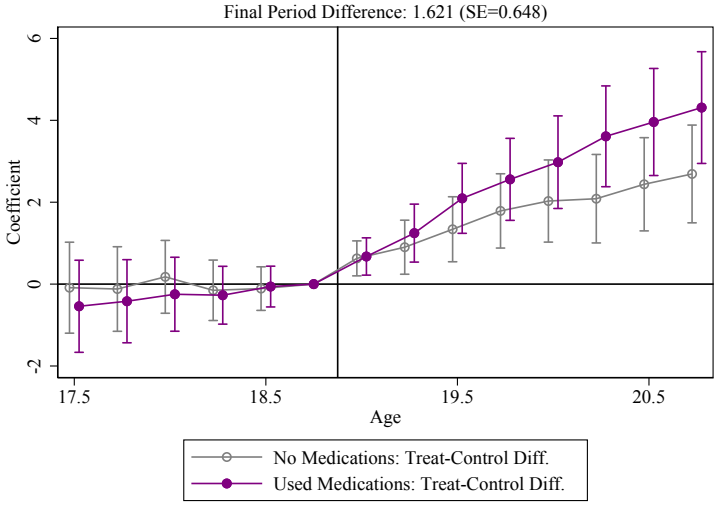
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who were recently using mental health services (in purple) and for men who were less recently using mental health services (in gray) using estimates from equation (2). "Final Period Difference" refers to the α_8 coefficient using that equation. Recent usage is defined as filing a mental health claim during the study pre-period. Standard errors are clustered at the individual level.

Figure A13: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage

(a) Incarcerated that quarter



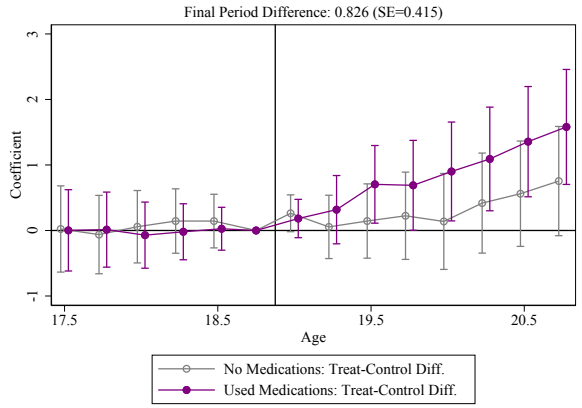
(b) Ever incarcerated



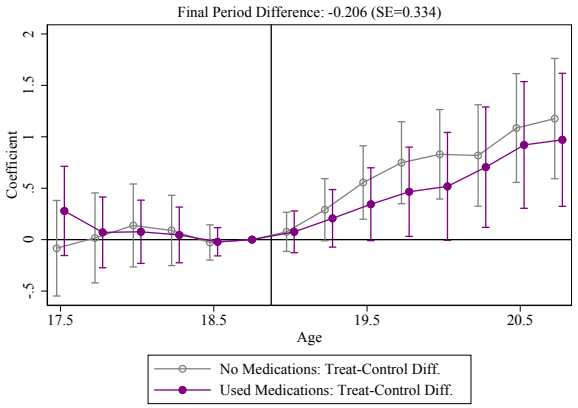
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who filed claims for psychotropic drugs between ages 14–18 (in purple) and for men who did not file claims for medications during these ages (in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure A14: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage & Offense Type

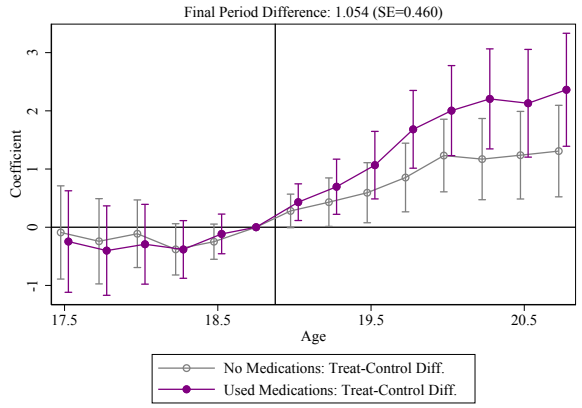
(a) Violent Offenses



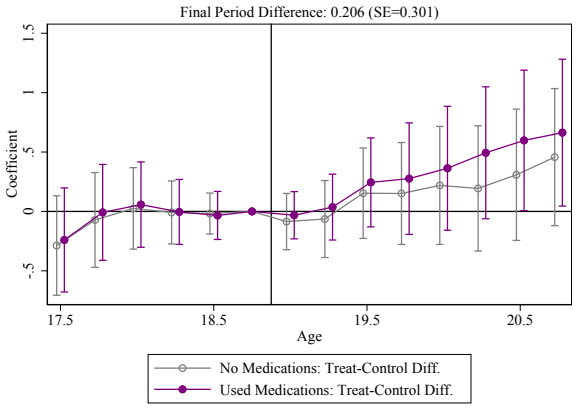
(b) Drug-Related Offenses



(c) Property Offenses



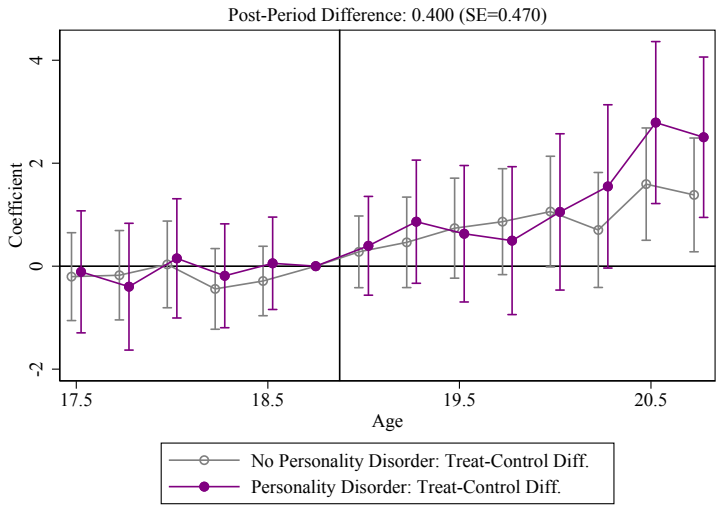
(d) Miscellaneous Offenses



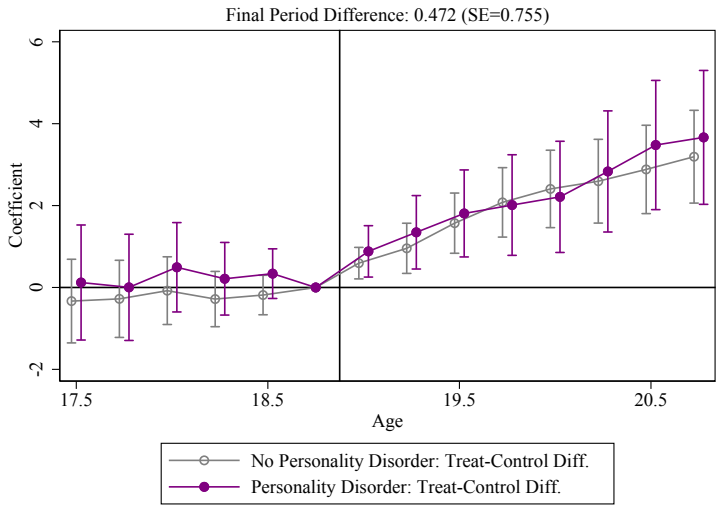
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who filed claims for psychotropic drugs between ages 14–18 (in purple) and for men who did not file claims for medications during these ages (in gray) using estimates from equation (2). "Final Period Difference" refers to the α_8 coefficient using that equation. Standard errors are clustered at the individual level.

Figure A15: Likelihood of Incarceration for Men with a Mental Health History, by Diagnosis

(a) Incarcerated that quarter



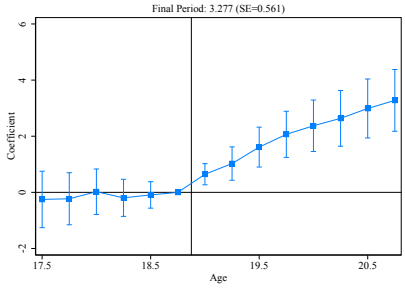
(b) Ever incarcerated



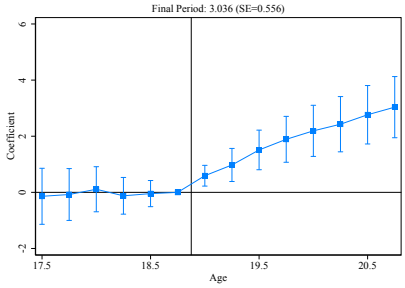
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men ever diagnosed with a personality disorder (in purple) and for men without a personality disorder (i.e., those with a clinical disorder only, in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure A16: Likelihood of Incarceration for Men with a Mental Health History, Excluding One Diagnosis At a Time

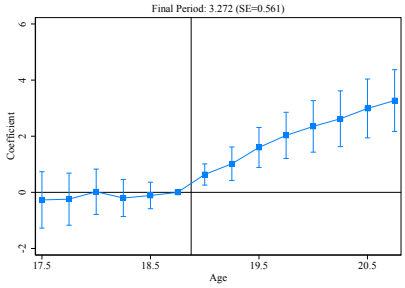
(a) Baseline (no exclusions)



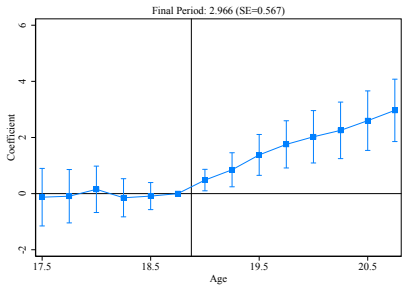
(b) Organic psychotic conditions (290–294)



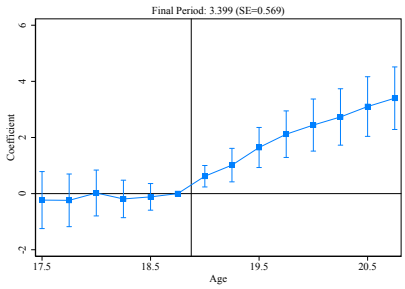
(c) Schizophrenic disorders (295)



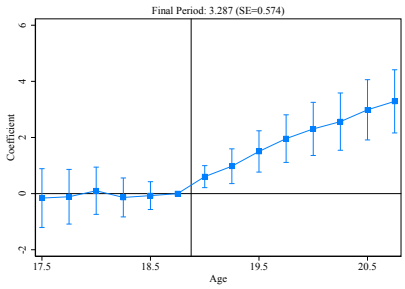
(d) Episodic mood disorders (296)



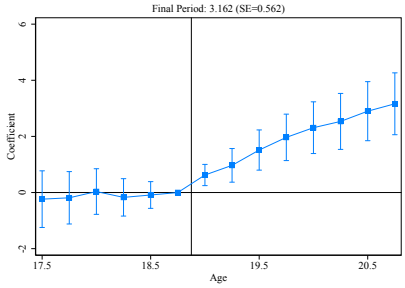
(e) Other psychoses disorders (297–299)



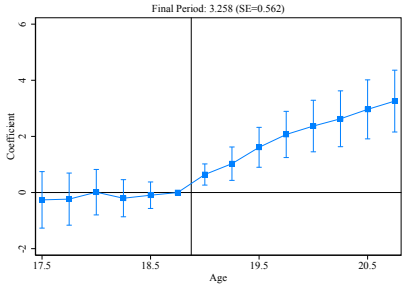
(f) Neurotic disorders (300)



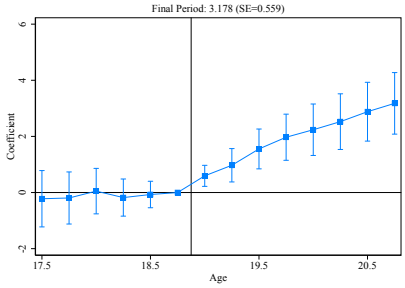
(g) Personality disorders (301)



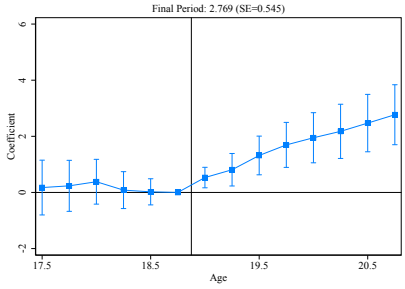
(h) Sexual disorders (302)



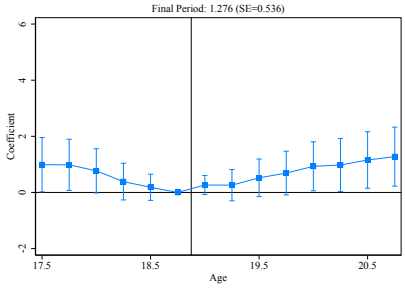
(i) Alcohol dependence syndrome (303)



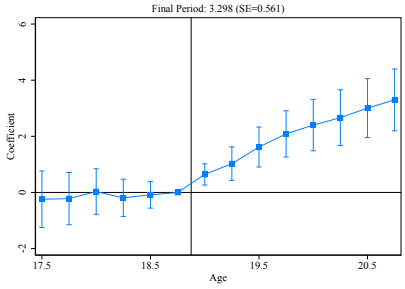
(j) Drug dependence (304)



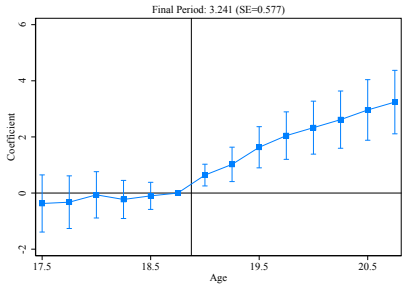
(k) Non-dependent drug abuse (305)



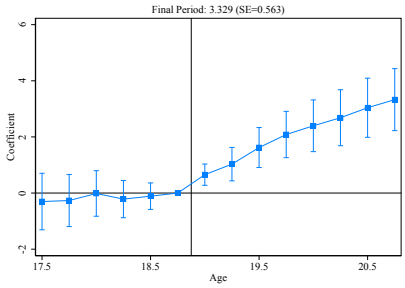
(l) Physiological malfunction (306)



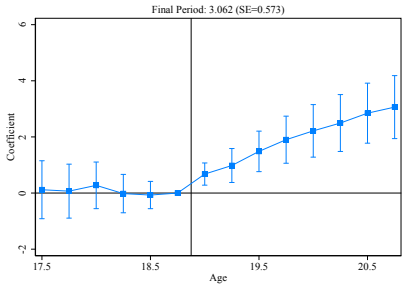
(m) Special syndromes (307)



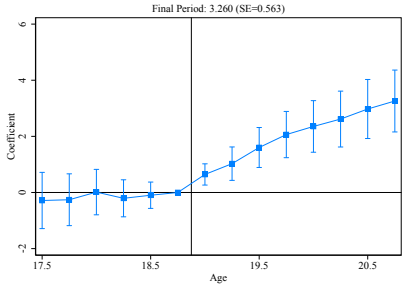
(n) Acute reaction to stress (308)



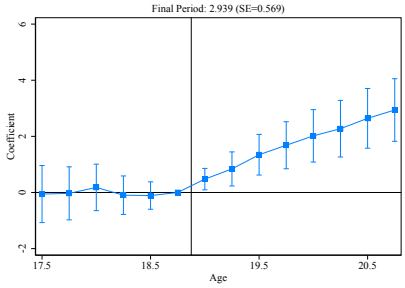
(o) Adjustment reaction (309)



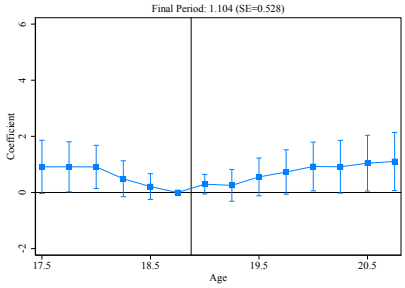
(p) Specific nonpsychotic disorders (310)



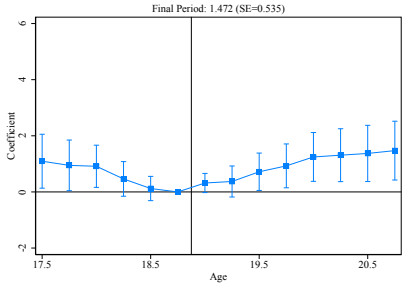
(q) Depressive disorder (311)



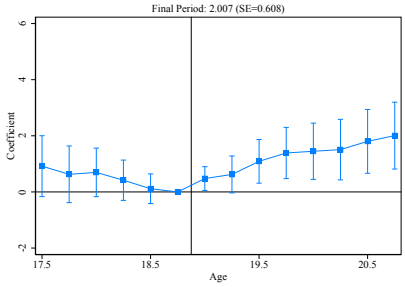
(r) Disturbance of conduct (312)



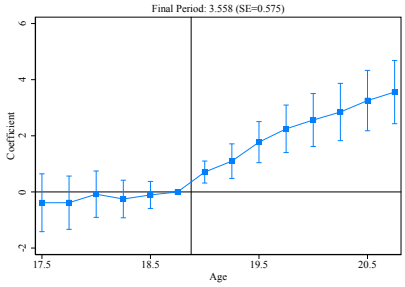
(s) Disturbance of emotions (313)



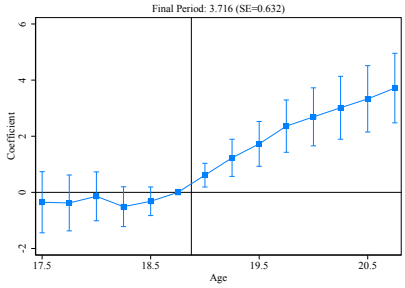
(t) Hyperkinetic syndrome (314)



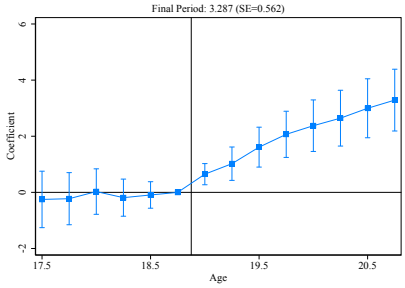
(u) Hyperkinetic syndrome only (314 only diagnosis)



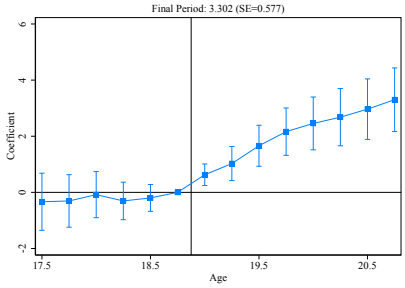
(v) Developmental delay (315)



(w) Other psychic factors (316)



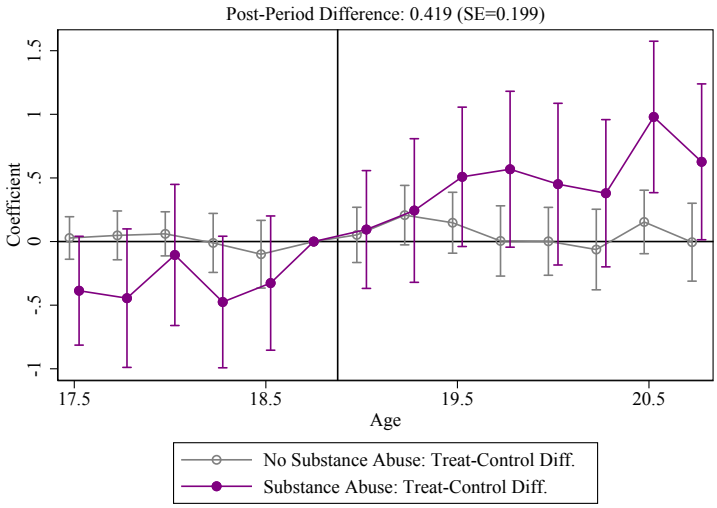
(x) Intellectual disabilities (317–319)



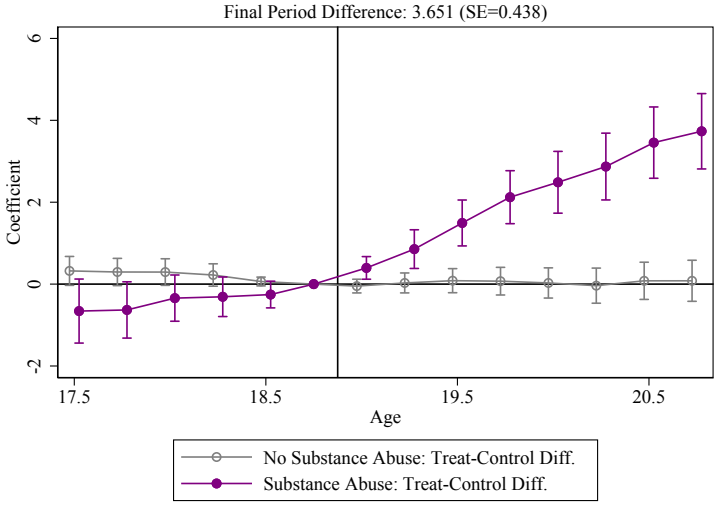
NOTE: The figures reproduce the main estimates from Figure 5, plotting the treat-control differences for men with a mental health history after dropping men ever diagnosed with a specific ICD-9 code (listed under each description). "Final Period" refers to α_8 in equation (2). Standard errors are clustered at the individual level. Panel (u) drops men whose sole diagnosis is hyperkinetic syndromes.

Figure A17: Likelihood of Drug-Related Incarceration for Men with a Mental Health History, by Substance Abuse Diagnosis

(a) Incarcerated that quarter

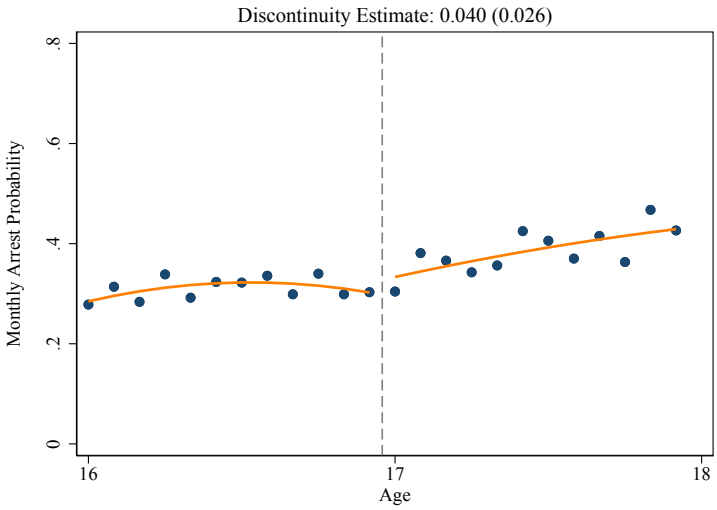


(b) Ever incarcerated



NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data for a drug-related offense. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men ever diagnosed with a substance abuse disorder (in purple) and for men without a substance abuse disorder(in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter for a drug-related offense. "Ever incarcerated" refers to having been detained in a correctional facility at least once before for a drug-related offense. Standard errors are clustered at the individual level.

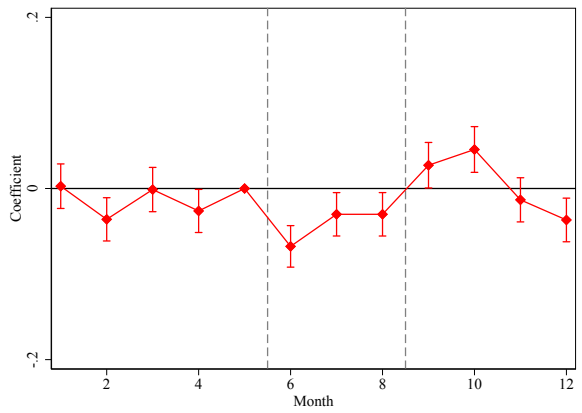
Figure A18: Share of Individuals Arrested for a Felony Around the Age of Criminal Majority



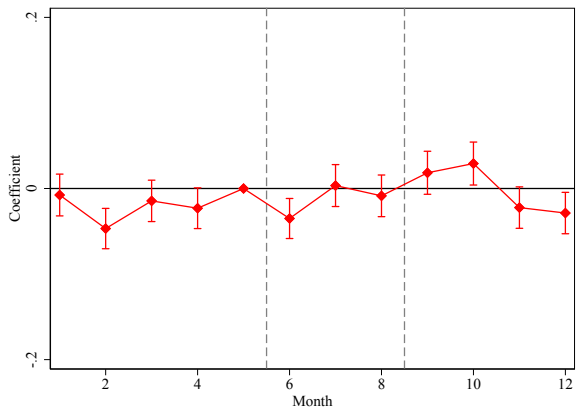
NOTE: These figures plot the monthly estimates of the likelihood of a felony arrest around an individual's 17th birthday. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (4). The estimate above the figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Figure A19: Likelihood of Felony Arrest or Death for Males Aged 12–16, by Month

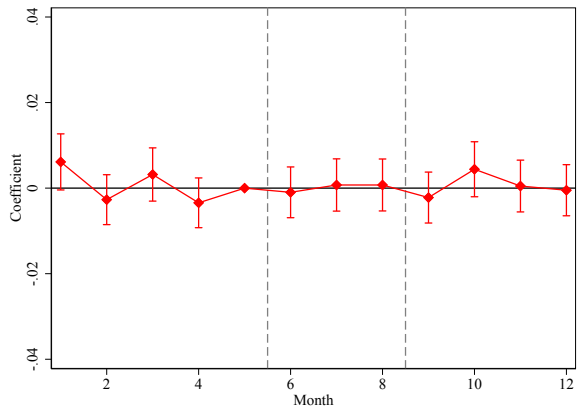
(a) All felonies



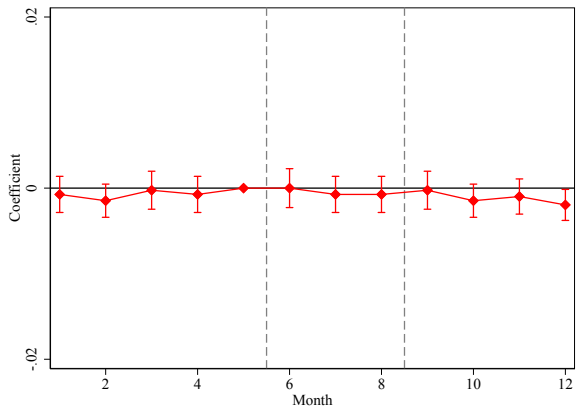
(b) Non-school-related felonies



(c) Adult felonies

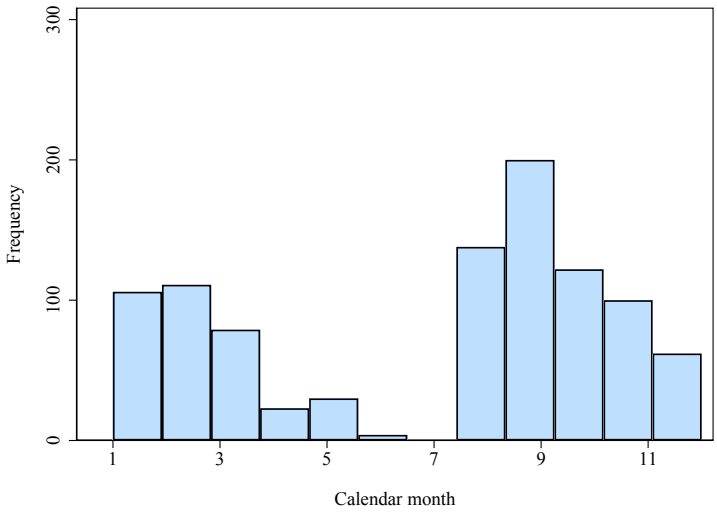


(d) Death



NOTE: This figure plots the likelihood that low-income male adolescents were arrested for committing a felony or passed away, by calendar month. The sample used is a balanced panel dataset of all male adolescents born in 1990–1999 constructed at the individual-age-month level for ages 12–16. "Non-school-related felonies" refers to felonies that do not have the word "school" in the charge description (e.g., carrying weapons on school property). "Adult felonies" refers to felonies that appear in the adult criminal justice records (i.e., the SLED data) despite these individuals being juveniles. "Death" is measured using death certificate records and is used as another proxy for adolescent risky behavior. Estimates come from regressing an indicator variable for a felony arrest or death on month fixed effects (omitting the month of May), age fixed effects, and individual fixed effects. Standard errors are clustered at the individual level.

Figure A20: Seasonality of Dropout

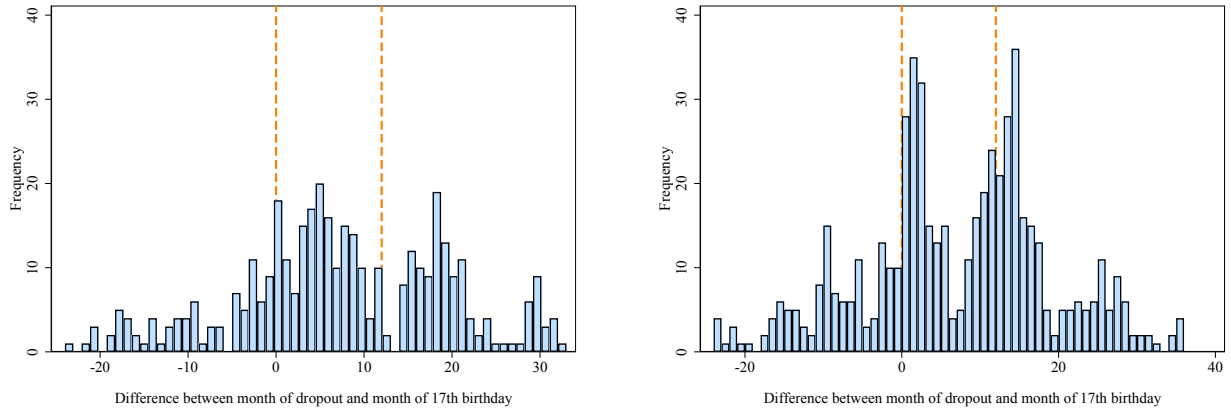


NOTE: This figure shows the dropout month recorded in the Department of Education administrative records for all individuals with an available dropout date.

Figure A21: Difference between 17th-Birthday Month and Dropout Month, by Birth Month

(a) January–June

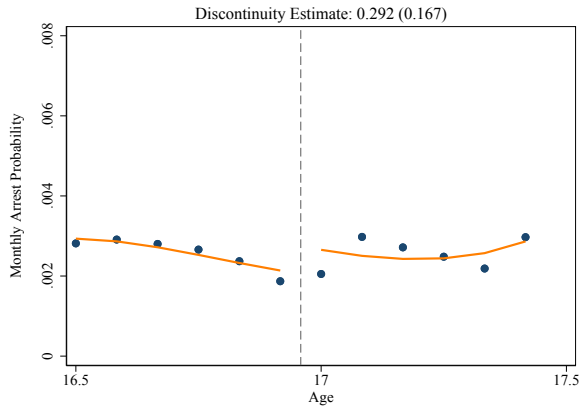
(b) July–December



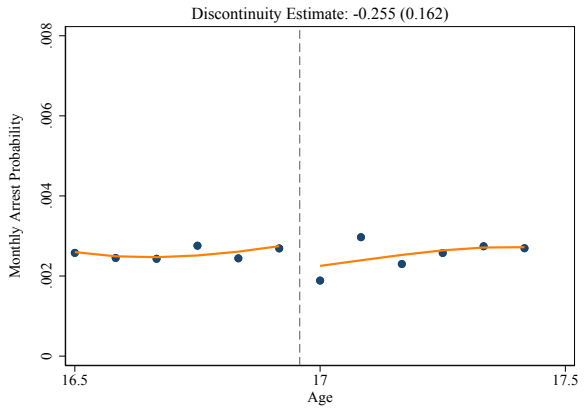
NOTE: This figure shows the difference between an individual's 17th-birthday month and dropout month, as recorded in the Department of Education administrative records for all individuals with an available dropout date. The dashed orange lines indicate the time between the individual's 17th-birthday month and their 18th-birthday month.

Figure A22: Felony Propensity Estimates Around the Age of Criminal Majority, by Birth Month

(a) January–June



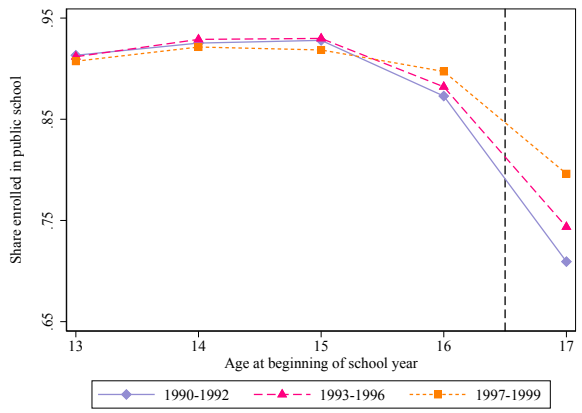
(b) July–December



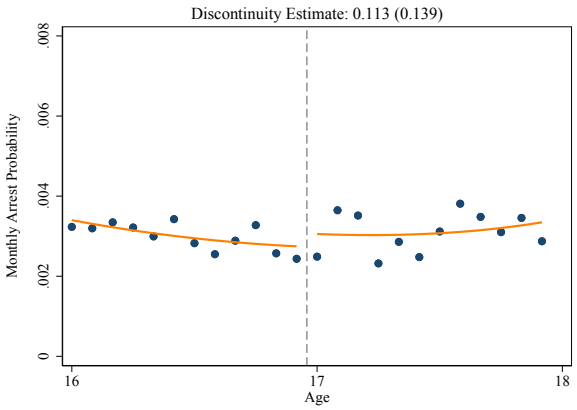
NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday, separately by birth month. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Figure A23: Likelihood of Dropout and Deterrence Estimates, by Cohort

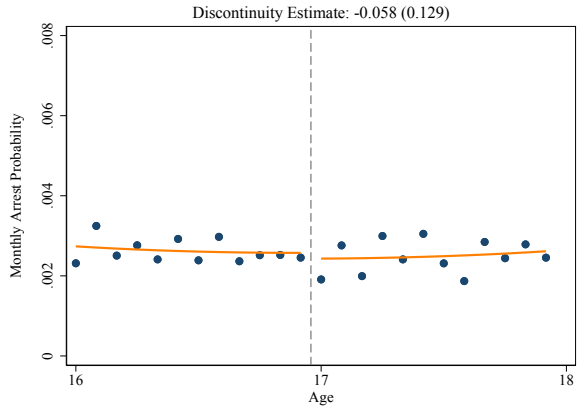
(a) Share enrolled by age



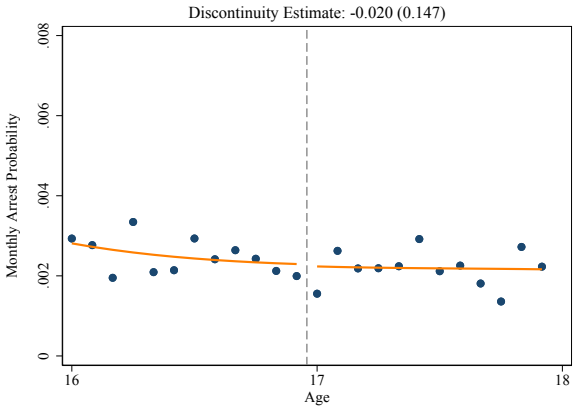
(b) 1990–1992 Cohorts



(c) 1993–1996 Cohorts



(d) 1997–1999 Cohorts



NOTE: The first panel shows the share of individuals born in that cohort who appeared at that age in the Department of Education administrative school records noting enrollment at the beginning of the school year. The remaining figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday, separately by birth cohort. The figures consider all men born who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Table A1: Summary Statistics of Matching Candidates and Matches

	(1) Candidate Controls	(2) Matched Controls	(3) Candidate Treated	(4) Matched Treated
Black	0.67	0.72	0.72	0.72
Medicaid (ever)	1.00	1.00	1.00	1.00
Medicaid (years)	4.01	4.24	7.80	7.80
MH claim (pre 16)	0.35	0.56	0.57	0.56
DJJ referral (ever)	0.29	0.34	0.41	0.41
Arrests (ever)	0.19	0.22	0.27	0.27
SNAP (ever)	0.72	0.74	0.89	0.89
SNAP (years)	3.30	3.50	5.73	5.75
Observations	8,980	8,927	22,063	21,455

NOTE: "Candidate" refers to individuals who were eligible for the matching procedure. "Matched" refers to individuals who were successfully matched. In column 2, individuals are weighted using the total weight that each individual gets in the sample (i.e., the sum of the regression weights corresponding to that individual). "MH claim" refers to any mental health claim between the ages of 10 and 15. "DJJ referral" refers to being referred to the Department of Juvenile Justice for any offense.

Table A2: Distribution of Mental Health Diagnoses

	Medicaid men	Treated men	Treated men with mental health history
Organic psychotic conditions (290–294)	2.43	3.17	4.98
Schizophrenic disorders (295)	0.68	0.85	1.38
Episodic mood disorders (296)	6.07	7.59	12.44
Other psychoses disorders (297–299)	2.36	2.91	4.84
Neurotic disorders (300)	7.90	9.79	15.40
Personality disorders (301)	1.38	1.74	2.91
Sexual disorders (302)	0.18	0.22	0.37
Alcohol dependence syndrome (303)	0.57	0.73	1.10
Drug dependence (304)	3.69	4.77	7.24
Non-dependent drug abuse (305)	14.15	18.39	25.68
Physiological malfunction (306)	0.26	0.34	0.51
Special syndromes (307)	4.27	5.11	8.46
Acute reaction to stress (308)	0.80	1.03	1.44
Adjustment reaction (309)	10.34	12.43	20.49
Specific nonpsychotic disorders (310)	0.34	0.44	0.66
Depressive disorder (311)	8.12	10.01	16.33
Disturbance of conduct (312)	16.45	19.99	32.87
Disturbance of emotions (313)	14.88	18.14	30.57
Hyperkinetic syndrome (314)	26.31	30.17	51.83
Developmental delay (315)	18.53	21.14	37.08
Other psychic factors (316)	0.08	0.10	0.17
Intellectual disabilities (317–319)	7.28	9.00	15.76
Number of individuals	31,533	21,455	12,046

NOTE: This table reports the share of men that have an insurance claim with that ICD-9 mental disorder diagnosis code at any point between the ages of 10 and 18 (inclusive). The first column considers all men who were ever enrolled in Medicaid between ages 10–18. The second column considers the high-enrollment group (i.e., the treated men). The final column considers men in the treatment group with a mental health history (prior to age 16). An individual can be given multiple diagnoses throughout adolescence, so the shares do not add up to 100.

Table A3: Common Mental Health Services

	Medicaid men	Treated men	Treated men with mental health history
Psychotropic drugs	37.48	43.25	69.04
Alcohol and drug services	11.81	14.79	22.04
Psychotherapy	14.07	17.24	28.35
Case management	44.98	52.43	59.14
Number of individuals	31,533	21,455	12,046

NOTE: This table reports the share of men that have an insurance claim for that type of service at any point between the ages of 10 and 18 (inclusive). The first column considers all men who were ever enrolled in Medicaid between ages 10–18. The second column considers the high-enrollment group (i.e., the treated men). The final column considers men in the treatment group with a mental health history (prior to age 16). An individual can use multiple services throughout adolescence, so the shares do not add up to 100. For more details, I refer the reader to Appendix B.

Table A4: Effect of Medicaid Loss on Men's Criminal Activity

	Full Sample		By Mental Health History	
	Control Avg.	Estimated Effect (β_τ or β_8)	Control Avg. (MH Sample)	Estimated Effect (α_τ or α_8)
Arrests				
Arrested that quarter	0.05	0.001	0.06	0.003
		(0.003)		(0.006)
Custody that quarter	0.98	0.232	1.18	0.383
		(0.161)		(0.301)
Ever taken into custody	10.54	2.026***	13.23	3.126***
		(0.347)		(0.659)
Incarcerations in State Prison				
Incarcerated	3.31	0.357	4.42	0.766*
		(0.224)		(0.416)
Ever incarcerated	6.61	1.168***	8.66	2.168***
		(0.307)		(0.575)

NOTE: Data on arrests come from the South Carolina Law Enforcement Division and data on incarcerations in state prison come from the South Carolina Department of Corrections. The average in column 1 reports the post-period average for the matched control observations in the full sample. The average in column 3 reports this same statistic for control observations with a mental health history. For the two cumulative variables, the averages correspond to the value measured in the last quarter of the post period (age 20, quarter 4). The estimated effects in column 2 correspond to the post-period average of the β_τ coefficients (or β_8 for cumulative variables) estimated using equation (1). The estimated effects in column 4 correspond to the post-period average of the α_τ coefficients (or α_8 for cumulative variables) estimated using equation (2). Standard errors are clustered at the individual level and reported under corresponding estimates in parentheses.

Table A5: Robustness to Specification, Matching, & Samples for Men's Likelihood of Incarceration

		Incarcerated		Ever Incarcerated	
	Treated Units	Full Sample	MH History	Full Sample	MH History
Baseline	21,455	0.547**	0.924**	1.954***	3.015***
		(0.250)	(0.463)	(0.357)	(0.677)
Cluster: match level	21,455	0.547***	0.924***	1.954***	3.015***
		(0.114)	(0.217)	(0.186)	(0.360)
Cluster: match & ind.	21,455	0.547**	0.924**	1.954***	3.015***
		(0.249)	(0.462)	(0.355)	(0.674)
One control per treat	21,455	0.558**	1.083**	1.954***	3.388***
		(0.271)	(0.506)	(0.383)	(0.734)
Downweight outliers	21,455	0.543**	0.917**	1.951***	3.012***
		(0.250)	(0.464)	(0.357)	(0.678)
Assign in pre-period	20,249	0.234	0.235	1.393***	1.934***
		(0.193)	(0.364)	(0.350)	(0.675)
Match on school	19,933	0.500*	0.322	1.565***	1.899**
		(0.299)	(0.563)	(0.410)	(0.778)
No matching	22,259	0.772***	0.932**	2.865***	2.738***
		(0.173)	(0.405)	(0.271)	(0.591)
DFL re-weighting	22,259	0.456*	0.729	1.875***	2.525***
		(0.249)	(0.455)	(0.344)	(0.641)
DFL re-weighting, school	22,259	0.645**	0.974*	1.907***	2.449***
		(0.295)	(0.531)	(0.376)	(0.694)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports the results from the robustness checks conducted in Section 6.3. The first row reproduces the baseline estimates seen in Figure 5. The second and third rows cluster the standard errors at the match level and at the match and individual level, respectively. The fourth row forces each treated unit to have one control unit. The fifth row re-weights control that have disproportionate weight in the baseline specification. The sixth row assigns individuals to the treatment and control group based on their Medicaid enrollment in the sample pre-period. The seventh row matches individuals using school instead of district. The eighth row uses all eligible treated and control individuals (i.e., no matching). The ninth and tenth rows use the DFL re-weighting approach to re-weight eligible control individuals. The ninth row uses the same baseline matching characteristics and the tenth row uses school instead of district. Except for the second and third rows, standard errors are clustered at the individual level and reported under their corresponding estimate in parentheses.

Table A6: Likelihood of Felony Arrest or Death for Males Aged 12–16 in Summer Months

	(1) Felony	(2) Non-school felony	(3) Adult felony	(4) Death
Summer month	-0.038***	-0.003	-0.000	0.004
	[0.006]	[0.006]	[0.001]	[0.005]
Observations	4,895,400	4,895,400	4,895,400	4,895,400

NOTE: This figure reports the likelihood that low-income adolescents were arrested for committing a felony or passed away in a summer month (defined as June, July, and August). The sample used is a balanced panel dataset of all male adolescents born in 1990–1999 constructed at the individual-age-month level for ages 12–16. "Non-school-related felonies" refers to felonies that do not have the word "school" in the charge description (e.g., carrying weapons on school property). "Adult felonies" refers to felonies that appear in the adult criminal justice records (i.e., the SLED data) despite these individuals being juveniles. "Death" is measured using death certificate records and is used as another proxy for adolescent risky behavior. Estimates come from regressing an indicator variable for a felony arrest or death on an indicator variable for summer month, age fixed effects, and individual fixed effects. Standard errors are clustered at the individual level.

B Medicaid Loss: Sample and Variable Construction

B.1 Selection of Sample

To select the individuals for this study, South Carolina's Revenue and Fiscal Affairs (RFA) Office used the characteristics of individuals and schools in the Department of Education records. First, they calculated the share of students receiving free or reduced-price lunch at each high school in the state, for every year between 2008 and 2014.[44] These shares were then averaged across the 2008–2014 school years and schools were ranked based on the average share. The high schools in the upper half of this distribution were chosen and if an individual ever attended one of these "low-income" high schools, he or she entered the sample.

Using the individuals in this sample, I construct a panel dataset of individuals starting at age 10. None of the datasets provide exact information on an individual's date of birth. For individuals ever enrolled in Medicaid (roughly 70 percent of the sample), I use the month and year of birth in the Medicaid recipient file to construct a panel at the person-age-quarter level (where quarter refers to one of the four quarters within an age). I also construct a dataset at the person-age level in order to include individuals who were never enrolled in Medicaid (their year of birth information is provided in the Department of Education data).

Throughout the analysis, I focus on individuals born between 1990 and 1993 because subsequent cohorts were affected by the introduction of an automatic enrollment program for Medicaid. Specifically, South Carolina implemented the Express Lane Eligibility (ELE) program in 2011–2012, resulting in increased Medicaid enrollment starting with the 1994 cohort. Among men born between 1990–1993, 29 percent were eligible control individuals, whereas among men born between 1994–1997, only 16 percent were eligible control units. The increase in enrollment prompted by ELE reduces the number of individuals eligible for the control group, thereby worsening the match rate and allowing fewer individuals to have a disproportionate impact on the estimated counterfactual outcome paths. Perhaps more importantly, this increase in enrollment also alters the composition of eligible treatment and control individuals. Prior to ELE, the control group included lower-income individuals who were likely eligible but not enrolled in Medicaid. However, starting with the 1994 cohort, many of these lower-income individuals were automatically enrolled into Medicaid, thereby becoming

[44] Students who attend private school or homeschool are not included in this data.

part of the high-enrollment group; the remaining control individuals therefore had relatively higher incomes and were potentially worse candidates for estimating counterfactual outcome paths. For these reasons, the analysis focuses on individuals born between 1990 and 1993.

B.2 Variable Construction

I use the Medicaid insurance claims to classify medical visits or medications as mental healthcare. Specifically, I classify insurance claims as mental healthcare if any of the diagnoses codes or any of the pharmacologic-therapeutic drug classifications are related to behavioral health. Mental health diagnoses are identified using ICD-9 codes 290–319 and ICD-10 codes F01–F99. In Section 7, I split the sample into two groups using the ICD-9 diagnosis codes available in their insurance claims. I identify individuals as having a personality disorder if they ever filed a mental health claim with an ICD-9 code of 301, 317, 318, or 319. Individuals with a substance abuse disorder are those who ever filed a claim with ICD-9 codes 303, 304, or 305. Finally, drugs are labeled as mental health medications if their code is 281604–282800, 281208, 281292, 283292, 284000, or 289200.

I also use the billing codes in the mental health claims to identify the types of services used. Alcohol or substance abuse services refers to mental health claims that have alcohol or substance abuse services in the billing codes. Psychotherapy refers to any mental health claims that list psychiatric services or other therapy in the billing codes. Case management refers to mental health claims that have case management listed in the billing codes.

When utilizing the SLED data, I distinguish all arrests from arrests that ended with an individual being taken into custody (15 percent of arrests in the data). Given the prevalence of arrests among this low-income sample—20 percent of high-enrollment men were arrested during ages 17–18 years old—I use these custody arrests in order to consider relatively more serious offenses. Using the information that describes the disposition of the arrest, I see that these custody arrests vary in severity but do tend to be more serious than the average arrest: 20 percent are burglaries, 15 percent are drug-related offenses, 9 percent are parole or probation violations or revocations, 9 percent are assaults, and 8 percent are robberies.

I classify the offenses in the SLED and DOC data into four categories: violent offenses, drug-related offenses, property offenses, and miscellaneous offenses. Violent-crime incarcerations in the DOC data are incarcerations for murder, assault, robbery, or sex offenses. Violent-crime incarcerations in the SLED data are those whose arrest dispositions are for murder, assault (excluding

simple assault, but including domestic violence as well as kidnapping charges), robbery, and sex offenses (excluding sex offender violations).[45] Drug offenses are those related to the possession, distribution, or manufacturing of drugs or alcohol (including DUIs). Property crimes are incarcerations for burglary, larceny/theft, or motor vehicle theft. Finally, miscellaneous offenses are all remaining offenses that do not fit into any of these categories (e.g., weapons offenses, parole or probation violations).

Finally, when considering academic achievement, I standardize the test scores at the grade, year, and test-type level to have mean zero and standard deviation one. I standardize at the test-type level because South Carolina transitioned across various standardized tests during the study's sample period: the Palmetto Achievement Challenge Test (PACT), the Palmetto Assessment of State Standards (PASS), the High School Assessment Program (HSAP), the ACT Aspire test, and the South Carolina College- and Career-Ready (SC READY) Assessment.

[45] Specifically, murders are all serious arrests with the words "murder," "homicide," "manslaughter," "lynching," or "death" in the disposition. Sex offenses are all serious arrests with the words "rape," "lewd," "sex," or "indecent exposure," in the disposition and I exclude sex offender registry violations. Assaults are all serious arrests with any variation of the word "assault" as well as the words "kidnapping," "ABHAN," or "domestic violence" in the disposition.

C Cost-Benefit Analysis: Providing Medicaid Eligibility

C.1 Increased Costs of Expanding Eligibility

In order to calculate the cost of expanding Medicaid eligibility for two extra years, I calculate the number of individuals in a cohort that would likely take-up the program if eligibility were expanded. Throughout this analysis, I focus on the costs and benefits for one cohort and only consider the poorest half of South Carolina's residents (i.e., the sample of this paper), assuming that the higher-income half of the state is unaffected by changes in Medicaid eligibility.

Overall, there are roughly 11,748 men in each cohort. I linearly interpolate the enrollment patterns of this group using the share enrolled in each quarter prior to age 19 (i.e., the shares in Figure 3). I find that roughly 30 percent of the cohort would take up Medicaid at age 19 and 24 percent of the cohort would take up Medicaid at age 20. Multiplying these numbers by the annual per-enrollee cost for children in South Carolina in 2011 ($2,008), I find that that the total cost of providing this group of low-income young men with Medicaid totals $12.5 million (Young et al. 2015).[46]

Crowd out of private insurance is less of a concern in this scenario because the income eligibility thresholds are not changing. Indeed, private insurance coverage was always an option for these individuals, so their enrollment patterns at ages 17 and 18 tend to reflect their insurance preferences. I therefore assume that none of these individuals would suddenly change from private insurance to public insurance simply as a result of expanding eligibility for two extra years (without changing eligibility thresholds).

I also consider more-conservative possibilities in terms of the share of men in these cohorts who would choose to take up the program. If the same share of individuals who was enrolled in the program in the quarter prior to age 19 remained enrolled through ages 19 and 20 (31 percent of the cohort), then the cost rises to $14.6 million. Finally, I consider a higher per-enrollee cost using the nationwide annual per-enrollee cost for children ($2,492). Assuming this higher cost *and* the higher level of program take-up implies a total cost of $18.2 million.

[46] In both this section and in Appendix E, I index dollar values to 2010 dollars, which is around the time that the individuals in this analysis turn 19. All costs are also discounted using a 3% discount rate.

C.2 Benefits of Expanding Eligibility

C.2.1 Lower Social Costs from Fewer Victimizations

To calculate the reduced social costs from fewer victimizations, I begin by using the estimates from Table 6 to calculate the number of violent, property, and drug-related crimes that occurred as a result of the Medicaid loss. I find that on average for one cohort, there were 35, 37, and 40 additional violent, property, and drug-related crimes, respectively.[47] I then use the victimization-to-arrest ratios calculated in Heckman et al. (2010) to estimate the average number of additional *incidents* that likely occurred as a result of the Medicaid loss. Specifically, I use the violent and property crime ratios from this source (4.0 and 15.4), and assume that the ratio for drug offenses is the same as that for property crimes.[48] These ratios imply that 139 violent incidents, 577 property crimes, and 623 drug-related offenses occurred after the loss of Medicaid eligibility.

I then calculate the cost of the average violent, property, and drug-related incident (similar to the approach in Mello 2019). First, I split each category of crime into subcategories. Then, I use the prevalence of these sub-crimes in the SLED data in conjunction with the victimization-to-arrest ratios to calculate the share of victimizations that fall into each category. Next, I use the average cost to victims (from Cohen & Piquero 2009 and Miller et al. 1996) for each subcrime in order to calculate the cost of each average violent, property, and drug offense (summarized in Table C1).[49] The upper-end figures use all of the costs from Cohen & Piquero (2009). To be conservative in terms of the statistical value of life, the lower-end estimates divide the cost of murder in half (Heller et al. 2017). For drug-related offenses, the upper-end figures assign DUI offenses the average cost from drunk driving crashes from Cohen & Piquero (2009). The lower-bound estimate assigns DUI offenses the cost of drunk driving incidents without injuries from Miller et al. (1996). For the remaining drug-related crimes, I conservatively assign them a cost of $0 because most of these offenses tend to be "victimless."[50] For both violent and drug offenses, the moderately conservative

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[47] I calculate these numbers by multiplying the average number of people in one cohort (5,364) by the estimated coefficients in Table 6 (divided by 100) and by 8 quarters.

[48] I average the victimization-to-arrest ratio across the 1996–2001 years to get an average ratio.

[49] I note that the costs in Cohen & Piquero (2009) are typically lower than the costs reported in Cohen et al. (2004), which come from valuation surveys of individuals. I rely on the former data source to be more conservative.

[50] Because Heckman et al. (2010) do not provide ratios for drug-related offenses, I assume that the share of DUI and non-DUI crimes I see in the arrest data reflects the same share of incidents.

estimate takes an average of the upper and lower bounds. Overall, the total social cost of crime that was averted ranges from $16.4 to $28.3 million.

C.2.2 Lower Fiscal Costs from Fewer Incarcerations

I begin by considering the reduced fiscal costs from fewer incarcerations. For the analysis in this subsection, I assume that local jails have no cost and only focus on the cost of incarcerating individuals in state prisons. This analysis also ignores the resources allocated to the criminal justice system to monitor individuals on probation or to arrest, charge, and convict offenders. If anything, omitting these costs will tend to understate the reduced fiscal costs.

To be conservative, I assume that not all of the men arrested for these serious crimes are sentenced to state prison spells. Using the full sample of low-income men born between 1990 and 1993, I estimate that 67 percent, 54 percent, and 42 percent of violent, property, and drug arrestees are sentenced to a prison spell.[51] For each offense, I then multiply the number of incarcerated individuals in state prisons by the average sentenced served in 2009 in South Carolina and by the daily cost per inmate (roughly $45 in 2011).[52] I allow for heterogeneity in the length of sentences served by type of offense: the average time served for violent, property, and drug crimes is 4 years, 1.9 years, and 2.2 years, respectively (Pew Center on the States 2012).[53] Overall, I find that the fiscal cost would have been reduced by $2.6 million.

If I can use the marginal—as opposed to the average—cost of incarcerating an individual, then the associated institutional costs of incarcerating one individual would be lower. Owens (2009) finds that the marginal cost of incarcerating an individual for one year is around $12,675 in 2010 dollars. If I use this estimate, I find that the total fiscal cost totals $2.1 million. To get a middle-ground estimate, I take an average of the upper and lower bounds.

C.2.3 Lower Social Costs from Fewer Incarcerations

Next, I calculate the reduced social costs from fewer incarcerations. Estimates from Mueller-Smith (2015) suggest that a two-year prison term has economic impacts (in terms of employment and

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[51] In other words, I calculate the share of individuals who were arrested for say, a violent offense and who have a documented prison spell within a year (i.e., assuming that the transition between arrest and incarceration is not immediate).

[52] For more information on correctional costs for the South Carolina Department of Corrections, see `http://www.doc.sc.gov/research/BudgetAndExpenditures/Per_Inmate_Cost_1988-2019.pdf`.

[53] Average sentences served were calculated using the National Corrections Reporting Program data.

public assistance) of around $34,650 per person and crime impacts (in terms of post-release criminal behavior) between $14,983 and $33,297.[54] Taking the lower-bound and upper-bound of these estimates suggests that a two-year prison spell has total social costs of $49,633 and $67,947, respectively. Prior calculations suggest that roughly 61 individuals in each cohort would have been sentenced to serve a state prison spell after losing Medicaid eligibility, implying a total cost between $3.0 and $4.1 million.

Finally, for the remaining individuals who were incarcerated but did not serve time in a state prison, I use the estimates from Mueller-Smith (2015) for 6-month prison terms. I find that the social costs for these individuals range from $1.3 to $1.7 million, which puts the overall total social costs between $4.3 and $5.8 million. To get a middle-ground estimate, I take an average of the upper and lower bounds.

C.3 Marginal Value of Public Funds

In this subsection, I use the causal estimates to form this policy's marginal value of public funds (MVPF) (Finkelstein & Hendren 2020, Hendren & Sprung-Keyser 2020). Specifically, the MVPF is a ratio of society's willingness to pay for this policy to the net cost of the policy to the government. Similar to the approach above, I construct both an upper and lower bound for this ratio based on different assumptions. Estimates are summarized in Table C2.

C.3.1 Willingness to Pay

I begin by calculating the numerator of the ratio, which measures the aggregate social willingness to pay for the policy change (i.e., extending Medicaid eligibility for two years). The primary component in this numerator is society's willingness to pay for fewer criminal victimizations v. I use the upper and lower bounds of the social cost of crimes averted outlined above and shown in Table C1. In constructing the upper bound of the MVPF, I also include the second round of criminal victimizations that would be averted (i.e., from men not recidivating).[55] Overall, the willingness

[54] Estimates from Mueller-Smith (2015) are deflated from 2015 to 2010 dollars based on correspondence with the author.

[55] Specifically, I assume that 30% of these men would have recidivated by age 24. I then use the raw data to calculate the share of second arrests that end with a prison spell (74%) to calculate the implied number of arrests that would have occurred. Finally, I use the arrest-to-victimization ratios as well as the share of victimizations that fall into each crime subcategory to calculate the number of implied incidents. I use these figures and the average cost of each crime to calculate the total social cost of this second round of victimizations.

to pay for fewer victimization ranges from $16.4 to $33.0 million.

Next, I consider the willingness to pay for improved labor market prospects, η, by the individuals who avoided incarceration. In other words, beneficiaries should be willing to pay for the increase in wages they experience from this policy change. To calculate this foregone income, I first use the 2009–2013 American Community Survey to calculate the employment rate and average annual income of employed men in South Carolina who were aged 19–25 and living under 200 percent of the FPL: 50 percent and $11,950, respectively (Ruggles et al. 2020).[56] I then calculate the total foregone income of affected individuals *during* incarceration: roughly $973,100.[57] For the upper bound of the MVPF, I also include the losses in income that follow the incarceration spell. I estimate the post-release employment rate of offenders (using Mueller-Smith 2015, Table 7) and use this figure to calculate foregone income in the five years after release: roughly $313,300.[58]

Second, the overall willingness to pay for this policy change includes the value of the public insurance transfer γ. In the upper bound MVPF estimate, I assume that beneficiaries would be willing to pay the average out-of-pocket spending amount for individuals ages 19–34: $370 dollars (Peterson-KFF 2020).[59] In the lower-bound estimate, I conservatively assume that beneficiaries would only be willing to pay $1 for this transfer. However, I note that the total value of the transfer γ also depends on who bears the cost of uncompensated care (Finkelstein et al. 2019). I therefore relegate the final calculation of this parameter to the following subsection.

Finally, it is worth noting that these calculations ignore the insurance value of Medicaid beyond the transfer value, society's willingness to pay for a reduction in bad health conditions, and affected individuals' willingness to pay to avoid being incarcerated (i.e., the value of freedom above and beyond improved labor market prospects). Adding such features would raise the overall willingness

[56] I do not use the estimates from Mueller-Smith (2015) quantifying the economic impact because those figures combine the effects on earnings and on public assistance.

[57] Specifically, I calculate this figure by multiplying the number of individuals who were incarcerated because of the Medicaid loss $\times$ the employment rate $\times$ the average annual income $\times$ the average sentence served, separately for each crime category.

[58] I allow the post-release employment rates to differ by crime category (i.e., individuals who served time for violent offenses have lower post-release employment rates than individuals who served time for property or drug offenses). I calculate post-release foregone income for five years, following Mueller-Smith (2015)'s approach, which estimates effects using five years of post-charge data.

[59] I verify this estimate using the Centers for Medicare and Medicaid Services estimates on out-of-pocket spending by age group, finding that males ages 19–44 on average spent $393 in 2010 (Centers for Medicaid and Medicaid Services 2020, Table 7).

to pay for this policy.

C.3.2 Net Cost to the Government

The denominator of the MVPF captures the cost to the government for this policy change, including both mechanical costs as well as fiscal externalities. The primary component is the cost of expanding Medicaid eligibility for two extra years, G. I use the middle-ground estimate discussed in Section C.1, so that G is approximately \$14.6 million. In determining the net cost to the government of this policy, however, it is important to consider the ultimate economic incidence of the transfers to external parties. Finkelstein et al. (2019) finds that 60 percent of Medicaid spending is a transfer to providers of uncompensated care for the low-income uninsured. If the government bears the cost of uncompensated care, then the total cost of this policy will only be $0.4G$. If individuals bear the cost of uncompensated care, then the total cost of this policy is G. I thus assume that the government and individuals bear the cost of uncompensated care when estimating the upper and lower bound of the MVPF, respectively. Finally, I note that in the scenario in which individuals bear the cost of uncompensated care (i.e., the lower bound of the MVPF), then society would also incorporate this component in their willingness to pay for the public insurance transfer (Finkelstein et al. 2019). In other words, γ would also include $0.6G$.

In calculating the total cost to the government, I also factor in the reduced fiscal cost from fewer incarcerations, μ, which ranges from \$2.1 to \$2.6 million (see Section C.2.2). For the upper bound of the MVPF, I also incorporate the fiscal cost of a second round of incarcerations (i.e., from recidivism), which amounts to roughly \$578,000.[60]

Finally, worse labor market prospects for individuals translate to reduced tax revenue for the government. Using a 20 percent tax rate following the approach of Hendren & Sprung-Keyser (2020), I find that the government loses between \$195,000 and \$257,000 in tax revenue.[61]

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[60] I assume that 30 percent of individuals recidivate within five years (around age 24) and that they serve an average sentence of 28 months.

[61] This analysis does not incorporate changes in spending on SNAP benefits. On the one hand, when individuals are incarcerated, the government does not need to pay for their SNAP benefits, suggesting a reduction in net costs. However, as shown in Mueller-Smith (2015), men who are incarcerated are more likely to rely on public assistance after their release. In South Carolina, offenders who have a drug-related felony conviction are not eligible, but other individuals with criminal histories are allowed to receive benefits. Furthermore, it is not clear how the incarceration of this population would affect the SNAP receipt of family members or dependents (e.g., if these men are less likely to be employed after release, then their families might be more likely to receive SNAP benefits).

Table C1: Social Costs per Crime

Offense	Percent	Ratio	Estimated Cost		
			Upper	Middle	Lower
Violent Crimes					
Murder	9.91	1.52	$4,839,653	—	$2,419,826
Sex Offenses	4.48	4.55	$142,033	—	$142,033
Robbery	37.24	5.94	$12,625	—	$12,625
Assault	48.36	4.06	$38,928	—	$38,928
Avg. Violent Crime			$190,243	$150,039	$109,836
Property Crimes					
Larceny	14.43	17.28	$473	—	$473
Burglary	65.33	15.94	$2,104	—	$2,104
MV Theft	20.24	6.75	$5,787	—	$5,787
Avg. Property Crime			$2,172	$2,172	$2,172
Drug Crimes					
DUI	5.18	—	$29,458	—	$4,075
All other	94.82	—	$0	—	$0
Avg. Drug Crime			$1,526	$869	$211
Total Social Cost			$28,267,455	$22,348,874	$16,430,294

NOTE: "Percent" refers to the share of each broad category that is classified as that particular sub-crime using all of the arrests that end with an individual being taken into custody in the SLED data. Among these arrests, 27% are violent crimes, 34% are property crimes, and 21% are drug-related offenses. "Ratio" refers to the average victimization-to-arrest ratio from Heckman et al. (2010) (Table H.6 in the Online Appendix). "MV theft" refers to motor vehicle theft. The estimated costs come from Cohen & Piquero (2009) (victim costs in Table 5, inflated to 2010 dollars) and Miller et al. (1996) (Table 2, inflated to 2010 dollars).

Table C2: Marginal Value of Public Funds (MVPF), Upper and Lower Bounds

	Estimated Cost	
	Upper	Lower
Willingness to Pay:		
Fewer crime victimizations, ν	\$32,957,163	\$16,430,294
Improved labor market prospects, η	\$1,286,410	\$973,082
Value of insurance transfer, γ	\$1,358,491	\$4,461,761
Aggregate willingness to pay	\$35,602,065	\$21,865,137
Costs to the Government:		
Cost of providing Medicaid, function of G	\$5,857,517	\$14,643,793
Fewer incarcerations , μ	–\$3,220,491	–\$2,069,931
Foregone tax revenue, 0.2η	–\$257,282	–\$194,616
Net cost	\$2,379,744	\$12,379,246
Marginal Value of Public Funds	14.96	1.77

NOTE: "Upper" and "lower" refer to the upper and lower bounds for the MVPF ratio. The upper bound deliberately biases the calibrations toward overstating the benefits and understating the costs (and vice versa for the lower bound).

D Measuring Deterrence Around Age of Criminal Majority

D.1 Sample and Variable Construction

I first restrict the full sample of men born between 1990 and 1999 to individuals who were ever enrolled in Medicaid (i.e., the individuals for whom I have an available month of birth). Because information on an individual's exact date of birth is not available, the month during which an individual turns 17 serves as the first month of age 17. The analysis thus focuses on the 12 months before an individual's 17th-birthday month as well as the eleven months that follow an individual's 17th-birthday month.

I focus on felonies throughout this analysis because the definition of felony is not a function of an individual's age. Moreover, because felonies tend to be relatively serious in nature, focusing on this group reduces the likelihood that individuals will be charged with that crime as juveniles, but not as adults (or vice versa).[62]

The next step is identifying felony offenses in the Department of Juvenile Justice's data. I classify offenses as felonies using a variable that indicates whether the referral was for a felony or misdemeanor. Because I do not have exact information on an individual's birthday, I will mistakenly label certain arrests as occurring at age 17 when they actually occurred at age 16 (i.e., offenses occurring during an individual's 17th-birthday month when the individual was still 16). To address this misclassification in ages, I (randomly) shift half of non-technical referrals occurring during any month t to month $t-1$.[63]

I then classify arrests in the South Carolina Law Enforcement Division data as felonies. First, I create a crosswalk between arrest charges and felony classifications; I use all possible arrest charge codes in the SLED files and classify that charge as a felony if a majority of those charges were labeled as felonies. I then use this crosswalk to classify the primary charge for each arrest

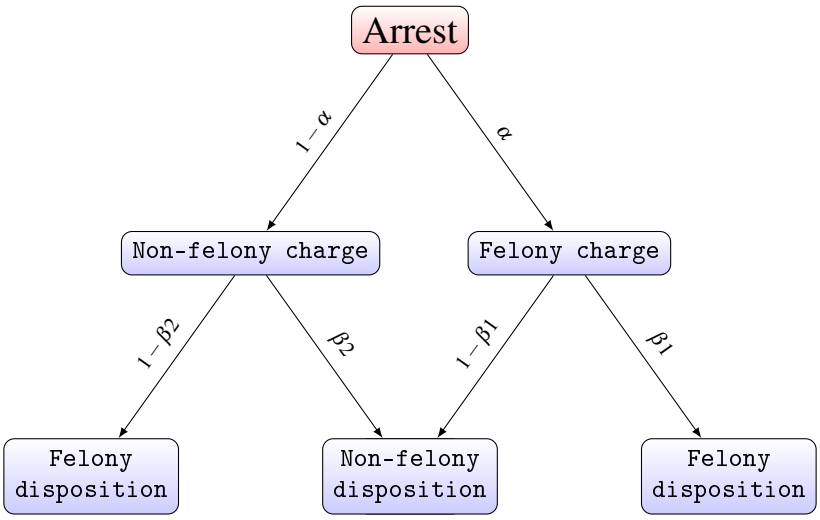
[62] To illustrate this logic, I consider the case of simple assaults: it appears that juveniles are much more likely to be referred to DJJ for simple assaults than to be charged as adults for this crime. If I included simple assaults in this analysis, it would appear as though there was a large deterrence effect, when in reality there is likely significant discretion on whether to charge individuals for certain offenses depending on their age. Focusing on felonies reduces the likelihood that the estimated discontinuity will be a product of changes in discretion.

[63] I define technical referrals as any DJJ referrals related to probation or aftercare program violations. Individuals who violate the terms of their probation or aftercare program can be referred to DJJ even after their 17th birthdays.

as a felony.[64] For the 10 percent of arrests that do not have available charge information, I use the information that describes the disposition of the arrest to classify that arrest as a felony. Specifically, I create a second crosswalk between arrest (literal) descriptions and felony classifications. I then use this second crosswalk to classify arrest dispositions as felonies.[65]

One concern with this classification method is that for the 10 percent of arrests that do not have available charge information, I may be over- or under-counting the number of felonies. For example, if between the charge and the disposition, many felony charges were not ruled to be felonies, then I would be undercounting the number of felony charges. To correct for potential misclassification, I use all of the arrests that have both a charge and a disposition available (52 percent of arrests for men ages 17 and younger) in order to gauge the magnitude of the misclassification. In other words, I use the arrests with an available charge and disposition to calculate α, β_1, and β_2 in Figure D1.

Figure D1: Likelihood of Felony Arrest Charge and Disposition



I then use these estimates to calculate the share of felonies that are incorrectly classified (i.e, they were likely non-felony charges) as well as the share of non-felonies that were mis-identified

[64] Of arrests that are labeled as felonies using this crosswalk, 99.6% would have been labeled as felonies using their original classification in the arrest data. I use this method in order to be consistent with the classification process used for the 10% of arrests that do not have available charge information.

[65] For any dispositions that were not able to be classified using this second crosswalk (e.g., because of spelling errors), I manually classified the disposition, referring back to the crosswalk for guidance.

(i.e., they were likely felony charges):

$$\text{Share of Felonies Incorrectly Classified } \gamma_1 = \frac{(1-\alpha)(1-\beta_2)}{(1-\alpha)(1-\beta_2)+(\alpha)(\beta_1)}$$

$$\text{Share of Non-Felonies Incorrectly Classified } \gamma_2 = \frac{(\alpha)(1-\beta_1)}{(\alpha)(1-\beta_1)+(1-\alpha)(\beta_2)}$$

I assume that these estimates of misclassification represent the degree of misclassification for the 10 percent of arrests with missing charge information. I then use these shares to randomly re-classify γ_1% of classified felonies in this group as non-felonies, and γ_2% of classified non-felonies as felonies. In the end, I have two measures of felony charges in the SLED data: one which uses the disposition for the missing-charge group, and one which corrects for the potential misclassification in the process.

Finally, in order to construct the figures I combine the two data sources, so that individual i is labeled as committing a felony at time t if he had a felony referral in the DJJ data *or* a felony arrest in the SLED data. I then restrict the sample one more time to individuals who had not committed a felony prior to age 16 (90 percent of individuals ever-enrolled in Medicaid). The baseline measure used in Figure 8 uses the felony classification that has been corrected for potential misclassification. I check the robustness of this result to the usage of the original felony classification in Figure D2. The results are hardly changed.

D.2 Elasticity Construction: Length of Sentences

In order to calculate the elasticity of crime with respect to sentence lengths, I calculate the expected number of days an individual is detained in juvenile detention versus in prison. First, because the DJJ data does not have information on sentence lengths, I use statistics from DJJ's Interactive Trend Reports.[66] I calculate that the expected number of days a juvenile was detained in 2011–2012 (the midpoint of the sample period) was 96 days. Specifically, I take a weighted average of the average daily population and the average stay length across the 18 facilities.

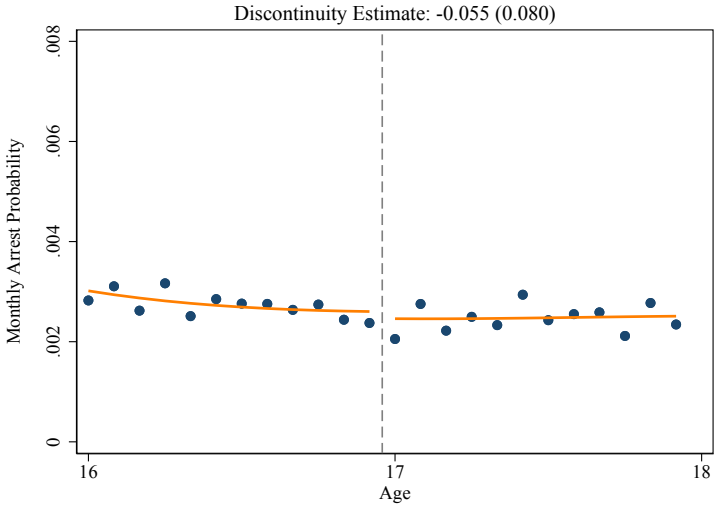
I then use the admissions data from the Department of Corrections files to calculate that the

[66] See the interactive reports at `https://publicreporting.scdjj.net/`.

average incarceration sentence for men admitted at age 17 is between 4.5–5 years. I confirm this estimate using the DOC's statistical reports, which show that the average sentence lengths for inmates admitted in 2015 (i.e., the earliest available year) is 4 years and 4 months.[67] I therefore conclude that the average sentence length for adults is approximately 1,580 days, which represents a 1,546 percent increase in incarceration lengths.

Finally, instead of considering the average punishment length, I consider the average punishment served. Using information from Pew Center on the States (2012), I conclude that men in South Carolina on average served prison spells that were 2.3 years (or roughly 28 months or 840 days), which represents a 775 percent increase in incarceration length.

Figure D2: Felony Propensity Estimates Around the Age of Criminal Majority



NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. This figure considers all men born between 1990 and 1999 who had not been arrested by the DJJ for a felony prior to age 16 ($N = 72,939$). The circles represent the share of individuals arrested for a felony in that month as a share of individuals who were at risk of being arrested for a felony. The solid line represents the estimate based on equation (6). The corresponding discontinuity estimate from this equation and its standard error (estimated by clustering at the individual level) are reported above the figure.

[67] For more statistical reports, see `http://www.doc.sc.gov/research/statistics.html`.

E Cost Comparison: Medicaid vs. Traditional Crime-Reduction Approaches

In this section, I compare the fiscal and social costs of three potential approaches for reducing crime: providing low-income young adults with access to public health insurance, increasing sentence lengths, and hiring more police officers.

E.1 Cost of Providing Medicaid Eligibility

In order to calculate the total cost of the Medicaid approach, I first consider the cost of providing insurance coverage, relying on the moderately conservative estimate calculated and discussed in Appendix C: $14.6 million. This estimate is a function of the take-up of Medicaid as well as the per-enrollee cost of Medicaid in South Carolina.

The next component is the fiscal cost of incarcerating men who were *not* deterred (i.e., men who still commit crimes). In other words, even if crime dropped by 10 percent, there would still be roughly 880 serious arrests per cohort.[68] Similarly to the calculations in Appendix C, I only consider the cost of incarcerating men in state prison. I begin by calculating the number of arrests that would have still occurred for each offense type. Specifically, I use the share of crimes that fall into each offense type—27 percent violent crimes, 34 percent property crimes, 21 percent drug-related offenses, and 18 percent miscellaneous offenses based on all serious arrests in the SLED data—to calculate the number of arrests for each offense type. I then use the raw data to calculate the share of serious arrests that have a corresponding state prison spell. I use the average sentence served for each offense type as well as the daily cost per inmate in South Carolina (Pew Center on the States 2012). The total fiscal cost of incarcerating these individuals: $20.6 million.

Finally, because these individuals are serving relatively shorter (i.e., the status quo) sentence lengths, there is a chance that they will re-offend after serving their prison spell.[69] To calculate the fiscal cost, I assume that 30 percent of the men who were incarcerated recidivate within five years and I use the average length of sentences served in South Carolina for all crimes (2.3 years).[70] The

[68] There were roughly 3,918 serious arrests among 19- and 20-year olds in the SLED data. Roughly 403 of these arrests would have been avoided if Medicaid had not expired, implying that 3,515 serious arrests would have still occurred among four cohorts of men.

[69] For simplicity, I only focus on the second round of incarcerations. The number of these men serving more than two prison spells is likely small, especially given that violent criminal behavior declines with age.

[70] Statistics come from South Carolina's Department of Corrections' reports on the recidivism rates of inmates.

fiscal cost of this second round of incarcerations totals $4.6 million.

When individuals re-offend, their crimes also impose social costs on victims. To calculate these costs, I use the share of serious *second* arrests that have an associated prison spell to back out the number of serious crimes committed. I then use the share of serious arrests by offense type and the victimization-to-arrest ratios to calculate the implied number of incidents. I use the upper-bound (i.e., the least conservative) estimate for violent crimes from Table C1 to estimate the total social costs of this second round of victimizations: $36.6 million.[71]

Summing these components together, I find that the total cost of this approach is roughly $76.5 million. Note that this approach is relatively conservative in assuming that the individuals who serve shorter prison spells do not generate additional benefits (e.g., tax revenue) after being released, and only takes into account the potential costs from re-offending.

E.1.1 Cost of Longer Prison Spells

To calculate the cost of this crime-reduction approach, I use the preferred estimate of the elasticity of crime with respect to sentence lengths from Table 8—$\varepsilon_{c,f} = -0.018$ using the full sample and the average sentence served—to estimate the degree to which sentence lengths would need to be extended to achieve a 10 percent reduction in crime. I find that sentences would need to be roughly 570 percent longer. I assume that this elasticity applies to all offense types uniformly and calculate the new average sentence length served for each type of crime. Finally, I follow the same approach as above for calculating the total fiscal cost, multiplying the number of incarcerations for each offense type by the longer sentence length and by the cost per inmate. I find that the total fiscal cost amounts to $109.6 million.

I then do a back-of-the-envelope calculation to calculate how large $\varepsilon_{c,f}$ would need to be in order for this approach to have the same cost as the provision of Medicaid. I find that $\varepsilon_{c,f}$ would need to be around -0.032, which is 78 percent larger than the preferred elasticity.

E.2 Providing Medicaid Eligibility versus Hiring Police Officers

Another favored crime-reduction approach for the past fifty years has been to hire more police officers. Indeed, a number of studies have estimated the effect of police presence on criminal activity (see e.g., Chalfin & McCrary 2018, Evans & Owens 2007, Mello 2019, Weisburst 2019).

[71] Throughout this section, I assume $0 for miscellaneous offenses and drug-related offenses.

In this subsection, I consider the cost of reducing crime by 10 percent by hiring more police officers. When comparing the cost of hiring police officers to the cost of providing Medicaid, I use the short-term cost of Medicaid provision (i.e., excluding the social and fiscal costs of recidivism): $36.1 million.[72]

Similarly to the analysis studying sentence lengths, I use the elasticity of crime to police (i.e., $\varepsilon_{c,p}$) to calculate how many police officers would need to be hired in order to achieve a 10 percent reduction in crime. Using the elasticity of crime to police officers from Evans & Owens 2007 ($\varepsilon_{c,p} = -0.3841$), I find that the state would need to increase the overall size of their police force by 31 percent, which implies hiring roughly 3,600 more police officers.[73,74] Assuming a marginal cost of $130,000 for hiring a police officer, the fiscal cost of this policy amounts to $913.9 million (Chalfin & McCrary 2018).

However, hiring police officers does not only reduce the criminal activity of 19- and 20-year olds, but it has spillover effects on the criminal activity of individuals of other ages. To calculate the number of additional crimes averted in other age groups, I begin by looking at the age distribution of admitted inmates in South Carolina (focusing on individuals ages 17 through 40, who make up the majority of individuals committing crime). I then use the share of serious arrests that end in state prison (calculated from the raw data) to back out the number of total arrests in this age group. Next, I use the share of arrests that are violent and property offenses in this sample to calculate the implied number of violent and property arrests for this age group. I then use the violent and property crime elasticities from Evans & Owens (2007) to estimate how many fewer arrests there would be given the increased number of police officers.[75] Finally, I use the victimization-to-arrest ratios from Heckman et al. (2010) to calculate how many fewer violent and property incidents there would be if more police officers were hired. Using the upper-bound social costs of violent crime from Table C1, I find that hiring police officers would reduce the social costs of violent

[72] When comparing Medicaid provision to longer sentence lengths, possible recidivism from using shorter sentence lengths needs to be accounted for a more accurate comparison. When comparing Medicaid provision to hiring more police officers, I can focus on short-term costs because individuals can recidivate under both policies.

[73] This estimate of $\varepsilon_{c,p}$ is a weighted average of the elasticities for violent and property crimes in Evans & Owens (2007), where the weights are the share of crimes belonging to each category.

[74] In 2008, the total number of sworn personnel in South Carolina was 11,674 (Reaves & Hickman 2011).

[75] Because the papers in this literature typically focus on violent and property crimes, I make the (plausible) assumption that the social cost of drug-related and miscellaneous offenses is $0 and thus ignore these offenses in the calculations.

Table E1: Summary of Estimated Net Cost of Hiring Police Officers (In Millions)

	Lower-bound cost	Upper-bound Cost
Marginal cost: $130,000		
Ages 18–40	$155.4	–$385.3
Ages 18–30	$509.8	$229.6
Marginal cost: $73,000		
Ages 18–40	–$245.3	–$786.0
Ages 18–30	$109.0	–$171.1

NOTE: This table reports the estimated net costs of hiring police officers (i.e., negative costs imply that the benefits outweigh the costs). The top and bottom panels use a higher and lower marginal cost of hiring police officers, respectively (Chalfin & McCrary 2018, Evans & Owens 2007). The two columns use the lower- and upper-bound costs of violent crimes from Table C1. The age range indicates the extent to which hiring police officers is assumed to deter criminal activity among individuals in other age groups.

and property victimizations by $1.3 billion and $18.6 million, respectively. Finally, there is an additional reduction in fiscal costs of $21.8 million from fewer individuals being incarcerated (after multiplying the number of individuals in these other age groups who would have likely been incarcerated by the daily inmate cost and average sentence served).

The results from this exercise suggest that the benefits of hiring more police officers outweigh the costs. However, I note that these results are sensitive to the cost of violent crimes—especially because the evidence from prior studies shows that violent crime is particularly responsive to police presence—as well as to the assumption that police reduces crime uniformly for individuals ages 17 through 40. If I use the lower-bound for the cost of violent crime in Table C1 and assume that the spillovers only affect men ages 18–30, then I find that this policy has an overall net cost of $509.8 million, which would favor Medicaid provision over hiring more police officers. Table E1 summarizes how the estimated costs change as I alter certain assumptions, including lowering the marginal cost of hiring police officers.

Overall, the calculations from this subsection suggest that in terms of crime reduction, Medicaid provision is likely less cost effective than hiring more police officers. There are two caveats to this conclusion. First, this analysis does not take into account the social costs of policing (for example, the social costs of police brutality, excessive force, or over-policing). Second, the calculations

ignore the non-crime-related benefits that health insurance might provide to individuals (e.g., in terms of financial stability, earnings and tax revenue) (Gallagher et al. 2019, Gross & Notowidigdo 2011, Hu et al. 2018). Consequently, even though I am making a relatively comparable comparison for these policies—estimating the cost of a 10 percent crime reduction—it is almost certain that these calculations are underestimating the associated benefits of providing low-income young adults with Medicaid access.