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

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Individuals with mental illness are over-represented in the incarcerated population. This paper studies whether improving access to mental healthcare can reduce men's criminal propensity using linked administrative data from South Carolina. Leveraging a break in Medicaid eligibility and a difference-in-differences strategy, I find that men who lose eligibility become more likely to be incarcerated relative to a matched comparison group. The effects are driven by men with mental illness, suggesting that decreased access to mental healthcare played an important role in affecting men's criminality. Cost-benefit analyses show that expanding Medicaid eligibility to low-income young men is a cost-effective crime-reduction policy.

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1 Introduction

Policymakers and academics in the U.S. have long 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 more therapeutic approaches (Menninger 1968). Another prominent perspective, put forth by economist Gary Becker, posited that offenders weigh the costs and benefits of engaging in crime, and can thus be deterred from committing offenses by either improving the alternatives to criminal activity or raising the expected cost of crime (Becker 1968).

In the decades that followed, crime rates rose in many cities across the country. Policymakers embraced the notion that raising the cost of crime would deter offenders (Donohue 2007), and thus increased the severity of punishments at both the state and federal level. These policies contributed to rapid growth in the incarcerated population (Raphael & Stoll 2013b). Today, incarceration is a common occurrence for low-income and minority men (Western & Pettit 2010). Recent research has documented the adverse effects of incarceration including increased barriers to employment and greater reliance on public assistance (see, e.g., Dobbie et al. 2018, Mueller-Smith 2015) in addition to its onerous fiscal costs (Kearney et al. 2014). 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 offenders.

This paper revisits the role of mental healthcare in reducing criminal behavior. To estimate the causal effect of mental healthcare on crime, I leverage a discrete break in public health insurance eligibility in South Carolina and I study how losing coverage impacts low-income men's likelihood of incarceration. South Carolina, like other states in the U.S. South, provides low-income children, but not childless adults, free health coverage via the Medicaid program. Individuals who are enrolled in Medicaid throughout adolescence therefore age out of their eligibility on their 19th birthdays. To consider the specific role of mental healthcare in affecting criminal behavior, I employ individual-level administrative data linking health insurance claims to criminal records, thereby allowing me to estimate the effect of this lapse in coverage on the criminal outcomes of men with mental health histories.

Specifically, I use a matched difference-in-differences approach, in which I study the outcomes of men who were impacted by the termination in eligibility. To account for age trends in crime, these men are compared to similar low-income men who were likely eligible, but not enrolled in the Medicaid program right before their 19th birthdays (e.g., because of hurdles to enrolling in the program or other idiosyncratic reasons) and who were therefore *less* affected by the loss in eligibility. I implement a matching procedure—using demographic characteristics, geographic location, and mental health histories—to select this group of observationally similar men that were not enrolled in the Medicaid program. The research design relies on the assumption that despite any remaining differences between the two groups, the comparison group aids in tracing out the counterfactual incarceration paths of the affected men after their 19th birthdays. I provide support for this assumption by showing that the two groups have very similar incarceration propensities for the year and a half prior to age 19, and only begin to diverge when affected men lose access to Medicaid services.

I find that men who lose access to Medicaid coverage are 0.5 percentage points (or 14% relative to the comparison group mean) more likely to be incarcerated in the two years following the loss in eligibility. These baseline results suggest a strong, positive relationship between Medicaid disenrollment and criminal activity among low-income young men. When I split the sample by men's mental health histories, I find that the effects are entirely driven by men with mental illness. By their 20th birthdays, men with mental health histories who lost access to services are 2.5 percentage points (or 22%) more likely to have ever been incarcerated relative to men in the comparison group. By age 21, these figures have risen to 3 percentage points (or 21%). I find increases in violent and property crimes, and especially financially motivated offenses, suggesting that losing access to mental healthcare impacts serious criminal involvement. I also find that the effects are particularly pronounced for men who were filing mental health claims right before their 19th birthdays and for those who relied on Medicaid for access to mental health medications. In contrast, I find no increases in incarceration among low-income men who lost access to Medicaid coverage, but who did not have a history of mental illness. Together, these findings suggest that decreased access to mental healthcare plays an important role in explaining the observed rise in criminal activity.

I validate these findings through five additional exercises, all of which suggest that losing access to mental health services increases criminal propensity. The central concern with the empirical strategy is that the comparison group may not be a suitable group for estimating the counterfactual outcomes of affected men in late adolescence. To consider this possibility, I first show that the results are robust to augmenting the matching procedure with additional characteristics to reduce observable differences between the two groups. Second, I estimate a separate matched difference-in-differences specification, comparing Medicaid enrollees who filed mental health claims before their 19th birthdays to other enrollees who did not file such claims. In this comparison, I also find an increase in the likelihood of incarceration starting at age 19 for the group of men filing mental health claims. Third, instead of relying on a comparison group to estimate causal effects, I implement a regression discontinuity approach in which I estimate individuals' likelihood of being arrested around their 19th birthdays. I find that men with mental health histories—and especially those filing mental health claims in late adolescence—are significantly more likely to be arrested upon reaching age 19, with no comparable increases for men without mental health histories. Fourth, I conduct falsification checks, in which I replicate the baseline empirical approach around earlier birthdays. I do not find increases in incarceration around ages 17 or 18, implying that the increase at 19 is driven by the loss in Medicaid eligibility. Finally, acknowledging that other important life transitions occur in late adolescence, I show that both groups had similar rates of school enrollment prior to age 19. These results thus suggest that the estimated effects are not confounded by shocks related to educational attainment. I also show that the effects are not driven by a small group of men aging out of other programs (i.e., foster care and the Supplemental Security Income program) at age 18.

In the last part of the paper, I use the estimates quantifying the effect of losing Medicaid eligibility on criminal behavior to conduct cost-benefit analyses. First, I use back-of-the-envelope calculations to show that the benefits of providing low-income young men with Medicaid eligibility (in terms of reduced fiscal costs from fewer incarcerations and reduced social costs from fewer victimizations) outweigh the program costs. Next, I benchmark the cost effectiveness of Medicaid provision 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 (Hjalmarsson 2009, Lee & McCrary 2017, Mueller-Smith et al. 2022) and leverage the age of criminal majority to show that low-income adolescents in this sample are relatively undeterred from engaging in criminal behavior when faced with harsher criminal sanctions. Given these limited effects, 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 might consider improving access to healthcare as an approach for reducing crime and lowering criminal justice expenditures.

This paper contributes to four strands of literature. The first is a growing literature studying the relationship between mental health, mental health services, and criminal activity. Seminal work in this literature has quantified the association between mental illness and criminal behavior (see, e.g., Swanson et al. 1990, Teplin et al. 2002). Subsequent studies have then considered the potential role of mental health services in influencing criminal activity.[1] Specifically, Heller et al. (2017) implements a randomized controlled trial in Chicago and shows that an intervention that included a cognitive behavioral therapy component curbed the criminal behavior of adolescent boys during the program.[2] Moreover, using cross-state and cross-county variation, related studies have estimated the effect of changes in the amount of mental healthcare provision on local crime rates (Deza et al. 2022 considers the number of office-based mental health providers, Bondurant et al. 2018 the number of substance abuse facilities, and Marcotte & Markowitz 2011 the number of psychotropic drug prescriptions). Finally, concurrent work has found that connecting former inmates with severe mental illness to mental health providers (Batistich et al. 2021) as well as referring offenders with substance abuse disorders to drug diversion programs (Arora & Bencsik 2021) can reduce recidivism rates.

Building on these findings, this paper quantifies the causal effect of mental healthcare on men's likelihood of incarceration by studying changes in Medicaid eligibility. Medicaid is the largest provider of mental healthcare in the United States, so this paper considers how reducing access to these behavioral health services shapes the criminal propensity of low-income young men, the population most likely to come into contact with the justice system.[3] Crucially, I leverage exogenous variation in access to mental healthcare at the individual level to study the outcomes of affected men relative to similar individuals in close geographic proximity who are less affected by changes in healthcare access. This study therefore does not rely on local policy variation or individual enrollment choices that may be correlated with other local-level or individual-level changes, respectively. Finally, this paper considers a broad and relatively young population of low-income men, highlighting how access to mental healthcare in late adolescence—during the peak of the age-crime profile—can influence their likelihood of being incarcerated for the *first* time.

[1] Numerous other studies consider the relationship between mental health and educational attainment (e.g., Busch et al. 2014, Cuellar & Dave 2016 and Currie & Stabile 2006), labor market outcomes (e.g., Biasi et al. 2021, Bütikofer et al. 2020, and Ridley et al. 2020), and other risky behaviors (e.g., Chorniy & Kitashima 2016).

[2] Similarly, Blattman et al. (2017) shows that in Liberia, interventions that combine therapy and cash reduce crime and violence.

[3] 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).

Understanding the factors that contribute to a person's first incarceration spell may be especially relevant given high rates of recidivism in the U.S. criminal justice system (Durose et al. 2014).

Second, this paper adds to a recent literature studying the effects of health insurance expansions on public safety, which finds that these expansions—for example, through the Affordable Care Act—reduce the incidence of certain violent and property offenses (Aslim et al. 2019, Fry et al. 2020, He & Barkowski 2020, Vogler 2020, Wen et al. 2017). An open question in this literature is the extent to which access to mental healthcare can explain the relationship between health insurance and crime rates. Yet, assessing the specific role of mental healthcare has been challenging due to the lack of data linkages between state agencies in the United States. The findings of this paper indicate that changes in offending are entirely driven by men with mental health histories, thereby providing new evidence on the role of mental healthcare in affecting public safety.

This paper further contributes to a broad literature studying factors that influence deterrence and desistance from crime (see, e.g., Chalfin & McCrary 2017 and Doleac 2020). This paper complements a number of recent studies quantifying the effects of public assistance programs on criminal activity and recidivism (Deshpande & Mueller-Smith 2021, Luallen et al. 2018, Tuttle 2019, Yang 2017) by focusing on the role of access to health services. In the same vein, this paper argues that criminal involvement is a function of health, similar to a number of papers that study the effect of cognitive health—via changes in lead exposure—on criminal behavior (Aizer & Currie 2019, Billings & Schnepel 2018, Feigenbaum & Muller 2016, Reyes 2007).

Finally, this paper adds to a growing literature quantifying the social returns to Medicaid (e.g., Arenberg et al. 2020, Boudreaux et al. 2016, Brown et al. 2020, Goodman-Bacon 2021). 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 among low-income adolescents. The findings can therefore help quantify the short-term returns to increasing Medicaid access for young adults, which are likely of interest to policymakers weighing the costs and benefits of expanding public insurance coverage.[4] Second, whereas previous studies typically focus on historical expansions of Medicaid for children, this study focuses on the provision of Medicaid eligibility to modern cohorts of young adults. Because young adults are relatively less likely to be insured—and are

[4] Most state constitutions include balanced budget requirements or debt limitations that prohibit policymakers from borrowing to fund expenses with long-term returns. Additionally, politicians seeking re-election might prefer to promote policies with short-term benefits (Jacobs 2016). Hence, quantifying a policy's short-term returns is likely of particular interest.

thus the group that stands most to gain from modern health insurance expansions—understanding the returns to this investment is of policy relevance.

The remainder of the paper is organized as follows: In Section 2, I discuss the relationship between mental illness and criminal behavior and the role that mental healthcare can play in tempering this relationship. Section 3 describes 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 Section 7 goes through a series of robustness checks. Section 8 explores heterogeneous effects. Section 9 conducts 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 (see, e.g., Menninger 1968). Individuals who held this view 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.

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 weigh the associated costs and benefits when deciding whether to commit a crime (Becker 1968). The implications of this 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 by raising the opportunity cost of crime (e.g., via improved employment opportunities).

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 five-fold increase in the incarceration rate (Pfaff 2017, Neal & Rick 2016, Raphael &

Stoll 2013b). 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.[5] Raphael & Stoll (2013a) finds that around 5% 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.

2.2 Prevalence of Mental Illness Among Criminal Justice Populations Today

Today, the relationship between mental illness and criminal behavior is considerably more well-established (see Frank & McGuire 2010 for a detailed review).[6] Individuals with mental illness are significantly over-represented in prisons and jails: 37% of prison inmates and 44% of jail inmates have been diagnosed with a mental disorder prior to incarceration (Bronson & Berzofsky 2017). Figure 1 plots the cumulative likelihood of incarceration for low-income men with and without prior mental health diagnoses using the primary data source in this paper. Low-income men *with* a mental health history are almost three times more likely to have been incarcerated by age 24 than men *without* a mental health history. Given the high prevalence of mental illness among the incarcerated population, the criminal justice system spends a significant share of its resources housing and treating these individuals, especially given their higher recidivism rates, longer sentences, and more expensive medical needs (Osher et al. 2012).

The persistent relationship between mental illness and criminal involvement raises the question of whether improved access to health services can reduce the likelihood that mentally ill individuals come into contact with the criminal justice system. It is worth mentioning 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, considerable scientific progress has been made in understanding and treating mental illness, including important developments and improvements of psychotropic drugs (e.g., antidepressants, mood stabilizers) as well as alternative modes of psychotherapy (see,

[5] 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).

[6] 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 (Glied & Frank 2014).

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 Mental Healthcare in Affecting Criminal Propensity

There are various ways in which losing access to health insurance could affect an individual's criminal behavior. This study focuses on the role of mental healthcare: for many individuals, losing Medicaid eligibility means losing access to mental health treatments, medications, or other resources.[7] Loss of access to mental healthcare could result in increased criminal behavior for multiple reasons.

First, individuals who lose access to behavioral health services might be more prone to making errors in judgment or decision-making, including decisions related to engaging in criminal behavior (for instance, individuals might underestimate the costs of committing an offense).[8] Second, 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 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. Criminal alternatives may also become more attractive if decreased access to mental healthcare disrupts an individual's human capital formation or labor market productivity. Biasi et al. (2021) finds that increased access to lithium—a psychiatric medication primarily used to treat mood or depressive disorders—improved the career trajectories of individuals with bipolar disorder. In the same vein, 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 mental healthcare can minimize these effects, then losing access could hinder an individual's academic achievement. Finally, health insurance coverage provides individuals with access to resources (e.g., social workers, community-based services) that could help de-escalate mental health crises or treat substance abuse, thereby preventing future criminal involvement. In Section

[7] Loss of insurance could also affect criminal behavior via changes in expected medical costs (i.e., an income effect). Previous studies have found that access to Medicaid reduces out-of-pocket medical spending, thereby freeing up additional resources for the household (Gallagher et al. 2019, Gross & Notowidigdo 2011, Hu et al. 2018).

[8] These reasons illustrate that mental healthcare affecting criminal behavior is not at odds with the canonical model of the economics of crime (Becker 1968). Indeed, if mental healthcare improves an individual's judgment or decision-making, then increased access to mental healthcare could complement incentive-based strategies for reducing crime.

8, I study changes in incarceration by offense type in order to gain insight into which of these channels might contribute to changes in criminal propensity.

3 Data and Sample

This paper studies the effect of health insurance coverage on criminal behavior 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.[9]

The data source is administrative records from various state agencies that are linked at the individual level. This dataset is relatively unique in the context of U.S. administrative data in that it includes information from both health and law enforcement agencies. South Carolina's Revenue and Fiscal Affairs (RFA) Office linked data from six state government agencies for this study. The linking enables me to identify the same person across datasets and over time starting at age ten. For more details on the sample selection, sample restrictions, and variable construction, I refer the reader to Appendix B.

3.1 Sample

The sample used in this study is a disproportionately low-income group of male adolescents born between 1990 and 1993.[10] Specifically, an individual is included in the sample if he ever attended a high school among the poorest half of high schools in the state. One can thus think of this sample as representing the male residents of the poorest half of neighborhoods in South Carolina. Furthermore, because I need information on an individual's birth month (only available in the Medicaid records) for the analysis, I restrict this sample to individuals who have ever been enrolled in the Medicaid program (71% of this low-income sample).

3.2 Medicaid Claims

Detailed information on 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 demographic characteristics as well as the dates of enrollment spells. Additional data files

[9] 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).

[10] Given the focus on criminal involvement, I focus on men in this paper because men are significantly more likely to commit crimes and come into contact with the criminal justice system (Freeman 1999). In South Carolina, among individuals who serve a prison spell before age 21, 96% are men.

contain claims information from visits to doctors and hospitals as well as pharmacy claims.

The insurance claims data allow me to classify visits and prescriptions as mental healthcare. Mental health diagnoses are those corresponding to the mental, behavioral, and neurodevelopmental disorders category. Mental health medications refer to antidepressant, antianxiety, and antipsychotic medications as well as medications used to treat attention-deficit/hyperactivity disorder (ADHD). 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. Data from the DOC provide details on incarceration spells in state prisons. Data from DJJ contain information on all contact between adolescents and the juvenile justice system. In South Carolina, individuals are legally treated as adults on their 17th birthdays.

The main outcome of interest is men's likelihood of incarceration, which peaks in prevalence in men's late teens and early twenties, and is particularly common in low-income communities (Freeman 1999, Lofstrom & Raphael 2016). I measure this outcome by combining the SLED records—which identify individuals who were arrested and subsequently detained in a correctional facility—with the DOC data, which track incarceration spells. This outcome therefore measures the likelihood that an individual is incarcerated in any adult correctional facility.

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 that provide information on enrollment in the Supplemental Nutrition Assistance Program (SNAP) and in the Temporary Assistance for Needy Families (TANF) program. Finally, I use death certificate records from the Department of Health and Environmental Control.

4 Empirical Strategy

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 context and the research design, a matched difference-in-differences framework. I then describe the matching procedure and discuss the characteristics of the matched sample.

4.1 Background: Aging out of Medicaid Eligibility

In South Carolina, children with household incomes up to 208% of the federal poverty level (FPL) are insured via the Children's Health Insurance Program (CHIP), which is operated through the Medicaid program (SCDHHS 2020b). Upon reaching 19 years of age, low-income residents age out of eligibility and childless adults have limited access to Medicaid services.

Because I do not see any other health data besides Medicaid records, 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. Appendix Figure A1 uses data from the American Community Survey to show that in South Carolina, the share of men who are uninsured increases rapidly once individuals reach 19 years of age.[11]

4.2 Research Design

In order to measure the effect 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 the Medicaid program before their 19th birthdays—and who were thus affected by the sudden termination in eligibility—serve as the "treated" individuals in the analysis (throughout the remainder of the paper, I refer to this group as the "high-enrollment" group). Importantly, because all high-enrollment men 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 account for age trends in crime, which are especially significant in late adolescence. I therefore use men who were enrolled in Medicaid earlier in adolescence, but who were *less* likely to be enrolled prior to their 19th birthdays as the comparison group (henceforth, the "low-enrollment" group).

A natural question is why men in the low-enrollment group were enrolled in Medicaid earlier, but not later in adolescence. Unfortunately, there is no information available in the datasets to answer this question, but there are a few potential explanations. First, many low-enrollment men were likely eligible for the Medicaid program, but were not enrolled because of hurdles to enrolling

[11] Moreover, 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).

in the program.[12] Indeed, Edwards & Kellenberg (2013) documents that families in South Carolina were deterred from applying to or re-enrolling in the Medicaid program every year due to long waits at the county office as well as costly administrative requirements (like needing to provide wage information from employers). Two other potential explanations are that the low-enrollment group's families experienced positive income shocks that made them ineligible for the program, or that these men (or their families) decided that they no longer wanted or needed access to Medicaid services. These latter two possibilities imply that on average, the low-enrollment group might have higher-income parents and might be relatively healthier in late adolescence than the high-enrollment individuals.[13] Regardless of the reason for not being enrolled, however, the key component for the research design is that the comparison group did not experience a sudden change in healthcare access at age 19. I can thus use this group to estimate counterfactual outcome paths around this age and disentangle the effect of the insurance loss from pure age effects.

In practice, 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 before and after their 19th birthdays. Figure 2 offers a graphical timeline of the approach.[14] Crucially, this empirical strategy allows me to compare the outcome paths of the two groups for a full year and a half prior to their 19th birthdays, and then see if their outcomes diverge precisely when the high-enrollment group loses access to health services. If the two groups have similar incarceration trajectories and *only* begin to diverge when high-enrollment men lose access to Medicaid, then I can attribute the differences in outcomes to the loss in eligibility. The effect of the termination in eligibility on incarceration is likely not immediate, so I follow the outcomes of the two groups for the eight quarters that follow their 19th birthdays.

[12] It is most likely the case that men who were eligible for, but not enrolled in the Medicaid program were uninsured. Among low-income children in South Carolina, 20% were uninsured during this time period (Edwards & Kellenberg 2013). Among low-income boys who were not insured via Medicaid between the ages of 16 and 18, slightly more than half reported that they had no health insurance coverage (Ruggles et al. 2020).

[13] In practice, individuals choosing to not enroll in the program could also be related to the enrollment hurdles. If families weigh the costs and benefits of enrolling in the program each year and the costs are high, then relatively healthier families will be deterred from enrolling (Shepard & Wagner 2021).

[14] I assign men into the two groups a year before the study pre-period to guarantee that the assignment is uncorrelated with their outcomes around age 19. First, because Medicaid enrollment is a function of income, I assign men into a group significantly before their 19th birthdays to ensure that any estimated effects at age 19 are coming from the loss in eligibility, and not from contemporaneous income shocks (i.e., to avoid Ashenfelter's dip). Second, men who are incarcerated do not have access to Medicaid services, and are therefore less likely to be enrolled in the program. In Section 7, I check the sensitivity of the main results to altering the timing of assignment into groups.

4.3 Matching Procedure

To ensure that the low-enrollment men serve as a suitable group for estimating high-enrollment men's counterfactual outcome paths around age 19, I implement a matching procedure that guarantees balance along key 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 men who might have left the state, I also restrict the sample to those who were present in any of the six data sources between ages 15–18. I then drop individuals who passed away or were incarcerated before age $17\frac{1}{2}$ because these outcomes could mechanically determine the assignment of an individual into a group. There are 22,063 eligible high-enrollment and 8,964 eligible low-enrollment men, which is 93% of the men in the low-income sample who are born in these cohorts and have an available birth month.[15]

I then match each high-enrollment person to all "counterfactual" low-enrollment individuals using a parsimonious set of characteristics (similar to the approach used in Smith et al. 2019). Specifically, I use year of birth, race (measured as Black or non-Black), school district, and mental health history prior to age 16 (measured as having a mental health claim between the ages of 10 and 15) as matching characteristics.[16] 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 high-enrollment individuals in the absence of the Medicaid loss. Importantly, Figure 1 illustrates that men with prior mental health histories are on different criminal trajectories than men without a mental health diagnosis. Matching on mental health history before age 16, when both groups were enrolled in Medicaid, is therefore useful for matching high-enrollment men to low-enrollment men with similar criminal propensities (as opposed to 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 7.

Finally, to ensure balance between the two groups, I re-weight each successfully matched low-enrollment individual by one over the number of comparison men that were successfully matched to the corresponding high-enrollment individual. In practice, this process re-weights the group of low-enrollment men, so that the high- and low-enrollment groups resemble each other

[15] 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. See Table B1 for the sample size as I impose each of these restrictions.

[16] The age of first diagnosis is relatively early (around ages 11–12) in this sample and most individuals with an adolescent history of mental illness receive a diagnosis prior to age 16. See Appendix B for more detail.

with respect to the matching characteristics.[17]

4.4 Characteristics of Matched Sample

Table 1 reports summary statistics for the full sample and for the high- and low-enrollment groups. All characteristics are measured starting at age 10 and before an individual's 19th birthday. On average, high-enrollment men (column 2) are observably different from the group of individuals who were ever enrolled in Medicaid (column 1). In particular, high-enrollment men are more likely to be Black and more likely to have a mental health history. These differences in observable characteristics reinforce the importance of implementing the matching procedure in order to identify low-enrollment men who are a suitable comparison group for estimating the counterfactual outcome paths of high-enrollment individuals.[18]

The matching procedure guarantees balance between the high- and low-enrollment groups in terms of race, cohort, geographic location, and mental health history. However, this table makes clear that even after matching on key observable characteristics, the two groups are not identical. Indeed, consistent with the potential reasons for not being enrolled—costs to enrolling, higher incomes, and better health—the low-enrollment men seem to have slightly higher-income parents and are somewhat more positively selected. To consider how the longer-term trajectories of the two groups might vary given these differences, Appendix Figure A2 plots the raw means of test scores as well as juvenile justice referrals for felony offenses for the two groups earlier in adolescence. These figures confirm that even though the two groups are not identical, they were relatively similar to each other in terms of their educational achievement and criminal propensity—both in levels and in trends—throughout adolescence. These figures thus provide reassuring evidence that the low-enrollment men will serve as a suitable group for estimating the counterfactual incarceration paths of the high-enrollment men around their 19th birthdays.[19] In Section 7, I more directly consider the remaining differences between the two groups and show robustness of the main results to including additional characteristics (enrollment in the SNAP program, prior juvenile justice

[17] If a low-enrollment individual is matched to more than one high-enrollment person, then his total weight in the sample is the sum of the weights from each match. All high-enrollment men are assigned a weight of one.

[18] Table A1 compares means for the matching candidates (i.e., the men who met the criteria for being eligible for the matching procedure) as well as the successfully matched individuals. This table shows that 97% of eligible high-enrollment individuals were successfully matched to at least one comparison individual.

[19] Buckles & Hungerman (2013) finds that children born in the winter are more likely to have mothers who are younger, less educated, and less likely to be married. I find no significant difference in the likelihood that high-enrollment men are born in the winter, providing reassurance that the family backgrounds of these two groups are not drastically different (see Figure A3 for the distribution of birth months by group).

contact, and school attended) in the matching procedure.

4.5 Loss of Medicaid Eligibility in Matched Sample

To confirm that high-enrollment men are indeed affected by the loss of Medicaid eligibility, Figure 3 plots the share of men enrolled in the Medicaid program before and after age 19. Given that individuals were assigned into groups prior to age $17\frac{1}{2}$, we see that a portion of men in the high-enrollment group become naturally disenrolled before age 19 and the share of low-enrollment men enrolled in the pre-period is not mechanically zero (i.e., mean reversion). Nevertheless, this figure highlights that high-enrollment men experience a large decline in enrollment after their 19th birthdays, while men in the low-enrollment group are significantly less likely to be affected by the loss of eligibility. Among high-enrollment men, the average share enrolled drops from 77% in the pre-period to 13% in the post-period, thereby confirming the presence of a large "first stage." To the extent that losing Medicaid eligibility impacts criminal activity, then we would expect to see significant changes in the outcomes of the high-enrollment group exactly around these individuals' 19th birthdays.

The second panel of Figure 3 plots the raw means separately by mental health history, showing that both groups of high-enrollment men experience a decline in Medicaid enrollment. The drop in enrollment among men with mental health histories is smaller in magnitude than the drop for men without mental health histories, which would bias me against finding an effect for the former group relative to the latter. Appendix Figure A4 shows analogous figures using claims data, confirming that both groups of men are more than 30 percentage points less likely to file claims in the post-period. Finally, Appendix Figure A5 shows that the high-enrollment men did not experience declines in SNAP or TANF enrollment around their 19th birthdays, illustrating that insofar as this group experienced changes in access to public assistance at this age, then it was only as a result of the lapse in Medicaid eligibility.

5 Estimating the Effect of Losing Medicaid on Criminal Behavior

5.1 Baseline Difference-in-Differences 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 before and after their 19th birthdays. The regression analogue of this comparison is a fully dynamic difference-in-differences regression of the following form:

$$Y_{it} = \eta + \sum_{\tau=-6}^{\tau=7} \Big[\beta_\tau(\text{Treat}_i \times \gamma_\tau) + \theta_\tau\gamma_\tau\Big] + \mu\text{Treat}_i + \delta_t + \varepsilon_{it} \tag{1}$$

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- and post-period are six and eight quarters, respectively. Treat_i is an indicator variable equal to 1 for high-enrollment men, δ_t are calendar time fixed effects, and ε_{it} is an error term. Standard errors are clustered at the individual level, and regressions are weighted using the weights derived from the matching procedure.

The coefficients of interest are β_τ, which estimate the differences in outcome Y at event time τ between the high- and low-enrollment men. 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 $\tau = -1$.

The two main outcomes I consider are whether an individual is incarcerated at time t and the cumulative analogue of this variable, measuring whether an individual has *ever* been incarcerated by time t. This latter variable allows me to differentiate whether individuals are recidivating, or whether they are being incarcerated for the first time upon losing access to health services.[20] Because these outcomes are relatively rare occurrences, outcome variables are multiplied by 100.

When presenting the results, I typically plot the estimates of β_τ in order to depict the dynamic evolution of outcomes before and after age 19. I also report the post-period average of the β_τ coefficients ($\frac{1}{8}\sum_{\tau=0}^{7}\beta_\tau$), or in other words, the average treatment effect in the post period. When considering the cumulative outcome variable, I instead report the last coefficient β_7, which measures the 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 comparison group's average outcome in the post-period. When using cumulative variables, I re-scale β_7 by the comparison group's average outcome in the final period of the post-period.[21]

[20] The use of a cumulative variable is also motivated by the imperfect information on an offender's full incarceration spell, making this measure my preferred outcome. Specifically, the SLED data do not contain information on when an individual was released from custody. By failing to capture an offender's detainment status, estimates that rely on an individual's incarceration in a given time period are likely underestimates of an individual's true likelihood of being incarcerated in any given quarter.

[21] Because criminal propensity tends to rise with time in this age range, re-scaling the estimated effect by the comparison group's average outcome 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 outcome. This choice is also motivated

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. Accordingly, the identifying assumption in this empirical strategy is that high-enrollment men would have trended similarly to the low-enrollment group in the absence of the Medicaid loss.

One concern with this identifying assumption might be that men in the high- and low-enrollment groups are fundamentally different and thus might have different outcome paths. For example, if high-enrollment men are lower-income than the comparison group, then we might expect the former group to be more likely to engage in criminal activity regardless of the loss in Medicaid eligibility. Matching on observable characteristics—in particular, cohort, race, and geographic location—helps alleviate this concern. Perhaps more importantly, however, even if the two groups differ in their overall levels of incarceration, the identifying assumption relies on the two groups *trending* similarly prior to their 19th birthdays. In practice, I plot the raw data and estimate non-parametric specifications in order to corroborate the plausibility of this parallel trends assumption (i.e., that low-enrollment men are a suitable comparison group for estimating the counterfactual outcomes of the high-enrollment men in the absence of the insurance loss). If the two groups begin to diverge right after high-enrollment men's 19th birthdays, then I can attribute that divergence to the sudden lapse in Medicaid coverage.

The second threat to the identifying assumption—standard to the difference-in-differences framework—is that high-enrollment men might be experiencing other shocks at the same time as the loss in Medicaid eligibility, thereby confounding the estimated effects. Three factors mitigate this concern. First, other laws and policies associated with the transition into adulthood (e.g., the school-leaving age, the age of medical consent) occur earlier in adolescence in South Carolina, so that the loss of eligibility on an individual's *19th* birthday is unique to the Medicaid program. Second, high- and low-enrollment men turn 19 at similar times (by construction). And third, 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 similar low-income men living in close proximity) *and* that the timing of these

by the identifying assumption: the comparison group is tracing out the counterfactual outcome paths of the high-enrollment men, so I re-scale the estimates by the average counterfactual outcome.

shocks coincided with men's 19th birthdays. The uniqueness of the Medicaid program's rules, the matching procedure, and the differences in the timing of birthdays therefore make it unlikely that the estimated effects are driven by high-enrollment men experiencing shocks unrelated to their loss in Medicaid eligibility around their 19th birthdays.

5.3 Triple-Differences Specification

Given the established connection between mental illness and crime, the effect of the Medicaid loss might be particularly salient for men with mental health histories. Indeed, among low-income men in this sample who served an incarceration spell in state prison by age 21, 76% of them had been diagnosed with a mental health disorder during adolescence. I therefore begin the analysis by showing the main results for the full sample, but I then estimate the effects separately by men's mental health history.

Specifically, I estimate a regression of the following form:

$$Y_{it} = \eta + \sum_{\tau=-6}^{\tau=7} \Big[\underbrace{\alpha_\tau(\text{Treat}_i \times \text{Hist}_i \times \gamma_\tau) + \nu_\tau(\text{Hist}_i \times \gamma_\tau)}_{\text{Mental health history}} + \underbrace{\lambda_\tau(\text{Treat}_i \times \gamma_\tau) + \theta_\tau \gamma_\tau}_{\text{No mental health history}} \Big] \tag{2}$$

$$+ \mu \text{Treat}_i + \rho \text{Hist}_i + \pi(\text{Treat}_i \times \text{Hist}_i) + \delta_t + \varepsilon_{it}$$

where Hist_i is a variable indicating whether individual i had a mental health history prior to age 16 (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 in a given quarter τ. However, this specification allows high-enrollment individuals with mental health histories to have differing treatment effects, captured by α_τ, than high-enrollment men without mental health histories (whose treatment effects are denoted by λ_τ). In that regard, this regression is equivalent to running a triple-differences specification.

When reporting results, I report the post-period average of the α_τ coefficients (or α_7 for the preferred cumulative variable), which measures whether high-enrollment men with a mental health history had a different average treatment effect than high-enrollment men without a mental health history. Given that both groups are aging out of Medicaid eligibility at age 19, larger treatment effects for men with mental health histories would suggest that losing access to *mental healthcare* is playing an important role in explaining changes in criminal behavior.

6 Main Results for Criminal Behavior

6.1 Full Sample

I first consider an individual's likelihood of being incarcerated in a given quarter. The first panel of Figure 4 plots the raw means using the full matched sample, and panel (b) plots the difference-in-differences estimates from equation (1). These figures confirm that high-enrollment men were trending similarly to low-enrollment men for the year and a half prior to age 19, thereby providing support for the parallel trends assumption. Then, precisely after their 19th birthdays, the two groups begin to diverge so that high-enrollment men were 0.5 percentage points (or 14%) more likely to be incarcerated in any given quarter of the post-period relative to low-enrollment men. The results from this and the following subsection are summarized in Table 2.

Next, I now use a cumulative variable that measures whether an individual has *ever* been incarcerated. Panels (c) and (d) once again show that high- and low-enrollment men were trending similarly and only begin to significantly diverge upon reaching age 19.[22] These findings thus indicate that the increase in incarceration was driven by new individuals having their *first* serious contact with the criminal justice system. By their 20th birthdays, men in the high-enrollment group who lost access to Medicaid were roughly 1 percentage point (or 11%) more likely to have been incarcerated than the low-enrollment men. Two years after losing eligibility, this figure rises to almost 2 percent points (or 16%).

A back-of-the-envelope calculation suggests that in the absence of the Medicaid loss, around 380 men would not have been incarcerated, implying a 10% reduction in these cohorts' likelihood of incarceration by age 21. Because this sample only represents half of the state's population, this number is almost certainly an undercount of the number of men in these cohorts who would have not been incarcerated if their Medicaid eligibility had not expired on their 19th birthdays. Moreover, to the extent that at least a portion of the low-enrollment men were affected by the loss in eligibility, then the estimates in this section would be underestimates of the effect of Medicaid disenrollment on incarceration.

[22] There appears to be a slight pre-trend in panels (c) and (d), which could be the result of mean reversion (given the way in which men were assigned into groups based on their Medicaid enrollment; see panel (a) of Figure 3). Nevertheless, this pre-trend is small in magnitude, suggesting that the men who were transitioning in and out of the program in the pre-period were less affected by changes in coverage than those who experienced a sudden termination in eligibility on their 19th birthdays.

Finally, it is worth noting that the high-enrollment group's likelihood of incarceration resembled that of the low-enrollment group's not only in its trending behavior, but also in its levels before age 19. A potential interpretation of these patterns is that having access to health services suppressed high-enrollment men's criminal propensities, keeping them comparable to those of their relatively higher-income and healthier peers.[23] Upon aging out of eligibility, high-enrollment men likely began to face constraints in accessing health services and became more likely to commit serious offenses. One explanation for this increase in criminal activity could be that Medicaid was an important source of mental healthcare for this group of low-income adolescents, an issue I now explore in greater detail.

6.2 Differences by Mental Health History

To consider the importance of mental healthcare, I now split the sample into two groups based on an individual's mental health history. Figure 5 displays the raw means as well as estimates from the triple-differences specification. Panel (b) plots the differences for men without a mental health history (45% of high-enrollment men, in orange) as well as the differences for men with a mental health history (in blue). These results suggest that the increase in crime is *entirely* driven by men with mental health histories.

Panels (c) and (d) consider men's cumulative likelihood of incarceration, and show that despite trending similarly before age 19, the share of high-enrollment men with mental health histories who have ever been incarcerated starts diverging from the analogous share of the comparison group exactly around individuals' 19th birthdays. One year after losing eligibility, high-enrollment men with a mental health history are 2.5 percentage points (or 22%) more likely to have ever been incarcerated. By their 21st birthdays, these estimates rise to 3 percentage points (or 21%). These results confirm that the rise in crime is not driven by a group of men being detained for long periods of time or consistently recidivating. Instead, the termination of Medicaid eligibility results in *new* individuals with mental illness becoming incarcerated for the first time.

Notably, despite losing Medicaid eligibility, high-enrollment men *without* mental health histories continue to trend similarly to their low-enrollment counterparts for the entire post-period. A high

[23] If both groups were identical except for their access to health services, then we might expect the criminal propensity of the comparison group to be higher than that of high-enrollment men. However, men in the comparison group likely have higher-income parents (see Table 1) and might be relatively healthier (i.e., if they decided that they no longer want or need health services). Both of these differences likely explain why the low-enrollment group's incarceration rate is not higher than that of the high-enrollment men.

share of these individuals are filing claims in the years prior to aging out of eligibility (see Figure A4) and more than one-quarter of them have been diagnosed with chronic physical conditions (e.g., asthma, diabetes, hypertension). If the effect of losing health coverage on criminal behavior was purely an income effect, then we would also expect to see an increase in incarceration for men *without* mental health histories who lost eligibility. The lack of an increase in incarceration for this group thus provides support for the notion that changes in criminal propensity are primarily driven by changes in access to mental healthcare.

Next, I use these reduced-form estimates to calculate the implied effect of Medicaid enrollment on men's likelihood of incarceration. Table 3 displays the results, showing that Medicaid disenrollment increases low-income men's likelihood of incarceration by around 25%.[24] Among men with mental illness, Medicaid enrollment decreases their likelihood of incarceration by 2 percentage points, roughly a 40% 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%. Billings & Schnepel (2018) find that early-life interventions to reduce lead exposure decrease the likelihood of arrests by around 40%. The estimates in this section thus suggest that providing health coverage to low-income men with mental illness has comparable effects to those of other interventions typically associated with crime reduction.

Overall, the results from this section indicate that there is a population of low-income young men—those with mental health histories—for whom access to mental healthcare seems to serve as an effective deterrence mechanism. Once these men lose access to health services, they are more likely to commit an offense and become incarcerated for the first time. These findings are consistent with Heller et al. (2017), which finds that low-income adolescents participating in after-school programming that includes cognitive behavioral therapy components were significantly less likely to be arrested, but that effects did not persist after the program ended. These results also complement the findings in Deshpande & Mueller-Smith (2021), which identifies a different

[24] 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, β_0, is the ratio of the reduced-form and the first-stage estimates. I re-scale the estimates using changes in enrollment—rather than changes in the share of men filing claims—because I want to measure the effect of having access to mental healthcare even for individuals that were not consistently using it. Finally, it is worth noting that if high-enrollment men were able to transition from Medicaid to other insurance coverage, then these magnitudes would be underestimates of the effect of losing coverage on incarceration.

population of low-income individuals—male and female adolescents with disabilities—who are more likely to commit crimes after being removed from the SSI program, though for this group the increase operates primarily via an income effect.

6.3 Larger Effects for Recent Beneficiaries of Mental Healthcare

If losing access to mental healthcare is indeed driving the increase in criminal activity, then we would expect the treatment effects to be more pronounced for men who were filing mental health claims right before aging out of eligibility. To test this hypothesis, I hone in on men with mental health histories and split this group based on how recently the individual was filing mental health claims. Specifically, I designate high-enrollment men as recent beneficiaries if they had a mental health claim in the year and a half before turning 19 (36% of high-enrollment men with mental health histories).

In practice, I maintain the same comparison group for each high-enrollment person and use a triple-differences specification—similar to equation (2)—to estimate whether men who were filing mental health claims *more* recently are more likely to become incarcerated than men filing mental health claims *less* recently. If the loss of mental health services is an important factor driving the increase in crime, then I should find a larger effect for more-recent beneficiaries. Figure 6 shows that men who were using behavioral health services right before their 19th birthdays (denoted by the purple circles) were significantly more likely to have been incarcerated by age 21 compared to men who were using these services *less* recently (gray markers). These findings thus provide additional evidence that decreased access to mental healthcare plays a key role in driving the rise in criminal activity.

7 Robustness of Main Result

In this section, I consider the robustness of the main result displayed in panel (d) of Figure 5. I group the robustness checks into three categories based on the underlying concern with the empirical approach. The first set of exercises considers robustness to the choices made when constructing the baseline sample and implementing the preferred specification. The second group of exercises considers the central concern with the identifying assumption, which is that despite matching on key characteristics and observing similar incarceration propensities prior to age 19, the low-enrollment men may not be a suitable group for estimating the counterfactual outcome paths of the high-enrollment men around their 19th birthdays. The final section then considers a separate concern: that other shocks—besides Medicaid disenrollment—that might influence an individual's criminal propensity may be occurring in late adolescence, thereby confounding the estimated effects. Appendix Table A2 summarizes results from this section.

7.1 Robustness to Clustering & Matching Procedure

I begin by changing the level at which I cluster the standard errors, first to the match level and then to the match and individual level. The statistical significance of the main results is preserved. Next, I test the sensitivity of the estimates to the matching and weighting procedure utilized to construct the comparison group. To verify the parallel trends assumption in these exercises, Appendix Figure A6 displays the graphical results. First, instead of relying on coarsened exact matching to construct a comparison group, I re-weight the low-enrollment men using the DFL re-weighting approach (DiNardo et al. 1995, Fortin et al. 2011). Second, I return to the baseline matching procedure and exclude from the sample the group of low-enrollment men receiving the greatest amount of weight in the regression (i.e., to ensure that the patterns are not driven by a small number of comparison units). Third, I change the timing of assignment into groups, so that men are assigned to a group based on their Medicaid enrollment in the study pre-period (as opposed to a year before the pre-period). Next, instead of matching each high-enrollment individual to all possible low-enrollment men, I force each high-enrollment unit to only have one randomly chosen comparison unit. The main results are robust to these four changes. Finally, I abstain from any matching and re-weighting, and instead estimate the specification using all eligible high- and low-enrollment men.[25] The main results by mental health history are very similar to those using the baseline matched sample.

7.2 Suitability of Low-Enrollment Men for Estimating Age Effects

7.2.1 Utilizing Additional Matching Characteristics

The main threat to the identifying assumption is that because high- and low-enrollment men are not identical, the latter may not be a suitable group for estimating the criminal propensity of the former around their 19th birthdays. To consider this possibility, I begin by augmenting the

[25] Appendix Figure A7 highlights that without any re-weighting, low-enrollment men in the full sample do not exhibit parallel trends to the high-enrollment men. Once mental health history is taken into account, however, the two groups exhibit more similar trends prior to age 19. This figure thus illustrates the importance of including mental health history as a matching characteristic if the goal is to find low-enrollment men who can serve as a suitable group for tracing out the counterfactual outcome paths of high-enrollment men.

matching procedure with additional observable characteristics, so that high-enrollment men are even *more* likely to resemble the low-enrollment group. If the estimated effects are driven by differences between the two groups that are unrelated to Medicaid disenrollment, then I would expect to see the difference in incarceration starting at age 19 decline as I make the two groups more similar.

First, I substitute an individual's school district with his school in the matching procedure, so that the comparison is essentially between classmates. I implement this change using both the baseline matching procedure and the DFL re-weighting approach, and find that matching on school does not alter the main findings (Appendix Figures A8 and A9).[26] Next, Table 1 shows that high-enrollment men are observably different than low-enrollment men along other dimensions: namely, high-enrollment men seem to have lower household incomes than their low-enrollment counterparts as well as higher prior contact with the justice system. To reduce the discrepancy in household income, I augment the matching procedure with a variable denoting enrollment in the SNAP program in adolescence. To give more weight to low-enrollment men with previous criminal justice contact, I instead utilize a variable that denotes whether an individual was referred to the juvenile justice system. In both cases, I continue to find that high-enrollment men with mental health histories diverge from their low-enrollment counterparts on their 19th birthdays and that the estimated magnitudes are relatively unchanged. Finally, I jointly minimize all of these differences by using an individual's school, his SNAP enrollment, *and* his previous contact with the juvenile justice system as matching characteristics. Even in this most restrictive scenario, I find that high-enrollment men with mental health histories were trending similarly to the low-enrollment group prior to age 19 and only became more likely to be incarcerated upon losing access to health services.

Together, the consistent patterns and estimated magnitudes in these four exercises provide reassuring evidence that the difference in incarceration that arises at age 19 cannot be explained by differences between the two groups in their location, income, or previous contact with the justice system. Moreover, insofar as these additional matching characteristics are proxies for other (perhaps unobservable) differences between the high- and low-enrollment men, then the robustness of the main result to their inclusion indicates that the estimated effects are not driven by differences

[26] Adding characteristics to the baseline matching procedure reduces the number of successfully matched high-enrollment men, so I also utilize the DFL re-weighting approach to keep the sample size constant regardless of the chosen matching characteristics.

between the groups.

7.2.2 Consistent Results Using Medicaid Enrollees as Alternative Comparison Group

Given the central concern about the suitability of the baseline comparison group for estimating age effects, I also consider the robustness of the main result to an alternative way of constructing the treatment and comparison groups altogether. Specifically, I focus on men with mental health histories who were enrolled in Medicaid between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$ (i.e., high-enrollment men with mental health histories in the baseline approach). I then split this sample of men into two groups: men filing mental health claims in the year and a half before age 19 serve as the "treated" group, and those who did not file mental health claims during this period become the *new* comparison group. Similar to the baseline empirical strategy, I implement a matching procedure to ensure balance between the two groups in terms of race, cohort, and geographic location.

Appendix Figure A10 shows that men filing mental health claims are nearly 2 percentage points (or 10%) more likely to be incarcerated by age 21 relative to this alternative comparison group. Importantly, the groups are trending similarly prior to age 19 and again begin to diverge in their incarceration propensity precisely when eligibility lapses. This exercise—which uses a different group of men to account for age trends in crime—provides additional evidence that losing access to mental healthcare seems to play an important role in explaining the rise in criminal activity. This result is consistent with Figure 6 showing that more recent-beneficiaries have larger treatment effects than less-recent beneficiaries. Finally, because both groups in this exercise experienced declines in Medicaid enrollment at age 19, this figure is likely an underestimate of the effect of losing access to health services on the likelihood of incarceration.

7.2.3 Alternative Empirical Strategy: Regression Discontinuity

Next, I refrain from using a comparison group and instead utilize a regression discontinuity approach as an alternative strategy for estimating the effect of losing Medicaid coverage on criminal propensity. Specifically, I compare arrest probabilities before and after individuals' 19th birthdays (similar to the strategy in Card et al. 2009 and Lee & McCrary 2017).[27] The analysis is based on

[27] Because this empirical approach focuses on the change in criminal propensity that occurs immediately upon reaching age 19, I use arrests as the outcome of interest (as opposed to using incarceration records from the Department of Corrections, which likely reflect longer-term judicial decisions). Moreover, because I do not expect the effect of losing eligibility to affect all individuals instantaneously, the estimates from this approach are likely underestimates of the overall effect of losing access to health insurance on criminal activity.

the following reduced-form regression-discontinuity model:

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

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, Post19$_t$ is an indicator variable for months after an individual's 19th birthday, and γ_m are calendar-month fixed effects. 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 aside from Medicaid disenrollment, no other factors that influence criminal involvement change discontinuously around an individual's 19th birthday.

Appendix Figure A11 displays the results, showing that men's likelihood of being arrested increases upon reaching age 19. Importantly, panels (b)–(d) highlight that the discontinuity is only present for men with mental health histories, and especially for those filing mental health claims in the years prior to aging out of eligibility. The point estimates suggest that men with recent mental health histories were 0.4 percentage points (or 10%) more likely to be arrested upon reaching their 19th birthdays.[28] I find consistent results when I consider the likelihood that an individual is incarcerated for the first time as the outcome: Appendix Figure A12 shows a discontinuity in this likelihood, but only for men filing mental health claims prior to age 19.

7.2.4 Falsification Checks Around Earlier Ages

Finally, a related concern with the empirical strategy might be that the high- and low-enrollment groups are fundamentally different and therefore bound to have diverging criminal propensities in late adolescence. This concern might be particularly relevant given the empirical strategy, in which I assign men into groups earlier in adolescence (ages 16.5–17.5) and then follow their natural evolution of outcomes for multiple years. To consider whether diverging outcomes are a concern, I conduct "placebo" checks in which I replicate the baseline empirical approach around

[28] Appendix Table A3 summarizes these results and shows robustness to using a cubic polynomial. The results in panel (c) of this table are less precisely estimated, but suggest that men with recent mental health histories are also more likely to be arrested and detained in a correctional facility, with no comparable effect for men without recent mental health histories. Appendix Table A4 displays results from analogous exercises around age 20, confirming that there is no increase in criminal propensity for individuals with mental health histories around this later age.

earlier ages. If the main findings are driven by naturally diverging paths, then I would also expect to see differences emerge between the two groups around earlier ages as these individuals are transitioning into adulthood. If the main findings are instead driven by the loss of Medicaid at age 19, then I should not find an increase in high-enrollment men's criminal propensities around earlier birthdays when there was not a break in eligibility.

I begin by shifting the approach back one year and estimating men's likelihood of incarceration around age 18. Appendix Figure A13 and Table A5 display the results from this exercise, showing a lack of an increase in the likelihood of incarceration for high-enrollment men with mental health histories after their 18th birthdays. Next, I expand the definition of incarceration to include juvenile detentions and estimate the triple-differences specification around ages 17, 18, and 19. The results show that an increase in incarceration only occurs around individuals' 19th birthdays, suggesting that this increase is indeed driven by the loss of Medicaid eligibility at this later age.

7.3 Robustness to Concurrent Changes in Late Adolescence

The second threat to causal identification is that high-enrollment men might be experiencing other shocks unrelated to Medicaid disenrollment that might influence their criminal propensity. An important change that occurs in late adolescence is that individuals graduate (or leave) high school and transition into higher education or the labor market. However, teens drop out or graduate from high school at different points in calendar time and there is no reason to think that the timing of these transitions would coincide with their 19th birthdays. Indeed, the age at which individuals can legally drop out of school in South Carolina is 17. Appendix Figure A14 plots the share of each group that is enrolled at a given age. These figures confirm that school enrollment begins to decline when individuals are 17 years old. Perhaps more importantly, though, the figures show that the share of high-enrollment men that was enrolled in school was comparable to the analogous share of low-enrollment men before age 19 for men with and without mental health histories. These figures thus provide evidence that high-enrollment men were not differentially experiencing shocks related to educational attainment right before their 19th birthdays that might be confounding the estimated effects of Medicaid disenrollment.[29]

In the same vein, one concern might be that these men are aging out of foster care or losing

[29] Appendix Figure A5 shows that the share of men enrolled in the SNAP program was also gradually declining in late adolescence and this decline, in turn, could impact men's criminal propensity (Tuttle 2019, Yang 2017). Nevertheless, the decline is significantly more pronounced for high-enrollment men *without* mental health histories, which would bias me against finding an effect for those with mental health histories relative to those without.

Supplemental Security Income (SSI) benefits at age 18, and these transitions might influence their criminal activity (Courtney et al. 2007 and Deshpande & Mueller-Smith 2021). Although the timing of these transitions differs from that of Medicaid disenrollment, I still take seriously this consideration and exclude from the sample the relatively small share (13%) of high-enrollment men who were in foster care or receiving SSI benefits. The main results are robust to their exclusion, suggesting that the increase in criminal activity is not driven by this group of men experiencing transitions unrelated to Medicaid enrollment during late adolescence (Appendix Figure A15).[30]

8 Heterogeneity by Crime Type, Medication Usage, & Diagnoses

The results up to this point indicate that increases in incarceration 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 subgroup within the high-enrollment men.

8.1 Effects by Crime Type

In line with the economics of crime literature, I begin by estimating whether high-enrollment men are more likely to be incarcerated for violent and property crimes. Figure 7 and Table 4 show that high-enrollment men with mental health histories are more likely to be incarcerated for both types of offenses relative to the low-enrollment group. Because violent and property crimes represent relatively serious offenses, these results imply increases in serious criminal offending, rather than an increase in the likelihood of incarceration for less serious crimes.[31] These results are consistent with papers studying the effect of Medicaid expansions on crime rates, which tend to find declines in violent crimes and in certain types of property offenses (Aslim et al. 2019, Fry et al. 2020, He & Barkowski 2020, Vogler 2020, Wen et al. 2017).

Next, I classify the offenses that result in incarceration into: (1) financially motivated offenses (e.g., burglary, robbery, drug distribution); (2) non-financial violent offenses (e.g., weapons-related offenses, assault); (3) drug and alcohol possession (e.g., marijuana possession, DUIs); and (4) miscellaneous offenses (e.g., resisting arrest, parole or probation violations). Increases in financially

[30] Given that youths in foster care and SSI maintain their Medicaid eligibility in adulthood, one alternative would be to use these men to account for age effects around age 19. Nevertheless, the criminal trajectories of SSI and foster care youths differ from those of other men with mental health histories at ages 17–18, indicating that the former is not a suitable comparison group for estimating the counterfactual outcomes of the latter. This difference is perhaps unsurprising given that SSI and foster care youth are experiencing other shocks that might impact their criminal involvement prior to their 19th birthdays.

[31] Table A6 considers all arrests and confirms that the increase in crime is coming from serious offending, in particular from an increase in felony offenses.

motivated offenses would indicate that the rise in crime is economic in nature, whereas changes in drug and alcohol possession might imply increases in self-medication or substance abuse. Increases in violent (non-financial) offenses might instead reflect that individuals are more likely to make errors in judgment or decision-making after losing access to mental healthcare.

The findings show that by age 21, high-enrollment men with mental health histories are 2.5 percentage points (or 32%) more likely to have been incarcerated for a financially motivated offense. Notably, the increase in these offenses seems to begin immediately after individuals' 19th birthdays. These results thus suggest that an important share of the rise in criminal activity is economic in nature.[32] Nevertheless, the results show that high-enrollment men with mental health histories are also more likely to be incarcerated for non-financial violent offenses, drug possession, as well as miscellaneous offenses. Hence, these findings suggest that there are likely multiple channels through which loss of access to insurance could result in increased criminal involvement for men with mental illness.

8.2 Differences by Medication Usage

More than half of recent beneficiaries were relying on Medicaid for access to psychotropic drugs, suggesting that decreased access to mental health medications might be an important reason behind the increase in criminal activity. To consider the specific role of psychotropic drugs, I split the sample of high-enrollment men with mental health histories based on their medication usage. In particular, I divide the sample into those who filed claims for mental health drugs in the years before aging out of eligibility (i.e., the 35% of high-enrollment men who had claims for psychotropic drugs between the ages of 16 and 18) and those who did not file such claims during these ages. Appendix Figure A16 shows that men relying on Medicaid for mental health medications were significantly more likely to be incarcerated than men with mental health histories who did not file claims for psychotropic drugs in late adolescence.

Furthermore, as Appendix Table A8 shows, a substantial share of men in the high-enrollment group filed claims for non-ADHD medications (namely, antidepressant, antianxiety, and antipsychotic medications). Losing access to non-ADHD medications could have more significant impacts on the likelihood of incarceration due to negative side effects like withdrawal (see, e.g., Lewis et al.

[32] Table A7 shows that when I split those with mental health histories into more- vs. less-recent beneficiaries, the former group is more likely to commit financially motivated offenses, providing additional evidence that the increase in criminal behavior for those most affected by the loss in eligibility was economic in nature.

2021) or deleterious effects on labor market outcomes (see, e.g., Biasi et al. 2021, Ridley et al. 2020). Appendix Figure A17 therefore splits the high-enrollment men based on the type of mental health medication for which they were filing claims. Indeed, the estimates in this figure show that the incarceration effects are particularly pronounced for the group of men filing claims for non-ADHD medications in the years prior to losing eligibility.

Finally, Appendix Table A9 shows that men relying on Medicaid for access to medications were more likely to be incarcerated for financially motivated offenses. If the self-medication channel were primarily driving the increase in crime, we might have expected to find a larger treatment effect for offenses related to drug possession among men who relied on Medicaid for psychotropic drugs. The lack of a difference in treatment effects between the two groups in this table thus confirms that the self-medication channel is not the primary mechanism underlying the relationship between mental healthcare and criminal behavior.

8.3 Differences by Diagnoses

I also use the diagnoses codes in the Medicaid claims to study which subgroups of men are more likely to contribute to the rise in crime. Specifically, Appendix Table A10 considers the sensitivity of the baseline estimates to excluding men ever diagnosed with a particular disorder.[33] First, it is worth noting that the baseline results are robust to excluding individuals with intellectual disabilities and other neurodevelopmental disorders, indicating that men with these diagnoses are not driving the increase in criminal activity. In contrast, panel (b) shows that the main result is sensitive to excluding men who have ever been diagnosed with conduct disorder, oppositional defiant disorder, as well as substance-related and addictive disorders.[34] The findings in this table thus suggest that men diagnosed with these disorders are those who are most likely to commit crime after losing access to Medicaid eligibility. These results are in line with findings in prior studies showing that adults diagnosed as children with conduct disorder as well as those with co-occurring substance use disorders are more likely to be violent (Glied & Frank 2014).

[33] Appendix Table A8 shows that neurodevelopmental disorders (including ADHD) as well as behavioral disorders (i.e., conduct and oppositional defiant disorders) are among the most common diagnoses in this sample. This table also highlights that most men with mental health histories are diagnosed with more than one disorder in adolescence.

[34] It is important to note that many of these diagnoses are co-occurring. For example, 42% and 27% of men with oppositional defiant disorder have also been diagnosed with conduct disorder and substance abuse disorder, respectively, prior to age 19. Because of the co-occurrence of diagnoses and the prevalence of ADHD in this sample, column 5 of panel (b) excludes men who have ADHD as their only diagnosis, showing that the increase in crime is not driven by this relatively small group.

Finally, Appendix Table A11 considers whether the increase in crime is more pronounced for men with mental health histories who have also been diagnosed with a chronic physical condition. This table shows that the rise in incarceration is comparable for men with and without physical conditions, confirming that it is the loss of mental healthcare, rather than physical healthcare, that is driving the increase in crime.

9 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 low-income men with Medicaid coverage until the 21st birthdays. 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.[35] In this section, I summarize the approach and estimates, but I refer the reader to Appendix C for a more detailed discussion.

9.1 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 for two extra years and conclude that this cost would amount to roughly $15 million. This cost is a function of the estimated take-up of the program (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. Because the income eligibility thresholds are being held constant in this scenario, crowding out of private insurance coverage is less of a concern.

9.2 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

[35] Because this cost-benefit calculation is not taking into account any of the associated benefits of providing women with Medicaid eligibility (e.g., reduced teen birth rates; Kearney & Levine 2015), I focus on quantifying the costs and benefits of providing young men with insurance access.

on serious arrests (as opposed to on a person's likelihood of incarceration) and focuses on the sample of men with mental health histories. The coefficient β_0 estimates the extent to which high- and low-enrollment men in this sample differ in their arrest propensity in the post-period relative to the pre-period. The results are reported in Table 5.[36]

Using these estimates and taking a relatively conservative approach, I find that the reduced social costs of crime are roughly $17 million.[37] Specifically, I use the estimates from Table 5 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.

Next, I calculate the benefits from fewer incarcerations, and estimate that providing Medicaid eligibility would result in $3.0 million and $7.2 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. To calculate the social costs generated by prison spells, I use estimates from Mueller-Smith (2015) quantifying the impact of prison on economic outcomes (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 around $1.90 in social and fiscal costs. Table C2 summarizes the estimates from this analysis, showing that even in the calculation with the most conservative assumptions, the estimated benefits of Medicaid provision outweigh the costs.

[36] It is not straightforward to compare these estimates to those from studies quantifying the effect of Medicaid expansions on crime rates because the former uses arrest records for low-income young men and the latter uses police incident reports for the whole population. However, a back-of-the-envelope calculation suggests that crime rates would have been 0.6% lower if eligibility had not lapsed for this group of adolescents, compared to increases in violent crime of around around 5% from Medicaid expansions (He & Barkowski 2020, Vogler 2020). I refer the reader to Appendix E for more details.

[37] In this calculation, I assume that expanding Medicaid eligibility prevents these crimes altogether. In Appendix C, I calculate the social cost of crime under an alternative assumption: that half of crimes are prevented, while the other half are delayed by two years until eligibility expires.

9.3 Marginal Value of Public Funds

I also use the estimates from this section to 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 (Hendren & Sprung-Keyser 2020). Similar to the cost-benefit exercise, I construct upper and lower bounds for this ratio. Using moderately conservative assumptions, I find that the MVPF of expanding eligibility for two years is 1.8 (with a lower- and upper-bound of 1.4 and 23.3, respectively). 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). Nevertheless, the findings from this exercise confirm that $1 of spending on this policy delivers more than $1 to its beneficiaries.

9.4 Comparing Medicaid Provision to Longer Punishments

Up to this point, I have shown that Medicaid seems to be a cost-effective policy for reducing criminal behavior. But how does Medicaid's cost effectiveness compare to that of other crime-reduction approaches? In this subsection, I benchmark the cost effectiveness of providing low-income young men with Medicaid eligibility against the cost effectiveness of harsher criminal sanctions, which has been a favored approach for reducing crime for the past fifty years. One motivation for this exercise is that cost-benefit calculations, like those discussed in the previous subsections, can be sensitive to the average cost assigned to crime. By comparing two crime-reduction approaches, I can abstract away from the cost assigned to crime and instead compare the cost of two policies intended to reduce crime by the same amount. More details are provided in Appendices D and E, but I provide a short recap of the comparison here.

I begin by calculating the extent to which sentence lengths would need to be increased for this group of low-income adolescents in order to achieve the same reduction in crime as extending Medicaid eligibility. Following prior studies in the literature (Hjalmarsson 2009, Lee & McCrary 2017, Mueller-Smith et al. 2022), I estimate the elasticity of crime with respect to sentence lengths for this sample by quantifying the degree to which these low-income adolescents commit fewer crimes upon reaching the age of criminal majority (i.e., when they transition from the juvenile to the adult justice system, the latter of which has more severe criminal sanctions). Figure D1 plots the hazard rates of a felony arrest and shows little indication of a systemic drop in felony arrests upon reaching age 17.

Using a conservative estimate of the elasticity of crime with respect to sentence lengths from this exercise ($\varepsilon_{c,f} = -0.017$), I find that the total cost of reducing crime by 15% via longer sentence lengths totals \$105 million, almost twice the cost of Medicaid provision (\$57 million).[38] In order for these two policies to have the same overall cost, $\varepsilon_{c,f}$ would need to be more than two times the estimated elasticity. Overall, this comparison allows me to conclude that low-income young men would need to be significantly more responsive to changes in sentence lengths in order for harsher punishments to be as cost effective as healthcare provision.[39]

10 Discussion & Conclusion

Motivated by the disproportionate representation of mentally ill individuals in today's criminal justice population, this paper studies the potential for mental healthcare to serve as a crime-reduction policy. To study this question, 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. To consider the specific importance of mental healthcare, I use rich administrative data linked across health and law enforcement agencies in South Carolina, which allows me to identify individuals with mental health histories and measure their criminal outcomes.

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 comparable low-income men who were less affected by the termination in eligibility. Using detailed information from insurance claims, I show that these effects are entirely driven by men with mental health histories. Moreover, I find that the effects are particularly pronounced for men who were filing mental health claims right before the loss in eligibility and for men who relied on Medicaid for access to mental health medications. I validate these results with of a battery of robustness checks and alternative empirical strategies, all of which suggest that decreased access to mental healthcare increases criminal propensity.

The findings of this study offer a number of takeaways and policy implications. First, this paper studies and identifies a population of individuals—low-income young men with mental health histories—for whom access to mental healthcare seems to serve as an effective deterrence mechanism. Policymakers might thus wish to consider improved access to healthcare among

[38] The cost of Medicaid provision is higher in this exercise than that outlined in Section 9.1 because it incorporates the fiscal cost of incarcerating individuals who are not deterred as well as the fiscal and social costs from recidivism.

[39] Appendix E also compares the cost of healthcare provision to hiring 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 officers reduces crime among individuals in older age groups. An important caveat to this finding is that the comparison does not account for the social costs of policing.

this population as one approach for reducing crime and decreasing criminal justice expenditures, especially given this program's relative cost effectiveness. Notably, the termination in eligibility occurs in late adolescence—around the peak of the age-crime profile—highlighting the potential importance of access to mental healthcare at this point of the life cycle. 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 more likely to be uninsured than the rest of the population. 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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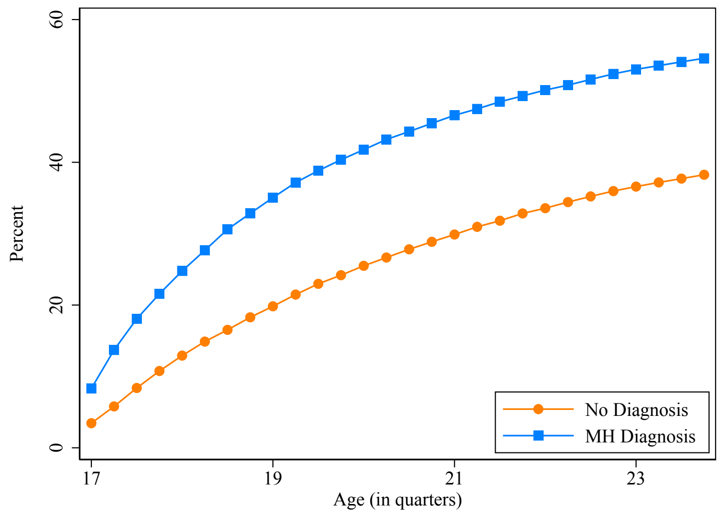
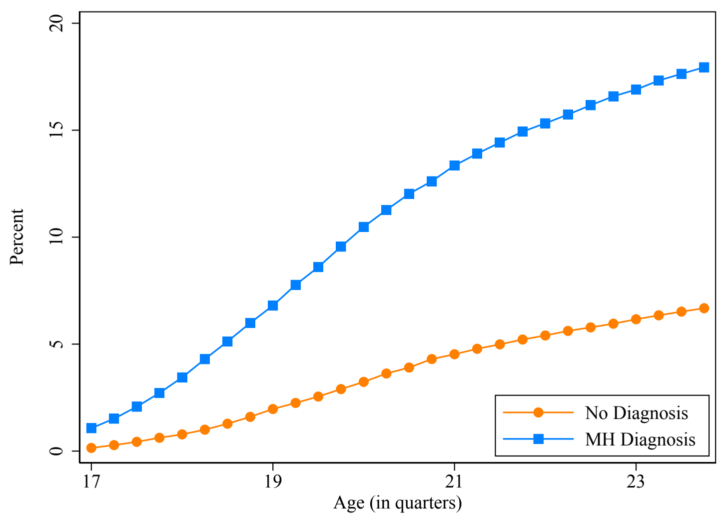
Figures and Tables

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

(a) Ever Arrested

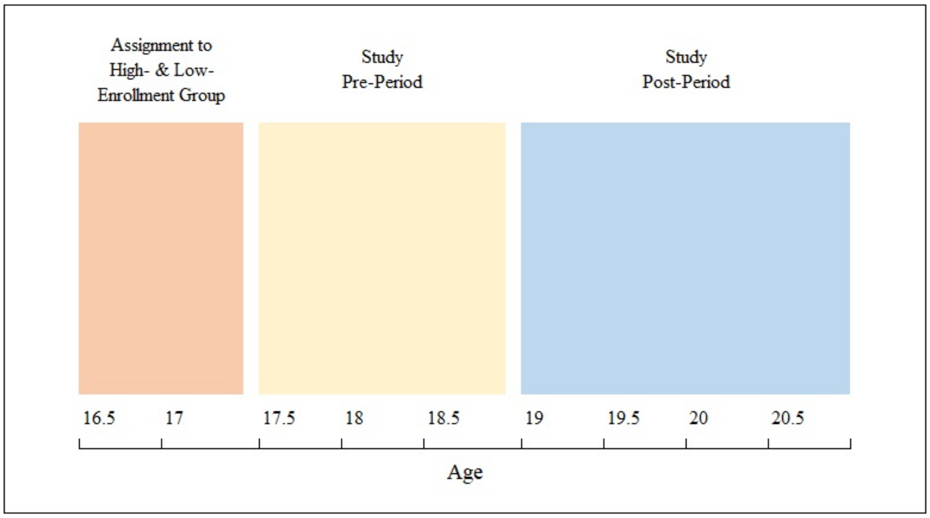


(b) Ever Incarcerated



NOTE: These figures plot the share of each group that has ever been arrested or incarcerated at a given age in South Carolina. "MH diagnosis" refers to having a Medicaid claim with a mental health diagnosis before the age of 16. The sample consists of male individuals in the 1990–1993 birth cohorts who were enrolled in Medicaid at some point between the ages of 10 and 18. "Ever arrested" refers to having an arrest record in the South Carolina Law Enforcement Division data. "Ever incarcerated" refers to having been detained in an adult correctional facility (either in the Law Enforcement Division data or the Department of Corrections data).

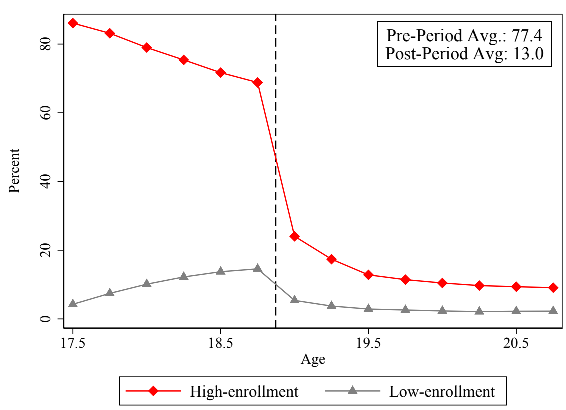
Figure 2: Timeline of Empirical Strategy



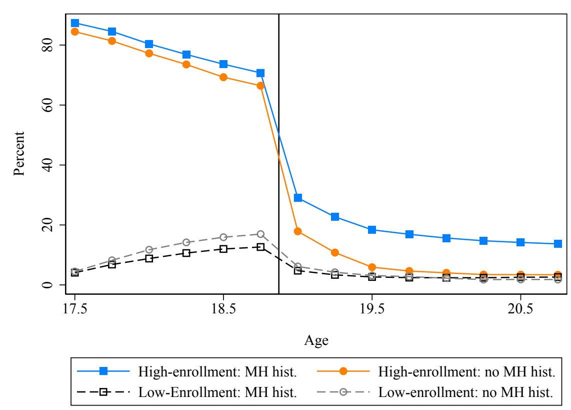
NOTE: Men are assigned to the high- and low-enrollment groups based on their enrollment in the Medicaid program between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$ (orange). Their outcomes are then allowed to evolve naturally in the study pre-period (yellow) and post-period (blue).

Figure 3: Share of High- and Low-Enrollment Men Enrolled in Medicaid (Raw Means)

(a) Full Sample



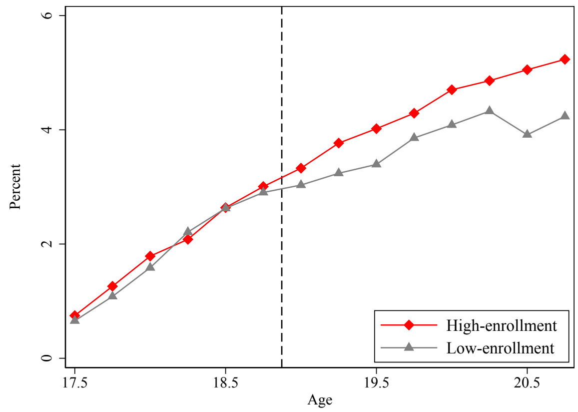
(b) By Mental Health History



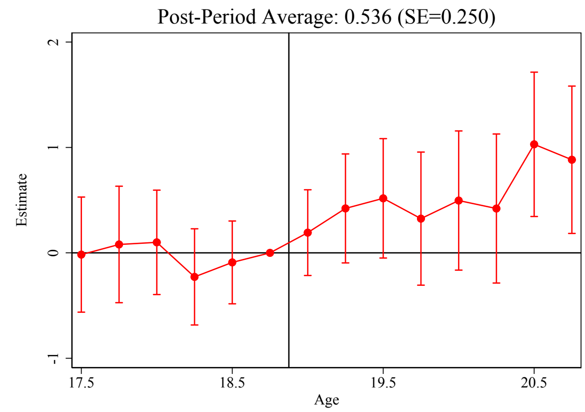
NOTE: These graphs use the sample of matched high- and low-enrollment men to plot the share of men enrolled in Medicaid in a given quarter. Panel (a) plots averages for the full sample and panel (b) plots averages separately by mental health history prior to age 16. The averages reported in panel (a) correspond to the pre- and post-period shares of high-enrollment individuals. In both panels, low-enrollment men are weighted using the corresponding weights from the matching procedure.

Figure 4: Difference in Likelihood of Incarceration Between High- and Low-Enrollment Men (Raw Means & Difference-in-Differences Estimates)

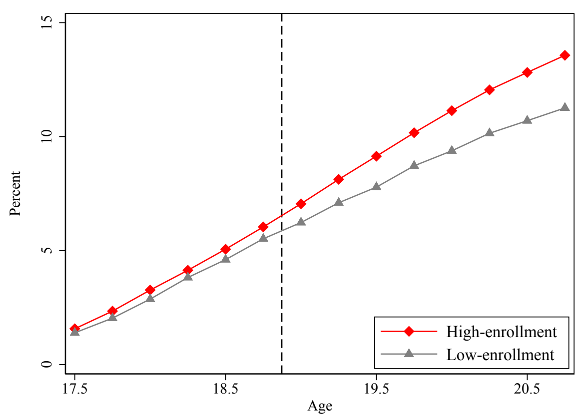
(a) Incarcerated that quarter



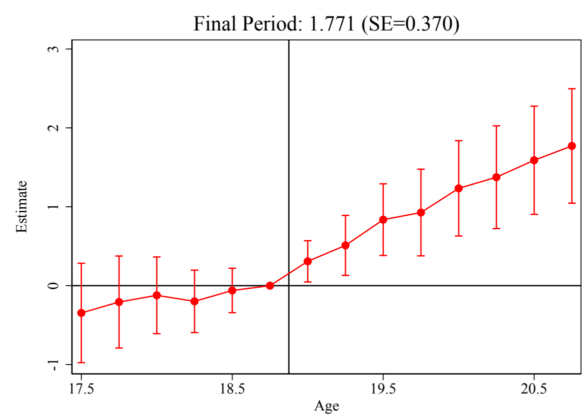
(b) Incarcerated that quarter



(c) Ever incarcerated



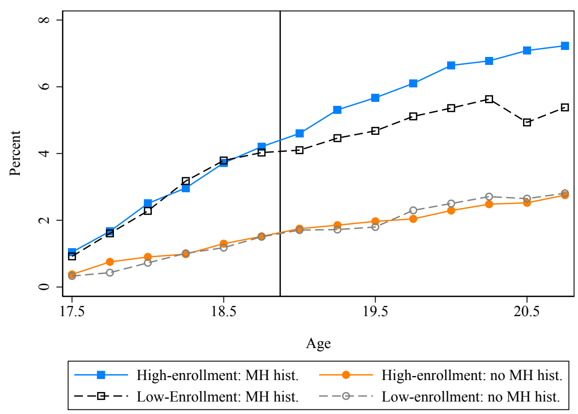
(d) Ever incarcerated



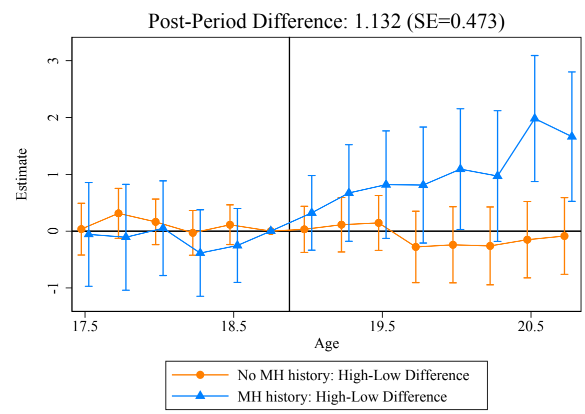
NOTE: The dependent variable measures whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). "Incarcerated that quarter" refers to being detained in an adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Panels (a) and (c) plot the raw means of the matched sample. Panels (b) and (d) plot the difference-in-differences estimates using equation (1). "Post-Period Average" and "Final Period" refer to the average of the post-period β_τ coefficients and to β_7, respectively. Low-enrollment men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level.

Figure 5: Difference in Likelihood of Incarceration Between High- and Low-Enrollment Men, by Mental Health History (Raw Means & Difference-in-Differences Estimates)

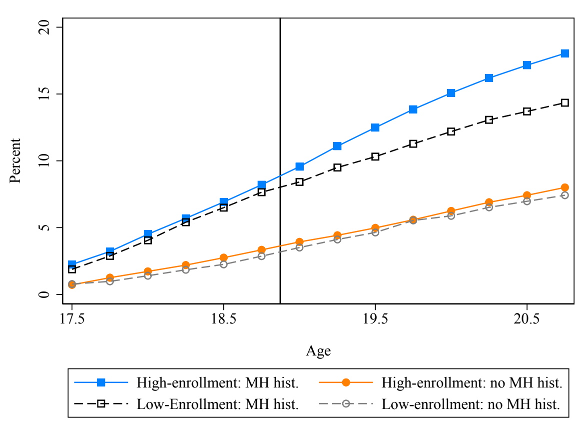
(a) Incarcerated that quarter



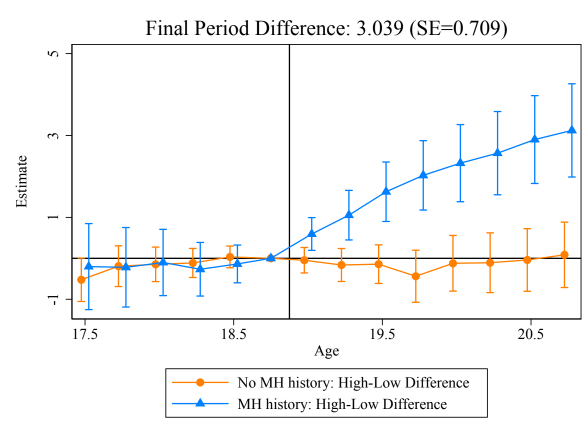
(b) Incarcerated that quarter



(c) Ever incarcerated



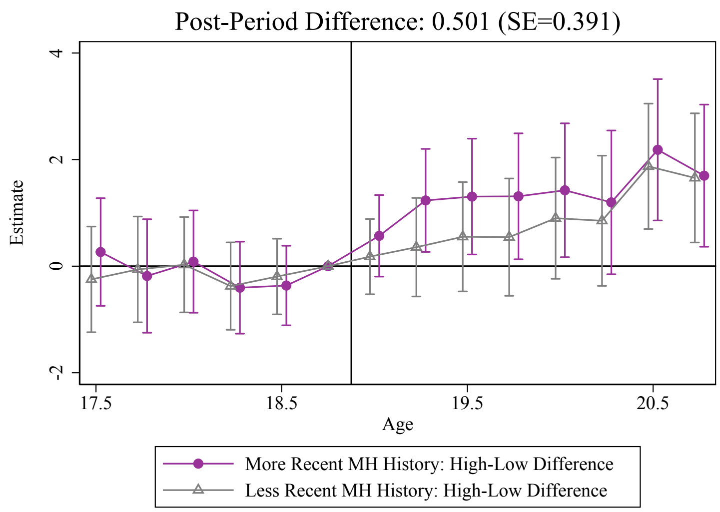
(d) Ever incarcerated



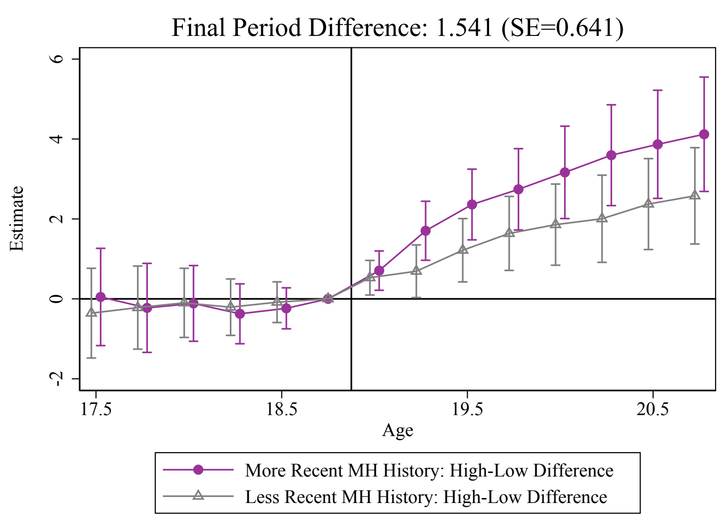
NOTE: The dependent variable measures whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). "Incarcerated that quarter" refers to being detained in an adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Panels (a) and (c) plot the raw means of the matched sample separately by mental health history. Panels (b) and (d) plot the difference-in-differences estimates using equation (2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients and to α_7, respectively. Low-enrollment men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level.

Figure 6: Difference in Likelihood of Incarceration for Men with a Mental Health History, by Recency of Mental Health Claims

(a) Incarcerated that quarter



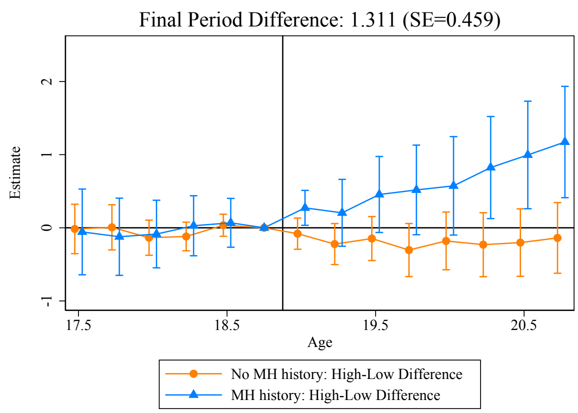
(b) Ever incarcerated



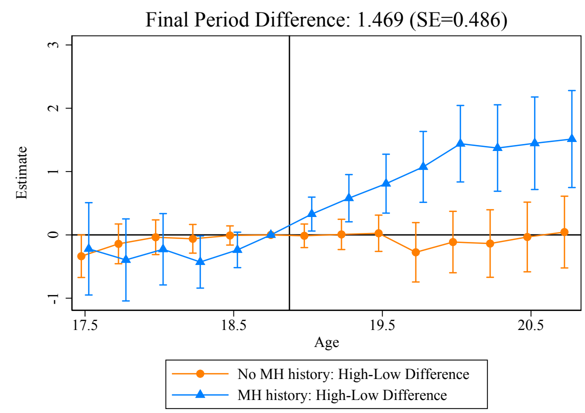
NOTE: The dependent variable measures whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). These figures only consider high-enrollment men with mental health histories and the corresponding matched low-enrollment men. Using equation (2), the figures plot the difference-in-differences estimates for high-enrollment men who were recently filing mental health claims (in purple) and for high-enrollment men who were less recently using mental health services (in gray). Recent usage is defined as filing a mental health claim during the pre-period. "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively, corresponding to the treatment effect for more-recent beneficiaries. "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 7: Likelihood of Ever Being Incarcerated, by Mental Health History and Offense Type

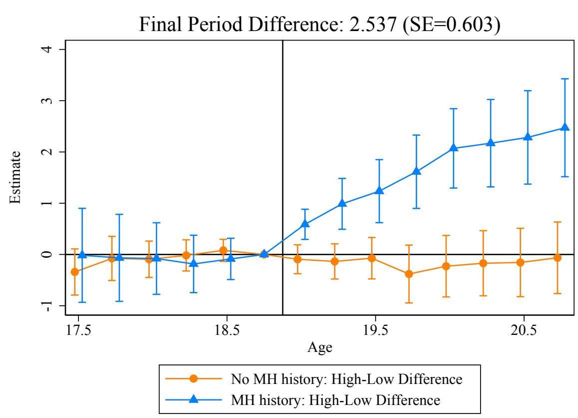
(a) Violent offenses



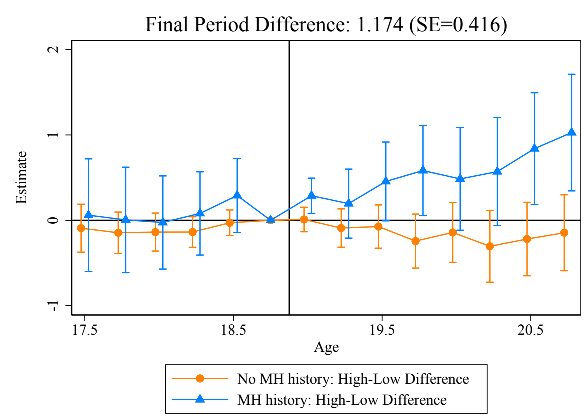
(b) Property offenses



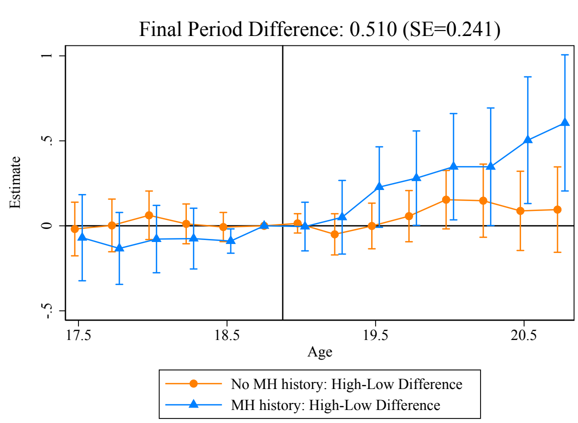
(c) Financially motivated offenses



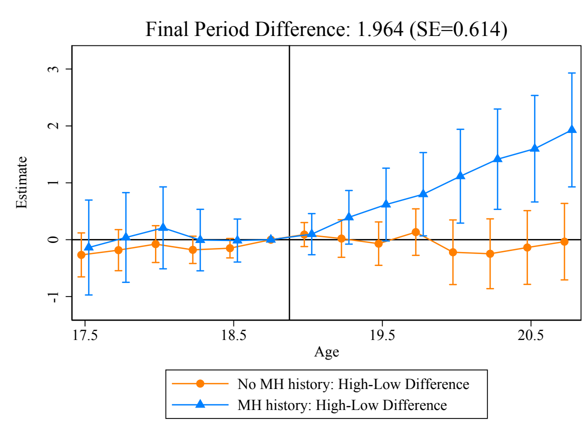
(d) Non-financial violent offenses



(e) Drug & alcohol possession



(f) Miscellaneous offenses



NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). These figures plot the difference-in-differences estimates using equation (2) and "Final Period" refers to the α_7 estimate from this equation. Standard errors are clustered at the individual level. "Miscellaneous offenses" refers to crimes that were not classified in panels (c)–(e). For more details on offense classifications, see Appendix B.

Table 1: Summary Statistics of Full and Matched Sample

	(1) All enrollees	(2) High-enrollment men	(3) Low-enrollment men
Black	69.61	72.52	72.52
Mental health history (pre 16)	47.42	55.40	55.40
Age of first diagnosis	11.99	12.12	11.33
Number of diagnoses (pre 16)	1.00	1.23	0.88
Juvenile justice referral (ever)	36.99	40.90	33.99
Arrests (ever)	24.54	26.70	21.93
SNAP (ever)	80.46	88.37	73.28
TANF (ever)	22.67	27.79	15.61
SSI (ever)	9.17	12.17	3.31
Foster care (ever)	3.41	4.51	1.94
Observations	33,252	21,418	8,911

NOTE: Column 1 reports percentages or averages for all men who were enrolled in Medicaid at some point between the ages of 10 and 18 (inclusive). Column 2 reports means for men enrolled in Medicaid at ages 16.5–17.5. Column 3 reports means for men enrolled in Medicaid earlier in adolescence, but not between the ages of 16.5 and 17.5. Low-enrollment men are weighted using the corresponding weights from the matching procedure. "Mental health history" refers to having a claim with a mental health diagnosis or for a mental health medication prior to age 16. "Age of first diagnosis" is only calculated for individuals who received a diagnosis between the ages of 10–18. "Number of diagnoses" refers to the number of different diagnoses received prior to age 16 (among the twelve diagnoses considered). Every other outcome is measured between the ages of 10 and 18. "Arrests" refers to having an arrest record in the South Carolina Law Enforcement Division data. For more details on the variable construction and diagnosis categories, see Appendix B. See Table A1 for summary statistics of eligible candidates for the matching procedure.

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

	Full Sample		By Mental Health History	
	(1) Incarcerated	(2) Ever Incarcerated	(3) Incarcerated	(4) Ever Incarcerated
Estimated Effect	0.536**	1.771***	1.132**	3.039***
	(0.250)	(0.370)	(0.473)	(0.709)
Low-Enrollment Avg.	3.76	11.26	4.96	14.35
Observations	424,606	424,606	424,606	424,606

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. Columns 1 and 2 report estimates using equation (1) and correspond to the post-period average of the β_τ coefficients and to the β_7 coefficient, respectively. Columns 3 and 4 report estimates using equation (2) and correspond to the post-period average of the α_τ coefficients and to the α_7 coefficient, respectively. In columns 1 and 3, "Low-Enrollment Avg." refers to the average post-period incarceration rate of the low-enrollment group in the full sample and among men with mental health histories, respectively. In columns 2 and 4, this statistic reports the incarceration rate of the low-enrollment group measured in the last quarter of the post period in the full sample and among men with mental health histories, respectively. Standard errors are clustered at the individual level.

Table 3: Two-Stage Least Squares Effect of Medicaid Enrollment on Men's Likelihood of Incarceration

	(1) Full Sample	(2) Mental Health History
Effect of Medicaid on Incarceration	-0.985***	-2.143***
	(0.363)	(0.612)
Post-Period Average	4.08	5.57
Observations	424,606	208,978

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 and the second column focuses on men with mental health histories. "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

	(1) Violent	(2) Property	(3) Financially motivated	(4) Non-financial violent	(5) Drug possession	(6) Misc.
Estimated Effect	1.311^{***}	1.469^{***}	2.537^{***}	1.174^{***}	0.510^{**}	1.964^{***}
	[0.459]	[0.486]	[0.603]	[0.416]	[0.241]	[0.614]
Low-Enrollment Avg.	4.20	5.18	7.99	3.68	0.95	6.57
Observations	424,606	424,606	424,606	424,606	424,606	424,606

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The estimated effects correspond to the α_7 coefficient in equation (2), analogous to the estimates presented in Figured 7. "Misc." refers to offenses that were not classified in the previous three categories (columns 3–5). "Low-Enrollment Avg." reports the share of low-enrollment men with mental health histories who had ever been incarcerated for that offense type in the last quarter of the post period. Standard errors are clustered at the individual level. For more details on offense classifications, see Appendix B.

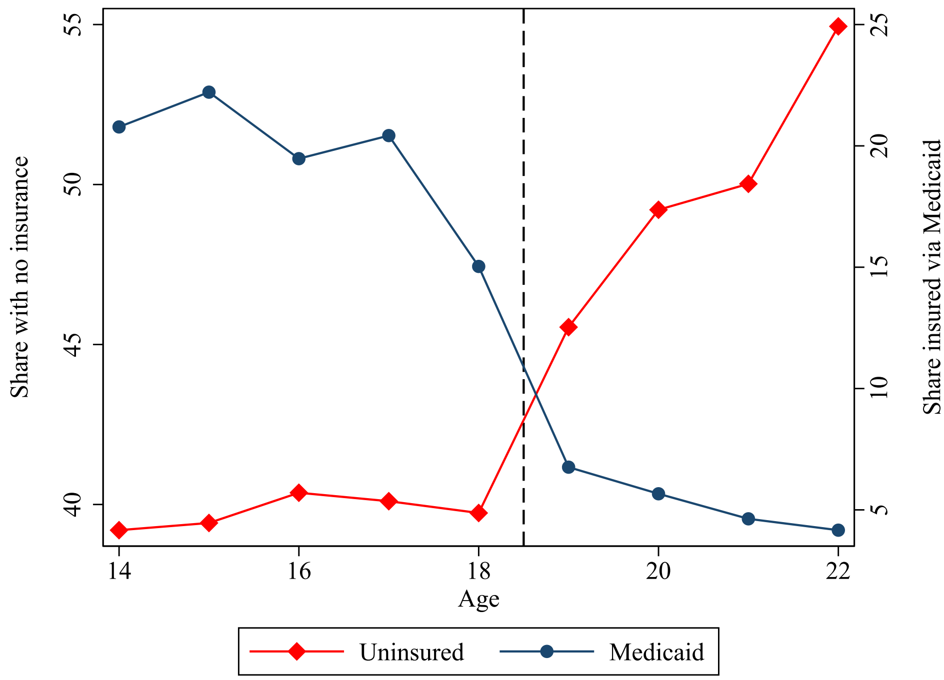
Table 5: Effect of Medicaid Disenrollment on Public Safety

	(1) All	(2) Violent	(3) Property	(4) Drug	(5) Other
Treat $\times$ Post	0.515^{***}	0.144^{**}	0.173^{**}	0.234^{***}	0.133^{*}
	(0.145)	(0.073)	(0.084)	(0.073)	(0.075)
Low-Enrollment Avg.	1.21	0.38	0.50	0.20	0.48
Observations	208,978	208,978	208,978	208,978	208,978

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers men with mental health histories and measures the effect of the loss in Medicaid eligibility on serious crime, as measured by arrests that resulted in an individual being taken into custody. This table reports the Treat $\times$ Post coefficient from equation (5). "Other" refers to offenses that were not classified in the previous three categories (columns 2–4). "Low-enrollment Avg." refers to the post-period average arrest rate of the low-enrollment group. For more details on offense classifications, see Appendix B.

A Appendix Figures and Tables

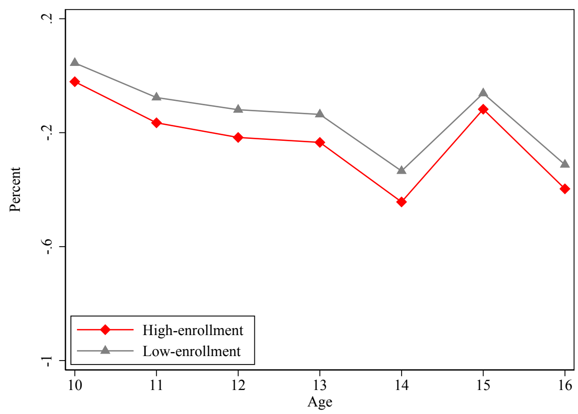
Figure A1: Share of Men in South Carolina Insured via Medicaid and Uninsured



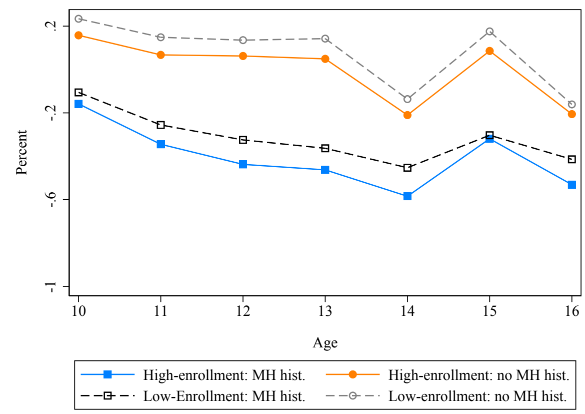
NOTE: This figure uses the 2004–2015 American Community Surveys (Ruggles et al. 2020) to plot the share of men in South Carolina who are insured via Medicaid as well as who do not have any health insurance coverage at a given age.

Figure A2: Standardized Test Scores and Juvenile Justice Felony Referrals for Full Sample and by Mental Health History (Raw Means)

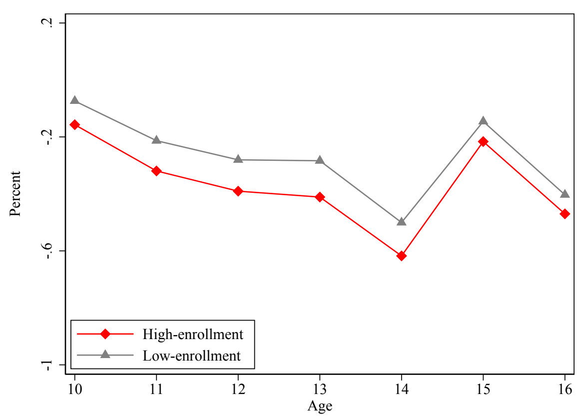
(a) Math score, full sample



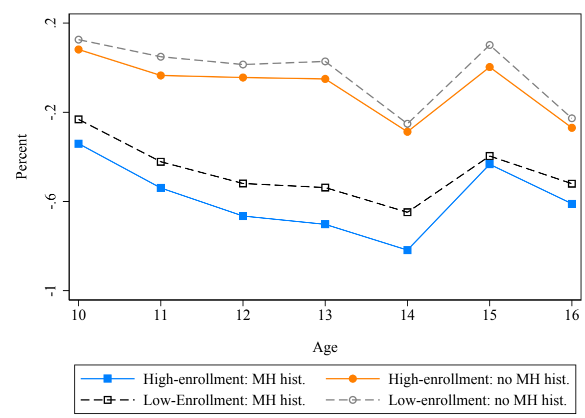
(b) Math score, by MH history



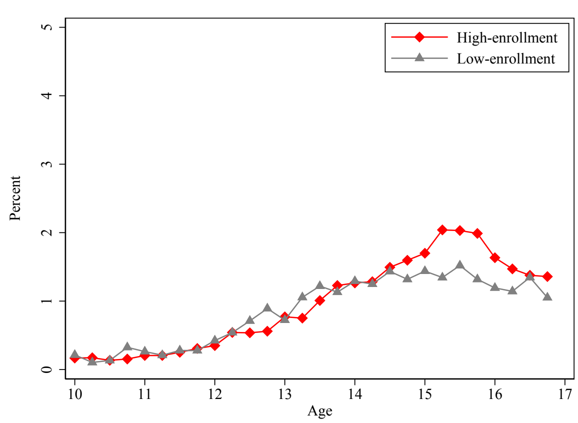
(c) ELA score, full sample



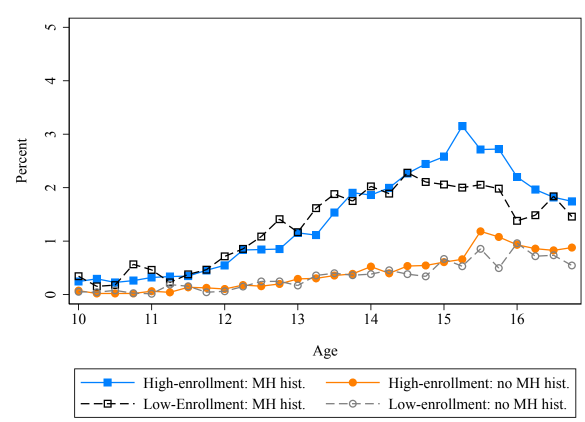
(d) ELA score, by MH history



(e) Felony referral, full sample

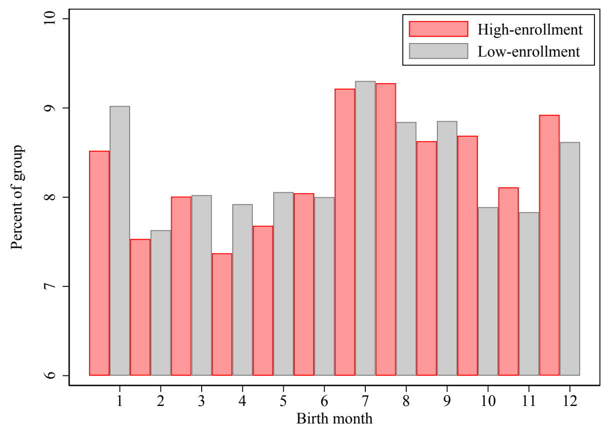


(f) Felony referral, by MH history



NOTE: These graphs plot the raw means of each outcome in a given year (or quarter in the last two panels) for the full sample of high- and low-enrollment men as well as separately by mental health history. Low-enrollment men are weighted using the corresponding weights from the matching procedure.

Figure A3: Share of High- and Low-Enrollment Men Born in Each Month

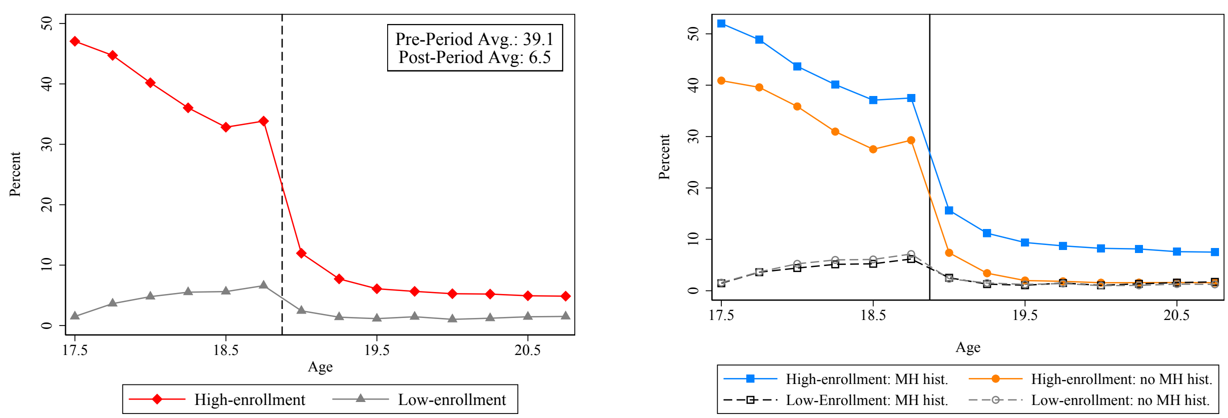


NOTE: This graph uses the sample of matched high- and low-enrollment men to plot the percent of individuals in each group that are born in a given month.

Figure A4: Share of High- and Low-Enrollment Men Filing Medicaid Claims (Raw Means)

(a) Full sample

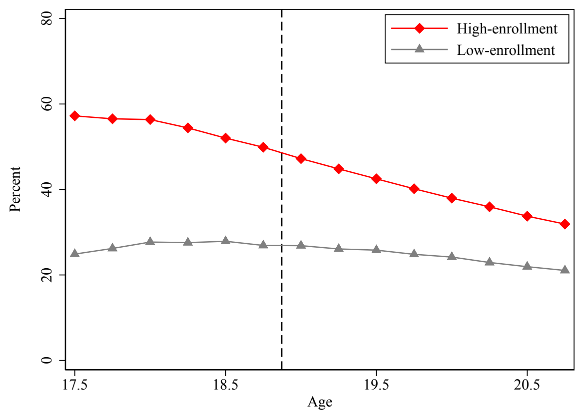
(b) By Mental Health History



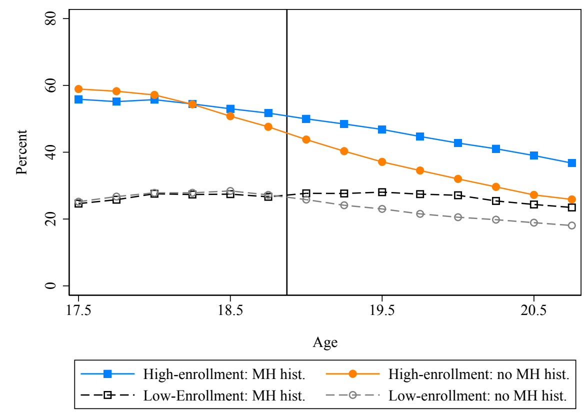
NOTE: These graphs use the sample of matched high- and low-enrollment men to plot the share of men filing any Medicaid claims in a given quarter. Panel (a) plots averages for the full sample and panel (b) plots averages separately by mental health history prior to age 16. The averages reported in panel (a) correspond to the pre- and post-period shares of high-enrollment individuals. In both panels, low-enrollment men are weighted using the corresponding weights from the matching procedure.

Figure A5: Share of High- and Low-Enrollment Men Enrolled in Public Assistance Programs (Raw Means)

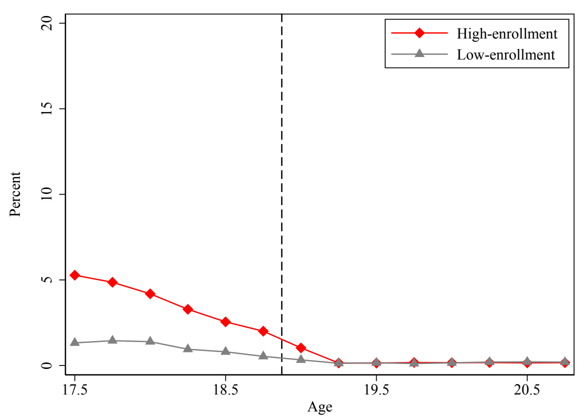
(a) SNAP, full sample



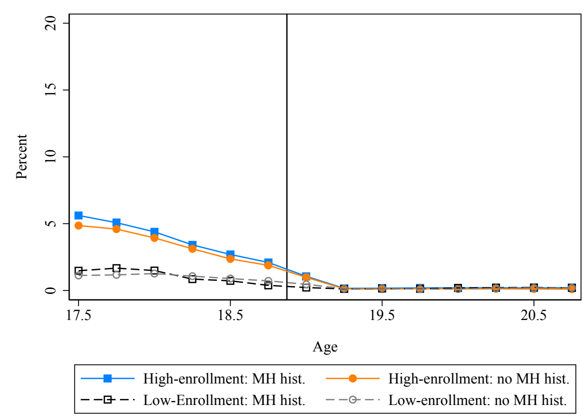
(b) SNAP, by MH history



(c) TANF, full sample



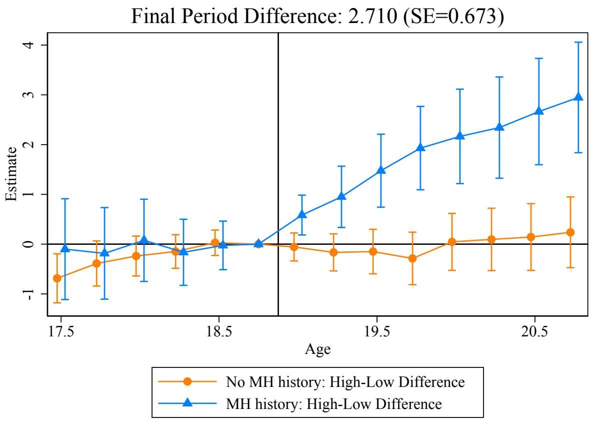
(d) TANF, by MH history



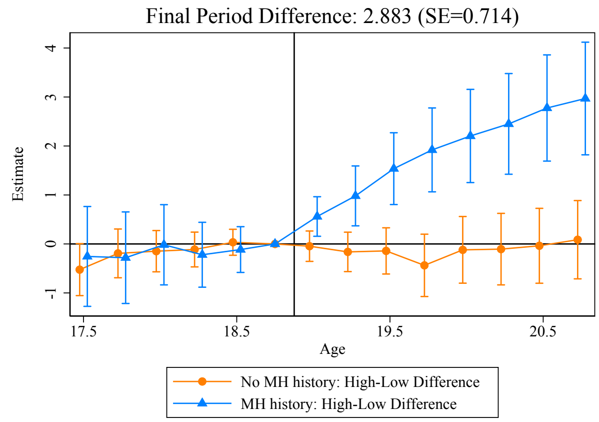
NOTE: These graphs use the sample of matched high- and low-enrollment men to plot the share of individuals enrolled in the SNAP and TANF programs in a given quarter. "SNAP" refers to the Supplemental Nutrition Assistance Program and "TANF" refers to the Temporary Assistance for Needy Families program. Panels (a) and (c) plot averages for the full sample and panels (b) and (d) plot averages separately by mental health history (prior to age 16). In all panels, low-enrollment men are weighted using the corresponding weights from the matching procedure.

Figure A6: Robustness of Main Result to Matching, Sample, and Variable Construction

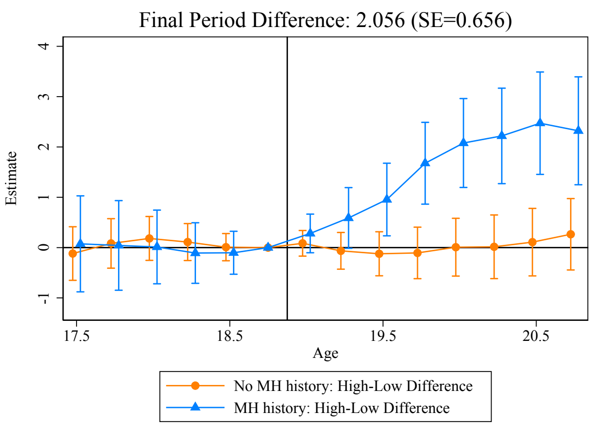
(a) DFL re-weighting approach



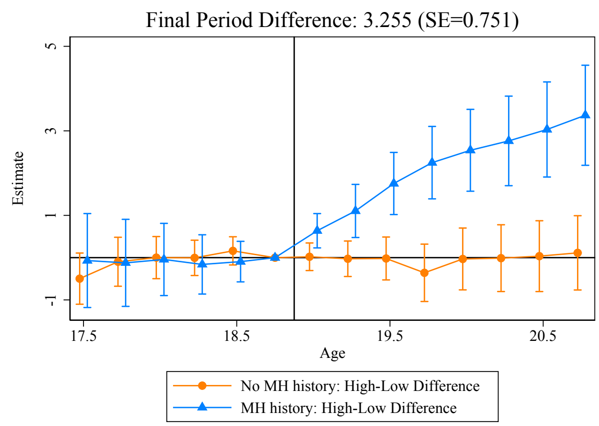
(b) Downweighting outliers



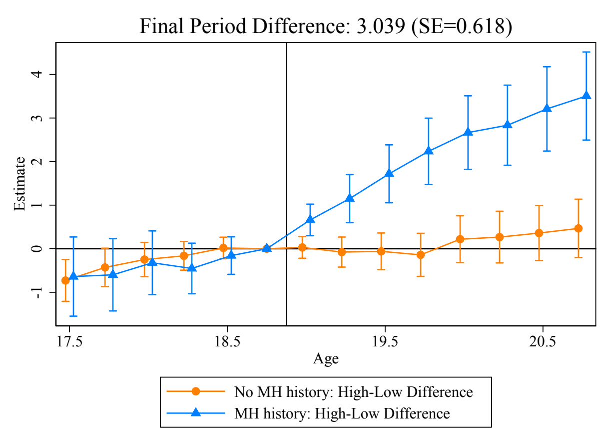
(c) Assignment in pre-period



(d) One match per high-enrollment individual



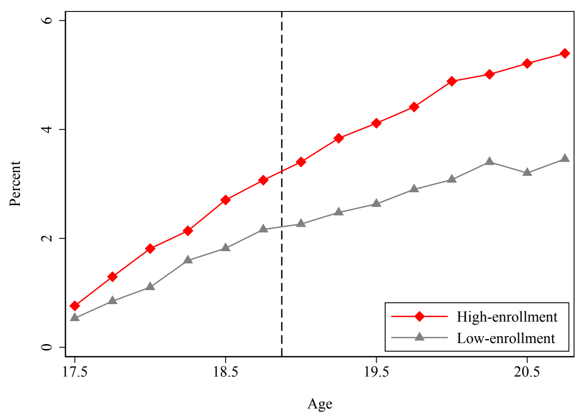
(e) No matching



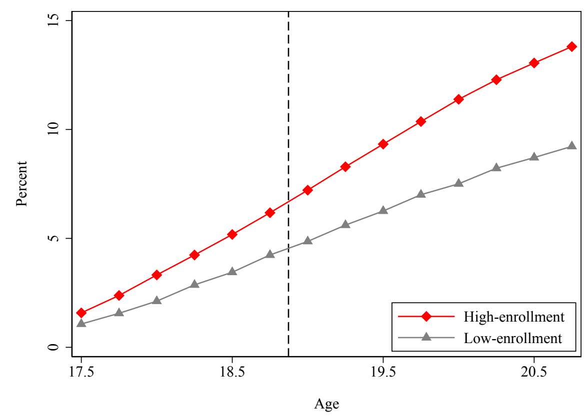
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. Each figure plots the differences between high- and low-enrollment men estimated using equation (2). "Final Period Difference" refers to the α_7 coefficient. Panel (a) re-weights low-enrollment men using the DFL re-weighting approach. For panel (b), I calculate the total weight each low-enrollment unit is given in the baseline sample and drop those whose weight is in the top 1% of that distribution. I then reconstruct weights for the remaining units so that there is balance on observables. Panel (c) assigns men to groups based on their Medicaid enrollment in the year and a half before age 19. Panel (d) randomly assigns one low-enrollment unit to each high-enrollment individual. Panel (e) uses all high- and low-enrollment men without any re-weighting.

Figure A7: Raw Means for All High- & Low-Enrollment Men (No Matching)

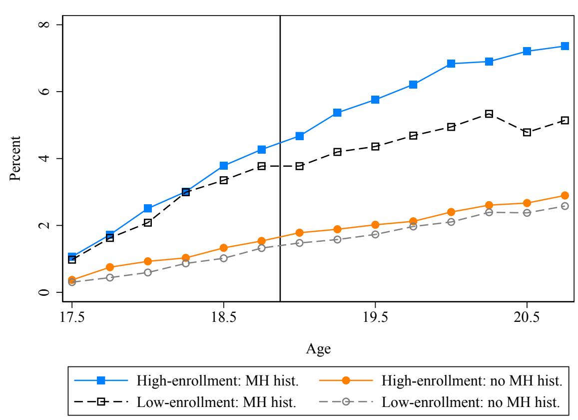
(a) Incarcerated that quarter



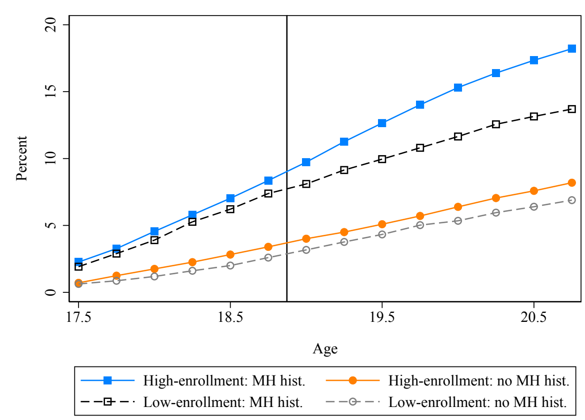
(b) Ever incarcerated



(c) Incarcerated that quarter



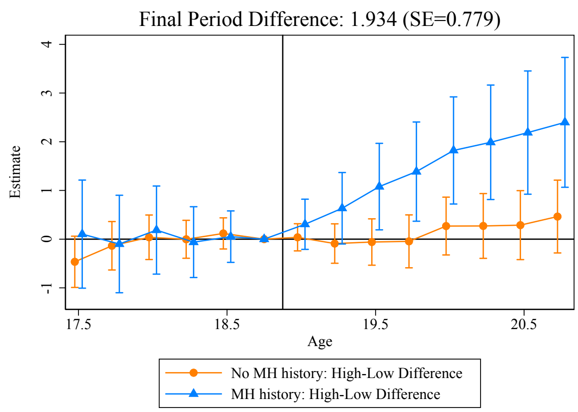
(d) Ever incarcerated



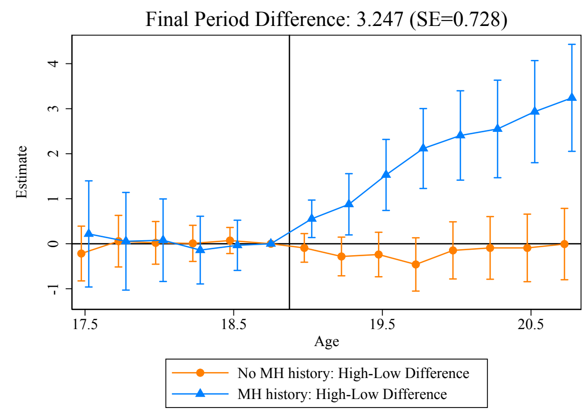
NOTE: The sample considered in this figure is all high- and low-enrollment men that are eligible for the matching procedure described in Section 4. The dependent variable is a measure of whether an individual was incarcerated. "Incarcerated that quarter" refers to being detained in an adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Panels (a) and (b) plot the raw means for all eligible high- and low-enrollment men, and panels (c) and (d) plot these means separately by men's mental health history prior to age 16.

Figure A8: Robustness of Main Result to Adding Matching Characteristics
(Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)

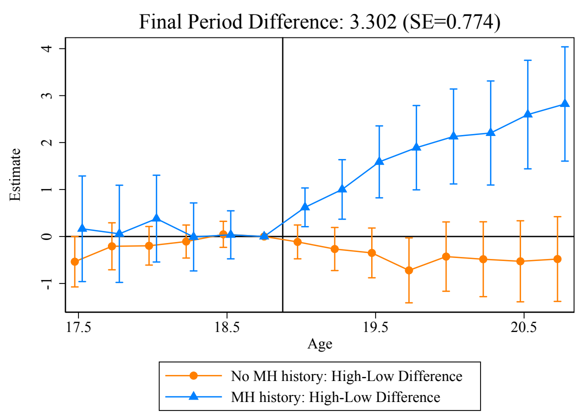
(a) Matching on school



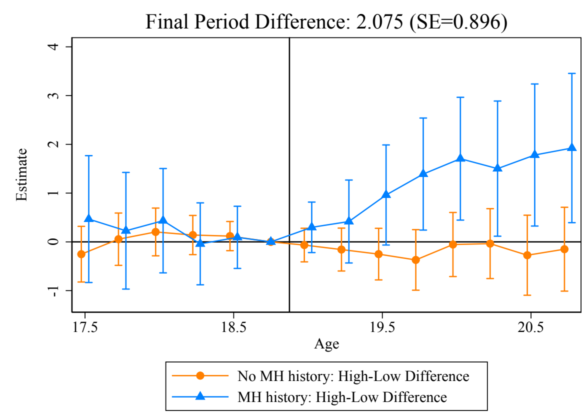
(b) Matching on SNAP enrollment



(c) Matching on juvenile justice contact



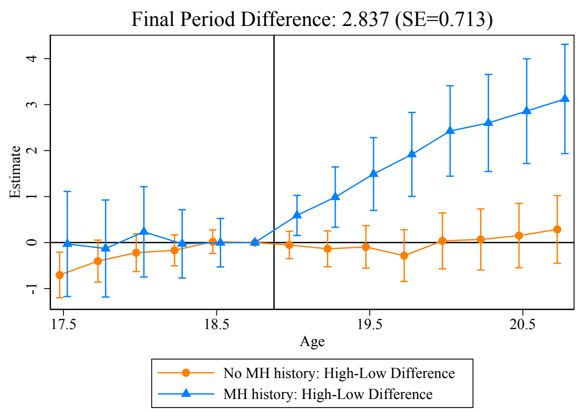
(d) Matching on school, SNAP, & DJJ contact



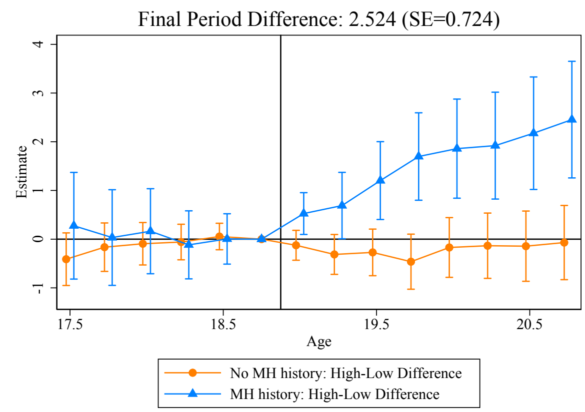
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. Each figure plots the differences between high- and low-enrollment men estimated using equation (2). “Final Period Difference” refers to the α_7 coefficient. Panel (a) matches men using school attended (instead of district). Panel (b) matches men based on the baseline characteristics and SNAP enrollment in adolescence (i.e., enrolled between ages 10–18). Panel (c) matches men based on the baseline characteristics and any juvenile justice involvement between ages 10–16. Panel (d) matches men based on school attended, SNAP enrollment, and Department of Juvenile Justice (DJJ) contact. All panels use coarsened exact matching to construct the comparison group of low-enrollment men. Standard errors are clustered at the individual level.

Figure A9: Robustness of Main Result to Adding Matching Characteristics Using DFL Re-Weighting Approach

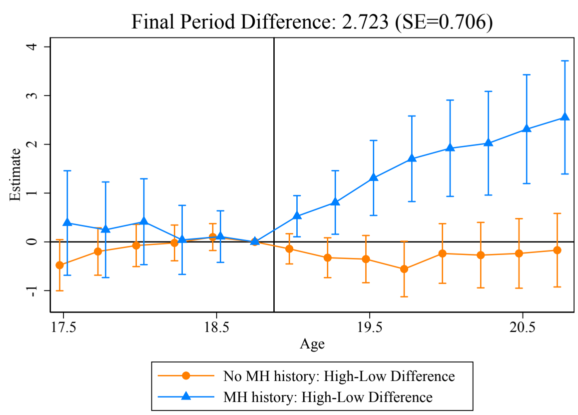
(a) Matching on school



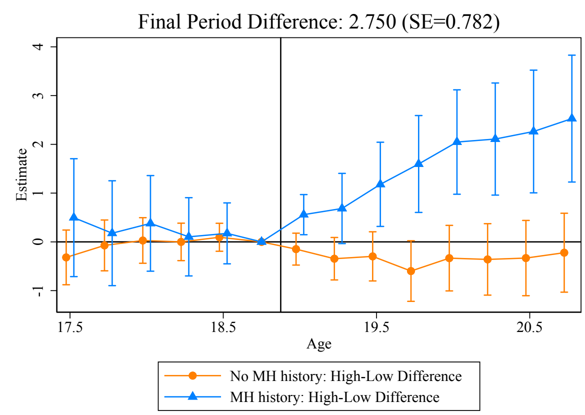
(b) Matching on SNAP enrollment



(c) Matching on juvenile justice contact

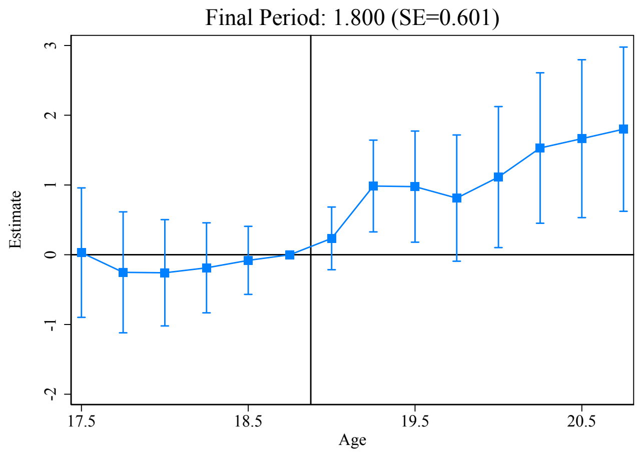


(d) Matching on school, SNAP, & DJJ contact



NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. Each figure plots the differences between high- and low-enrollment men estimated using equation (2). "Final Period Difference" refers to the α_7 coefficient. Panel (a) matches men using school attended (instead of district). Panel (b) matches men based on the baseline characteristics and SNAP enrollment in adolescence (i.e., enrolled between ages 10–18). Panel (c) matches men based on the baseline characteristics and any juvenile justice involvement between ages 10–16. Panel (d) matches men based on school attended, SNAP enrollment, and Department of Juvenile Justice (DJJ) contact. All panels use the DFL re-weighting approach to construct the comparison group of low-enrollment men. Standard errors are clustered at the individual level.

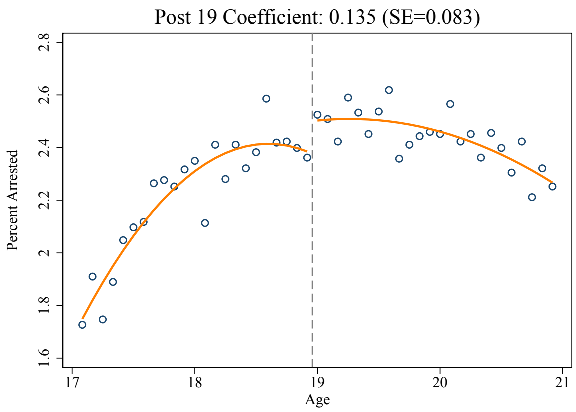
Figure A10: Likelihood of Ever Being Incarcerated among Medicaid Enrollees with Mental Health Histories, by Recency of Mental Health Claims



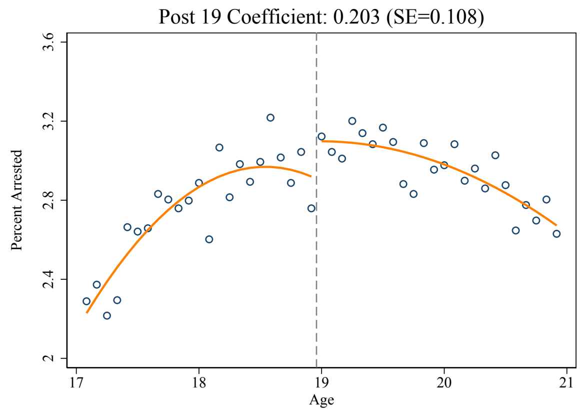
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. This figure considers men with mental health histories who were enrolled in Medicaid between the ages of 16.5–17.5. The figure plots the difference-in-differences estimates using equation 1, in which "treated" men are those with mental health claims in the six quarters before their 19th birthdays and comparison men are those with no mental health claims in that same period. Similar to the baseline approach, I implement a matching procedure prior to estimation so that the two groups are balanced on observable characteristics (i.e., race, year of birth, school district). Standard errors are clustered at the individual level.

Figure A11: Regression Discontinuity: Arrest Probability Around Age 19

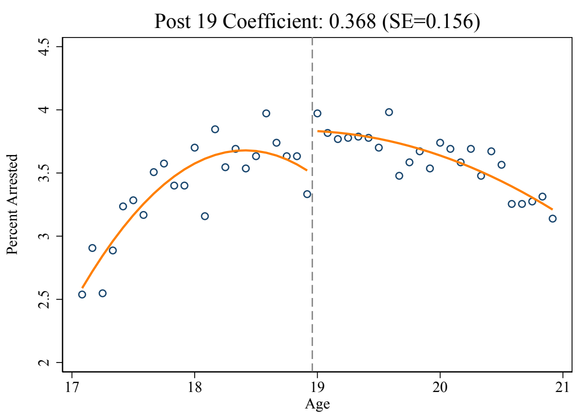
(a) Enrolled in Medicaid, ages 10–18



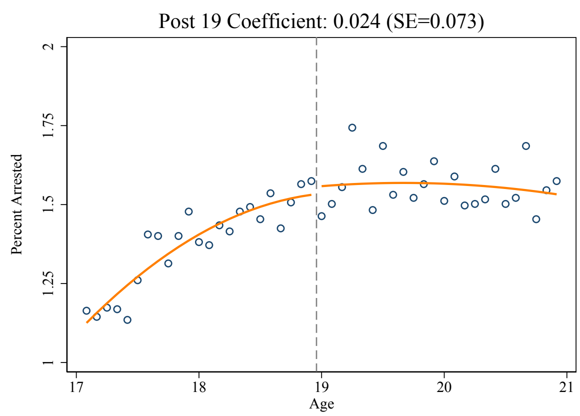
(b) Mental health history, ages 10–18



(c) Mental health history, ages 16–18



(d) No mental health history, ages 16–18

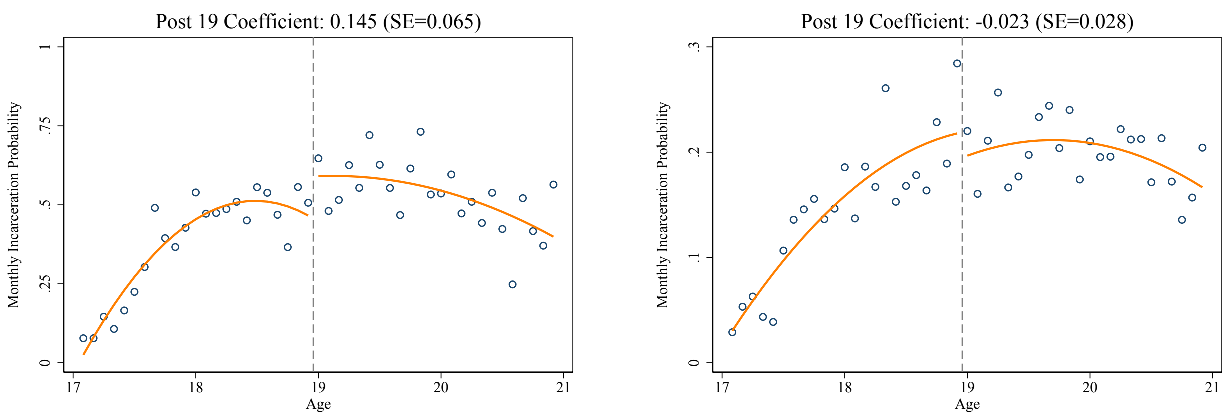


NOTE: This figure plots men's probability of being arrested around their 19th birthday using data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (4). The numbers reported above each figure are the discontinuity estimate from this equation and its standard error (clustering at the individual level). Panel (a) considers all men enrolled in Medicaid between the ages of 10 and 18. Panel (b) considers men with a mental health diagnosis or medication in this age range. Panel (c) considers men with a mental health diagnosis or medication between the ages of 16 and 18, and panel (d) considers men with no mental health diagnosis nor medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Figure A12: Regression Discontinuity: First-Time Incarceration Propensity Around Age 19

(a) Mental health history, ages 16–18

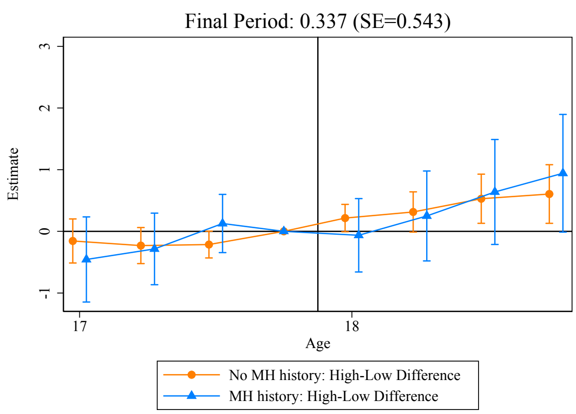
(b) No mental health history, ages 16–18



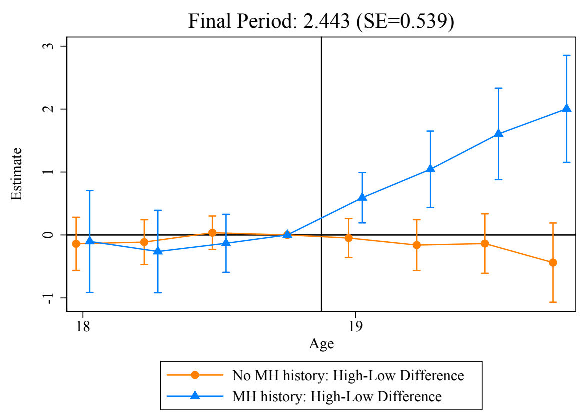
NOTE: This figure plots men's probability of being incarcerated for the first time using data on arrests that culminated with an individual being taken into custody. The circles represent the share of men incarcerated for the first time in that month as a share of men who were still at risk of being incarcerated. The solid line represents the estimate derived using equation (4). The numbers reported above each figure are the discontinuity estimate from this equation and its standard error (clustering at the individual level). Panel (a) considers men with a mental health diagnosis or medication between the ages of 16 and 18, and panel (b) considers men with no mental health diagnosis or medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Figure A13: Placebo Check: Likelihood of Ever Being Incarcerated Around Ages 18 and 19, by Mental Health History

(a) Age 18



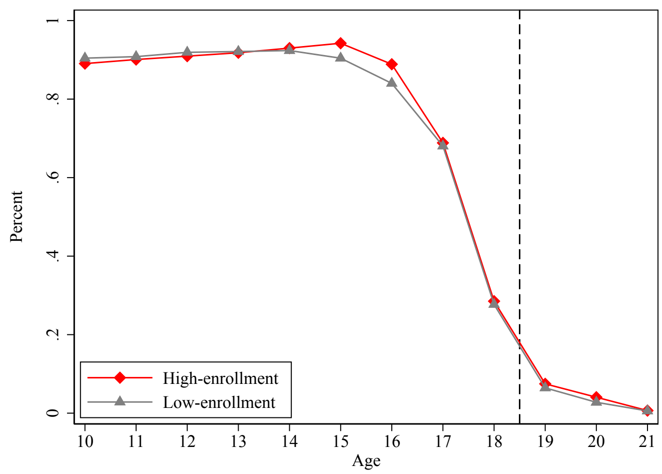
(b) Age 19



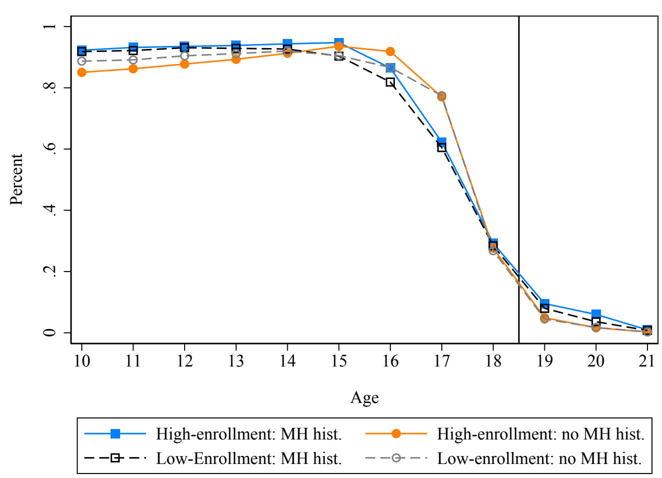
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). Panel (a) presents the results from the placebo check measuring the likelihood of incarceration around age 18. Men are assigned into groups based on their Medicaid enrollment at ages 15.5–16.5 (instead of 16.5–17.5) and then the matching procedure is implemented. Panel (b) presents the results from the baseline approach. In both panels, difference-in-differences estimates come from estimating equation (2) with three and four quarters in the pre- and post-period, respectively. “Final Period” refers to the α_4 coefficient using that equation. Standard errors are clustered at the individual level.

Figure A14: Share of High- and Low-Enrollment Men Enrolled in Public School (Raw Means)

(a) Full sample

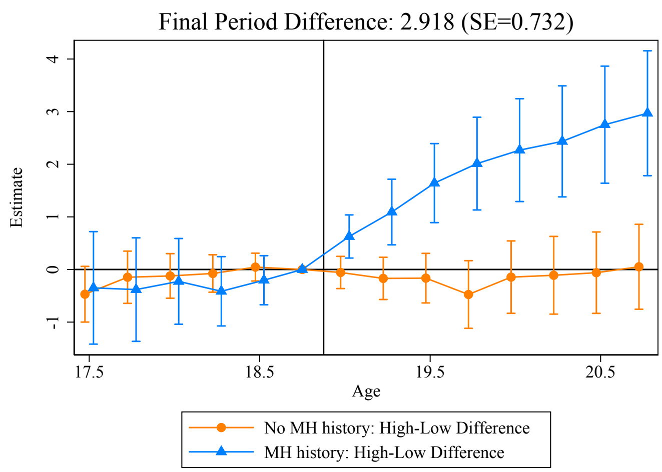


(b) By Mental Health History



NOTE: These graphs plot the share of men enrolled in public school at a given age for the full sample of matched high- and low-enrollment men as well as separately by mental health history. Data on school enrollment comes from South Carolina's Department of Education, and being enrolled refers to appearing in the administrative dataset that contains information on all public school students. In both panels, low-enrollment men are weighted using the corresponding weights from the matching procedure.

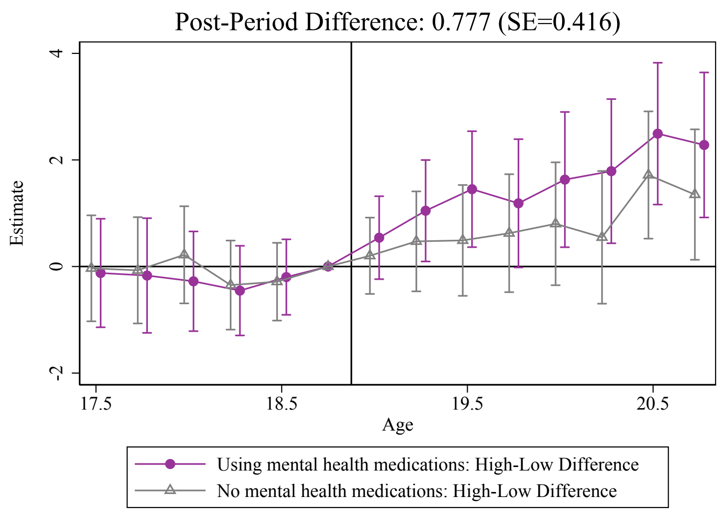
Figure A15: Robustness of Main Result to Excluding Foster Care & SSI Youth



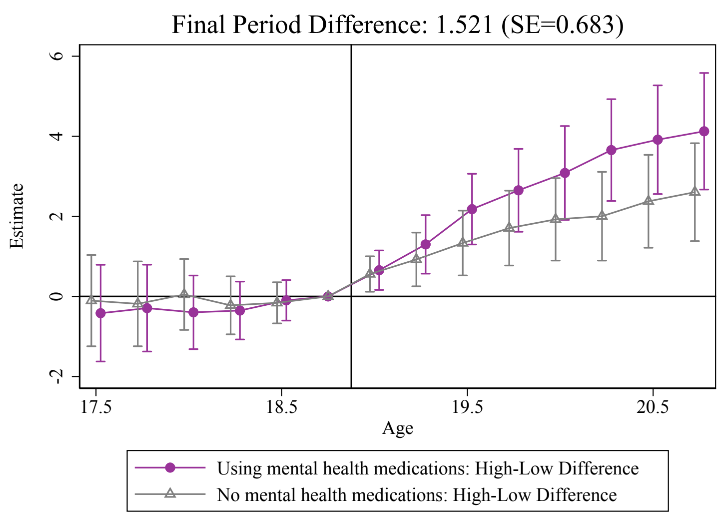
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. The sample utilized excludes foster care and SSI youth (and their corresponding comparison units) from the baseline sample. The figure plots the differences between high- and low-enrollment men estimated using equation (2). "Final Period Difference" refers to the α_7 coefficient. Standard errors are clustered at the individual level.

Figure A16: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage

(a) Incarcerated that quarter



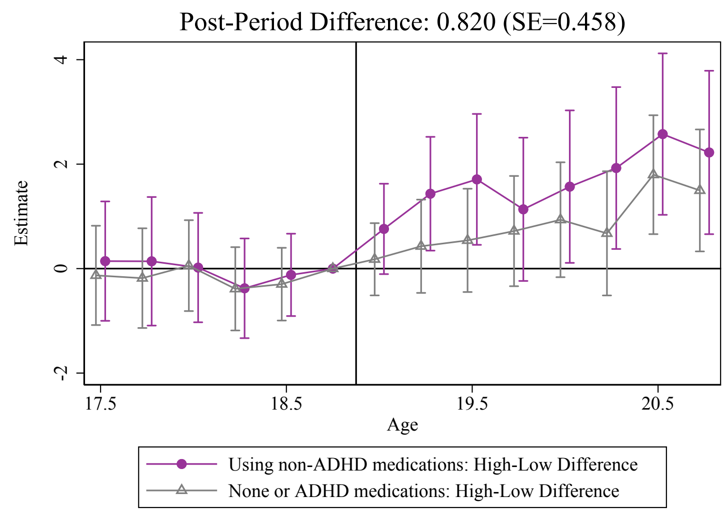
(b) Ever incarcerated



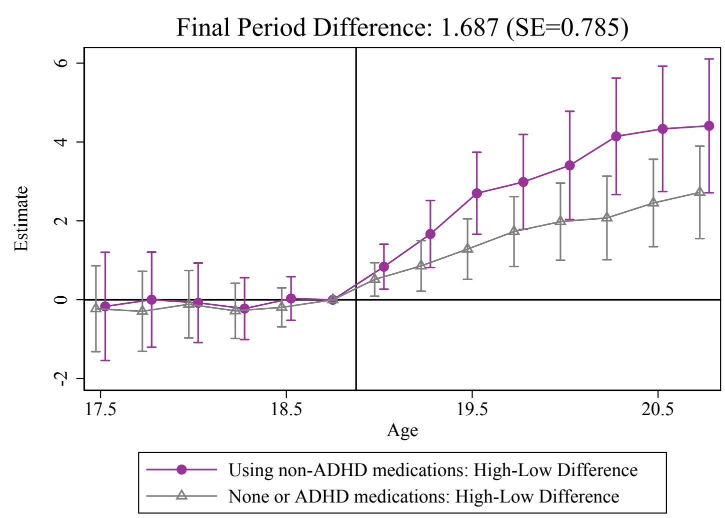
NOTE: The dependent variable measures whether an individual was incarcerated. These figures only consider high-enrollment men with mental health histories and the corresponding matched low-enrollment men. Using equation (2), the figures plot the difference-in-differences estimates for high-enrollment men who were taking mental health medications (in purple) and for those who were not filing claims for medications (in gray) relative to their matched comparison group. Medication utilization is defined as filing a claim for a psychotropic drug between the ages of 16 and 18. "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively, corresponding to the treatment effect for men taking mental health medications. "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 A17: Likelihood of Incarceration for Men with a Mental Health History, by Type of Mental Health Medication

(a) Incarcerated that quarter



(b) Ever incarcerated



NOTE: The dependent variable measures whether an individual was incarcerated. These figures only consider high-enrollment men with mental health histories and the corresponding matched low-enrollment men. Using equation (2), the figures plot the difference-in-differences estimates for high-enrollment men who were taking non-ADHD mental health medications (in purple) and for those who were not filing claims for such medications (in gray) relative to their matched comparison group. Non-ADHD medication utilization is defined as filing a claim for antianxiety, antidepressant, or antipsychotic medications between the ages of 16 and 18. "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively, corresponding to the treatment effect for men taking mental health medications. "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.

Table A1: Summary Statistics of Matching Procedure Candidates and Matches

	(1) Candidate low-enrollment	(2) Matched low-enrollment	(3) Candidate high-enrollment	(4) Matched high-enrollment
Black	67.25	72.52	71.59	72.52
Mental health history (pre 16)	34.28	55.40	56.03	55.40
Age of first diagnosis	11.52	11.33	12.10	12.12
Number of diagnoses (pre 16)	0.54	0.88	1.24	1.23
Juvenile justice referral (ever)	28.88	33.99	41.08	40.90
Arrests (ever)	19.48	21.93	26.90	26.70
SNAP (ever)	71.34	73.28	88.23	88.37
TANF (ever)	14.13	15.61	27.78	27.79
SSI (ever)	2.60	3.31	12.39	12.17
Foster care (ever)	1.05	1.94	4.56	4.51
Observations	8,964	8,911	22,063	21,418

NOTE: All columns report the percent of individuals with a particular attribute. “Candidate” refers to men who were eligible for the matching procedure described in Section 4. “Matched” refers to men who were successfully matched. In column 2, men are weighted using the corresponding weights from the matching procedure. “Mental health history” refers to having a claim with a mental health diagnosis or for a mental health medication prior to age 16. “Age of first diagnosis” is only calculated for individuals who received a diagnosis between the ages of 10–18. “Number of diagnoses” refers to the number of different diagnoses received prior to age 16 (among the twelve diagnoses considered). Every other outcome is measured between the ages of 10 and 18. “Arrests” refers to having an arrest record in the South Carolina Law Enforcement Division data. For more details on the variable construction and diagnosis categories, see Appendix B.

Table A2: Robustness to Specification, Matching, & Samples for Men's Likelihood of Ever Being Incarcerated

	(1) High enrollment	(2) Low enrollment	(3) Full sample	(4) By mental health history
Baseline	21,418	8,911	1.771***	3.039***
			(0.370)	(0.709)
Cluster: match level	21,418	8,911	1.771***	3.039***
			(0.187)	(0.363)
Cluster: match & ind.	21,418	8,911	1.771***	3.039***
			(0.368)	(0.706)
DFL re-weighting, baseline	22,063	8,964	1.761***	2.710***
			(0.356)	(0.673)
Downweighting outliers	21,418	8,911	1.675***	2.883***
			(0.372)	(0.714)
Assignment in pre-period	20,820	9,597	1.385***	2.056***
			(0.340)	(0.656)
One match	21,418	7,418	1.919***	3.255***
			(0.391)	(0.751)
No matching	22,063	8,964	2.617***	3.039***
			(0.292)	(0.618)
Matching on school	19,675	8,581	1.504***	1.934**
			(0.406)	(0.779)
Matching on SNAP enrollment	20,889	8,612	1.782***	3.247***
			(0.380)	(0.728)
Matching on DJJ contact	20,501	8,776	1.331***	3.302***
			(0.398)	(0.774)
Matching on school, SNAP, & DJJ	16,486	7,458	0.900**	2.075**
			(0.450)	(0.896)
DFL re-weighting, school	22,063	8,964	1.888***	2.837***
			(0.377)	(0.713)
DFL re-weighting, SNAP	22,063	8,964	1.341***	2.524***
			(0.384)	(0.724)
DFL re-weighting, DJJ	22,063	8,964	1.364***	2.723***
			(0.372)	(0.706)
DFL re-weighting, all	22,063	8,964	1.322***	2.750***
			(0.415)	(0.782)
Excluding SSI & foster care	18,605	8,894	1.522***	2.918***
			(0.368)	(0.732)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The first row reproduces the baseline estimates in Figure 5. The next two rows cluster the standard errors at the match level and at the match and individual level, respectively. The 4th row uses the DFL re-weighting approach to re-weight eligible low-enrollment men. The 5th row drops low-enrollment men with disproportionate weight in the regression. The 6th row assigns men to a group based on their enrollment during the pre-period. The 7th row forces each high-enrollment individual to have one randomly chosen low-enrollment comparison unit. The 8th row uses all eligible high- and low-enrollment men (no matching). The 9th row matches men based on school attended. The 10th row matches men based on the baseline characteristics and SNAP enrollment. The 11th row matches men based on the baseline characteristics and DJJ contact. The 12th row matches men based on school, SNAP enrollment, and DJJ contact. Rows 13–16 change the matching characteristics analogously to rows 9–12, respectively, but using the DFL re-weighting approach. The 17th row excludes foster care and SSI youth (and their comparison units). Except for the 2nd and 3rd rows, standard errors are clustered at the individual level and reported under their estimate in parentheses. Appendix Figures A6, A8, A9, and A15 show graphical depictions of rows 4–17.

Table A3: Estimated Discontinuities in Criminal Behavior Around Age 19

(a) Arrest propensity, quadratic polynomial

	Enrolled, Age 10–18	MH claim, Age 10–18	MH claim, Age 16–18	No MH claim, Age 16–18
Discontinuity at 19	0.135	0.203*	0.368**	0.024
	[0.083]	[0.108]	[0.156]	[0.073]
Pre-period Avg.	2.22	2.76	3.39	1.38
Observations	1,154,132	839,890	485,134	973,135

(b) Arrest propensity, cubic polynomial

	Enrolled, Age 10–18	MH claim, Age 10–18	MH claim, Age 16–18	No MH claim, Age 16–18
Discontinuity at 19	0.121	0.257*	0.455**	-0.060
	[0.112]	[0.145]	[0.210]	[0.097]
Pre-period Avg.	2.22	2.76	3.39	1.38
Observations	1,154,132	839,890	485,134	973,135

(c) Incarceration propensity, quadratic polynomial

	Enrolled, Age 10–18	MH claim, Age 10–18	MH claim, Age 16–18	No MH claim, Age 16–18
Discontinuity at 19	0.029	0.060	0.103	-0.029
	[0.035]	[0.046]	[0.065]	[0.031]
Pre-period Avg.	0.27	0.36	0.43	0.17
Observations	1,154,132	839,890	485,134	973,135

NOTE: This table considers men's probability of being arrested and incarcerated for each month around their 19th birthdays using data from the South Carolina Law Enforcement Division. Each estimate reports the discontinuity estimate from equation (4) and its standard error (clustering at the individual level). Panels (a) and (c) use a quadratic polynomial, whereas panel (b) uses a cubic polynomial. The first column considers men enrolled in Medicaid between the ages of 10 and 18. The second column considers men with a mental health diagnosis or medication in this age range. The third column considers men with a mental health diagnosis or medication between ages 16–18, and the fourth column considers men with no mental health diagnosis nor medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Table A4: Estimated Discontinuities in Criminal Behavior Around Age 20

(a) Arrest propensity, quadratic polynomial

	Enrolled, Age 10–18	MH claim, Age 10–18	MH claim, Age 16–18	No MH claim, Age 16–18
Discontinuity at 20	0.097	0.098	0.163	-0.030
	[0.084]	[0.108]	[0.157]	[0.072]
Pre-period Avg.	2.43	2.99	3.68	1.52
Observations	1,178,688	857,760	495,456	993,840

(b) Incarceration propensity, quadratic polynomial

	Enrolled, Age 10–18	MH claim, Age 10–18	MH claim, Age 16–18	No MH claim, Age 16–18
Discontinuity at 20	-0.023	-0.044	-0.112	0.043
	[0.037]	[0.049]	[0.072]	[0.031]
Pre-period Avg.	0.41	0.53	0.65	0.24
Observations	1,178,688	857,760	495,456	993,840

NOTE: This table considers men's probability of being arrested and incarcerated for each month around their 20th birthdays using data from the South Carolina Law Enforcement Division. Each estimate reports the discontinuity estimate from equation (4) and its standard error (clustering at the individual level). Both panels use a quadratic polynomial. The first column considers men enrolled in Medicaid between the ages of 10 and 18. The second column considers men with a mental health diagnosis or medication in this age range. The third column considers men with a mental health diagnosis or medication between ages 16–18, and the fourth column considers men with no mental health diagnosis or medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Table A5: Placebo Check: Likelihood of Ever Being Incarcerated Around Different Ages, by Mental Health History

	(1) Age 17	(2) Age 18	(3) Age 19
Adult incarcerations	—	0.337	2.443***
		(0.543)	(0.539)
All incarcerations	-0.092	-0.014	1.699***
	(0.737)	(0.441)	(0.539)
Observations	238,088	241,712	242,632

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 7.2.4 in which the baseline empirical approach is replicated and shifted back two years (for column 1) and one year (for column 2). All of the estimates correspond to the α_4 coefficient using equation (2) with three and four quarters in the pre- and post-period, respectively. In the first row, the dependent variable is a measure of whether an individual has ever been incarcerated in an adult correctional facility (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). In the second row, the outcome variable is a measure of whether an individual has ever been incarcerated in an adult correctional facility or detained in a juvenile facility. Standard errors are clustered at the individual level.

Table A6: Effect of Medicaid Loss on Likelihood of Arrest for Men with Mental Health Histories

	(1) All arrests	(2) Serious arrests	(3) Felony offenses	(4) Non-felony offenses	(5) Felony incarceration	(6) Non-felony incarceration
Treat × Post	0.179	0.515***	0.423*	-0.244	0.424***	0.091
	(0.325)	(0.145)	(0.229)	(0.240)	(0.123)	(0.063)
Low-Enrollment Avg.	6.44	1.21	2.75	3.69	0.93	0.29
Observations	208,978	208,978	208,978	208,978	208,978	208,978

NOTE: Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1. This table considers men with mental health histories and measures the effect of the loss in Medicaid eligibility on criminal activity after their 19th birthdays using arrest data from the South Carolina Law Enforcement Division. This table reports the Treat × Post coefficient from a difference-in-differences equation analogous to equation (1), with arrests as the outcome variable and with the *Post* coefficient capturing the difference between high- and low-enrollment men in the post-period relative to the pre-period. The first column considers all arrests. The second column considers serious arrests that ended with an individual being detained in a correctional facility. The third column considers all arrests in which any of the charges or disposition information indicated a felony offense. The fourth column considers all arrests with no associated felony offense. The fifth column considers serious arrests that ended with an individual being detained in a correctional facility with an associated felony offense. The sixth column considers arrests that ended with an individual being detained in a correctional facility, but with no associated felony offense. "Low-Enrollment Avg." refers to the post-period average arrest rate of low-enrollment men with mental health histories.

Table A7: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for Men with Mental Health Histories, by Recency of Mental Health Claims

	(1) All	(2) Violent	(3) Property	(4) Financially motivated	(5) Non-financial violent	(6) Drug possession	(7) Misc.
Less recent	2.578***	1.119***	1.183***	2.046***	1.095***	0.629***	1.600***
	[0.615]	[0.419]	[0.406]	[0.516]	[0.383]	[0.223]	[0.546]
More recent	1.541**	0.163	0.934**	1.226**	-0.145	-0.078	0.893*
	[0.641]	[0.405]	[0.464]	[0.561]	[0.365]	[0.255]	[0.542]
Post-Period Avg.	17.95	6.87	8.75	12.90	6.01	1.94	11.20
High-Enrollment Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers high-enrollment men with mental health histories and their matched low-enrollment men, and splits the samples into two groups based on whether high-enrollment men filed mental health claims in the six quarters before their 19th birthdays. “Less recent” refers to high-enrollment men without mental health claims in the pre-period, and the reported estimates correspond to the λ_7 coefficient using equation (2) relative to their matched comparison group. “More recent” refers to high-enrollment men who filed mental health claims in the pre-period, and the estimates correspond to the α_7 coefficient using the same equation relative to their matched comparison group. The estimates in the first column correspond to panel (b) of Figure 6. The remaining columns consider the likelihood of incarceration for different offense types. “Misc.” refers to offenses that were not classified in the previous three categories (columns 4–6). “Post-Period Avg.” reports the share of high-enrollment men with less-recent mental health histories who had ever been incarcerated for that offense type in the last quarter of the post period. Standard errors are clustered at the individual level. For more details on offense classifications, see Appendix B.

Table A8: Distribution of Mental Health Medications and Diagnoses

	(1) All men	(2) High-enrollment men	(3) High-enrollment men, MH history
Mental health medications:			
Any psychotropic drug	33.86	41.15	67.21
ADHD drug	22.67	27.09	47.45
Antidepressant drug	13.72	17.21	28.70
Antipsychotic drug	9.12	11.80	20.17
Antianxiety drug	11.30	14.71	22.68
Mental health diagnoses:			
Attention-deficit/hyperactivity disorder (ADHD)	24.95	30.10	52.35
Depressive disorder	9.61	12.55	20.75
Anxiety disorder	7.12	9.13	14.74
Adjustment disorder	7.58	9.69	15.99
Conduct disorder	15.05	19.26	32.03
Oppositional defiant disorder	12.49	16.12	27.43
Bipolar disorder	3.20	4.21	7.04
Intellectual disabilities	7.17	9.32	16.53
Other neurodevelopmental disorder	18.04	21.68	38.53
Substance-related and addictive disorder	15.33	20.90	29.40
Post-traumatic stress disorder	1.46	1.90	3.18
Other unclassified disorder	5.96	7.58	12.19
Number of diagnoses	1.28	1.62	2.70
Age of first diagnosis	11.99	12.12	11.35
Number of individuals	33,252	21,418	11,866

NOTE: This table reports the share of men that have an insurance claim with that diagnosis or for that mental health medication at any point between the ages of 10 and 18. The first column considers all men who were enrolled in Medicaid between the ages of 10 and 18. The second column considers men in the high-enrollment group. The final column considers high-enrollment men with a mental health history prior to age 16. "Age of first diagnosis" is only calculated for individuals who received a diagnosis between the ages of 10–18. For more details on the variable construction, see Appendix B.

Table A9: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for Men with Mental Health Histories, by Mental Health Medication Usage

(a) All mental health medications

	(1) All	(2) Violent	(3) Property	(4) Financially motivated	(5) Non-financial violent	(6) Drug possession	(7) Misc.
No MH medications	2.605***	0.864**	1.198***	1.975***	0.832**	0.601***	1.638***
	[0.624]	[0.430]	[0.414]	[0.530]	[0.391]	[0.229]	[0.551]
MH medications	1.521**	0.910**	0.924*	1.476**	0.613	0.000	0.816
	[0.683]	[0.422]	[0.496]	[0.590]	[0.389]	[0.272]	[0.584]
Post-Period Avg.	17.76	6.65	8.35	12.56	5.76	1.97	10.83
High-Enrollment Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

(b) Non-ADHD medications

	(1) All	(2) Violent	(3) Property	(4) Financially motivated	(5) Non-financial violent	(6) Drug possession	(7) Misc.
None or ADHD meds.	2.723***	1.003**	1.343***	2.106***	0.913**	0.569***	1.772***
	[0.598]	[0.407]	[0.396]	[0.504]	[0.370]	[0.216]	[0.523]
Non-ADHD meds.	1.687**	0.724	0.723	1.575**	0.544	0.133	0.612
	[0.785]	[0.474]	[0.578]	[0.681]	[0.433]	[0.315]	[0.656]
Post-Period Avg.	17.58	6.53	8.45	12.48	5.68	1.91	10.66
High-Enrollment Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

NOTE: Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1. This table considers high-enrollment men with mental health histories and compares them to their matched low-enrollment group. Both panels report estimates using equation (2). The estimate in the first row corresponds to the λ_7 coefficient in that equation, whereas the estimate in the second row corresponds to the α_7 coefficient. In panel (a), "MH medications" and "No MH medications" refer to high-enrollment men who did and did not file a claim for a psychotropic drug between the ages of 16–18, respectively. "Post-Period Avg." reports the average outcome for the latter group in the last quarter of the post period. In panel (b), "Non-ADHD meds." refers to high-enrollment men filing claims for non-ADHD medications and "None or ADHD meds." refers to high-enrollment men who did not file such claims between the ages of 16–18. "Post-Period Avg." reports the average outcome for the latter group in the last quarter of the post period. "Misc." refers to offenses that were not classified in the previous three categories (columns 4–6). Standard errors are clustered at the individual level. For more details on offense and drug classifications, see Appendix B.

Table A10: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for Men with Mental Health Histories, Excluding Men with Specific Diagnoses

Panel (a):

	(1) Baseline	(2) Intellectual disability	(3) Other neurodev.	(4) Depression	(5) Anxiety	(6) Bipolar	(7) PTSD
Ever detained	3.129***	3.222***	3.746***	2.747***	3.047***	2.820***	3.084***
	[0.583]	[0.598]	[0.658]	[0.590]	[0.599]	[0.587]	[0.584]
Observations	208,978	181,496	144,746	174,244	184,324	197,246	203,616

Panel (b):

	(1) Conduct	(2) Opp. defiant	(3) Adjustment	(4) ADHD	(5) ADHD only	(6) Substance abuse	(7) Other
Ever detained	1.065*	1.475***	3.113***	1.868***	3.411***	0.807	3.259***
	[0.557]	[0.556]	[0.590]	[0.640]	[0.598]	[0.553]	[0.587]
Observations	155,512	163,282	182,322	121,212	196,070	159,894	188,678

NOTE: Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1. This table considers high-enrollment men with mental health histories and their matched low-enrollment men. The estimates correspond to the difference-in-differences estimate in the final quarter of the sample period ($alpha_7$ in equation 1 but using these subsamples). The estimate in column 1 of panel (a) reports the baseline estimate using all men with mental health histories. The remaining columns exclude high-enrollment men with a specific diagnosis as well their corresponding low-enrollment units. Column 5 in panel (b) excludes high-enrollment men whose only diagnosis is ADHD (and their comparison units). Standard errors are clustered at the individual level. For more details on the classification of diagnoses, see Appendix B.

Table A11: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for Men with Mental Health Histories, by Chronic Physical Conditions

	(1) Baseline	(2) Physical conditions	(3) No physical conditions
Estimated Effect	3.129***	2.176*	3.636***
	[0.583]	[1.176]	[0.674]
Low-Enrollment Average	14.35	13.08	14.68
High-Enrollment Men	11,866	3,707	8,159

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers high-enrollment men with mental health histories and their matched low-enrollment men. The estimates correspond to the difference-in-differences estimate in the final quarter of the sample period ($alpha_7$ in equation 1 but using these subsamples). The estimate in column 1 reports the baseline estimate using all men with mental health histories. Columns 2 and 3 split the sample of high-enrollment men into those diagnosed with and without chronic physical conditions before the age of 16. In both columns, the high-enrollment men are compared to their matched comparison group. Standard errors are clustered at the individual level. For more details on the classification of diagnoses, see Appendix B.

B Loss in Medicaid Eligibility: Data Appendix

This appendix describes the sample selection and restrictions as well as the variable construction for estimating the effect of losing health insurance coverage on criminal behavior, leveraging the fact that individuals age out Medicaid eligibility on their 19th birthdays.[40]

B.1 Selection of Sample

The goal of the sample selection was to hone in on low-income communities that are likely eligible for the Medicaid program and have a high propensity of coming into contact with the criminal justice system. Given the lack of information on household incomes, South Carolina's Revenue and Fiscal Affairs (RFA) Office instead used records from the Department of Education as an alternative way to identify low-income communities, and thus adolescents that were likely living in low-income households. 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.[41] Then, these shares were averaged across the school years, schools were ranked based on the average share, and the 142 high schools in the upper half of this distribution were chosen. Finally, if an individual was ever enrolled in of these "low-income" high schools—not necessarily in the 2008–2014 range, but at any point—he or she was chosen to be in the sample.

Once the sample was selected, RFA pulled information on these individuals starting at age 10 (i.e., starting in the year 2000) from the six state government agencies that approved data sharing for this project. RFA links individuals across datasets using a person's first and last name, date of birth, and social security number whenever available.[42] Each individual is then assigned a unique identifier, which I then use to identify the same individual across datasets and over time.

Due to confidentiality concerns, none of the datasets provide me with information on an individual's exact date of birth. For individuals ever enrolled in Medicaid (roughly 70% of the sample), I use the month and year of birth in the Medicaid recipient file to construct a panel dataset at the person-age-quarter level (where quarter refers to each of the four quarters within an age). I

[40] 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 individuals with a disability (SCDHHS 2020a, KFF 2019).

[41] Students who attend private school or home school are not included in this data. In the 2014–15 school year, South Carolina introduced the Community Eligibility Provision program, through which certain schools began providing free lunch to all students regardless of an individual student's eligibility for free or reduced price lunch.

[42] Statisticians at RFA noted that the match rate is typically high (around 97%) and is even higher conditional on having a valid name and date of birth.

then merge in information at this level of granularity using data from the other agencies.

B.2 Sample restrictions

The analysis focuses on low-income male residents with an available birth date (i.e., ever enrolled in the Medicaid program) in the 1990–1993 birth cohorts.[43] To remain in the sample, men must (1) be enrolled in the Medicaid program prior to age 19; (2) appear in any administrative records between the ages of 15–18 to avoid including individuals who may have moved out of the state; (3) be alive during the period in which individuals are assigned into the high- and low-enrollment groups (ages 16.5–17.5); and (4) not be incarcerated during the assignment period. Table B1 indicates how the sample size changes as I impose each additional restriction. The remaining 31,027 individuals (two-thirds of the initial sample) are then eligible for the matching procedure described in Section 4.3.

B.3 Variable Construction

B.3.1 Health History

Mental health diagnoses are those belonging to the mental, behavioral, or neurodevelopmental category of diagnoses (ICD-9 codes 290–319 and ICD-10 codes F01–F99). Furthermore, I identify an individual as having a specific diagnosis (e.g., depressive disorder) if any of the ICD-9 or ICD-10 diagnosis codes in his claims match the corresponding diagnosis codes for that disorder in the Diagnostic and Statistical Manual of Mental Disorders (Fifth Edition). I categorize mental health disorders into the following twelve categories: ADHD, depression, anxiety, adjustment, conduct, oppositional defiant, bipolar, intellectual disability, other neurodevelopmental, substance abuse, PTSD, and other unclassified disorders. "Other neurodevelopmental" refers to disorders in the neurodevelopmental disorders category that are neither intellectual disabilities nor ADHD.

[43] I focus on men born in these four cohorts because subsequent cohorts were impacted by the introduction of an automatic enrollment program for Medicaid. South Carolina implemented the Express Lane Eligibility (ELE) program in 2011, resulting in substantial increases in Medicaid enrollment during adolescence starting with the 1994 cohort. The increase in enrollment prompted by ELE would reduce the number of men eligible for the low-enrollment group (i.e., the comparison group in the difference-in-differences approach), thereby worsening the match rate and allowing fewer individuals to have disproportionate impact on the estimated counterfactual outcome paths. Perhaps more importantly, this increase in enrollment would also alter the composition of eligible high- and low-enrollment men. The low-enrollment group includes 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 part of the high-enrollment group. The remaining low-enrollment men would therefore have relatively higher incomes and would be worse candidates for estimating counterfactual outcome paths.

"Other unclassified disorder" refers to having a mental health diagnosis not captured by the previous eleven categories. Finally, drugs in the pharmacy claims are classified as mental health medications if the therapeutic class or active ingredient corresponds to antianxiety, antidepressant, antipsychotic (including mood stabilizer), or ADHD medications. The last category includes both stimulant and non-stimulant cognitive-enhancing medications. I then classify individuals as having a mental health history if they file a claim with a mental health diagnosis or for a mental health medication between the ages of 10 and 15 (inclusive).

Given the focus on ages 10–15 to identify adolescents with mental health histories, it is reasonable to wonder whether most diagnosing of mental illness tends to occur in this age range. Of males who are enrolled in Medicaid and receive a mental health diagnosis prior to their 19th birthdays, 87% receive a diagnosis between the ages of 10–15. More specifically, 46% received a diagnosis before age 11 and two-thirds received a diagnosis before age 13, implying that the diagnosing of mental illness occurs quite early in this population.[44] The most common first diagnoses are ADHD and neurodevelopmental disorders, but many individuals then receive another diagnosis later in adolescence. Among male adolescents who are enrolled in Medicaid and receive a diagnosis prior to age 19, 62% have more than one diagnosis based on the twelve categories listed above (with the average and median number of diagnoses being 2.5 and 2, respectively). These statistics thus suggest that despite the differing age of onset of different disorders (see, e.g., Kessler et al. 2007), most individuals with mental illness during adolescence will be identified as having a mental health history using the 10–15 age range.

Furthermore, I identify an individual as having a physical condition if he has ever been diagnosed with asthma, diabetes, infantile cerebral palsy, hypertension, and other central nervous system disorders (e.g., epilepsy). Finally, I identify foster care and SSI youth using the payment categories listed in the Medicaid recipient file.

B.3.2 Criminal Involvement

I use both the South Carolina Law Enforcement Division (SLED) as well as the Department of Corrections (DOC) files to measure incarceration. In particular, the SLED records include information on all arrests, including the subset of more serious arrests that culminate with an individual being detained in an adult correctional facility (e.g., a local jail). The DOC records

[44] Note that I only see data starting at age 10, so if anything, these statistics overstate the age of the first diagnosis.

track spells in state prisons. The outcome of interest—which measures an individual's likelihood of incarceration—thus combines information from both agencies and measures the likelihood that an individual is detained in any adult correctional facility. The main difference between these two sets of records is that the SLED data only record when an individual is arrested and detained, but they do not provide information on the length of the incarceration spell. Hence, instead of relying on an outcome variable measuring whether an individual is incarcerated at any point in time (which would suffer from measurement error), the preferred outcome variable throughout the paper is one that measures whether an individual has *ever* been incarcerated.

In order to classify SLED arrests into offense types, I use offense codes and information from the disposition. In order to classify DOC prison spells into offense types, I use information on the most serious offense committed. Violent offenses include murder, robbery, assault, sex offenses (excluding sex offender registry violations), as well as any other violent offenses (e.g., trafficking persons, taking hostages). Property offenses include burglary, larceny, and arson. Drug offenses are those related to the possession, distribution, or manufacturing of drugs or alcohol (including DUIs). I then split drug-related offenses into those related to possession and those related to distribution. Financially-related offenses refer to robbery, burglary, larceny, and drug distribution as well as forgery, fraud, theft (e.g., card theft), blackmail/extortion, selling products (e.g., weapons, stolen vehicles), and prostitution. Non-financial violent offenses resemble violent offenses, except that they include weapons offenses and exclude robbery.

B.3.3 Other Characteristics

Information on an individual's race and sex come from the Department of Education records. I also use these administrative records to identify a person's main district and school. District refers to the modal school district attended between the ages of 15 and 18. If there is no modal district, I use the last school district attended before age 19. School refers to the last school attended before age 19. Finally, when considering academic achievement, I standardize math and English Language Arts (ELA) test scores at the grade, year, and test-type level to have mean zero and standard deviation one.

Table B1: Sample Restrictions

Sample	Sample size
Lower-income male residents in 1990–1993 cohorts	46,990
+ Ever enrolled in Medicaid	33,252
+ Enrolled in Medicaid, ages 10–18	31,533
+ In state records, ages 15–18	31,354
+ Alive during assignment period	31,296
+ Not incarcerated during assignment period	31,027
High-enrollment group	21,418
Low-enrollment group	8,911

NOTE: "In state records" refers to the individual being present in any administrative records (from any of the six state agencies) between the ages of 15–18 (inclusive). "Assignment period" refers to the year during which individuals are aged 16.5–17.5 (see Figure 2). The last two rows show the number of high- and low-enrollment men after implementing the matching procedure described in Section 4.

C Cost-Benefit Analysis: Providing Medicaid Eligibility

In this appendix, I calculate and discuss the costs and benefits of expanding Medicaid eligibility for two extra years. For both costs and benefits, I construct upper and lower bounds corresponding to different assumptions.

Throughout this analysis, I focus on the costs and benefits for one cohort and only consider the poorest half of South Carolina's residents who are enrolled in Medicaid at some point in adolescence (i.e., the sample of this paper), assuming that the higher-income half of the state is unaffected by changes in Medicaid eligibility. Moreover, 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.[45] Throughout this exercise, I also assume that Medicaid only has crime-related benefits; to the extent that expanding Medicaid eligibility impacts individual's outcomes beyond their criminal involvement, then if anything, I would be understating the benefits of this policy.

C.1 Increased Costs of Expanding Eligibility

I begin by calculating the number of individuals in a cohort that would likely take-up the program if eligibility were expanded. I linearly interpolate the enrollment patterns at ages 19 and 20 using the share enrolled in each quarter prior to age 19 (i.e., the shares in Figure 3). I find that roughly 42% of the cohort would take up Medicaid at age 19 and 35% 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 using Young et al. 2015), I find that that the total cost of providing this group of low-income young men with Medicaid totals $12.8 million.[46] The estimates from this and the next subsection are summarized in Table C2.

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 thus assume that none of these individuals would suddenly change from private insurance to public insurance simply as a result of expanding eligibility without also changing eligibility thresholds.

[45] As an example, prior research has shown that expanded access to family planning services through Medicaid have reduced teen birth rates (Kearney & Levine 2015).

[46] In both this appendix 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.

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 (44% 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 $18.1 million.

Finally, is is worth mentioning that in this exercise, I consider the costs of expanding eligibility to all low-income men. Nevertheless, alternative policies could be considered, such as expanding eligibility only to individuals who have been diagnosed with a mental health disorder or implementing a limited benefit program that allows individuals to keep accessing behavioral health services (but are not eligible for full insurance coverage).[47] These more targeted policies would likely be less expensive than extending Medicaid eligibility to full cohorts of low-income men, although identifying and verifying the desired group of beneficiaries would likely impose its own costs.

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 5 to calculate the number of violent, property, and drug-related crimes that occurred as a result of the Medicaid loss.[48] I then use the average 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, respectively), and assume that the ratio for drug offenses is the same as that for property crimes.

I then calculate the cost of the average violent, property, and drug-related incident (similar to the approach in Mello 2019) and summarize the estimates in Table C1. 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 correspond to 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,

[47] Similar policies already exist in South Carolina as well as in other states for family planning-related services.

[48] I calculate these numbers by multiplying the average number of people in one cohort (2,967) $\times$ the estimated coefficients in Table 5 (divided by 100) $\times$ 8 quarters.

property, and drug offense.[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 (similar to Heller et al. 2017). For drug-related offenses, the upper-end estimate assigns 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 these offenses are typically "victimless."[50] For both violent and drug offenses, the moderately conservative estimate is an average of the upper and lower bounds.

Putting all of these components together, I conclude that the total social cost of crime that was averted ranges from $13.1 to $21.2 million.

C.2.2 Lower Fiscal Costs from Fewer Incarcerations

I then consider the reduced fiscal costs from fewer incarcerations. For the analysis in this subsection, I assume that local jails have no operating costs 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 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 SLED and DOC data, I estimate that 65, 55, 41, and 44 percent of individuals who committed violent, property, drug, and miscellaneous offenses are sentenced to a prison spell, respectively.[51] For each offense, I then multiply the number of incarcerated individuals 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 offense type: the average time served for violent, property, drug, and miscellaneous crimes is 4 years, 1.9 years, 2.2 years, and 2.3 years, respectively (Pew Center on the States 2012).

[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.

[51] In particular, I calculate the share of individuals who were arrested for a specific type of offense and who have a documented prison spell within a year (assuming that the transition between arrest and incarceration is not immediate). In these calculations—unlike in the previous section estimating the social cost of crime—I include individuals who are incarcerated for miscellaneous offenses.

[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`.

Overall, I find that the fiscal cost would have been reduced by $3.4 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 $2.6 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 public assistance) of around $34,650 per person and crime impacts (in terms of post-release criminal behavior) between $14,983 and $33,297.[53] Multiplying these costs by the number of individuals in each cohort that would have been sentenced to serve a state prison spell after losing Medicaid eligibility implies a total cost between $4.0 and $5.5 million.

For the remaining individuals who were detained 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 $2.0 to $2.7 million, which puts the overall total social costs between $6.1 and $8.2 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 calculate this policy's marginal value of public funds (MVPF) (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 upper and lower bounds for this ratio as well as a preferred middle-ground estimate based on various assumptions. Estimates are summarized in Table C3.

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. Given the age range

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

and socioeconomic status of this population, it seems reasonable to think that this policy's MVPF would likely fall between these two categories.

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 and which includes three main components.

The primary component in this numerator is society's willingness to pay for fewer criminal victimizations v. I begin by using the estimates of the social cost of crimes averted, which are discussed above and shown in Table C1, but I make two additional adjustments to these calculations. First, one important consideration is the extent to which extending Medicaid eligibility prevents crimes altogether, or whether they are simply delayed by two years until eligibility expires. When constructing the lower- and middle-ground estimates of the MVPF, I assume the policy is able to prevent half of the incidents, whereas the other half still occur, but just two years later.[54] Second, I incorporate the role of recidivism in these calculations. In other words, I include the second round of criminal victimizations that would be averted from men not recidivating.[55] Overall, the willingness to pay for fewer victimizations ranges from \$8.7 to \$26.4 million.

Second, 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% of the federal poverty level: 50% and \$11,950, respectively (Ruggles et al. 2020).[56] I then calculate the total foregone income of affected individuals *during* incarceration, allowing for heterogeneity by crime type (given the different average sentence lengths). For the middle- and

[54] I not only make this assumption when re-calculating the total social cost of crimes averted, but also when calculating the costs of recidivism, improved labor market prospects, and the fiscal costs of incarceration (given that all of these components rely on the number of individuals incarcerated in their calculations).

[55] Specifically, I assume that 30% of incarcerated individuals recidivate within three years. I then use the raw data to calculate the share of second arrests that end with a prison spell, and use this figure to calculate the implied number of arrests that would have occurred because of recidivism. 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.

[56] In this section, I do not use the estimates from Mueller-Smith (2015) quantifying the economic impact of incarceration because those figures combine the effects on earnings (which enter the numerator of the MVPF) and on public assistance (which enter the denominator).

upper-bound of the MVPF, I also consider 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.[57]

Finally, 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: roughly \$370 dollars (Peterson-KFF 2020).[58] In the lower-bound estimate, I conservatively assume that beneficiaries would only be willing to pay \$1 for this transfer. The middle-ground estimate takes an average of the two values. Last, the total value of the transfer γ also depends on who bears the cost of uncompensated care (Finkelstein et al. 2019). I therefore defer the final calculation of this estimate to the following subsection.

It is worth noting that these calculations ignore various other components including the insurance value of Medicaid beyond the transfer value, society's willingness to pay for improvements in health (beyond the effects on criminal activity), as well as individuals' willingness to pay to avoid being incarcerated (beyond improved labor market prospects).[59] Adding such features would raise the overall willingness 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% of Medicaid spending is a transfer to providers of uncompensated care for

[57] I also 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.

[58] 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).

[59] As an example, individuals may be willing to pay to avoid the trauma of solitary confinement, deterioration in health status, and high rates of violence within prisons (Western 2021).

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 bears the cost of uncompensated care when estimating the upper bound of the MVPF and that individuals bear this cost otherwise. Finally, I note that in the scenario in which individuals bear the cost of uncompensated care, 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 the lower-bound and middle-ground estimates.

In calculating the total cost to the government, I also factor in the reduced fiscal cost from fewer incarcerations, μ (see Section C.2.2). This calculation also incorporates the fiscal cost of a second round of incarcerations (i.e., from recidivism).[60]

Finally, improved labor market prospects for individuals translate to higher tax revenue for the government. Following the approach of Hendren & Sprung-Keyser (2020), I use a 20% tax rate and find that the government recoups between \$135 and \$328 thousand in tax revenue.[61]

[60] I assume that 30% of individuals recidivate 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

			Estimated Cost		
Offense	Percent	Ratio	Upper	Middle	Lower
Violent Crimes					
Murder	6.8	1.5	$4,837,696	—	$2,418,848
Sex Offenses	3.9	4.6	$141,976	—	$141,976
Robbery	31.8	5.9	$12,620	—	$12,620
Assault	57.5	4.1	$38,912	—	$38,912
Avg. Violent Crime			$142,286	$114,471	$86,656
Property Crimes					
Larceny	14.8	17.3	$473	—	$473
Burglary	72.5	15.9	$2,103	—	$2,103
MV Theft	12.7	6.7	$5,784	—	$5,784
Avg. Property Crime			$2,037	$2,037	$2,037
Drug Crimes					
DUI	2.9	—	$29,447	—	$4,074
All other	97.1	—	$0	—	$0
Avg. Drug Crime			$854	$486	$118
Total Social Cost			$21,185,402	$17,122,885	$13,060,368

NOTE: "Percent" refers to the share of each broad category that is classified as that particular sub-crime using the arrests that end with an individual being taken into custody in the SLED data. "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: Summary of Costs and Benefits Associated with Extending Medicaid Eligibility (In Millions of $2010)

	Estimated Cost		
	Most conservative	Moderately conservative	Least conservative
Costs			
Medicaid costs	$18.1	$14.6	$12.8
Benefits			
Victimization costs	$13.1	$17.1	$21.2
Fiscal costs	$2.6	$3.0	$3.4
Social costs	$6.1	$7.2	$8.2
Total	$21.7	$27.3	$32.8

NOTE: This table reports the calculations from the cost-benefit analysis in Section 9. "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 C3: Marginal Value of Public Funds (MVPF), Upper and Lower Bounds

	Estimated Cost		
	Upper	Middle	Lower
Willingness to Pay:			
Fewer crime victimizations, v	26,401,476	11,405,477	8,711,337
Improved labor market prospects, η	1,639,416	889,267	673,829
Value of insurance transfer, γ	2,665,765	10,086,578	8,757,327
Aggregate willingness to pay	30,706,657	22,381,322	18,142,493
Costs to the Government:			
Cost of providing Medicaid, function of G	5,833,376	14,583,441	14,583,441
Fewer incarcerations, μ	–4,191,245	–2,207,411	–1,752,917
Foregone tax revenue, 0.2η	–327,883	–177,853	–134,766
Net Cost	1,314,248	12,198,176	12,695,758
Marginal Value of Public Funds	23.36	1.83	1.43

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). "Middle" refers to the middle-ground, preferred estimate using moderately conservative assumptions. The upper-bound estimate assumes all crimes avoided at ages 19 and 20 are averted altogether. The middle and lower estimates assume that only half of crimes are averted, while the remaining half are delayed. The upper-bound assumes that the government bears the cost of uncompensated care, while the other estimates assume that individuals bear the cost.

D Deterrence Effect of Harsher Sanctions Around Age of Criminal Majority

In this appendix, I study the criminal propensity of individuals around the age of criminal majority. The goal is to exploit the fact that the average sentence length discontinuously changes when adolescents transition from the juvenile to the adult justice system on their 17th birthdays. Studying criminal behavior around this age will thus allow me to calculate the elasticity of crime with respect to sentence lengths for this sample of low-income adolescents, which I can then use to compare the cost of Medicaid provision to that of harsher punishments.

I begin by discussing the sample and variable construction for this analysis. I then discuss the main results from this exercise and associated robustness checks. Next, I calculate the increase in average sentence lengths that occurs at age 17 in South Carolina, and use these estimates to calculate the elasticity of crime with respect to sentence lengths.

D.1 Sample and Variable Construction

I first restrict the sample of men 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 (DJJ) data. I classify offenses as felonies using a variable that indicates whether the referral was for a felony. Because I do not have exact information on an individual's birthday, I will mistakenly label certain referrals as occurring at age 17 when they actually occurred at age 16 (i.e., offenses that occurred during an individual's 17th-birthday month when the individual was still 16). To address the

[62] To illustrate this logic, consider the case of assaults: it appears that juveniles are much more likely to be referred to the Department of Juvenile Justice for assaults than to be charged as adults for this crime. If I included all assaults in this analysis, it would appear as though harsher sanctions had a large deterrence effect, when instead there is likely significant discretion on whether to charge individuals for certain offenses before and after age 17. Focusing on felonies reduces the likelihood that the estimated discontinuity will be a function of changes in discretion.

potential misclassification in ages, I shift all non-technical referrals that occurred at age 17–month 1 to age 16–month 12.[63] Correspondingly, I then (randomly) shift half of non-technical referrals occurring during any month t to month $t-1$.

Next, I classify arrests in the South Carolina Law Enforcement Division (SLED) data as felonies using the South Carolina's Judicial Branch CDR codes.[64] For the 10% of arrests that do not have available charge information, I use the offense information from the disposition of the arrest to classify that offense as a felony.

Finally, 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. Throughout the analysis, I exclude the 10% of individuals who committed a felony prior to age 16.

There are numerous advantages to using this sample of low-income adolescents to estimate the deterrence effect of harsher sanctions. 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, adolescents are selected into the sample based on their public school enrollment, as opposed to their past criminal behavior. Finally, I can restrict the sample to individuals who have not yet committed a felony offense, so that the estimates are less likely to be confounded by determinants of recidivism.

D.2 Empirical Approach for Estimating Deterrence Effect of Sanctions

To estimate the deterrence effect of harsher sanctions, I combine the juvenile and adult arrest records and compare men's likelihood of committing a felony before and after their 17th birthdays. 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. Specifically, 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 their first felony. I then summarize the hazard of a felony arrest in a given month and the corresponding discontinuity with the following

[63] Technical referrals refers to 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.

[64] For any offenses that were not able to be classified using the CDR codes (e.g., retired or missing codes), I manually classified the charge or disposition using the CDR codes for guidance.

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. In Section D.5, I discuss this potential confounder and provide evidence against the notion that the minimum dropout age is confounding the estimate of θ.

D.3 No Evidence of Deterrence

Figure D1 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 not yet been arrested for a felony. The solid lines plot predicted probabilities of arrest 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. These estimates suggest that *at most* there was a 13% decrease in the probability of felony arrest (after dividing the most negative marginal effect estimate by the age-16 average). Panel (b) splits the sample by an individual's mental health history.

To check the robustness of this result, I consider alternative ways of classifying offenses as felonies and the results are shown in Table D1. First, I use data on the arrest decision (i.e., decisions in the DJJ data and dispositions in the SLED data, rather than information on referrals and charges, respectively). Second, I classify an offense as a felony if *any* of the associated referrals, charges, decisions, or dispositions were felonies. In both of these checks, the estimate of θ, which represents the deterrence effect of harsher sanctions, remains statistically insignificant.

As a final check, Appendix Figure D2 shows the likelihood of being arrested for a felony around age 17 using an analogous approach to the one outlined in Section 7.2.3, which uses the probability of arrest rather than hazard rates. The statistically insignificant estimate from this figure confirms that individuals are equally likely to be arrested for a felony offense upon reaching the age of criminal majority.

D.4 Change in Length of Sentences

In order to calculate the elasticity of crime with respect to sentence lengths, I need to calculate the change in average sentence lengths that occurs when an individual transitions from the juvenile to the adult justice system. First, because the DJJ data does not have information on sentence lengths, I use statistics from DJJ's Interactive Trend Reports.[65] 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 (DOC) files and find that the average 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 length for inmates admitted in 2015 (i.e., the earliest available year) is 4 years and 4 months. I therefore conclude that the average sentence length for adults is approximately 1,580 days, which represents a 1,546% increase in incarceration lengths at age 17. Furthermore, instead of only considering the average sentence length, I also consider the average sentence *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 (roughly 28 months or 840 days), which represents a 775% increase in incarceration length.

Using these calculations as well as the most conservative estimates of deterrence, I calculate the elasticity of crime with respect to sentence lengths for this sample of low-income adolescents. The estimates using average sentence length imply an elasticity of -0.008, which is very close to Lee & McCrary (2017)'s reduced-form elasticity. If I instead use the average time served, then the corresponding elasticity is -0.017.

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

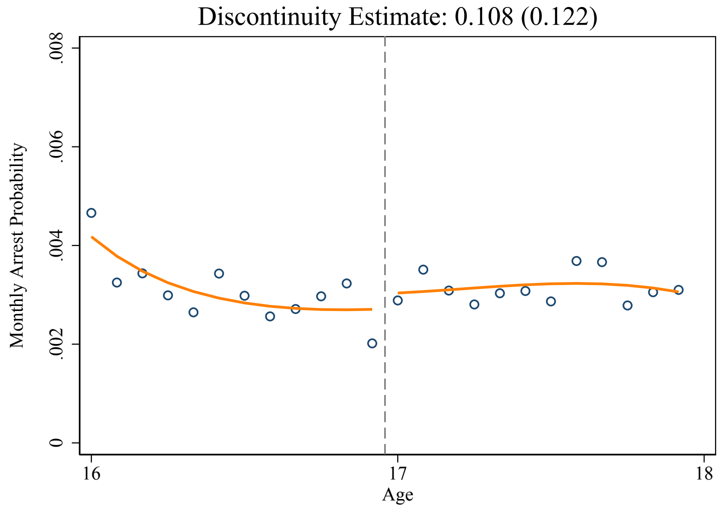
D.5 Potential Confounding Effect of Minimum Dropout Age

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 effect of the harsher punishments with the effect of adolescents dropping out of school upon reaching age 17. In other words, to the extent that schools have an "incapacitation" effect (i.e., they reduce the likelihood that teens commit offenses; see, e.g., Anderson 2014, Berthelon & Kruger 2011, Fischer & Argyle 2018), then one concern might be that the small and statistically insignificant estimates of deterrence at age 17 are obfuscating the negative effect of harsher criminal sanctions on crime and the positive effect of individuals dropping out of school on crime.

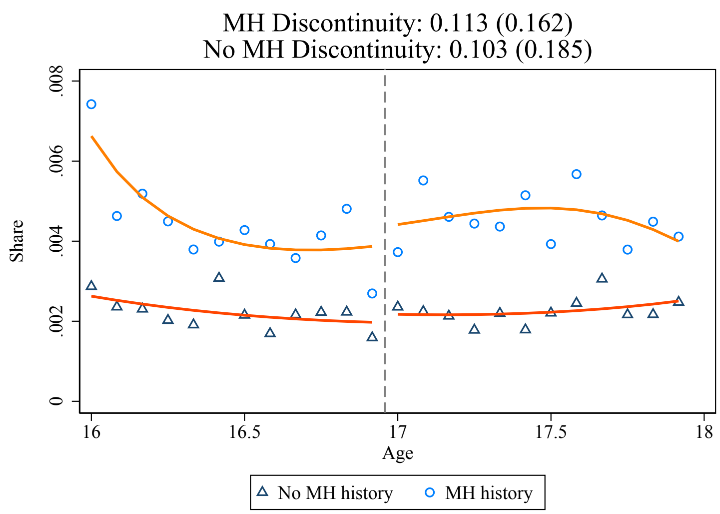
I test for the presence of schools' incapacitation effect in this setting. Specifically, I estimate men's likelihood of being arrested for a felony during each month of the calendar year. Appendix Figure D3 displays the graphical results and Appendix Table D2 summarizes the estimates, showing that adolescents 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.

Figure D1: Felony Propensity Estimates Around the Age of Criminal Majority

(a) Full Sample



(b) By Mental Health History



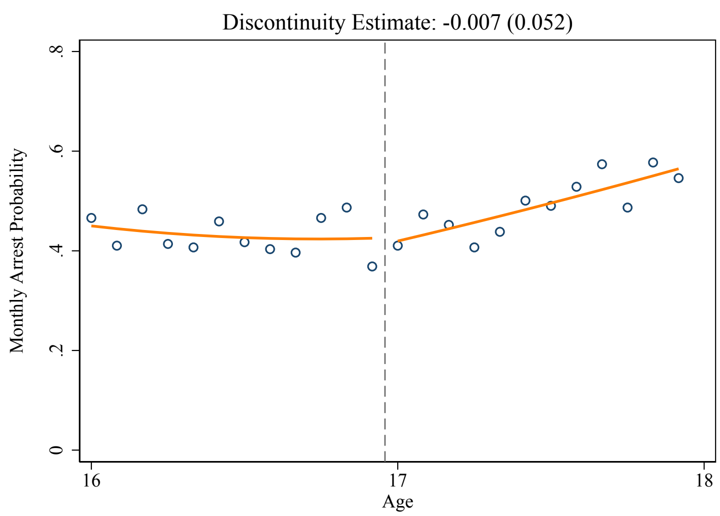
NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. The sample consists of men born between 1990–1993 who had not been arrested for a felony prior to age 16. The bottom panel splits the sample based on an individual's mental health history 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 estimates reported above each figure correspond to the discontinuity estimates from this equation and their standard errors (clustering at the individual level).

Table D1: Estimates of Felony Propensity Around Age 17, Using Alternative Ways of Classifying Felonies

	Using charges	Using decisions	Using all information
Discontinuity at 17	0.108	0.105	0.173
	[0.122]	[0.122]	[0.121]
Observations	665,728	672,764	665,492

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The estimates in this table are calculated using equation (6) and represent the discontinuous change in log-odds of committing a felony offense upon reaching age 17. The first column uses the baseline method for classifying offenses as felonies (i.e., using referrals and charges from the DJJ and SLED data, respectively). The second column uses information from the DJJ decision and SLED disposition to classify offenses as felonies. The third column classifies an offense as a felony if any of the referral, charge, decision, or disposition codes were felonies. Standard errors are clustered at the individual level.

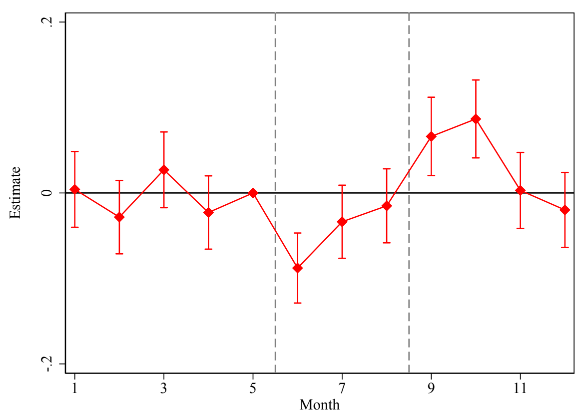
Figure D2: Share of Individuals Arrested for a Felony Around the Age of Criminal Majority



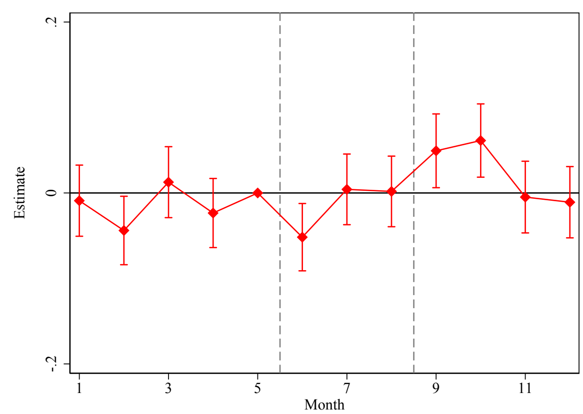
NOTE: These figures plot the monthly estimates of the likelihood of a felony arrest around an individual's 17th birthday. The figures consider all men born between 1990–1993 who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of all individuals. 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 D3: Likelihood of Felony Arrest or Death for Males Aged 12–16, by Month

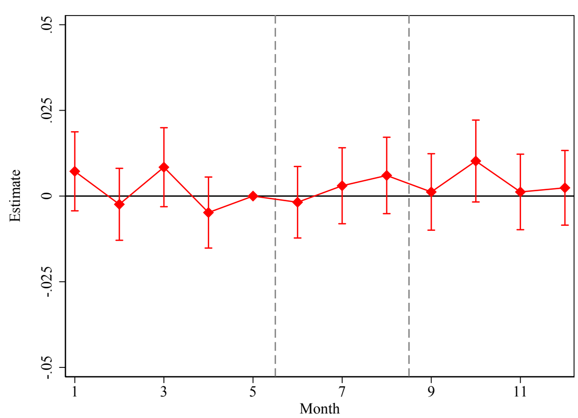
(a) All felonies



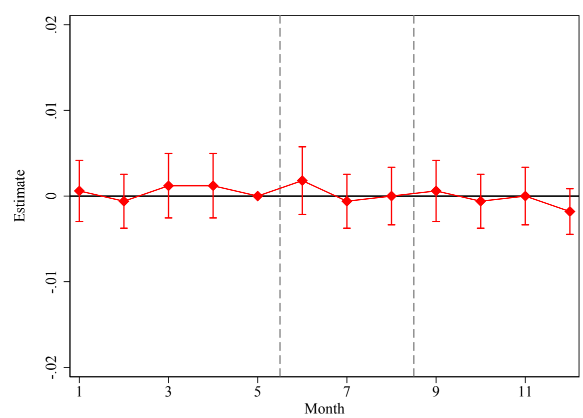
(b) Non-school-related felonies



(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 male adolescents in the 1990–1993 cohorts and the data is a balanced panel 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 despite these men 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 given outcome on month fixed effects (omitting the month of May), age fixed effects, and individual fixed effects. Standard errors are clustered at the individual level.

Table D2: 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	-0.058***	-0.019*	-0.000	0.000
	[0.010]	[0.010]	[0.003]	[0.001]
Observations	1,995,120	1,995,120	1,995,120	1,995,120

NOTE: This table 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 male adolescents in the 1990–1993 cohorts and the data is a balanced panel at the individual $\times$ age $\times$ 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 despite these men 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.

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

In this appendix, I compare the costs of three potential approaches for reducing crime: providing low-income young adults with access to Medicaid, increasing sentence lengths, and hiring more police officers. In particular, I consider the cost of each policy for reducing crime by 15%.[66]

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 of $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 and still commit crimes. In other words, even if some offenders were deterred from committing offenses, there would still be roughly 700 serious arrests per cohort. Similar 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. I then use the raw data to calculate the share of serious arrests that have a corresponding state prison spell. I also 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 totals $15.5 million.

Importantly, 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.[67] To calculate the fiscal cost, I assume that 30% 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).[68] The fiscal cost of this second round of incarcerations totals $3.7 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 overall serious arrests that would occur. I then use the share of serious arrests

[66] The estimate in column 1 of Table 5 suggests that among men born in these cohorts, there were roughly 489 excess serious arrests (11,866 $\times$ 0.00515 $\times$ 8 quarters). I then use the raw data to calculate that there were 3,282 serious arrests among 19- and 20-year old men born in these cohorts, which implies that serious crime would have been 15% lower in this age group if Medicaid eligibility had not suddenly expired at age 19.

[67] 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 criminal behavior declines with age.

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

by offense type and the victimization-to-arrest ratios to calculate the implied number of incidents that would occur. I use the upper-bound (i.e., the least conservative) estimate for violent crimes from Table C1 as well as the middle-ground estimates for property and drug offenses to estimate the total social costs of this second round of victimizations: \$23.1 million.

Summing these components together, I find that the total cost of this approach is roughly \$56.9 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 Comparison to 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 ($\varepsilon_{c,f} = -0.017$ using the average sentence served) to estimate the degree to which sentence lengths would need to be extended for 19- and 20-year-old men in order to achieve the same reduction in crime as extending Medicaid eligibility. I find that sentences would need to be 890% 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. I follow the same approach outlined 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 \$104.5 million, which is almost twice the cost of Medicaid provision.

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 -0.044, which is more than 2.5 times as large as the preferred elasticity.

E.2 Comparison to 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). In this subsection, I consider the cost of reducing crime 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): \$30.1 million.[69]

Similar to the analysis investigating the cost of longer 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 the same percent reduction in crime as Medicaid provision. Using the elasticity of crime to police officers from Evans & Owens 2007 ($\varepsilon_{c,p} = -0.34$), I find that the state would need to increase the overall size of their police force by around 45%, which implies hiring roughly 5,161 more police officers.[70,71] Assuming a marginal cost of \$130,000 for hiring a police officer (Chalfin & McCrary 2018), the fiscal cost of this policy amounts to \$1.3 billion. I sum this cost to the cost of incarcerating individuals who were not deterred (discussed in Section E.1).

However, hiring police officers not only reduces the criminal activity of 19- and 20-year-old men, 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 17–40, 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 each age group. 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. Next, I 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.[72] 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 and property victimizations by \$1.4 billion and \$23.5 million, respectively. Finally, there is an additional reduction in fiscal costs of \$30.0 million from fewer individuals being incarcerated (after multiplying the number of individuals in other age groups who would have been incarcerated by the daily inmate cost and average sentence served).

[69] 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.

[70] 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.

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

[72] 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.

These results suggest that the benefits of hiring more police officers outweigh the costs. However, I note that these results are sensitive to the cost assigned to violent crime—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 for individuals ages 17–40. 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 $866.5 million, which would favor Medicaid provision over hiring more police officers. Table E1 summarizes how the costs change as I alter certain assumptions, including lowering the marginal cost of hiring police officers.

Overall, these calculations 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 (e.g., the costs of police brutality, excessive force, or over-policing; see, e.g., Ang 2021). Second, the calculations ignore the non-crime-related benefits that health insurance might provide to individuals (e.g., in terms of financial stability or earnings and tax revenue; Gallagher et al. 2019, Gross & Notowidigdo 2011, Hu et al. 2018). Consequently, even though I am making a relatively parallel comparison for these policies—estimating the cost of a 15% crime reduction—it is likely the case that these calculations are underestimating the benefits of providing low-income young adults with access to Medicaid.

E.3 Comparison to Medicaid Expansions

As noted above, the sudden termination in Medicaid eligibility results in 15% more serious crimes among low-income young men. A natural question is how the size of this effect compares to magnitudes in related studies quantifying the effect of Medicaid expansions on public safety. First, it is worth noting that this comparison is not straightforward, as this study relies on arrest and incarceration records of young adults, whereas the latter group of studies rely on *all* incident reports to the police (which include crimes committed by individuals of all ages and most of which do not culminate with an arrest or incarceration spell). Nevertheless, it is still informative to translate the findings of this study in order to compare the effect of losing Medicaid eligibility with expanding Medicaid coverage.

I begin by using the number of arrests among 19–20 year-old men and find that the arrest rate for this group fell from 4.9% to 4.2% (i.e., a 15% decline in arrest rates).[73] Next, to back out the

[73] As noted above, there were 3,282 serious arrests among 19- and 20-year old men born in these cohorts, 33,252 total men (see Table 1), and 498 excess serious arrests. I multiply the population by two to account for the fact that I

arrest rate of all other individuals (non-19–20-year-old men), I utilize the fact that men ages 19–20 comprise 4% of all violent and property crime arrestees in the United States, but only around 1.2% of the population.[74] I combine these shares with the number of arrests of men ages 19–20 and the number of individuals in these cohorts to calculate the arrest rate of the rest of the population: roughly 1.5%. Finally, I use the arrest and population shares to conclude that the overall arrest rate would change from 1.55% to 1.54%, implying a 0.59% change in the arrest rate if Medicaid eligibility was not suddenly terminated.[75]

Assuming that changes in arrest rates are relatively good approximations of changes in crime rates (i.e., the likelihood of arrest conditional on an offense is unchanged), then this back-of-the-envelope calculation implies that overall crime rates would have been 0.6% lower if the loss in eligibility had not occurred. He & Barkowski (2020) and Vogler (2020) find that Medicaid expansions decreased violent crime rates by roughly 5% and also decreased the incidence of certain types of property crimes. The difference in magnitudes is unsurprising given that Medicaid expansions affect individuals of all ages, including those who have never offended as well as former inmates, whereas this exercise assumes that the lapse in eligibility *only* affects low-income men ages 19–20.

only see the lower-income half of the state in this sample, and I assume that the higher-income half is not arrested for serious offenses. Indeed, among serious arrests in the full sample, 92% were for individuals ever enrolled in the Medicaid program, confirming that low-income individuals are disproportionately more likely to come into contact with the criminal justice system.

[74] Arrest data are collected through the Uniform Crime Reporting program and available through the FBI's official website (Tables 38–39). Population data comes from the American Community Survey and estimates are made available via KFF's State Health Facts on Demographics and the Economy.

[75] The same back-of-the-envelope calculation just focusing on violent, not all serious, arrests yields a similar result (0.62%) for the change in the arrest rate.

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

	Lower-bound cost of violent crime	Upper-bound cost of violent crime
(a) Marginal cost: \$130,000		
Ages 17–40	\$455.3	−\$76.8
Ages 17–30	\$866.5	\$582.2
(b) Marginal cost: \$73,000		
Ages 17–40	−\$124.5	−\$656.6
Ages 17–30	\$286.7	\$2.5

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, from Chalfin & McCrary (2018) and 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.