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

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Abstract

Individuals with mental illness are over-represented in the incarcerated population. This paper studies the relationship between mental illness and criminal offending using administrative data from South Carolina that links health claims to criminal records. I leverage a discrete break in Medicaid eligibility at age 19, which results in a sudden termination of healthcare coverage, and importantly, a sharp reduction in access to mental healthcare for young adults with mental illness. Using a triple-differences strategy, I find that men with mental health histories who lose access to care are significantly more likely to be incarcerated following the termination of eligibility. Cost-benefit analyses show that expanding mental healthcare access to low-income young men is a cost-effective policy for reducing crime.

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1 Introduction

Policymakers and academics have long debated the root determinants of criminal behavior. In the 1960s, one dominant strain of thought argued that criminal behavior stems from mental illness, prompting 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, policymakers in the U.S. embraced the notion that raising the cost of crime would deter offenders (Donohue, 2007), and increased the severity of punishments at both the state and federal level. These policies contributed to rapid growth in the incarcerated population (Raphael and Stoll, 2013b), and incarceration is now a common occurrence for low-income and minority men (Finlay et al., 2023). However, the high economic and social costs of enforcement policies have called into question the cost effectiveness of the criminal justice system[1] and have prompted policymakers to consider alternative strategies for lowering criminal activity and criminal justice expenditures. Indeed, understanding the causes of crime and developing an efficient portfolio of crime-reduction approaches remains a key priority for governments given the significant costs that crime imposes on individuals and communities (Bindler et al., 2020).

This paper investigates the effect of mental health on criminal behavior and studies the degree to which mental healthcare reduces criminal offending. One motivation for returning to the decades-old conversation surrounding mental illness and crime is the disproportionate representation of mentally ill individuals in today's criminal justice population. In the U.S., 37% of prison inmates and 44% of jail inmates have been diagnosed with a mental disorder prior to incarceration.[2] This strong correlation between mental health and crime raises the question of whether these two outcomes are causally linked, and if so, whether mental healthcare can play a role in reducing criminal activity.

[1] See Chalfin and McCrary (2017) for a review of economics research studying the effect of police and sanctions on crime. Recent work has documented the adverse effects of incarceration (e.g., Dobbie et al., 2018; Garin et al., 2023) and policing (e.g., Ang, 2021; Tebes and Fagan, 2022). Ludwig (2006) discusses associated fiscal costs.

[2] See Bronson and Berzofsky (2017). For context, roughly one in five American adults are afflicted with a mental illness (National Institute of Mental Health, 2023).

Given the lack of experimental variation in mental health, one approach for studying these questions is to consider whether changes in access to mental healthcare, which implicitly affect an individual's mental health, impact criminal behavior.[3] However, there are two empirical challenges to quantifying the impact of mental healthcare on the criminal propensity of individuals with mental illness. First, datasets linking health claims and law enforcement records are relatively rare, making it difficult to identify individuals diagnosed with mental illness and measure their contact with the criminal justice system. Second, there is limited quasi-experimental variation in access to mental healthcare.

To address the data challenge, I obtained access to individual-level administrative data linking public health insurance (Medicaid) claims to criminal records in the state of South Carolina. To leverage quasi-experimental variation, I study a discrete break in access to healthcare that occurs when individuals age out of Medicaid eligibility. South Carolina 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. Crucially, Medicaid is the largest provider of mental healthcare in the United States, so this break in coverage represents a sudden reduction in access to mental healthcare for men with mental illness.

To estimate causal effects, I use the detailed information in the Medicaid claims data to compare the outcomes of enrollees with mental health histories to those of enrollees *without* mental health histories, before and after their 19th birthdays. This comparison allows me to study whether losing access to *mental* healthcare impacts criminal behavior. Importantly, because these two groups may have different criminal trajectories that are unrelated to the loss of mental healthcare, I incorporate a third difference into the research design. I consider men with and without mental health histories who were enrolled in Medicaid earlier in adolescence, but who were not affected by the loss of eligibility (non-enrollees). Including this group in the empirical strategy allows me to account for natural differences in criminal trajectories between men with and without mental illness. The identification assumption in this triple-differences model is that in the absence of the loss of mental healthcare, the difference in crime outcomes between men with and without mental illness would have evolved

[3] If mental healthcare was not effective in treating mental illness, then studying access to mental healthcare might not shed any light on the relationship between mental health and crime. However, significant scientific progress has been made over the past fifty years in understanding and treating mental illness, including important advances in the development and availability of mental health medications and services (Frank and Glied, 2006; Kendler, 2019).

similarly for enrollees and non-enrollees. Any increase in incarceration that occurs right after enrollees with mental illness turn 19 can thus be attributed to the loss of access to mental healthcare.

I find that men with mental illness who lose access to mental healthcare are 0.9 percentage points (or 19%) more likely to be incarcerated in the year following the loss of eligibility. Importantly, I study the dynamic evolution of outcomes and show that enrollees with mental health histories begin to diverge in their incarceration propensity *precisely* after reaching their 19th birthdays. Further, the results suggest that losing access to mental healthcare in late adolescence increases men's likelihood of being incarcerated for the first time. By age 21, these men are 3 percentage points (or 21%) more likely to have ever been incarcerated. I also show that the increase in incarceration is particularly pronounced for men who filed mental health claims right before their 19th birthdays and for those who relied on Medicaid for access to mental health medications. Together, these results indicate a close link between changes in mental health and offending behavior.

Moreover, I show that men with mental illness are more likely to be incarcerated for violent and property crimes, implying that forgoing mental healthcare impacts serious criminal involvement. These results suggest that both a psychological stress channel as well as an economic channel seem to be the key mechanisms underlying the relationship between mental health and criminal offending.

To complement the triple-differences design, I implement a regression discontinuity approach in which I estimate men's arrest propensities around their 19th birthdays. Using this alternative strategy, I find that men with mental health histories are significantly more likely to be arrested upon turning 19. I find no comparable increase for men *without* mental health histories. Similar to the triple-difference results, the increase in arrest probability is larger for men filing mental health claims late in adolescence.

I validate these findings through additional exercises, which yield consistent results that losing access to mental healthcare increases criminal propensity. I show that the baseline increase in incarceration is robust to a variety of checks, including falsification checks around earlier ages and excluding a small group of men aging out of other programs (i.e., foster care and the Supplemental Security Income program) at age 18. Additionally, I estimate an alternative specification, a difference-in-differences model, in which I focus on enrollees with mental health histories and compare men who filed mental health claims soon before their 19th birthdays with men who did not file mental health claims right before the termination of coverage. In this new comparison, I also find a divergence in incarceration propensities precisely at age 19, and that men filing mental health claims are 15% more likely to be incarcerated in the year after losing eligibility.

In the final part of the paper, I put the causal estimates into context by considering the cost effectiveness of mental healthcare provision. First, I use back-of-the-envelope calculations to show that the benefits of providing low-income young men with mental healthcare via the Medicaid program—in terms of reduced fiscal and social costs from fewer incarcerations and reduced social costs from fewer victimizations—outweigh the program costs. The marginal value of public funds (MVPF), which reflects the value of Medicaid eligibility to recipients for every dollar spent by the government, is roughly 2. I also benchmark the cost effectiveness of mental healthcare provision to that of longer punishments and of hiring police officers, both of which have been favored crime-reduction policies for the past fifty years. The results from these exercises suggest that providing access to mental healthcare via the Medicaid program is relatively cost effective, and that policymakers might consider improving access to mental healthcare in order to reduce crime and lower criminal justice expenditures.

The primary contribution of this paper is providing novel evidence that mental health affects criminal behavior and that access to mental healthcare mitigates this relationship. This study adds to a relatively small literature investigating the role of mental health services in affecting criminality.[4] Prior work has estimated the effect of mental health drug prescriptions (Marcotte and Markowitz, 2011) as well as openings/closings of facilities (Bondurant et al., 2018; Deza et al., 2022) on local crime rates. Though not specifically about mental illness, Heller et al. (2017) and Blattman et al. (2017) show that interventions that included a cognitive behavioral therapy component—which fosters noncognitive skills like slowing down and being patient—can curb the offending behavior of crime-prone young men. I advance this literature in two ways. First, I leverage exogenous variation in access to mental healthcare at the individual level; I therefore do not rely on individual choices or local variation that may be correlated with other individual- or local-level changes, respectively. Second, unlike prior work, the richness of the administrative data allows me to hone in on men with health histories to study how access to mental healthcare tempers the relationship between

[4] Seminal work in medicine and sociology has quantified the correlation between mental illness and crime (Swanson et al., 1990; Teplin et al., 2002). A related literature in economics considers the relationship between mental health and non-criminal outcomes; see, e.g., Currie and Stabile (2006) and Busch et al. (2014) on human capital and Biasi et al. (2021) on labor market outcomes.

mental illness and criminality. These findings complement concurrent work showing that referring offenders to substance use treatment (Arora and Bencsik, 2021) and connecting former inmates to mental health providers (Batistich et al., 2023) can reduce recidivism rates.

More broadly, this paper contributes to a large literature in the economics of crime studying the determinants of criminal behavior (Chalfin and McCrary, 2017; Doleac, 2023). While prior studies have shown that offenders respond to the enforcement regime and to labor market conditions, this paper argues that criminal involvement is also a function of mental health. The findings of this paper also underscore that mental health in late adolescence—during the peak of the age-crime profile—can influence the likelihood of committing serious crime for the *first* time.[5] Understanding the factors that contribute to a person's first incarceration spell may be especially relevant given the high rates of recidivism in the U.S. criminal justice system.

Finally, this paper adds to a recent literature studying the effects of health insurance provision on public safety, which finds that changes in access—for example, via the Affordable Care Act—impact local crime rates (Wen et al., 2017; Aslim et al., 2019; Fry et al., 2020; He and Barkowski, 2020; Vogler, 2020; Deza et al., 2024). An open question in this literature is the extent to which access to mental healthcare can explain these effects. The findings in this paper indicate that mental healthcare is a key channel underlying the relationship between health insurance and public safety. By highlighting the crime-reduction potential of mental healthcare provision via the Medicaid program, this study also complements recent work quantifying the effects of government assistance programs, like food stamps, on crime (Yang, 2017; Tuttle, 2019; Deshpande and Mueller-Smith, 2022).[6]

[5] Related work shows that lead exposure in early childhood, which is associated with cognitive impairments, increases violent crime in adolescence and adulthood (Reyes, 2007; Feigenbaum and Muller, 2016; Billings and Schnepel, 2018; Aizer and Currie, 2019).

[6] Given its focus on the Medicaid program, this paper also adds to the literature quantifying the social returns to public health insurance (Brown et al., 2020; Goodman-Bacon, 2021; Arenberg et al., 2024). These papers focus on historical expansions of Medicaid among children and document long-term benefits. In contrast, I emphasize the short-term returns of mental healthcare access among young adults.

2 Mental Health & Criminal Activity

2.1 Historical Background: Policy Responses to Crime in the U.S.

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 (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 posits 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 in the U.S. 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 that contributed to a nearly five-fold increase in the incarceration rate (Raphael and Stoll, 2013b; Neal and Rick, 2016; Pfaff, 2017). 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. Raphael and 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, the correlation between mental illness and criminal behavior is well-established (see Frank and McGuire, 2010, for a detailed review).[7] On any given day, over one million Americans with mental illness are in jail, prison, probation, or parole (Frank and McGuire, 2010). 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).[8]

The persistent relationship between mental illness and criminal activity raises the question of whether these outcomes are causally linked and whether improved access to mental healthcare 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, mental healthcare may not have been an effective way to reduce criminal behavior. However, in the intervening decades, 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, e.g., Frank and Glied, 2006; Hofmann et al., 2012; Kendler, 2019). Acknowledging this progress, this paper revisits the relationship between mental health, mental healthcare, and crime.

3 Data and Sample

This paper studies the effect of mental health on criminal behavior in the state of South Carolina. South Carolina has lower income levels than other states in the U.S. and it

[7] Even though a significant share of criminal offenders have mental health histories, it is not the case that most mentally ill individuals commit crimes (Glied and Frank, 2014).

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

also has low levels of health insurance coverage among non-elderly adults.[9]

The data source is administrative records from state agencies that are linked at the individual level. This dataset is 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.

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. Information on an individual's birth month is only available in Medicaid records, so I further restrict this sample to individuals who have ever been enrolled in the Medicaid program (71% of the low-income sample). For more details on the sample selection, sample restrictions, and variable construction, see Appendix B.

To measure an individual's healthcare coverage and mental health (MH), I use Medicaid enrollment and claims records from South Carolina's Department of Health and Human Services. These data include demographic characteristics, dates of enrollment spells, information from visits to doctors and hospitals, as well as pharmacy claims. The 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.

To measure crime-related outcomes in adulthood, I use records from the South Carolina Law Enforcement Division (SLED) and the Department of Corrections (DOC). Indi-

[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] 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.

viduals in South Carolina are legally treated as adults on their 17th birthdays. Data from SLED provide information on all arrests in the state, and they identify individuals who were arrested and subsequently detained in a correctional facility. Data from the DOC provide details on incarceration spells in state prisons. Among men who served an incarceration spell in state prison by age 21, 76% of them had been diagnosed with a mental health disorder during adolescence, once again highlighting the strong correlation between mental illness and criminal activity.

The main outcome of interest is men's likelihood of incarceration, which is a proxy for serious criminal offending. Incarceration peaks in prevalence in men's late teens and early twenties, and is particularly common in low-income communities (Freeman, 1999; Lofstrom and Raphael, 2016). To construct this measure, I combine SLED and DOC records, so that this measure reflects the likelihood that an individual is incarcerated in any adult correctional facility. Because SLED records identify individuals who were detained in a correctional facility upon being arrested, so these data allow me to capture serious offending as soon as it occurs.

I augment these sources with records from the Department of Juvenile Justice (DJJ), which contain information on all contact between adolescents and the juvenile justice system. Records from the Department of Education 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 enrollment information in the Supplemental Nutrition Assistance Program (SNAP) and in the Temporary Assistance for Needy Families (TANF) program. Finally, I use death certificate data from the Department of Health and Environmental Control.

4 Empirical Strategy

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 have limited access to Medicaid services.

The health records data only include Medicaid claims, so I cannot verify whether individuals who lose access to Medicaid become uninsured or transition to private insurance. However, it is likely that a high share of this low-income young population becomes uninsured. 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.

How does aging out of the Medicaid program affect these low-income young adults? Medicaid provides free health coverage to beneficiaries, so losing eligibility may reduce an individual's expected income. In particular, youth who lose access to Medicaid services may have to start paying out of pocket for medical care. Previous work has found that access to Medicaid reduces out-of-pocket medical spending and improves financial health (Gross and Notowidigdo, 2011; Hu et al., 2018).

For individuals with mental illness, losing Medicaid eligibility also means losing access to mental health treatments, medications, and other resources. Indeed, Medicaid is the largest payer for behavioral health services, covering both inpatient and outpatient services.[11] This break in eligibility therefore represents a sudden reduction in access to free mental healthcare for individuals with mental illness. Outside of the Medicaid program, mental healthcare is prohibitively expensive for many individuals, so the uninsured population typically forgoes care rather than pay for services themselves (Rowan et al., 2013; Walker et al., 2015; Ortega, 2023). Accordingly, the primary change that men with mental illness likely experience is not a reduction in income, but rather a relinquishing of mental healthcare. This policy thus creates a natural setting for studying the effect of changes in mental health on incarceration.

4.2 Research Design

The goal of this study is to quantify how a change in mental health—stemming from a change in access to mental healthcare—impacts criminal involvement. To fix ideas, suppose that there are two groups: men with and without mental illness. Their criminal propensity can be expressed as a function of age, income, and mental health, as follows:

$$\text{Crime}(t)^h = \text{Age}(t)^h + \text{Income}(t) + \text{MH}(t) \tag{1}$$

where $t \in \{18, 19\}$ represents ages before and after an individual's 19th birthday, respectively, and $h \in \{0, 1\}$ denotes whether an individual has a mental health history. Note that the

[11] In 2009, the program accounted for 26% of nationwide behavioral health spending. Behavioral health services are a significant component of 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).

effect of age on criminal offending depends on an individual's mental health h (see, e.g., Figure 1). The effect of income on crime is common across individuals. However, if men with mental illness have a different relationship between income and crime due to their mental illness, then this added component enters through the MH(t) term. For individuals without mental illness, the MH(t) term equals zero.

Losing Medicaid eligibility may impact offending both through changes in expected income and changes in mental health. To isolate the mental health effect, I use the granular information in the Medicaid claims data to split the sample into men with and without mental illness. The former group experiences changes in income and mental health, while the latter only experiences changes in income. Comparing the outcomes of these two groups around age 19, akin to a difference-in-differences framework, yields:

$$\begin{aligned}\Delta 1 &= \Big(\text{Crime}(19)^1 - \text{Crime}(18)^1\Big) - \Big(\text{Crime}(19)^0 - \text{Crime}(18)^0\Big) \\ &= \underbrace{\Big(\text{MH}(19) - \text{MH}(18)\Big)}_{\text{(1) Mental health effect}} + \underbrace{\Big(\text{Age}(19)^1 - \text{Age}(18)^1\Big) - \Big(\text{Age}(19)^0 - \text{Age}(18)^0\Big)}_{\text{(2) Different crime trajectories}} \qquad (2)\end{aligned}$$

The first term in this equation captures the change in the offending probability of men with mental illness because of forgone mental healthcare, which is the effect of interest in this study. The second term reflects differences in the criminal trajectories of men with and without mental illness that are unrelated to changes in mental health.

This expression makes clear that in order to estimate the impact of mental health on criminal offending, it is necessary to account for different age trajectories between men with and without mental illness. Hence, I incorporate a third difference into the research design, comparing men with and without mental health histories who were enrolled in Medicaid earlier in adolescence, but who were less affected by the loss of eligibility at age 19 (henceforth, non-enrollees). Because this group is unaffected by the break in eligibility, a comparison of their outcomes—again, akin to a difference-in-differences framework—captures the natural differences in criminal trajectories around age 19. Put differently, this third difference helps me estimate the second term in equation (2) and thus isolate the impact of mental health on criminal behavior.

Figure 2 summarizes the logic of this triple-differences approach. The triple-differences strategy relaxes the parallel trends assumption in difference-in-differences settings. Instead, it requires that the difference in the evolution of crime outcomes between enrollees with and without mental health histories be the same as the analogous difference among non-enrollees.

Importantly, the research design does not require that enrollees and non-enrollees have similar incarceration levels or trends, only that the differences between men with and without mental illness trend similarly across enrollment groups (i.e., parallel trends in differences).

A natural question in this setting is why some low-income adolescents were enrolled in Medicaid in late adolescence, while others were not. There is no information in the datasets to answer this question, but there are a few potential reasons. Non-enrollees might have experienced positive income shocks that made them ineligible for the program, or these men (or their families) decided that they no longer wanted or needed access to Medicaid services. Related to this last point, a likely explanation is that many non-enrollees were likely eligible for Medicaid, but were not enrolled because of administrative hurdles to enrolling in the program.[12] Edwards and 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 and costly administrative requirements (e.g., needing to provide wage information from employers).

However, regardless of the reason for not being enrolled, the key component for the research design is that non-enrolled men did *not* experience a sudden change in access to mental healthcare at age 19. I can thus use differences between men with and without mental health histories in this group to account for different criminal trajectories across mental health status. After presenting the main triple-differences results, I also present estimates from a complementary strategy, a regression discontinuity design, around individuals' 19th birthdays.

4.3 Implementation of Triple-Differences Framework

I identify individuals with mental illness as those who received a mental health claim before age 16. I then assign men into enrollment groups based on their enrollment in Medicaid between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$.[13] This approach allows me to follow the natural evolution

[12] Men who are eligible for, but not enrolled in the Medicaid program are likely uninsured. 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). Note that individuals in need of a hospitalization who are eligible for but not enrolled in the program are enrolled at the hospital (Aizer, 2003).

[13] I assign men into enrollment groups a year before the study pre-period to guarantee that the assignment is uncorrelated with their outcomes around age 19. 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 of eligibility, and not from contemporaneous income shocks. Further, incarcerated men do not have access to Medicaid

of outcomes of the four groups—by enrollment and mental health status—before and after their 19th birthdays. Specifically, I can examine incarceration propensities for a full year and a half prior to these men's 19th birthdays, and see if outcomes diverge *precisely* when enrollees with mental health histories lose access to mental healthcare. Appendix Figure A2 offers a graphical timeline of the approach.

The triple-differences design relies on non-enrollees being a suitable group for estimating the difference in criminal trajectories across mental health status among enrollees (i.e., the second term in equation (2)). I therefore implement a matching procedure, so that non-enrollees more closely resemble enrolled men. In practice, this process re-weights the group of non-enrollees, so that the two groups resemble each other with respect to birth cohort, race (Black or non-Black), school district, and mental health history. Specifically, I match each enrolled individual to all "counterfactual" non-enrollees based on this parsimonious set of characteristics. I then re-weight each matched non-enrollee by one over the number of men who were successfully matched to the corresponding enrolled individual.[14] Appendix Table A1 provides summary statistics on enrolled and non-enrolled men by mental health history. In Section 6, I show robustness to alternative matching approaches (including using additional matching characteristics) as well as to not matching at all. For more details on this procedure, I refer the reader to Appendix B.4.

To confirm that enrolled men are indeed affected by the loss of Medicaid eligibility, Appendix Figure A3 plots the share of men enrolled in the Medicaid program separately by enrollment status and mental health history. Given that individuals were assigned into groups prior to age $17\frac{1}{2}$, a portion of men in the enrolled group becomes naturally disenrolled before age 19 and the share of non-enrollees enrolled in the pre-period is not mechanically zero (mean reversion). Nevertheless, this figure highlights that both groups of enrolled men experience a decline in Medicaid enrollment after their 19th birthdays, while men in the non-enrolled group are significantly less affected by the loss of eligibility. Panel (b) shows that both groups of enrollees were filing claims in the pre-period and then experienced a 30 percentage point decline in their propensity to file claims after their 19th birthdays.[15] This

services, and are therefore less likely to be enrolled in the program. In Section 6, I check the sensitivity of the main results to altering the timing of assignment into enrollment groups.

[14] If a non-enrolled individual is matched to more than one enrollee, then his total weight in the sample is the sum of the weights from each match. All enrolled men are assigned a weight of one. I intentionally avoid matching on outcome variables.

[15] Indeed, it is not the case that enrollees without mental health histories had insignificant medical needs compared to enrollees with mental health histories. More than one-quarter of the former

reduction in claims reinforces that both groups' healthcare utilization was affected by the loss of eligibility.

4.4 Triple-Differences Specification

With these four groups in hand, I estimate the impact of the loss of mental healthcare on criminal activity using a triple-differences specification of the following form:

$$\begin{aligned} Y_{it} = {} & \underbrace{\beta_1(\text{Treat}_i \times \text{Hist}_i \times \text{Post}_t) + \beta_2(\text{Treat}_i \times \text{Post}_t) + \beta_3(\text{Treat}_i \times \text{Hist}_i) + \beta_4 \text{Treat}_i}_{\text{Enrollees}} \\ & + \underbrace{\beta_5(\text{Hist}_i \times \text{Post}_t) + \beta_6 \text{Post}_t + \beta_7 \text{Hist}_i + \eta}_{\text{Non-enrollees}} + \delta_m + \epsilon_{it}. \end{aligned} \tag{3}$$

where Y_{it} is an outcome variable for individual i at time t. Treat_i is an indicator variable equal to one for enrolled men, Hist_i indicates whether individual i had a mental health history, and Post_t is an indicator variable equal to one for time periods after an individual's 19th birthday. δ_m are calendar time (year$\times$month) fixed effects and ϵ_{it} is an error term. A unit of observation is a person-age-quarter. Standard errors are clustered at the individual level, and regressions are weighted using the weights from the matching procedure.

The coefficient of interest is β_1, representing the impact of losing mental healthcare on incarceration. Throughout the paper, I report one- and two-year effects.[16] The identifying assumption in this empirical strategy is that the MH/non-MH difference in crime among enrollees would have trended similarly to the MH/non-MH difference among non-enrollees in the absence of the loss of mental healthcare.[17] Any difference between enrollees with and without mental health histories that arises precisely after their 19th birthdays can thus be attributed to the loss of mental healthcare.

The two main outcomes are whether an adult male 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 between individuals recidivating versus being incarcerated for the first time.[18] For ease of exposition, outcome

group have chronic physical conditions like asthma, diabetes, and hypertension.

[16] To estimate one-year effects, I include two variables for the post-period: one for age 19 and one for age 20. To estimate two-year effects, I include a single post-period indicator for ages 19–20.

[17] Note that this identifying assumption holds regardless of whether enrolled men experience an income effect due to the loss of eligibility.

[18] The use of a cumulative variable is also motivated by the imperfect information on an offender's incarceration spell. Specifically, the SLED data measure the initial detention date, but do not

variables are multiplied by 100.

Finally, dynamic regressions are crucial for examining the trajectories of enrollees with mental health histories prior to age 19 and for seeing whether the divergence in outcomes occurs right after this group reaches their 19th birthdays. To visually assess the evolution of outcomes, I estimate a fully dynamic version of the triple-differences specification:

$$Y_{it} = \sum_{\tau=-6,\tau\neq-1}^{\tau=7} \Big[\beta_\tau(\text{Treat}_i \times \text{Hist}_i \times \gamma_\tau) + \nu_\tau(\text{Treat}_i \times \gamma_\tau) + \lambda_\tau(\text{Hist}_i \times \gamma_\tau) + \theta_\tau\gamma_\tau\Big] \quad (4)$$
$$+ \mu\text{Treat}_i + \rho\text{Hist}_i + \pi(\text{Treat}_i \times \text{Hist}_i) + \eta + \delta_m + \epsilon_{it}$$

where γ_τ is the quarter relative to an individual's 19th birthday. The pre- and post-period are six and eight quarters, respectively, and I omit the quarter before a person's 19th birthday. The coefficients of interest are β_τ, estimating the difference in outcome Y at event time τ between enrollees with and without mental health histories, after accounting for natural differences in criminal trajectories by mental health status. When presenting the results, I plot the β_τ estimates in order to depict the dynamic evolution of outcomes before and after age 19. When using the cumulative incarceration outcome, I report β_3 and β_7, which represent the difference in incarceration by age 20 and 21, respectively.

4.5 Discussion of Threats to Identification Assumption

One concern with the identifying assumption may be that enrolled and non-enrolled men are fundamentally different and thus might have different trends by mental health status. Matching on observable characteristics helps alleviate this concern, as the third difference is estimated using the enrolled men's close peers. Indeed, Appendix Figure A4 plots the raw means of test scores earlier in adolescence for each group; these figures show that within mental health status, enrollees and non-enrollees were relatively similar to each other in educational achievement (in levels and trends) throughout adolescence. This figure thus provides reassuring evidence that non-enrollees serve as a suitable group for estimating the counterfactual MH/non-MH trends of enrollees around their 19th birthdays. Moreover, throughout the analysis, I plot the raw data and estimate non-parametric specifications to corroborate the plausibility of the identifying assumption. If crime differences across mental

contain information on when an individual was released from custody. Hence, estimates that consider an individual's likelihood of being incarcerated in a given quarter likely underestimate an individual's true likelihood of being incarcerated.

health status are similar between enrollees and non-enrollees for a full year and a half, but begin to diverge right after individuals turn 19, then these patterns would indicate that the loss of mental healthcare is the key driver of changes in criminal behavior.

A second threat to the identifying assumption is that enrollees with mental health histories might be experiencing other shocks at the same time as the loss of mental healthcare, thereby confounding the estimated effects. Various 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.[19] Further, enrollees with mental health histories turn 19 in the same years as the other men in the sample, and they reach 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 enrollees with mental health histories—and not enrollees without mental health histories or similar low-income men living in close proximity—*and* that the timing of these 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 enrollees with mental health histories experiencing shocks unrelated to their loss of mental healthcare right after their 19th birthdays.

5 Effect of Loss of Mental Healthcare on Criminal Behavior

5.1 Increase in Incarceration for Men with Mental Illness

I begin by plotting the raw means of incarceration propensities separately by enrollment group and mental health status. Panel (a) of Figure 3 considers an individual's likelihood of being incarcerated in a given quarter, and panel (b) considers the cumulative variable measuring whether an individual has *ever* been incarcerated. These figures show that within mental health status, enrollees and non-enrollees had similar incarceration propensities before age 19. In other words, the difference between enrollees with and without mental health histories is evolving in the same way as the analogous difference among non-enrollees for the full year and a half prior to age 19, providing support for the identifying assumption. Then, precisely after reaching their 19th birthdays, enrolled men with mental health histories begin diverging and are more likely to be incarcerated.

[19] Appendix Figure A5 shows that the enrolled men with mental health histories did not experience declines in SNAP or TANF enrollment around their 19th birthdays.

Figure 4 plots the triple-differences estimates, also summarized in Table 1. The estimates indicate that men with mental health histories are 0.9 percentage points, or 19%, more likely to be incarcerated in a given quarter in the year after losing eligibility.[20] Turning to the cumulative measure, the estimates suggest that one year after losing eligibility, enrollees with a mental health history are 2.5 percentage points (22%) more likely to have *ever* been incarcerated. By their 21st birthdays, these estimates rise to 3 percentage points (21%). Appendix Figure A6 and Table A2 show that the point estimates are comparable across white and Black men, though the implied percent increases are larger for white men given their lower baseline incarceration rates.

The increase in incarceration within a year of losing access to mental healthcare suggests a close link between forgone mental healthcare and criminal offending. Furthermore, the cumulative results imply that the rise in incarceration is not driven by a group of men being detained for long periods of time or consistently recidivating. Instead, the loss of mental healthcare results in *new* individuals with mental health histories becoming incarcerated for the first time.

A back-of-the-envelope calculation suggests that in the absence of the loss of mental healthcare, around 360 men with mental health histories would not have been incarcerated, implying a 10% reduction in these cohorts' likelihood of incarceration by age 21. To put these magnitudes into perspective, Lochner and Moretti (2004) finds that one extra year of schooling reduces men's likelihood of incarceration by approximately 10%. Billings and Schnepel (2018) finds that early-life interventions to reduce lead exposure decrease the likelihood of arrests by around 40%. The findings in this paper thus suggest that providing mental healthcare to low-income men with mental illness has comparable effects to those of other interventions typically associated with crime reduction.

An additional point is worth noting regarding the patterns in the raw data in Figure 3. Before reaching their 19th birthdays, the incarceration propensities of enrollees with mental health histories resemble those of their non-enrolled peers in both trends and levels. A potential interpretation of these patterns is that having access to mental healthcare suppressed enrollees' criminal propensities, keeping them comparable to those of their non-

[20] I re-scale the effect in the post-period by the average outcome of non-enrollees with mental health histories in the corresponding moment of the post-period. Because criminal propensity tends to rise with time in this age range, re-scaling the effect by the average outcome in the pre-period would yield larger effects. This choice is also motivated by the identifying assumption that the MH/non-MH trends among non-enrollees are suitable counterfactuals for the enrolled men.

enrolled peers.[21] Upon aging out of eligibility, enrollees with mental health histories began forgoing mental healthcare and became more likely to commit serious offenses.[22]

Overall, these findings indicate that the availability of mental healthcare influences the offending behavior of men with mental health histories. Once these men lose access to mental healthcare, they are more likely to commit an offense and become incarcerated for the first time.

5.2 Larger Effects for Recent Beneficiaries of Mental Healthcare

If the increase in criminal activity is driven by the disruption in access to mental healthcare, then we would expect the treatment effects to be more pronounced for enrollees who were filing mental health claims right before aging out of eligibility. To test this hypothesis, I designate enrollees as recent beneficiaries if they had a mental health claim in the year and a half before turning 19 (36% of enrollees with mental health histories). If the loss of mental healthcare is an important factor driving the increase in crime, then I should find a larger effect for this group.

In practice, I split enrollees with mental health histories into more- vs. less-recent beneficiaries, and estimate the triple-differences specification for each group. In each regression, I maintain the same sample of men without mental health histories, but restrict non-enrollees with mental health histories to the corresponding matches of enrolled men to preserve balance on observable characteristics.

Figure 5 shows that men who were using mental healthcare right before their 19th birthdays (circle markers) were more likely to be incarcerated compared to men who were using mental healthcare *less* recently (triangle markers). These findings, summarized in Appendix Table A4, provide additional evidence that decreased access to mental healthcare plays a key role in driving the observed rise in criminal activity.

5.3 Consistent Results with Complementary Regression Discontinuity Strategy

So far, the analysis has relied on a triple-differences empirical strategy. In this subsection, I utilize an alternative, complementary approach for estimating the causal effect of

[21] Non-enrollees likely have higher-income parents (see Appendix Table A1) 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 non-enrolled men's incarceration rate is not higher than that of the enrolled men despite not being enrolled in the Medicaid program.

[22] Appendix Table A3 confirms that the increase in offending is driven by incidents with an associated felony offense.

mental healthcare on criminal behavior.

I use a regression discontinuity approach, which compares arrest probabilities before and after individuals' 19th birthdays, similar to the strategy in Card et al. (2009) and Lee and McCrary (2017). Because this approach estimates changes in criminal propensity that occur immediately upon reaching age 19, I rely on arrests as the main outcome of interest (as opposed to also using incarceration records, which can reflect longer-term judicial decisions). It is not ex-ante clear that the effect of losing mental healthcare on crime would be instantaneous, so any estimates from this analysis are likely underestimates of the overall effect of losing access to mental healthcare on crime. Nevertheless, this analysis relies on different identifying assumptions than the baseline approach, and thus serves as a complementary strategy for investigating the role of mental healthcare in affecting criminal activity in the immediate short-term.

Specifically, I estimate the following regression-discontinuity model:

$$Y_{it} = f(a_i) + \beta_1 \text{Post19}_t + \beta_2[\text{Post19}_t \times f(a_i)] + \gamma_m + \epsilon_{it} \tag{5}$$

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 mental healthcare 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.

I begin by estimating this equation separately for men with and without mental health histories, based on whether they filed any mental health claims throughout adolescence. Table 2 shows that the likelihood of being arrested increases upon reaching age 19 for men with mental health histories, whereas the discontinuity is not present for the latter group. Next, I focus on more- vs. less-recent beneficiaries: Figure 6 displays the results for men who were filing mental health claims in the two years before losing eligibility versus those who were not. Again, the discontinuity is only present for the former group. The point estimates suggest that men with recent mental health histories were 0.37 percentage points more likely to be arrested upon reaching their 19th birthdays, or 10% relative to the average arrest rate prior to age 19.

The findings in this subsection suggest that the loss of mental healthcare has a rapid effect on a share of men with mental illness. These results are consistent with the triple-differences results, which show an increase in crime relatively quickly after the loss of mental healthcare and that the effects are particularly pronounced for recent beneficiaries.

6 Robustness of Increase in Criminal Activity

6.1 Robustness to Clustering & Matching Procedure

I begin by considering robustness to the choices made when constructing the baseline sample and implementing the preferred specification. First, I change the level at which I cluster the standard errors, first to the match level and then to the match and individual level. I also run the baseline specification using individual-level fixed effects. The statistical significance of the main results is preserved (Appendix Table A5).

Next, I test the sensitivity of the estimates to the matching/weighting procedure used to select the group of non-enrollees. Appendix Table A6 summarizes the results and Appendix Figure A7 displays the dynamic evolution of outcomes. First, instead of relying on coarsened exact matching to select the non-enrolled men, I re-weight observations using the DFL re-weighting approach (DiNardo et al., 1995; Fortin et al., 2011). Second, I return to the baseline procedure and exclude non-enrollees receiving the greatest amount of weight in the regression to ensure that the patterns are not driven by a small number of observations. Third, I change the timing of assignment into enrollment groups, so that men are assigned to a group based on their enrollment in the pre-period (as opposed to a year before the pre-period). The main results are robust to these changes. Finally, I refrain from *any* matching and re-weighting, and instead estimate the baseline specification using all enrollees and non-enrollees. The main results are very similar to those using the matched sample.

6.2 Suitability of Third Difference in Triple-Differences Strategy

The baseline empirical strategy relies on using non-enrolled men to account for differences in crime across mental health status around age 19. I now conduct three exercises to consider whether the results are driven by this group not serving as a suitable counterfactual for estimating differences in age trajectories.

Using Additional Matching Characteristics. I begin by augmenting the matching procedure with additional variables so that non-enrollees are even *more* likely to resemble enrollees along observable characteristics. In particular, if the estimated effects are driven by differences between the enrollees and non-enrollees arising at age 19 that are *unrelated* to Medicaid disenrollment, then I might expect the increase in incarceration to diminish in magnitude as I make the groups more observably similar.[23]

First, I substitute an individual's school district with his school in the matching procedure. 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 Figure A8).[24] Next, I separately add SNAP program enrollment as well as contact with the juvenile justice system as matching characteristics. Finally, I use an individual's school, SNAP enrollment, *and* contact with the juvenile system as matching characteristics. In all of these exercises, I find that the MH/non-MH differences between enrollees and non-enrollees were trending similarly for the full year and a half prior to age 19, and that it was only after turning 19 that enrollees with mental health histories became more likely to be incarcerated. The consistent patterns and estimated magnitudes in these exercises provide reassuring evidence that the increase in incarceration is not driven by differences between enrollment groups in their location, income, or prior contact with the justice system.

Alternative Double-Differences Framework. I now use an alternative approach that abstracts away from non-enrollees altogether. Specifically, I focus on enrollees with mental health histories and implement a difference-in-differences strategy. 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 are the comparison group. The identifying assumption is that treated men would have continued to trend similarly to the comparison group in the absence of the loss of mental healthcare. Similar to the baseline strategy, I implement a matching procedure to ensure balance between the two groups in terms of race, cohort, and geographic location.

Appendix Figure A9 and Table A7 show that men filing mental health claims are 0.8 percentage points (or 6%) more likely to have been incarcerated by age 20 relative to the comparison group. Importantly, the groups are trending similarly prior to age 19 and again begin to diverge in their incarceration propensities precisely around their 19th birthdays.

[23] A caveat is that if adding characteristics increases unobservable differences between the groups, then the increase in incarceration could become larger.

[24] Adding characteristics to the baseline matching procedure reduces the number of successfully matched enrolled men, so I also utilize the DFL re-weighting approach to keep the sample constant across specifications.

Because both groups experienced a loss of access to mental healthcare, these estimates are likely underestimates of the overall effect of losing mental healthcare on criminal activity.

This exercise—which uses an alternative comparison group for estimating counterfactual outcomes and thus relies on a different identifying assumption—provides additional evidence that losing access to mental healthcare plays an important role in explaining the rise in criminal activity. This result is consistent with Figures 5 and 6 showing that more recent-beneficiaries have larger treatment effects than less-recent beneficiaries.

Falsification Checks Around Earlier Ages. Next, I conduct "placebo" checks in which I replicate the baseline empirical strategy around earlier ages. If I see treatment effects emerge around earlier ages—when there was no break in access to mental healthcare—then these results would imply that the non-enrolled men are not a suitable group for accounting for differences in crime across mental health status.

I begin by shifting the approach back one year and estimating men's likelihood of incarceration around age 18. Appendix Table A8 reports the results from this exercise, showing a lack of an increase in the likelihood of incarceration for enrollees 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. Again, the results show that an increase in incarceration only occurs around men's 19th birthdays. The results from this exercise suggest that non-enrolled men are a useful group for accounting for MH/non-MH crime differences, and that the increase in criminal activity at age 19 is driven by the loss of mental healthcare at this later age.

6.3 Lack of Concurrent Changes in Late Adolescence

A potential threat to causal identification is that enrollees with mental health histories might be experiencing shocks at age 19 unrelated to the loss of mental healthcare 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, this transition occurs at different points in calendar time and there is no reason to think that its timing coincides precisely with men's 19th birthdays. Appendix Figure A10 plots the share of each group that is enrolled in school at a given age. This figure shows that enrollment begins to decline when individuals are 17 years old, the age at which they are legally allowed to drop out in South Carolina. By age 18, only 30% of men with mental health histories remain enrolled in school. These patterns suggest that if the transition to higher education or the labor market were driving the increase in crime, we might expect to see effects prior to individuals' 19th birthdays. Further, the figure shows that within mental health status, the share of enrollees that were enrolled in school was comparable to the analogous share of non-enrollees. This figure thus provides evidence that enrolled men with mental health histories were not differentially graduating high school right before their 19th birthdays in a way that might confound the estimated effects.

A related concern might be that these men are aging out of foster care or losing Supplemental Security Income (SSI) benefits at age 18, and that these transitions might influence their criminal activity (Courtney et al., 2007; Deshpande and Mueller-Smith, 2022). Although the timing of these transitions differs from the loss of mental healthcare, I still take seriously this consideration and exclude from the sample the relatively small share (13%) of enrollees 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 dis-enrollment during late adolescence (Appendix Table A6 and Figure A7).

6.4 Robustness of RD estimates

The results from the regression discontinuity approach show that men's arrest probability increases upon turning 19 for individuals with mental illness, and not for those without mental health histories. Appendix Table A9 shows the robustness of this result to using a cubic polynomial in equation (5) and to using non-parametric estimates. Next, Appendix Figure A11 replicates the regression discontinuity approach in the six months before and after an individual's 19th birthday. I only find a statistically significant increase in arrest probability when considering individuals' 19th birthdays, consistent with the loss of mental healthcare being a central driver of the increase in crime.

7 Effects by Crime Type

Why might forgoing mental healthcare affect men's criminal propensity? There are several channels that might drive this relationship. First, individuals might begin making more errors in judgment or decision-making (a psychological stress channel). Second, individuals who lose access to medications might begin to self-medicate with illicit drugs (a self-medication channel). Busch et al. (2014) finds that following a regulatory policy that decreased antidepressant prescriptions, adolescents with depression were more likely to use illegal drugs. Third, decreased access to mental healthcare could disrupt an individual's economic outcomes, like their human capital formation[25] or labor market opportunities (an economic channel). For example, 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. To consider these mechanisms, I now consider changes in incarceration propensities for different types of offenses.

In line with the economics of crime literature, I begin by estimating whether men with mental health histories are more likely to be incarcerated for violent and property crimes. Table 3 and Appendix Figure A12 indicate that these men are significantly more likely to be incarcerated for both types of offenses. Because violent and property crimes represent serious incidents, these results imply increases in serious criminal offending, rather than an increase in the likelihood of incarceration for less serious crimes. The increase in violent crime suggests a role for the psychological stress channel. The increase in property crime—which could be a function of poor decision-making or economic need—provides support for both the psychological stress and economic channels. Appendix Table A10 conducts an analogous exercise using the regression discontinuity strategy, and similarly finds a sudden increase in the likelihood of being arrested for a property crime. These findings are consistent with papers studying the effect of Medicaid expansions on crime rates, which find declines in violent and property offenses (Wen et al., 2017; Aslim et al., 2019; Fry et al., 2020; He and Barkowski, 2020; Vogler, 2020).

As an alternative way to explore mechanisms, I classify offenses into financially motivated offenses (e.g., burglary, robbery, drug distribution); non-financial violent offenses (e.g., assault, weapons-related offenses); drug and alcohol possession (e.g., marijuana possession, DUIs); and miscellaneous offenses (e.g., resisting arrest, parole or probation violations). The findings show that by age 20, men with mental health histories are 22–25% more likely to have been incarcerated for financially motivated offenses and for non-financial violent offenses. However, they are no more likely to be incarcerated for drug possession, suggesting that the self-medication channel is not the primary mechanism. These findings corroborate the notion that the psychological stress and economic channels seem to be key mechanisms underlying the relationship between mental health and criminal behavior.

[25] Currie and Stabile (2006, 2007) find that mental health conditions can have deleterious effects on educational attainment.

8 Heterogeneity

8.1 Differences by Medication Usage

More than half of recent beneficiaries were relying on Medicaid for access to mental health medications, suggesting that decreased access to medications might be an important reason behind the increase in criminal activity. To consider this possibility, I split the sample of enrollees with mental health histories based on their medication usage. Specifically, I divide this group into those who filed claims for mental health drugs in the years before aging out of eligibility (i.e., the 35% of 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. The first two panels of Appendix Figure A13 show that the incarceration effects are particularly pronounced for the group of men filing claims for mental health drugs in late adolescence (Appendix Table A11 summarizes these estimates).[26]

Furthermore, a substantial share of enrollees with mental health histories filed claims for non-ADHD medications (antidepressant, antianxiety, and antipsychotic medications; see Appendix Table A13). The bottom panels of Appendix Figure A13 thus hone in on enrollees filing claims for non-ADHD medications. The estimates show that the more pronounced incarceration effects for men using psychotropic drugs are driven by those filing claims for non-ADHD medications in the years prior to losing eligibility.

8.2 Differences by Diagnoses

The Medicaid claims records include diagnoses codes, which allow me to study which subgroups of men are driving the rise in crime. To do so, Appendix Table A14 considers the sensitivity of the baseline estimates to excluding men ever diagnosed with a particular disorder.[27] First, the baseline 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.[28] These findings thus suggest that men diagnosed with these disorders

[26] Appendix Table A12 reports estimates using the regression discontinuity approach. These findings suggest that the immediate increase in arrests at age 19 is not driven by this group of men filing claims for mental health medications.

[27] Appendix Table A13 shows that neurodevelopmental disorders (including ADHD) as well as behavioral disorders (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.

[28] 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

are most likely to commit crime after losing access to mental healthcare. These results are in line with findings in prior studies showing that adults diagnosed as children with behavioral disorders as well as those with co-occurring substance use disorders are more likely to be violent (Glied and Frank, 2014). In contrast, the baseline results are robust to excluding individuals with intellectual disabilities and other neurodevelopmental disorders. To the extent that this group struggles with their transition to higher education or the labor market, then this result provides additional evidence that such transitions are not confounding the estimated effects. Appendix Table A12 replicates this analysis using the regression discontinuity approach and finds consistent evidence that the immediate increase in incarceration is most sensitive to excluding individuals with oppositional defiant and substance use disorders.

Finally, the last row in these tables considers the robustness of results to excluding individuals with mental illness who have also been diagnosed with a chronic physical condition. The results are comparable to the baseline estimates, reinforcing that it is the loss of mental healthcare—rather than physical healthcare—that is driving the increase in criminal activity among this group.

9 Cost Effectiveness of Mental Healthcare via Medicaid

The results up to this point suggest that forgoing mental healthcare increases criminal involvement. In this section, I put the causal estimates into context by considering the cost effectiveness of providing low-income young men with access to mental healthcare via the Medicaid program.

9.1 Costs and Benefits of Expanding Medicaid Eligibility

I begin by quantifying the cost of providing mental healthcare via the Medicaid program for two additional years. I compare this cost against the associated benefits, which include lower social costs from fewer victimizations as well as reduced fiscal and social costs from fewer incarcerations. I summarize the approach here and report the estimates from these calculations in Appendix Table A15. For more details on this cost-benefit analysis, I refer the reader to Appendix C.

substance abuse disorder, respectively, prior to age 19. Because of the co-occurrence of diagnoses and the prevalence of ADHD in this sample, I also exclude men who have ADHD as their only diagnosis, showing that the increase in crime is not driven by this relatively small group.

Costs. 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.

Benefits. I begin by estimating the social benefits accrued as a result of fewer criminal victimizations. To do so, I estimate the effect of the loss of mental healthcare on serious arrests (Table 4). Using these figures, I estimate the number of "excess" violent, property, and drug arrests that occurred as a result of the termination of Medicaid eligibility. I then use arrest-to-victimization ratios (Heckman et al., 2010) as well as estimates of the average cost per crime (Cohen and Piquero, 2009; Miller et al., 1996) to calculate the reduction in the total social costs of victimization. Taking a relatively conservative approach, I find that the reduced social costs of crime would be roughly $20 million.[29]

Next, I calculate the benefits from fewer incarcerations, and estimate that providing Medicaid eligibility would result in $3.5 million and $8.6 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.

Costs vs. Benefits. After comparing the costs of Medicaid provision with the potential benefits generated from reduced criminal activity and fewer incarcerations, I conclude that providing mental healthcare to low-income young men via the Medicaid program is a cost-effective way to reduce crime. Using moderately conservative estimates, the findings suggest that for every dollar spent on insuring low-income young men via Medicaid, society recoups around $2.17 in social and fiscal costs. Even in the calculation with the most conservative assumptions, the estimated benefits outweigh the costs.

[29] 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.

Marginal Value of Public Funds. I also 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 and 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 roughly 2 (Appendix Table C2). 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 beneficiaries.

9.2 Comparing Mental Healthcare Provision to Other Policies

Up to this point, I have shown that providing mental healthcare via Medicaid seems to be a cost-effective policy for reducing criminal behavior. But how does this cost effectiveness compare to that of other crime-reduction approaches? In Appendices D and E, I benchmark the cost effectiveness of providing low-income young men with Medicaid eligibility against the cost effectiveness of other favored crime-reduction policies: namely, harsher criminal sanctions and hiring police officers. One motivation for these exercises is that cost-benefit calculations 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 policies intended to reduce crime by the same amount.

Overall, I find that the cost of reducing crime via longer sentence lengths is twice the cost of reducing crime by the same amount via Medicaid eligibility. Put differently, 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 the provision of mental healthcare. I find that hiring police officers is potentially more cost effective as a crime-reduction approach, but the comparison is sensitive to the degree to which hiring officers reduces crime, especially violent crime, among individuals in older age groups.[30]

[30] An important caveat to this finding is that the comparison does not account for the social costs of policing (e.g., Ang, 2021; Tebes and Fagan, 2022), thereby understating the overall costs of hiring police officers.

10 Discussion & Conclusion

Criminal activity imposes significant economic and social costs on individuals and communities. Accordingly, a key priority for governments is to understand the causes of crime and develop effective crime-reduction approaches. Motivated by the disproportionate representation of mentally ill individuals in today's criminal justice population, this paper studies the effect of mental health on criminal behavior and the potential for mental healthcare to serve as a crime-reduction strategy. To study this question, I leverage a discrete break in access to mental healthcare that occurs on an individual's 19th birthday and employ a triple-differences research design. I use rich administrative data linking health and law enforcement records in South Carolina, which allows me to identify individuals with mental health histories and measure their criminal outcomes.

I find that men with mental illness who lose access to mental healthcare on their 19th birthdays are more likely to be incarcerated in the two subsequent years. I find that the effects are particularly pronounced for men who were filing mental health claims right before the loss of eligibility, and that these individuals are more likely to be incarcerated for violent and property offenses. I validate these results with a battery of robustness checks and alternative empirical strategies, all of which yield consistent evidence that forgoing mental healthcare increases criminal propensity.

The findings of this study offer a number of takeaways and policy implications. First, this paper identifies a population of individuals—low-income young men with mental health histories—whose criminal activity is affected by the availability of mental healthcare. Policymakers might thus wish to consider improved access to mental healthcare as one approach for lowering crime rates and decreasing criminal justice expenditures. Notably, the termination of eligibility occurs in late adolescence—during the peak of the age-crime profile—highlighting the potential importance of ensuring access to mental healthcare at this point in the life cycle. To the extent that mental healthcare improves an individual's decision making, then providing mental healthcare might complement traditional, incentive-based strategies for deterring crime.

More broadly, the findings of this paper speak to the short-term returns of healthcare provision for low-income young adults, which are likely of interest to policymakers for two reasons. First, young adults are relatively less likely to be insured and are the group that stands most to gain from modern health insurance expansions. Understanding the returns to this investment is thus of particular relevance. Second, policymakers may be particularly interested in understanding a policy's *short-term* returns. Politicians seeking re-election might prefer to promote policies with short-term benefits (Jacobs, 2016) and most U.S. state constitutions include balanced budget requirements that prohibit policymakers from borrowing to fund expenses with long-term returns. Policymakers should therefore incorporate these findings into their valuations of the Medicaid program and when weighing the costs and benefits of expanding health insurance access, and in particular mental healthcare access, to low-income young adults.

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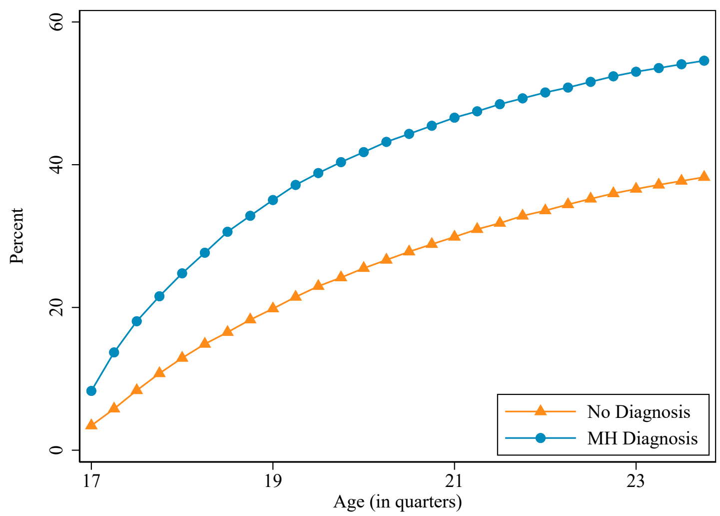
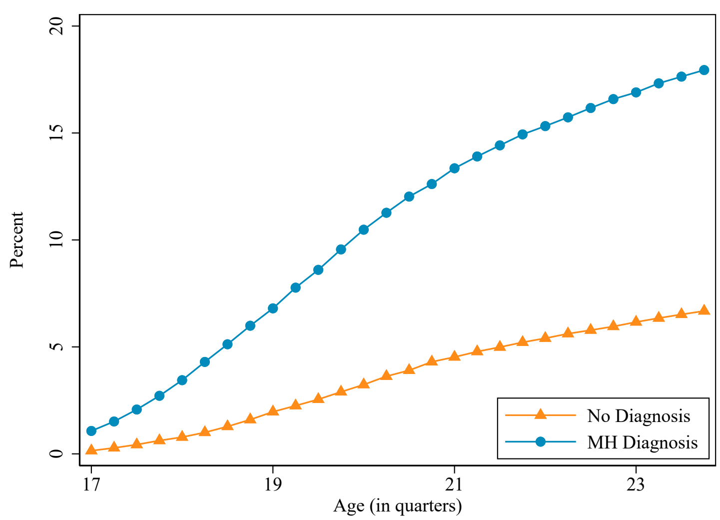
Figures and Tables

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 ever enrolled in Medicaid 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.

Figure 2: Illustration of Triple-Differences Research Design

(A) Enrollees

	<19	≥19
MH history	a	b
No MH history	c	d

→ **Δ1 = (b – a) – (d – c)**

(B) Non-enrollees

	<19	≥19
MH history	e	f
No MH history	g	h

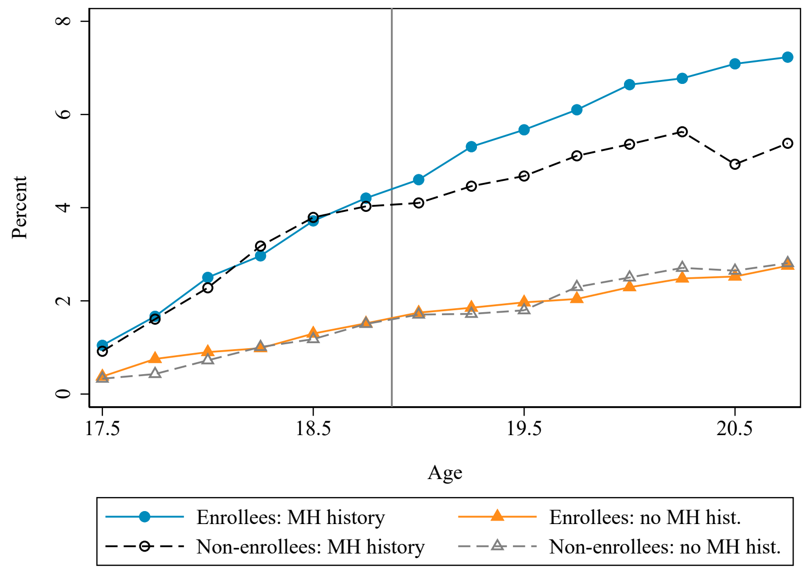
→ **Δ2 = (f – e) – (h – g)**

→ **Triple Difference = Δ1 – Δ2**

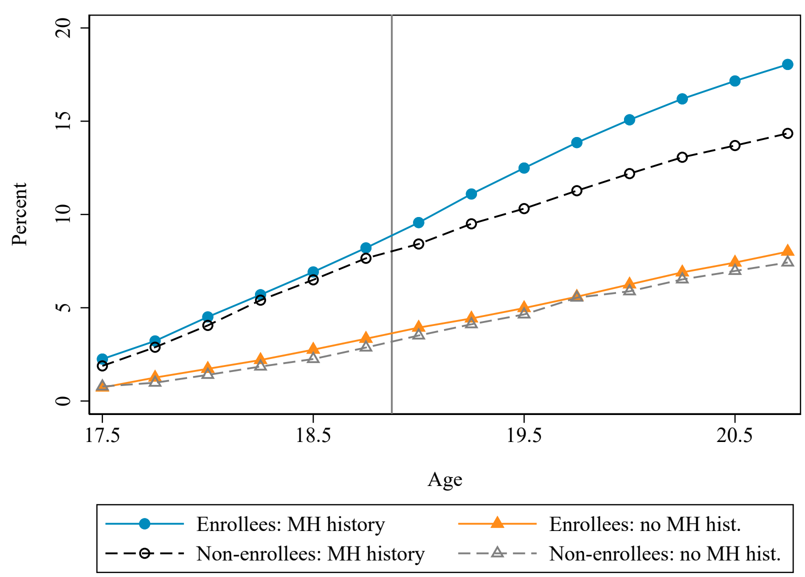
NOTE: This figure illustrates the triple-differences specification described in Section 4. Panels (a) and (b) illustrate the difference-in-differences comparison for enrollees and non-enrollees, respectively.

Figure 3: Raw Means of Incarceration Propensity, by Enrollment Status and Mental Health History

(a) Incarcerated that quarter



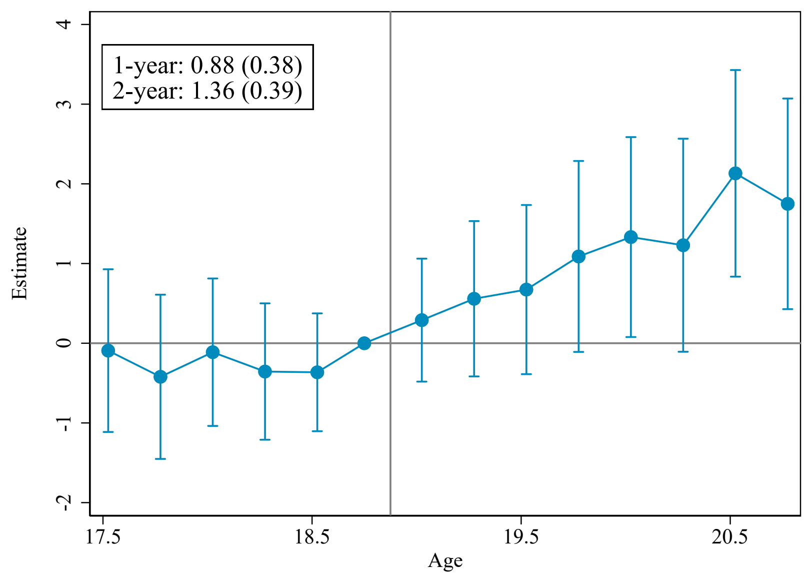
(b) Ever incarcerated



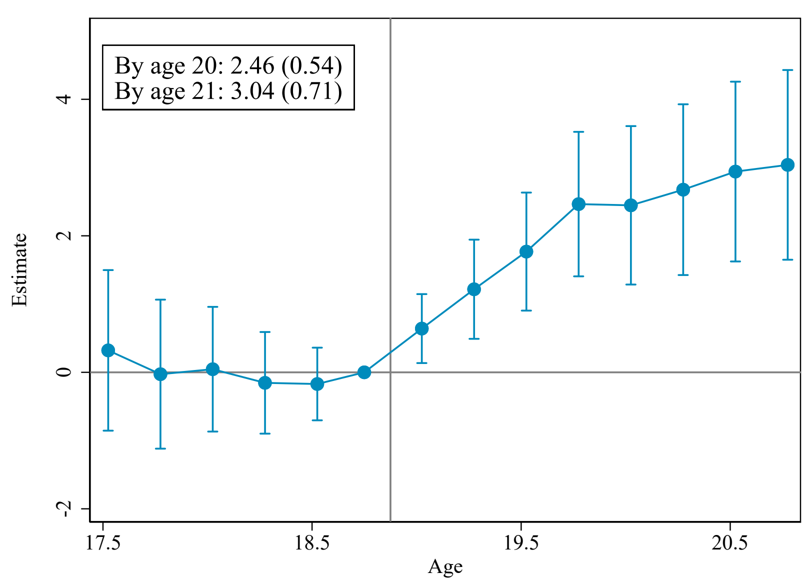
Note: These graphs plot incarceration propensities by Medicaid enrollment status and mental health history. "Incarcerated that quarter" refers to being detained in an adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Non-enrolled men are weighted using the corresponding weights from the matching procedure.

Figure 4: Triple-Differences Estimates for Effect of Losing Mental Healthcare on Likelihood of Incarceration

(a) Incarcerated that quarter



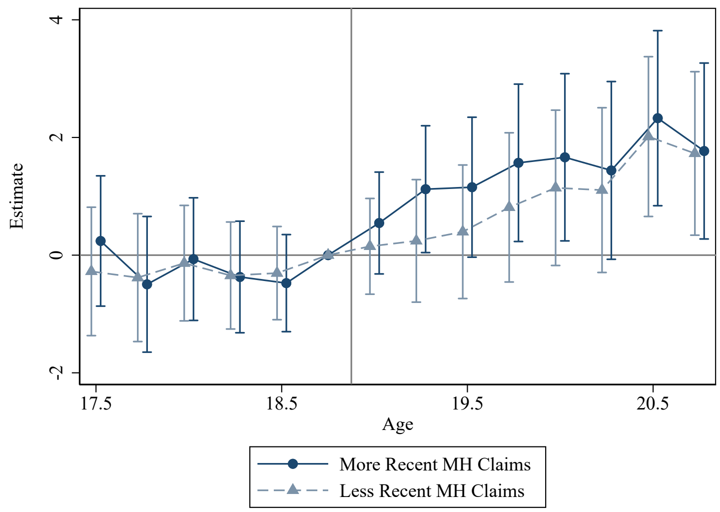
(b) Ever incarcerated



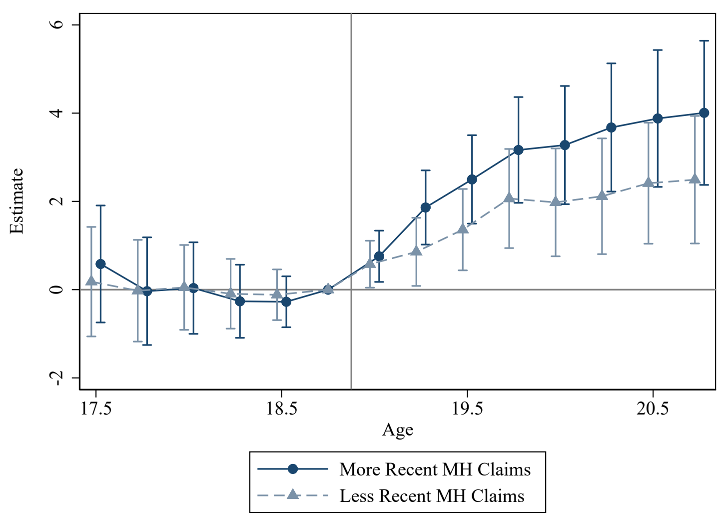
NOTE: These graphs plot triple-differences estimates using equation (4). In panel (a), the reported one- and two-year effects correspond to β from equation (3) using one- and two-year post-periods, respectively. In panel (b), the reported estimates correspond to β_3 and β_7 from equation (4). "Incarcerated 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. Non-enrolled men are weighted using the corresponding weights from the matching procedure. Standard errors are reported in parentheses and are clustered at the individual level.

Figure 5: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Mental Health Claims

(a) Incarcerated that quarter



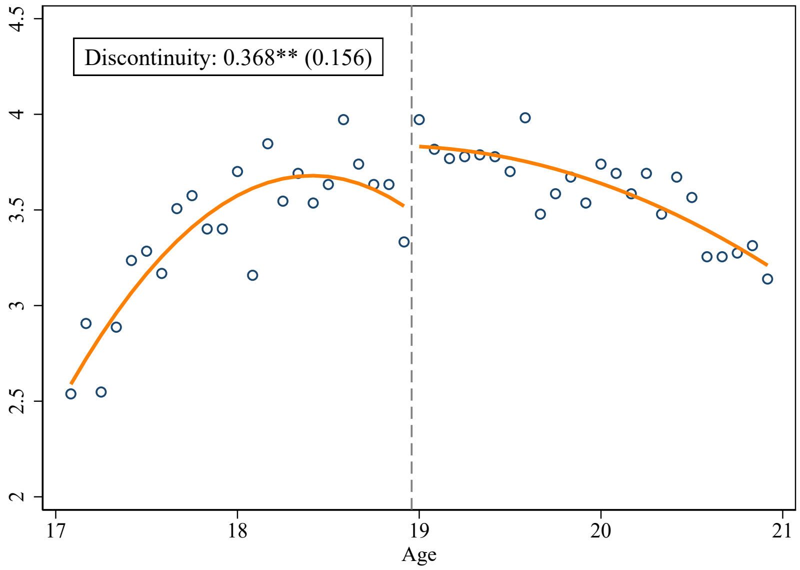
(b) Ever incarcerated



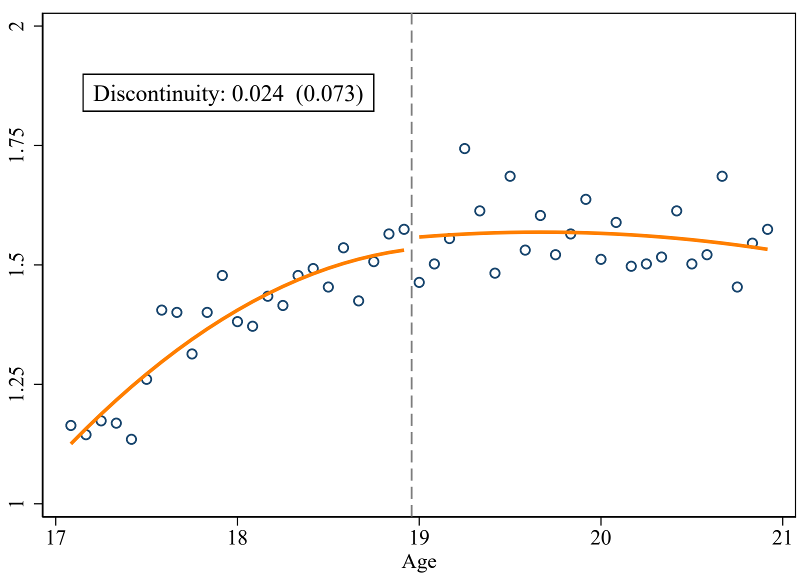
NOTE: These figures plot triple-differences estimates using equation (4). "More Recent" and "Less Recent" refer to models that restrict enrollees with mental health histories based on whether they did or did not file a mental health claim in the year and a half before age 19, respectively. In both sets of models, the group of matched non-enrollees with mental health histories is also restricted to maintain balance on observable characteristics. "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. Non-enrolled men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level.

Figure 6: Discontinuity in Arrest Probability 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 arrested around their 19th birthdays. The circles represent the share of individuals arrested in that month. The solid line plots the estimate using equation (5) using a quadratic polynomial. Each figure reports the discontinuity estimate and 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 a lack of information on an individual's exact birth date and the timing of the age of criminal majority. Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1.

Table 1: Triple-Differences Estimates for Effect of Losing Mental Healthcare on Likelihood of Incarceration

	(1) Incarcerated	(2) Ever Incarcerated
One-year effect	0.878**	2.465***
	(0.378)	(0.540)
Two-year effect	1.356***	3.039***
	(0.390)	(0.709)
Outcome mean: 1-year	4.59	11.28
Outcome mean: 2-year	4.96	14.35
Observations	424,606	424,606

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. Column one reports triple-differences estimates using equation (3). "One-year effect" quantifies the effect on incarceration at age 19. "Two-year effect" quantifies the effect on incarceration at ages 19–20. Column two reports estimates using equation (4). The one- and two-year estimates correspond to the β_3 and β_7 coefficients, respectively. "Outcome mean" refers to the average incarceration rate of non-enrollees with mental health histories in the corresponding time period. Standard errors are clustered at the individual level.

Table 2: Discontinuity in Arrest Probability Around Age 19

	(1) MH claim, Age 10–18	(2) No MH claim, Age 10–18	(3) MH claim, Age 16–18	(4) No MH claim, Age 16–18
Discontinuity	0.203*	0.051	0.368**	0.024
	(0.108)	(0.081)	(0.156)	(0.073)
Pre-period Average	2.76	1.07	3.39	1.38
Observations	839,890	618,379	485,134	973,135

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers men's probability of being arrested around their 19th birthdays separately by mental health history. Estimates report the discontinuity from equation (5) using a quadratic polynomial. The first and second columns consider men with and without a mental health claim between the ages of 10 and 18, respectively. The third and fourth columns consider men with and without a mental health claim between the ages of 16 and 18, respectively. To be conservative, estimates exclude the first month of age 17 due to a lack of information on an individual's exact birth date and the timing of the age of criminal majority. Standard errors are clustered at the individual level.

Table 3: Effect of Losing Mental Healthcare on Likelihood of Incarceration, by Offense Type

	(1) Violent	(2) Property	(3) Finally Motivated	(4) Non-financial violent	(5) Drug possession	(6) Other
A. Incarcerated						
One-year effect	0.666***	0.225	0.551***	0.235**	0.036	0.136
	(0.254)	(0.233)	(0.167)	(0.105)	(0.057)	(0.348)
Two-year effect	0.629**	0.369	0.513***	0.153*	0.067	0.695**
	(0.260)	(0.238)	(0.136)	(0.088)	(0.050)	(0.354)
Outcome mean: 1-year	1.58	1.97	0.92	0.44	0.10	3.32
Outcome mean: 2-year	1.92	2.03	0.85	0.42	0.11	3.77
B. Ever Incarcerated						
By age 20	0.822**	1.349***	1.995***	0.827***	0.224	0.666
	(0.362)	(0.372)	(0.464)	(0.314)	(0.161)	(0.427)
By age 21	1.311***	1.469***	2.537***	1.174***	0.510**	1.964***
	(0.459)	(0.486)	(0.603)	(0.416)	(0.241)	(0.614)
Outcome mean: 1-year	4.29	5.35	8.02	3.84	0.75	6.34
Outcome mean: 2-year	5.35	6.77	10.17	4.79	1.11	8.72
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. Panel (a) reports one- and two-year estimates using equation (3). Panel (b) reports the β_3 and β_7 coefficients from equation (4). "Other" refers to offenses that were not classified in columns 3–5. "Outcome mean" refers to the average incarceration rate of non-enrollees with mental health histories in the corresponding time period. Standard errors are clustered at the individual level. For more details on offense classifications, see Appendix B.

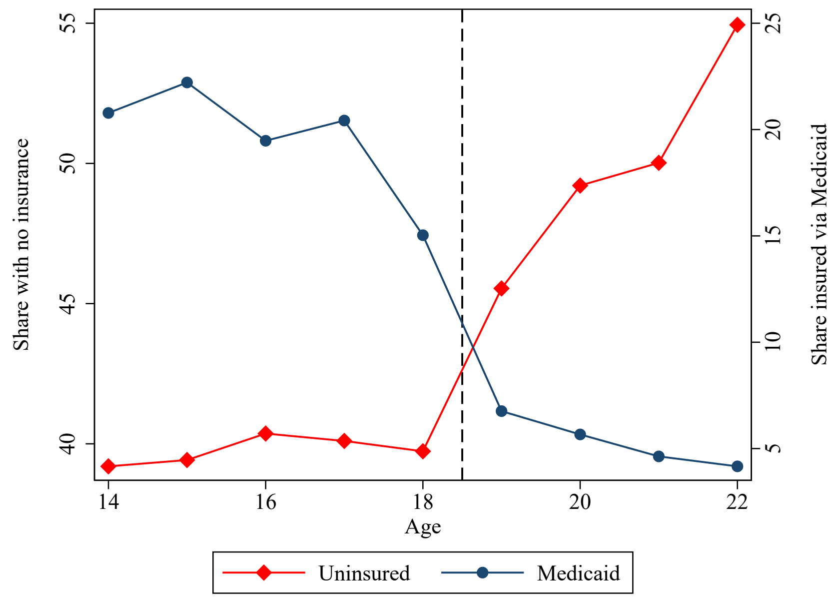
Table 4: Effect of Loss of Mental Healthcare on Public Safety (Serious Arrests)

	(1) All	(2) Violent	(3) Property	(4) Drug	(5) Other
Two-year effect	0.626***	0.162*	0.230**	0.255***	0.170*
	(0.164)	(0.084)	(0.098)	(0.080)	(0.090)
Outcome mean	1.21	0.38	0.50	0.20	0.48
Observations	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. This table reports triple-differences estimates using equation (3) for the baseline sample. All columns consider serious arrests that resulted in an individual being taken into custody. "Other" refers to offenses that were not classified in columns 2–4. "Outcome mean" refers to the post-period average arrest rate of non-enrollees with mental health histories. For more details on offense classifications, see Appendix B.

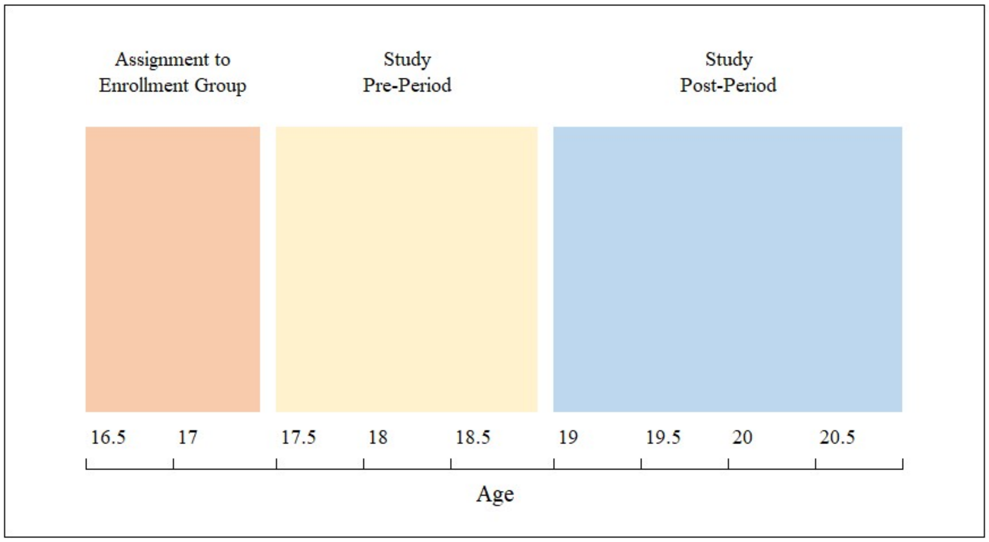
A Online Appendix

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 those who do not have any health insurance coverage at a given age.

Figure A2: Timeline of Empirical Strategy

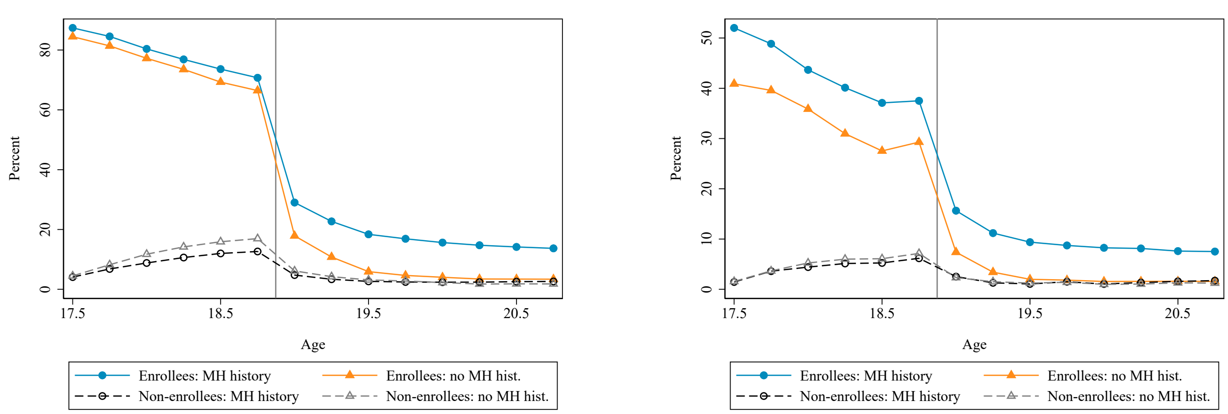


Note: Men are assigned to enrollment groups based on their enrollment in the Medicaid program between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$ (orange). Their outcomes are then allowed to evolve naturally in the study pre-period (yellow) and post-period (blue).

Figure A3: Medicaid Utilization, by Enrollment Status and Mental Health History (Raw Means)

(a) Share Enrolled

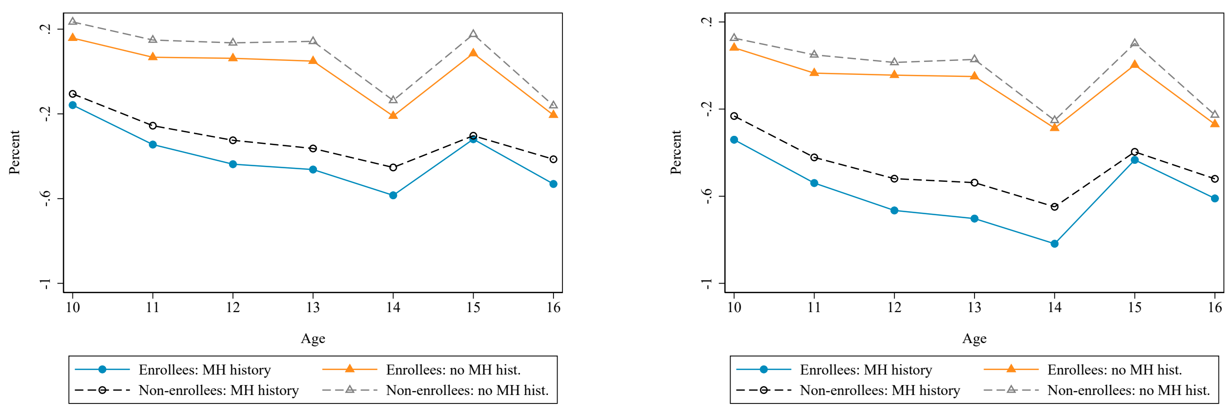
(b) Share Filing Claims



Note: These graphs plot healthcare utilization in a given quarter by enrollment status and mental health history. Panel (a) plots the share of men enrolled in Medicaid and panel (b) plots the share of men filing any Medicaid claims. Non-enrolled men are weighted using the corresponding weights from the matching procedure.

Figure A4: Standardized Test Scores Earlier in Adolescence, by Medicaid Enrollment and Mental Health History (Raw Means)

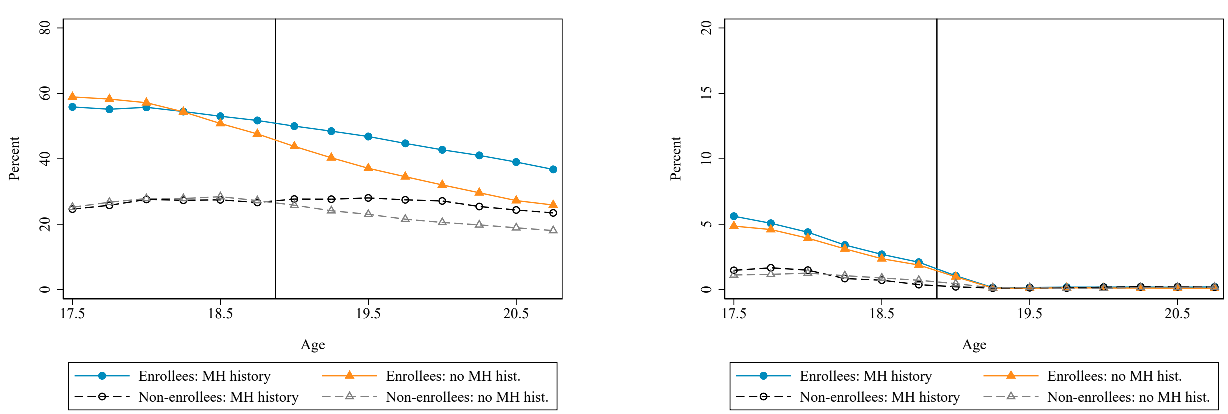
(a) Math score (b) ELA score



NOTE: These graphs plot average standardized test scores in a given year by enrollment status and mental health history. Panel (a) plots average math scores, and panel (b) plots average ELA scores. Non-enrolled men are weighted using the corresponding weights from the matching procedure.

Figure A5: Enrollment in Public Assistance Programs, by Medicaid Enrollment and Mental Health History (Raw Means)

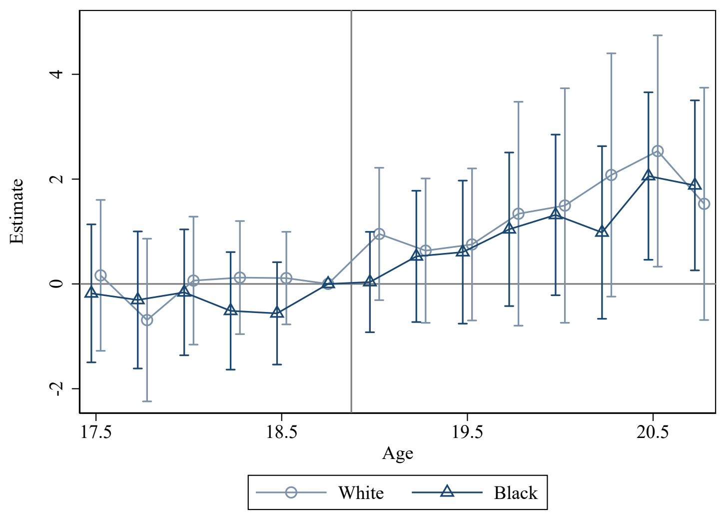
(a) SNAP (b) TANF



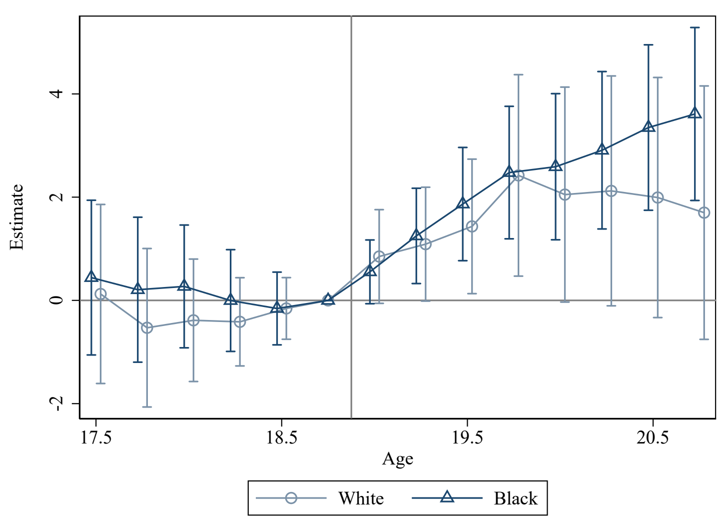
NOTE: These graphs plot program enrollment in a given quarter by enrollment status and mental health history. Panel (a) plots the share of men enrolled in the Supplemental Nutrition Assistance Program (SNAP) and panel (b) plots the share of men enrolled in the Temporary Assistance for Needy Families (TANF) program. Non-enrolled men are weighted using the corresponding weights from the matching procedure.

Figure A6: Likelihood of Incarceration for Men with a Mental Health History, by Race

(a) Incarcerated that quarter

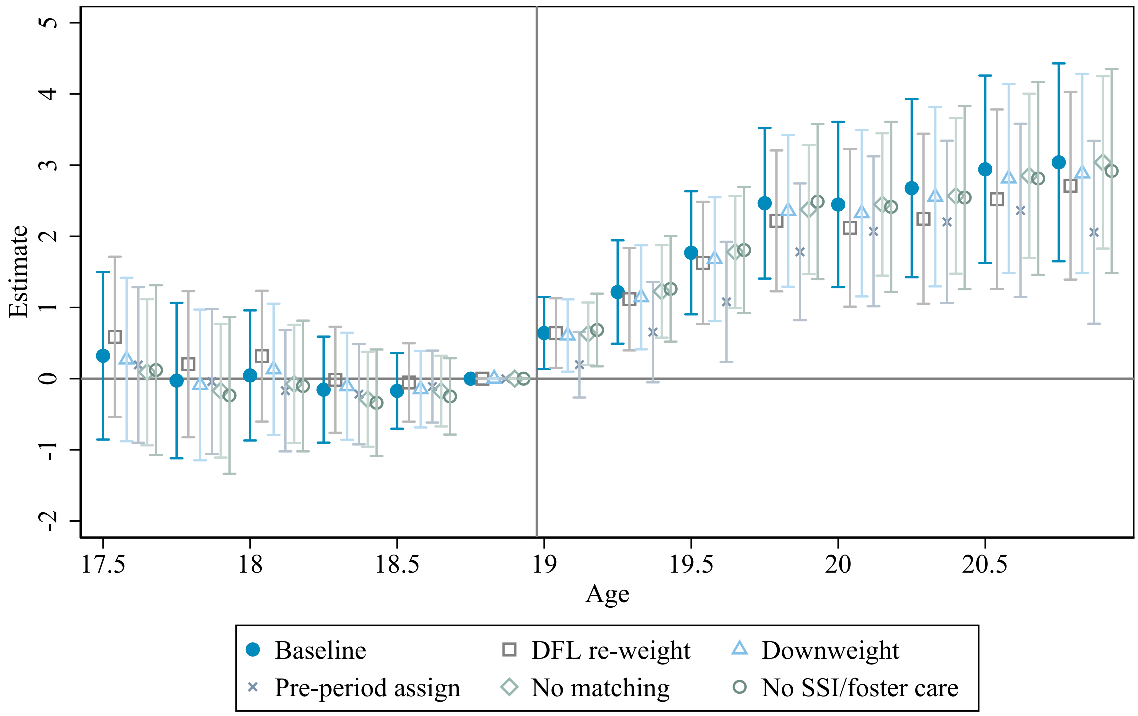


(b) Ever incarcerated



NOTE: These figures plot triple-differences estimates using equation (4) separately by race. "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. Non-enrolled men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level.

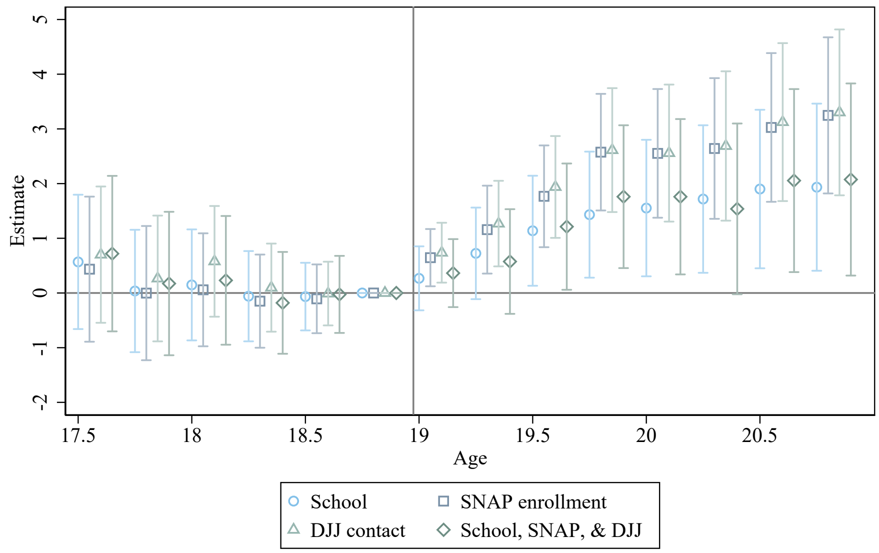
Figure A7: Robustness to Matching, Weighting, & Samples



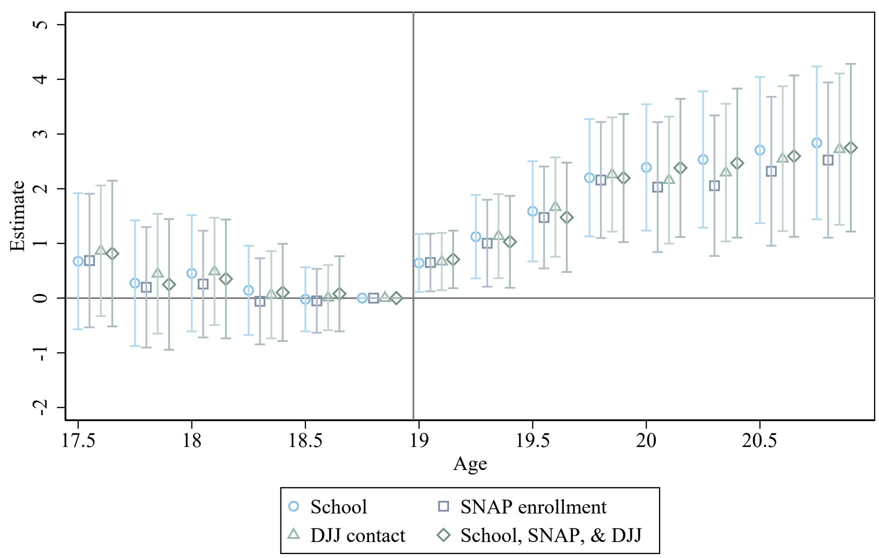
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. Each series plots the β estimates from equation (4). The first series reproduces the baseline estimates from Figure 4. The second series re-weights non-enrollees using the DFL re-weighting approach. The third series drops non-enrollees with large weights from the matching procedure. I calculate the total weight each non-enrolled unit is given in the baseline sample and drop men 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. The fourth series assigns men to enrollment groups based on their Medicaid enrollment in the year and a half before age 19. The fifth series uses all men—enrollees and non-enrollees—without any matching or re-weighting. The sixth series excludes foster care and SSI youth and their matched non-enrolled units. Standard errors are clustered at the individual level.

Figure A8: Robustness of Main Result to Adding Matching Characteristics

(a) Baseline matching procedure



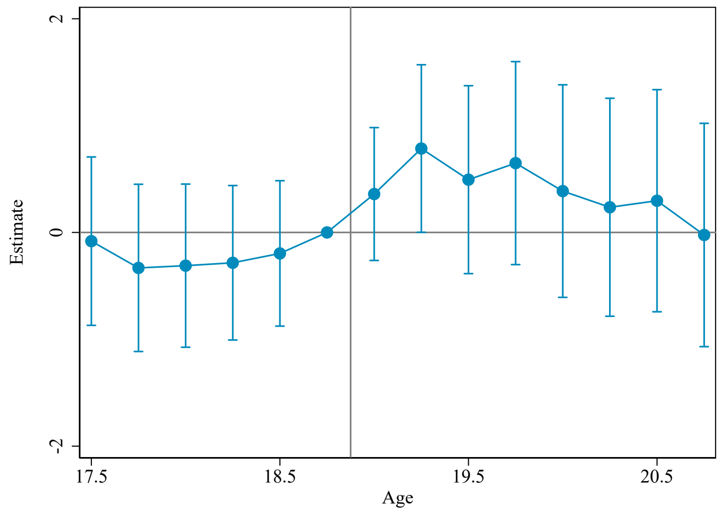
(b) DFL re-weighting



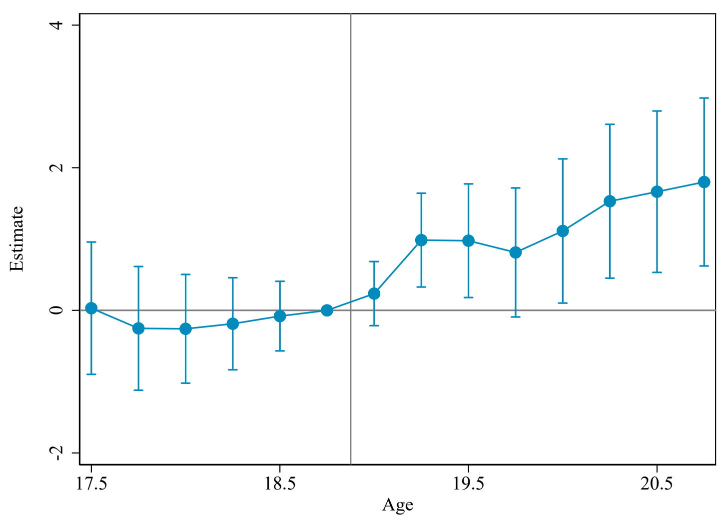
Note: The dependent variable is a measure of whether an individual has ever been incarcerated. Each series plots the β estimates from equation (4). Panel (a) uses the baseline matching procedure described in Section 4.3 and Appendix B.4. Panel (b) uses the DFL re-weighting procedure (DiNardo et al., 1995; Fortin et al., 2011). In both panels, the first series plots estimates that use school attended, instead of school district, as a matching characteristic. The second series matches men based on the baseline characteristics and SNAP enrollment in adolescence (ever enrolled between ages 10–18). The third series matches men based on the baseline characteristics and any Department of Juvenile Justice (DJJ) contact between ages 10–16. The fourth series matches men based on school attended, SNAP enrollment, and DJJ contact. Standard errors are clustered at the individual level.

Figure A9: Difference-in-Differences Estimates for Likelihood of Incarceration among Enrollees with Mental Health Histories Using Recency of Mental Health Claims

(a) Incarcerated that quarter

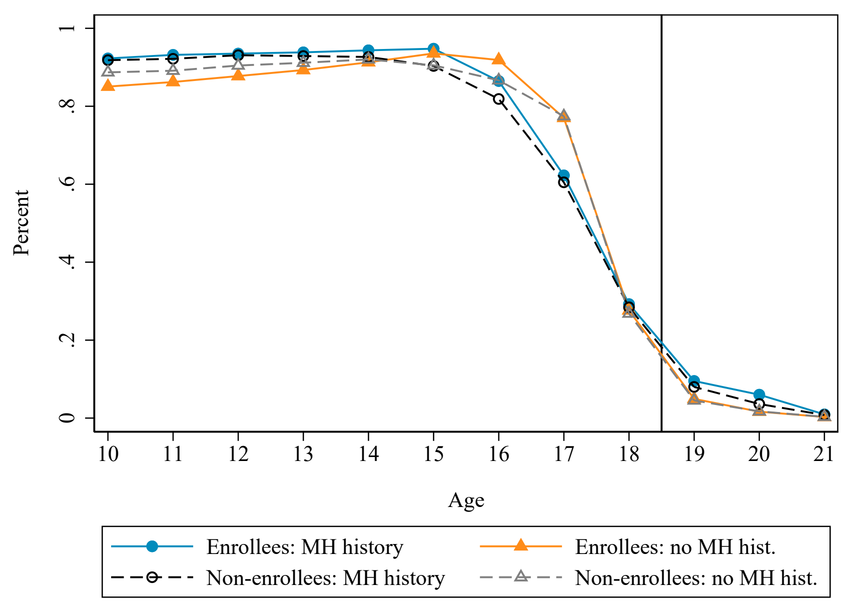


(b) Ever incarcerated



Note: This figure considers enrollees with mental health histories in the baseline sample and plots difference-in-differences estimates in which "treated" men are those with mental health claims in the six quarters before their 19th birthdays and "control" men are those with no mental health claims in that same period. Similar to the baseline approach, a matching procedure is implemented 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. Estimates and more details on the regression specifications are reported in Appendix Table A7.

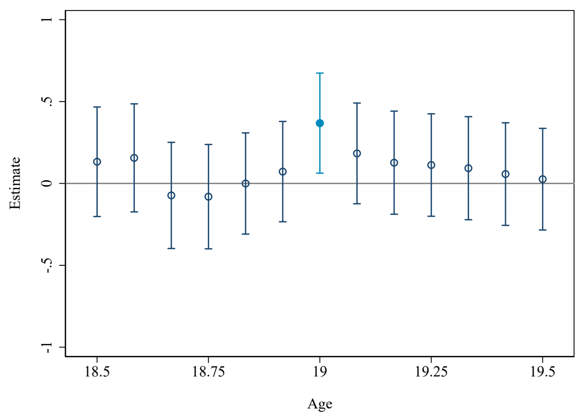
Figure A10: Share of Men Enrolled in Public School, by Medicaid Enrollment and Mental Health History (Raw Means)



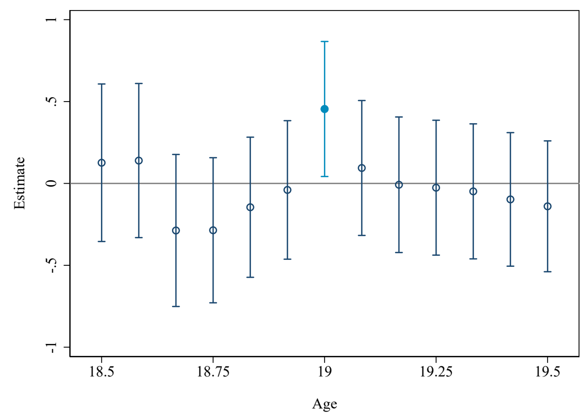
Note: This graph plots public school enrollment in a given year by enrollment status and mental health history. Data on school enrollment comes from South Carolina's Department of Education, and being enrolled refers to appearing in that year's student census. Non-enrolled men are weighted using the corresponding weights from the matching procedure.

Figure A11: Regression Discontinuity Estimates Around Months Before And After 19th Birthday

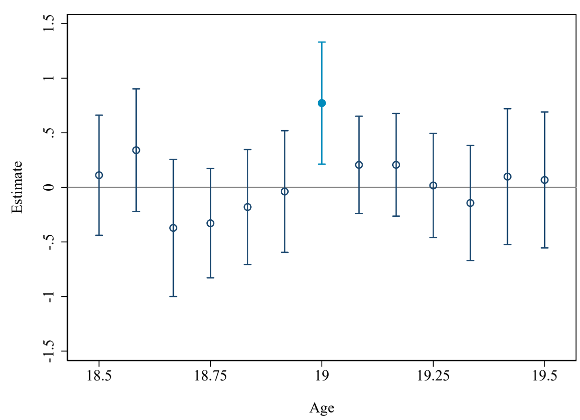
(a) Quadratic



(b) Cubic



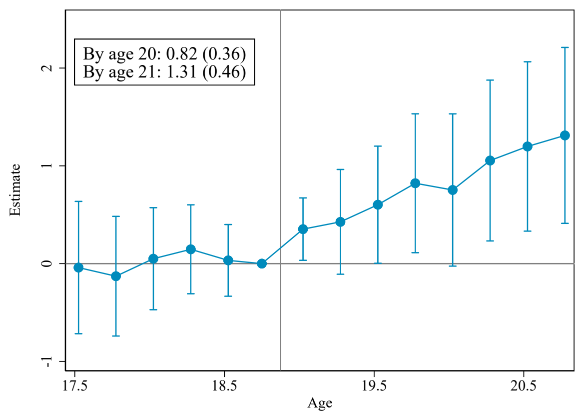
(c) Non-parametric



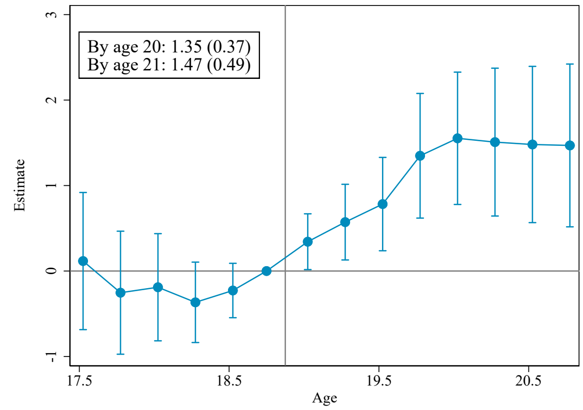
Note: This figure considers arrest probabilities for men with mental health histories (i.e., men with a mental health diagnosis or medication between ages 16–18). Each estimate corresponds to the estimated discontinuity around a specific age in the six months before and after an individual's 19th birthday. Panels (a) and (b) use equation (5) and quadratic and cubic polynomials, respectively. Panel (c) presents non-parametric estimates using the Stata program "rdrobust" (Calonico et al., 2022). Standard errors are clustered at the individual level. To be conservative, estimates exclude the first month of age 17 due to a lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Figure A12: Likelihood of Ever Being Incarcerated, by Offense Type

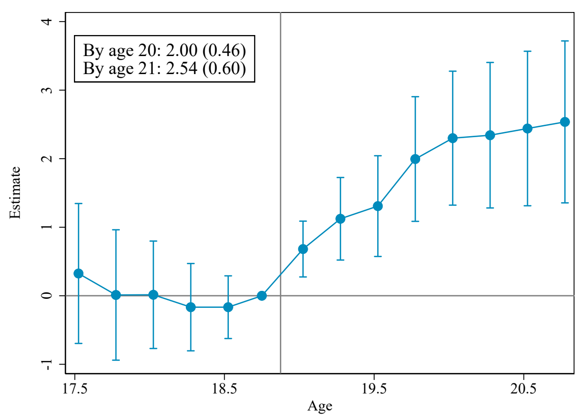
(a) Violent offenses



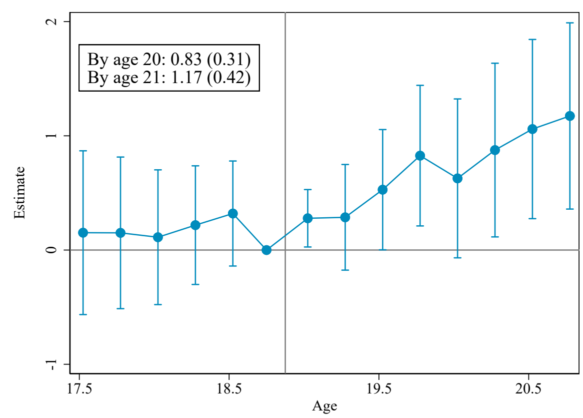
(b) Property offenses



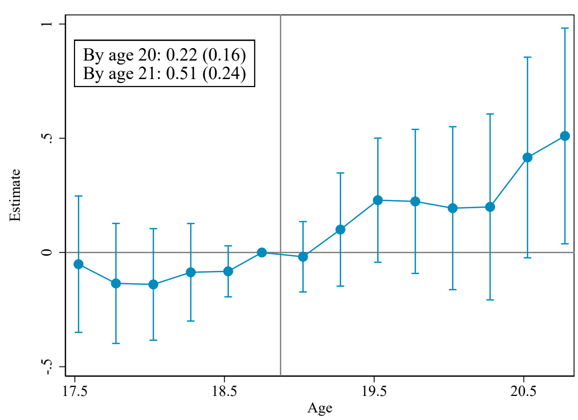
(c) Financially motivated offenses



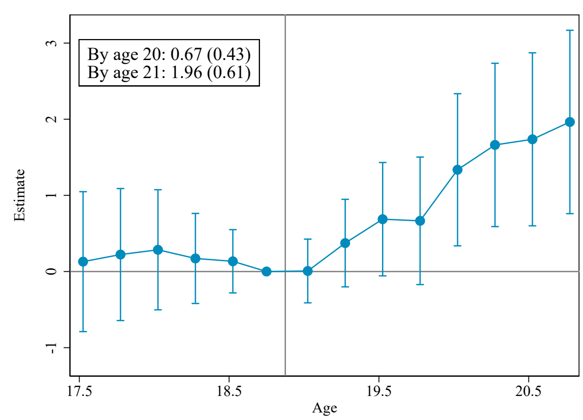
(d) Non-financial violent offenses



(e) Drug & alcohol possession



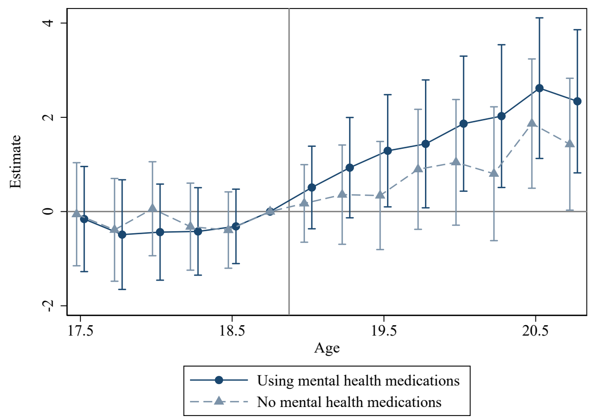
(f) Other offenses



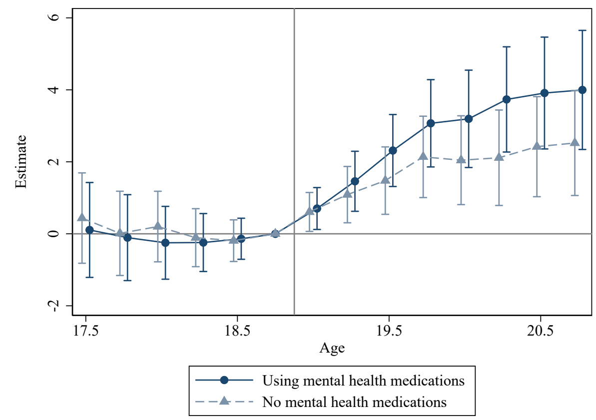
Note: The dependent variable is a measure of whether an individual has ever been incarcerated. These figures plot triple-differences estimates using equation (4) and the reported estimates refer to the β_3 and β_7 coefficients. Standard errors are clustered at the individual level. "Other offenses" refers to crimes that were not classified in panels (c)–(e). For more details on offense classifications, see Appendix B.

Figure A13: Effect of Losing Mental Healthcare on Likelihood of Incarceration, by Mental Health Medication Usage

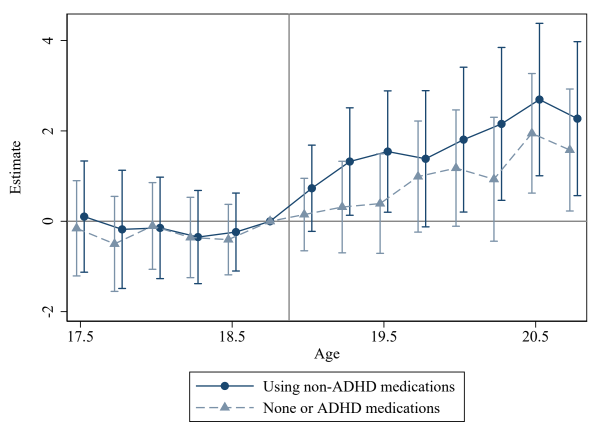
(a) Incarcerated – all drugs



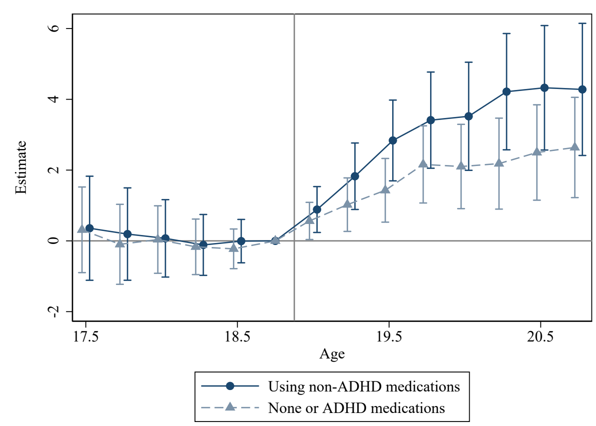
(b) Ever incarcerated – all drugs



(c) Incarcerated – non-ADHD drugs



(d) Ever incarcerated – non-ADHD drugs



NOTE: These figures plot triple-differences estimates using equation (4). Each model restricts the sample of enrollees with mental health histories based on their mental health medication usage. In all models, the matched group of non-enrollees with mental health histories is also restricted to maintain balance on observable characteristics. In panels (a) and (b), medication utilization is defined as filing a claim for a mental health drug between the ages of 16 and 18. In panels (c) and (d), medication utilization is restricted to antianxiety, antidepressant, or antipsychotic medications between the ages of 16 and 18. "Incarcerated" 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. Non-enrolled men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level.

Table A1: Summary Statistics by Medicaid Enrollment and Mental Health History

	Enrollees		Non-enrollees	
	(1) MH history	(2) No MH history	(3) MH history	(4) No MH history
Black	70.19	75.41	70.19	75.41
Age of first diagnosis	11.35	.	11.19	.
Number of diagnoses	2.21	.	1.58	.
Juvenile justice referral	52.12	26.97	43.14	22.63
Arrests	32.93	18.97	25.89	17.00
SNAP	88.70	87.97	74.95	71.21
TANF	30.18	24.83	18.17	12.43
SSI	19.59	2.95	4.88	1.35
Foster care	7.13	1.25	3.25	0.30
Observations	11,866	9,552	3,061	5,850

NOTE: Columns 1 and 2 report means for men enrolled in Medicaid at ages 16.5–17.5 (enrollees). Columns 3 and 4 report means for men enrolled in Medicaid earlier in adolescence, but not between the ages of 16.5 and 17.5 (non-enrollees). This table only includes individuals who were successfully matched using the matching procedure. Non-enrolled men are weighted using the corresponding weights from the matching procedure. "MH 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 categorized disorders. Every other outcome is measured between the ages of 10 and 18. For more details on the matching procedure and variable definitions, see Appendix B.

Table A2: Effect of Losing Mental Healthcare on Likelihood of Incarceration, by Race

	(1) Incarcerated	(2) Ever Incarcerated
A. White Men		
One-year effect	0.954*	2.422**
	(0.546)	(0.995)
Two-year effect	1.449**	1.700
	(0.666)	(1.252)
Outcome mean: 1-year	2.64	7.30
Outcome mean: 2-year	3.02	10.35
Observations	122,948	122,948
B. Black Men		
One-year effect	0.844*	2.476***
	(0.487)	(0.655)
Two-year effect	1.346***	3.611***
	(0.484)	(0.855)
Outcome mean: 1-year	5.42	12.96
Outcome mean: 2-year	5.78	16.04
Observations	301,658	301,658

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports triple-differences estimates separately by race. Column one reports estimates using equation (3). "One-year effect" quantifies the effect on incarceration at age 19. "Two-year effect" quantifies the effect on incarceration at ages 19–20. Column two reports estimates using equation (4). The one- and two-year estimates correspond to the β_3 and β_7 coefficients, respectively. "Outcome mean" refers to the average incarceration rate of non-enrolled men with mental health histories in the corresponding sample and time period. Standard errors are clustered at the individual level. The estimates in this table are analogous to the estimates presented in Figure A6.

Table A3: Effect of Losing Mental Healthcare on Likelihood of Arrest

	(1) All arrests	(2) Serious arrests	(3) Felony offenses	(4) Non-felony offenses
Two-year effect	0.083	0.626***	0.500***	0.126*
	(0.373)	(0.164)	(0.140)	(0.073)
Outcome mean	6.44	1.21	0.93	0.29
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. This table uses arrest data from the South Carolina Law Enforcement Division. Each column reports the β_1 estimate using equation (3). The first column considers all arrests. The second column considers serious arrests for which the individual was detained in a correctional facility. The third column considers serious arrests for which the individual was detained in a correctional facility with an associated felony offense. The fourth column considers arrests that ended with an individual being detained in a correctional facility, but with no associated felony offense. "Outcome mean" refers to the post-period average outcome of non-enrollees with mental health histories. Standard errors are clustered at the individual level.

Table A4: Effect of Losing Mental Healthcare on Likelihood of Incarceration, by Recency of Mental Health Claims

	(1) Incarcerated	(2) Ever Incarcerated
A. Less Recent		
One-year effect	0.641	2.065***
	(0.406)	(0.573)
Two-year effect	1.190***	2.492***
	(0.415)	(0.737)
Outcome mean: 1-year	4.72	11.48
Outcome mean: 2-year	5.01	14.39
Observations	364,700	364,700
B. More Recent		
One-year effect	1.295***	3.166***
	(0.416)	(0.612)
Two-year effect	1.643***	4.007***
	(0.435)	(0.833)
Outcome mean: 1-year	4.35	10.90
Outcome mean: 2-year	4.86	14.27
Observations	317,100	317,100

NOTE: Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1. This table reports triple-differences estimates separately by recency of mental health claims. "More Recent" and "Less Recent" refer to models that restrict enrollees with mental health histories based on whether they did or did not file a mental health claim in the year and a half before age 19, respectively. In both sets of models, the group of matched non-enrollees with mental health histories is also restricted to maintain balance on observable characteristics. Column 1 reports estimates using equation (3). "One-year effect" quantifies the effect on incarceration at age 19. "Two-year effect" quantifies the effect on incarceration at ages 19–20. Column 2 reports estimates using equation (4). The one- and two-year estimates correspond to the β_3 and β_7 coefficients, respectively. "Outcome mean" refers to the average incarceration rate of non-enrolled men with mental health histories in the corresponding sample and time period. Standard errors are clustered at the individual level. The estimates in this table are analogous to the estimates presented in Figure 5.

Table A5: Robustness to Clustering of Standard Errors and Fixed Effects

	Incarcerated		Ever Incarcerated	
	One-year	Two-year	One-year	Two-year
	(1)	(2)	(3)	(4)
Baseline	0.88**	1.36***	2.46***	3.04***
	(0.38)	(0.39)	(0.54)	(0.71)
Cluster: match level	0.88***	1.36***	2.46***	3.04***
	(0.18)	(0.18)	(0.27)	(0.36)
Cluster: match & individual	0.88**	1.36***	2.46***	3.04***
	(0.38)	(0.39)	(0.54)	(0.71)
Individual fixed effects	0.88**	1.36***	2.46***	3.03***
	(0.38)	(0.39)	(0.54)	(0.71)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. Columns 1 and 2 report one- and two-year effects using equation (3). Columns 3 and 4 report the β_3 and β_7 coefficients from equation (4), respectively. The first row reproduces the baseline estimates in Figure 4. The next two rows cluster the standard errors at the match level and at the match and individual level, respectively. The final row estimates the baseline regression models with individual-level fixed effects.

Table A6: Robustness to Matching, Weighting, & Samples

	Incarcerated		Ever Incarcerated	
	One-year	Two-year	One-year	Two-year
	(1)	(2)	(3)	(4)
(1) Baseline	0.88**	1.36***	2.46***	3.04***
	(0.38)	(0.39)	(0.54)	(0.71)
(2) DFL re-weighting, baseline	0.84**	1.07***	2.22***	2.71***
	(0.39)	(0.39)	(0.51)	(0.67)
(3) Downweighting outliers	0.86**	1.27***	2.36***	2.88***
	(0.38)	(0.39)	(0.54)	(0.71)
(4) Assignment in pre-period	0.39	0.60*	1.78***	2.06***
	(0.35)	(0.35)	(0.49)	(0.66)
(5) No matching	0.97***	1.34***	2.38***	3.04***
	(0.34)	(0.34)	(0.46)	(0.62)
(6) Matching on school	0.63	0.86**	1.43**	1.93**
	(0.41)	(0.42)	(0.59)	(0.78)
(7) Matching on SNAP enrollment	0.96**	1.39***	2.58***	3.25***
	(0.41)	(0.40)	(0.54)	(0.73)
(8) Matching on DJJ contact	0.71*	1.16***	2.61***	3.30***
	(0.40)	(0.42)	(0.58)	(0.77)
(9) Matching on school, SNAP, & DJJ	0.54	0.83*	1.76***	2.08**
	(0.46)	(0.45)	(0.67)	(0.90)
(10) DFL re-weighting, school	0.73*	1.11***	2.20***	2.84***
	(0.42)	(0.42)	(0.55)	(0.71)
(11) DFL re-weighting, SNAP	0.75*	0.94**	2.16***	2.52***
	(0.41)	(0.42)	(0.54)	(0.72)
(12) DFL re-weighting, DJJ	0.82**	1.07***	2.26***	2.72***
	(0.40)	(0.41)	(0.53)	(0.71)
(13) DFL re-weighting, all	0.73*	1.09**	2.19***	2.75***
	(0.42)	(0.44)	(0.60)	(0.78)
(14) Excluding SSI & foster care	0.98**	1.39***	2.49***	2.92***
	(0.39)	(0.40)	(0.56)	(0.73)

Note: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. Columns 1 and 2 report one- and two-year effects using equation (3). Columns 3 and 4 report the β_3 and β_7 coefficients from equation (4), respectively. Row 1 reproduces the baseline estimates. Row 2 uses the DFL re-weighting approach to re-weight non-enrollees. Row 3 drops non-enrolled men with disproportionate weight in the regression. Row 4 assigns men to an enrollment group based on their enrollment in the year and a half before their 19th birthdays. Row 5 uses all eligible enrollees and non-enrollees (no matching). Row 6 matches men based on school attended. Row 7 matches men based on baseline characteristics and SNAP enrollment. Row 8 matches men based on baseline characteristics and DJJ contact. Row 9 matches men based on school, SNAP enrollment, and DJJ contact. Rows 10–13 change the matching characteristics analogously to rows 6–9, respectively, but use the DFL re-weighting approach. Row 14 excludes foster care and SSI youth and their matched non-enrolled units. Standard errors are clustered at the individual level. Appendix Figures A7 and A8 show graphical depictions of columns 3 and 4.

Table A7: Difference-in-Differences Estimates for Likelihood of Incarceration among Enrollees with Mental Health Histories Using Recency of Mental Health Claims

	(1) Incarcerated	(2) Ever Incarcerated
One-year effect	0.775**	0.812*
	(0.312)	(0.462)
Two-year effect	0.597*	1.800***
	(0.320)	(0.601)
Outcome mean: 1-year	5.16	13.70
Outcome mean: 2-year	6.11	17.50
Observations	168,756	168,756

Note: Stars report statistical significance: *** = p-value $<$ 0.01, ** = p-value $<$ 0.05, * = p-value $<$ 0.1. This table considers enrollees with mental health histories in the baseline sample. "Treated" and "control" individuals refer to men with and without mental health claims in the six quarters before their 19th birthdays, respectively. The table reports estimates using equation:

$$Y_{it} = \beta_0 \text{Treat}_i + \beta_1 \text{Post}_t + \beta_2 \text{Treat}_i \times \text{Post}_t + \delta_m + \epsilon_{it}$$

Similar to the baseline approach, a matching procedure is implemented prior to estimation so that the treatment and control groups are balanced on race, year of birth, school district. "One-year effect" quantifies the effect on incarceration at age 19. "Two-year effect" quantifies the effect on incarceration at ages 19–20. Column (2) reports estimates using a cumulative variable and analogous dynamic specification:

$$Y_{it} = \sum_{\tau=-6}^{\tau=7} [\beta_\tau (\text{Treat}_i \times \gamma_\tau) + \theta_\tau \gamma_\tau] + \mu \text{Treat}_i + \delta_m + \epsilon_{it}$$

The one- and two-year estimates correspond to the β_3 and β_7 coefficients, respectively. "Outcome mean" refers to the average incarceration rate of comparison men in the corresponding time period. Standard errors are clustered at the individual level. Appendix Figure A9 shows graphical depictions of the estimates.

Table A8: Placebo Check of Likelihood of Incarceration Around Earlier Ages

	(1) Age 17	(2) Age 18	(3) Age 19
A. Incarcerated			
Adult incarcerations	—	-0.089	0.849**
		(0.379)	(0.380)
All incarcerations	0.366	-0.133	0.849**
	(0.440)	(0.379)	(0.380)
B. Ever incarcerated			
Adult incarcerations	—	0.337	2.443***
		(0.543)	(0.539)
All incarcerations	-0.092	-0.014	1.699***
	(0.737)	(0.441)	(0.470)
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 results in which the baseline empirical approach, depicted in Figure A2, is replicated around ages 17 and 18. In all models, there are four quarters in the pre-period and four quarters in the post-period. In the first panel, the dependent variable measures whether an individual is incarcerated in a given quarter and estimates come from equation (3). In the second panel, the dependent variable is the cumulative analogue and estimates correspond to the β_3 coefficient using equation (4). “Adult incarcerations” refer to being detained in an adult correctional facility. “All incarcerations” refer to being detained in an adult correctional facility or juvenile facility. Standard errors are clustered at the individual level.

Table A9: Robustness of Discontinuity in Arrest Probability Around Age 19

	MH claim, Age 10–18 (1)	No MH claim, Age 10–18 (2)	MH claim, Age 16–18 (3)	No MH claim, Age 16–18 (4)
Parametric: quadratic	0.203*	0.051	0.368**	0.024
	(0.108)	(0.081)	(0.156)	(0.073)
Parametric: cubic	0.257*	-0.088	0.455**	-0.060
	(0.145)	(0.110)	(0.210)	(0.097)
Non-parametric: local	0.349*	-0.147	0.772***	-0.062
	(0.212)	(0.151)	(0.285)	(0.108)
Pre-period Average	2.761	1.074	3.386	1.377
Observations	839,890	618,379	485,134	973,135

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers men's probability of being arrested around their 19th birthdays. Parametric estimates report the discontinuity using equation (5) and its standard error (clustering at the individual level). The first two rows use a quadratic and cubic polynomial, respectively. The third row presents non-parametric estimates using the Stata program "rdrobust" (Calonico et al., 2022). The first and second columns consider men with and without a mental health claim between the ages of 10 and 18, respectively. The third and fourth columns consider men with and without a mental health claim between the ages of 16 and 18, respectively. To be conservative, estimates exclude the first month of age 17 due to a lack of information on an individual's exact birth date and the timing of the age of criminal majority.

Table A10: Discontinuity in Arrest Probability Around Age 19 by Crime Type

	MH claim			No MH claim		
	(1) Violent	(2) Property	(3) Other	(4) Violent	(5) Property	(6) Other
Discontinuity	0.014	0.185**	0.154	0.008	0.009	0.023
	(0.074)	(0.080)	(0.122)	(0.034)	(0.036)	(0.058)
Pre-period Average	0.71	0.94	1.96	0.27	0.35	0.81
Observations	485,134	485,134	485,134	973,135	973,135	973,135

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers men's probability of being arrested around their 19th birthdays separately by crime type. Estimates report the discontinuity from equation (5) using a quadratic polynomial. The first three columns consider men with a mental health claim between ages 16–18, and the second three columns consider men with no mental health claim in this age range. "Other" refers to arrests that are neither for violent nor property crimes. To be conservative, estimates exclude the first month of age 17 due to a lack of information on an individual's exact birth date and the timing of the age of criminal majority. Standard errors are clustered at the individual level.

Table A11: Effect of Losing Mental Healthcare on Likelihood of Incarceration, by Mental Health Medication Usage

	No Medications		Medications	
	(1) Incarcerated	(2) Ever Incarcerated	(3) Incarcerated	(4) Ever Incarcerated
A. All Medications				
One-year effect	0.627	2.137***	1.346***	3.070***
	(0.408)	(0.577)	(0.416)	(0.619)
Two-year effect	1.047**	2.523***	1.930***	3.997***
	(0.417)	(0.744)	(0.439)	(0.844)
Outcome mean: 1-year	4.74	11.58	4.30	10.70
Outcome mean: 2-year	5.09	14.60	4.71	13.88
Observations	366,814	366,814	314,748	314,748
B. Non-ADHD Medications				
One-year effect	0.716*	2.161***	1.380***	3.412***
	(0.390)	(0.556)	(0.472)	(0.693)
Two-year effect	1.189***	2.639***	1.872***	4.280***
	(0.400)	(0.723)	(0.496)	(0.953)
Outcome mean: 1-year	4.57	11.24	4.66	11.37
Outcome mean: 2-year	4.91	14.24	5.12	14.70
Observations	384,356	384,356	296,590	296,590

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports triple-differences estimates in which the sample of enrollees with mental health histories is restricted based on their mental health medication usage. In all models, the matched group of non-enrollees with mental health histories is also restricted to maintain balance on observable characteristics. Columns 1 and 3 report estimates using equation (3). "One-year effect" quantifies the effect on incarceration at age 19. "Two-year effect" quantifies the effect on incarceration at ages 19–20. Columns 2 and 4 report estimates using equation (4). The one- and two-year estimates correspond to the β_3 and β_7 coefficients, respectively. In panel (a), medication utilization is defined as filing a claim for a mental health drug between the ages of 16 and 18. In panel (b), medication utilization is defined as filing a claim for antianxiety, antidepressant, or antipsychotic medications between the ages of 16 and 18. "Outcome mean" refers to the average incarceration rate of non-enrolled men with mental health histories in the corresponding sample and time period. Non-enrolled men are weighted using the corresponding weights from the matching procedure. Standard errors are clustered at the individual level. The estimates in this table are analogous to the estimates presented in Appendix Figure A13.

Table A12: Discontinuity in Arrest Probability Around Age 19 for Recent Beneficiaries, by Mental Health Medication and Diagnosis

	Arrest Estimate
A. Mental health medications:	
Mental health drug	0.10
	(0.20)
No mental health drug	0.66***
	(0.25)
B. Excluding Diagnosis:	
Intellectual disabilities	0.38**
	(0.17)
Neurodevelopmental disorder	0.29
	(0.19)
Depressive disorder	0.30*
	(0.17)
Anxiety disorder	0.44**
	(0.17)
Bipolar disorder	0.38**
	(0.16)
Post-traumatic stress disorder	0.39**
	(0.16)
Conduct disorder	0.30*
	(0.17)
Oppositional defiant disorder	0.21
	(0.17)
Adjustment disorder	0.40**
	(0.17)
ADHD	0.56**
	(0.22)
ADHD Only	0.36**
	(0.16)
Substance-related & addictive disorder	0.16
	(0.16)
Other unclassified disorder	0.42**
	(0.17)
Physical condition	0.43**
	(0.21)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers men's probability of arrest around their 19th birthdays. The sample is recent mental health beneficiaries (i.e., men filing a mental health claim between ages 16–18). Estimates report the discontinuity from equation (5) using a quadratic polynomial. Panel (a) splits the sample by mental health medication usage. Panel (b) excludes individuals who have ever received that diagnosis. "ADHD only" excludes individuals whose sole diagnosis is ADHD. To be conservative, estimates exclude the first month of age 17 due to a lack of information on an individual's exact birth date and the timing of the age of criminal majority. Standard errors are clustered at the individual level. For details on the classification of diagnoses, see Appendix B.

Table A13: Distribution of Mental Health Medications and Diagnoses

	(1) All men	(2) Enrollees with MH history
A. Mental health medications		
Any psychotropic drug	33.86	67.21
ADHD drug	22.67	47.45
Antidepressant drug	13.72	28.70
Antipsychotic drug	9.12	20.17
Antianxiety drug	11.30	22.68
B. Mental health diagnoses		
ADHD	24.95	52.35
Depressive disorder	9.61	20.75
Anxiety disorder	7.12	14.74
Adjustment disorder	7.58	15.99
Conduct disorder	15.05	32.03
Oppositional defiant disorder	12.49	27.43
Bipolar disorder	3.20	7.04
Intellectual disabilities	7.17	16.53
Other neurodevelopmental disorder	18.04	38.53
Substance-related and addictive disorder	15.33	29.40
Post-traumatic stress disorder	1.46	3.18
Other unclassified disorder	5.96	12.19
Number of diagnoses	1.28	2.70
Age of first diagnosis	11.99	11.35
Number of individuals	33,252	11,866

NOTE: This table reports the share of men who have a 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 enrollees with a mental health history in the baseline sample. "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 A14: Effect of Losing Mental Healthcare on Likelihood of Incarceration, Excluding Specific Diagnoses

	Incarcerated		Ever Incarcerated	
	One-year	Two-year	One-year	Two-year
	(1)	(2)	(3)	(4)
Baseline	0.88**	1.36***	2.46***	3.04***
	(0.38)	(0.39)	(0.54)	(0.71)
Excluding:				
Intellectual disabilities	0.99***	1.40***	2.58***	3.13***
	(0.38)	(0.40)	(0.55)	(0.72)
Neurodevelopmental disorder	1.18***	1.62***	2.88***	3.65***
	(0.42)	(0.43)	(0.59)	(0.77)
Depressive disorder	0.73*	1.22***	2.03***	2.75***
	(0.38)	(0.39)	(0.55)	(0.71)
Anxiety disorder	0.84**	1.29***	2.44***	2.92***
	(0.39)	(0.40)	(0.54)	(0.72)
Bipolar disorder	0.80**	1.24***	2.22***	2.70***
	(0.38)	(0.39)	(0.54)	(0.71)
Post-traumatic stress disorder	0.81**	1.34***	2.40***	2.99***
	(0.38)	(0.39)	(0.54)	(0.71)
Conduct disorder	0.16	0.59*	1.38***	1.24*
	(0.34)	(0.34)	(0.51)	(0.67)
Oppositional defiant disorder	0.30	0.82**	1.51***	1.53**
	(0.34)	(0.35)	(0.51)	(0.68)
Adjustment disorder	0.86**	1.34***	2.37***	2.95***
	(0.38)	(0.39)	(0.55)	(0.71)
ADHD	0.46	0.84**	1.79***	1.86**
	(0.40)	(0.40)	(0.57)	(0.75)
ADHD only	0.95**	1.42***	2.66***	3.32***
	(0.39)	(0.40)	(0.55)	(0.72)
Substance-related & addictive disorder	0.23	0.65*	1.23**	1.37**
	(0.33)	(0.33)	(0.49)	(0.66)
Other unclassified disorder	0.83**	1.36***	2.38***	3.15***
	(0.39)	(0.40)	(0.55)	(0.71)
Physical condition	0.96**	1.19***	1.98***	2.33***
	(0.41)	(0.41)	(0.61)	(0.83)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports triple-difference estimates excluding certain subgroups. The first row reproduces the estimates from the baseline sample. All subsequent rows exclude enrollees with mental health histories who had ever received that diagnosis as well as their corresponding non-enrolled matches. "ADHD only" excludes individuals whose sole diagnosis is ADHD. The final row excludes individuals diagnosed with a chronic physical condition. Columns 1 and 2 report one- and two-year effects using equation (3). Columns 3 and 4 report the β_3 and β_7 coefficients, respectively, using equation (4). Standard errors are clustered at the individual level. For details on the classification of diagnoses, see Appendix B.

Table A15: 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	\$14.9	\$19.5	\$24.1
Fiscal costs	\$3.1	\$3.5	\$4.0
Social costs	\$7.3	\$8.6	\$9.9
Total	\$25.3	\$31.6	\$37.8

NOTE: This table reports the calculations from the cost-benefit analysis in Section 9.1. "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.

B 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.[31]

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 who 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.[32] Then, these shares were averaged across the school years, schools were ranked based on the average share, and high schools in the upper half of this distribution were chosen (142 total high schools). Finally, if an individual was ever enrolled in one of these "low-income" high schools—not necessarily in the 2008–2014 range, but at any point during their academic life—he or she was chosen to be in the sample. Individuals attending "higher-income" high schools are not included in this 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. 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. 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 then merge in information at this level of granularity using data from the other agencies.

The six agencies providing data for this project are the Department of Health and Human Services, the South Carolina Law Enforcement Division, the Department of Juvenile Justice, the Department of Corrections, the Department of Social Services, the Department of Education, and the Department of Health and Environmental Control. None of these agencies measure outcomes related to higher education or the labor market, so examination of these outcomes is not possible in this analysis.

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

[32] 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.

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. 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 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

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 diagnoses 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. “Other unclassified disorder” refers to having a mental health diagnosis not captured by the previous eleven categories. Note that individuals can have more than one mental health diagnosis in an insurance claim. 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.[33] 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 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.

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

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. In Appendix Figure A7 and Table A6, I exclude individuals whose payment categories were associated with foster care and SSI during ages 16.5-17.5 when individuals are assigned into enrollment groups.

Given that individuals in foster care and SSI maintain their Medicaid eligibility in adulthood, one alternative would be to use this group of men as a comparison group to estimate the causal effect of the loss of mental healthcare. 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 unsurprising given that SSI and foster care youth are experiencing other shocks that might impact their criminal involvement prior to their 19th birthdays.

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 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.

Other Characteristics. Information on an individual's race and sex comes 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 a mean of zero and a standard deviation of one.

B.4 Matching Procedure

To ensure that the non-enrolled men serve as a suitable group for estimating enrollees' counterfactual outcome paths around age 19, I implement a matching procedure that guarantees balance along key observable characteristics between the two groups. After implementing the sampling restrictions detailed above, there are 22,063 eligible enrollees and 8,964 eligible non-enrollees, which is 93% of the men in the low-income sample who are born in these cohorts and have an available birth month. 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.

I then match each enrollee to all "counterfactual" non-enrollees using a parsimonious set of characteristics and coarsened exact matching, similar to the approach used in Smith et al. (2019). Specifically, I match on 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).

Table B2 reports summary statistics for enrollees and non-enrollees separately by mental health history and prior to implementing the matching procedure. All characteristics are measured starting at age 10 and before an individual's 19th birthday. Table A1 reports means for the successfully matched individuals. A comparison of these tables shows that 96% and 98% of eligible enrollees with and without mental health histories, respectively, were successfully matched to at least one non-enrolled individual.

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
Enrollees	21,418
Non-enrollees	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 assigned to enrollment groups (i.e., aged 16.5–17.5; see Appendix Figure A2). The last two rows show the number of enrollees and non-enrollees after implementing the matching procedure.

Table B2: Summary Statistics of Full Sample by Enrollment and Mental Health Status – Before Matching

	Enrollees		Non-enrollees	
	(1) MH history	(2) No MH history	(3) MH history	(4) No MH history
Black	69.11	74.74	61.86	70.06
Age of first diagnosis	11.34	.	11.19	.
Number of diagnoses	2.22	.	1.57	.
Juvenile justice referral	52.04	27.12	41.13	22.49
Arrests	32.97	19.18	24.70	16.75
SNAP	88.50	87.89	74.03	69.94
TANF	30.11	24.81	17.90	12.17
SSI	19.75	3.02	4.69	1.51
Foster care	7.14	1.27	2.64	0.22
Observations	12,361	9,702	3,073	5,891

NOTE: Columns 1 and 2 report means for men enrolled in Medicaid at ages 16.5–17.5 (enrollees). Columns 3 and 4 report means for men enrolled in Medicaid earlier in adolescence, but not between the ages of 16.5 and 17.5 (non-enrollees). This table includes all eligible candidates for the matching procedure. "MH 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 categorized disorders. Every other outcome is measured between the ages of 10 and 18. Summary statistics for successfully matched individuals are in Table A1.

C Cost-Benefit Analysis of Providing Mental Healthcare

In this appendix, I calculate and discuss the costs and benefits of expanding mental healthcare via 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.[34] 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 shares enrolled in each quarter prior to age 19 (i.e., the shares in Appendix Figure A3). 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 the total cost of providing this group of low-income young men with Medicaid totals $12.8 million.[35] The estimates from this and the next subsection are summarized in Table A15.

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.

I also consider more conservative possibilities in terms of the share of men who would choose to take up the program at ages 19–20. If the same share of individuals who were 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.

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 does not grant them eligibility for full insurance coverage.[36] These more targeted policies would likely be less expensive than

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

[35] In this appendix and 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 discounted using a 3% discount rate.

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

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 4 to calculate the number of violent, property, and drug-related crimes that occurred as a result of the loss of mental healthcare.[37] 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 and Piquero (2009) and Miller et al. (1996) for each subcrime in order to calculate the cost of each average violent, property, and drug offense.[38] The upper-end figures use all of the costs from Cohen and 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 and 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."[39] 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 \$14.9 to \$24.1 million.

C.2.2 Lower Fiscal Costs from Fewer Incarcerations

I then consider the reduced fiscal costs from fewer incarcerations. For this analysis, 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.

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[37] Note that it is not straightforward to compare these estimates to those from studies quantifying the effect of Medicaid expansions on local crime rates. This study relies on arrest and incarceration records of young adults, whereas the latter group of studies rely on incident reports to the police, most of which do not lead to an arrest or incarceration, and which include incidents committed by individuals of all ages.

[38] I note that the costs in Cohen and 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.

[39] 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.

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 40–65% of individuals who are arrested for an offense are sentenced to a prison spell. For each offense, I then multiply the number of incarcerated individuals by the average sentence served in 2009 in South Carolina and by the daily cost per inmate (roughly $45 in 2011).[40] I allow for heterogeneity in the length of sentences served by offense type (Pew Center on the States, 2012). Overall, I find that the fiscal cost would have been reduced by $4 million.

If I use the marginal—as opposed to the average—cost of incarcerating an individual, then the associated 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 $3.1 million. To get a middle-ground estimate, I take an average of the upper and lower bounds.

C.2.3 Lower Social Costs from Fewer Incarcerations

Next, I calculate the reduced social costs from fewer incarcerations. Estimates from Mueller-Smith (2015) suggest that a two-year prison term has economic impacts (in terms of employment and public assistance) of around $34,650 per person and crime impacts (in terms of post-release criminal behavior) between $14,983 and $33,297.[41] 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 $4.8 and $6.6 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 six-month prison terms. I find that the social costs for these individuals range from $2.4 to $3.2 million, which puts the overall total social costs between $7.3 and $9.9 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 and Sprung-Keyser, 2020). Specifically, the MVPF is a ratio of society's willingness to pay for this policy (numerator) to the net cost of the policy to the government (denominator). 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 C2. 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.

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 ν. I begin by using the estimates of the social cost of crimes averted, which are

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[40] 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.

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

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.[42] 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.[43]

Next, I consider the willingness to pay for improved labor market prospects, η, by the individuals who avoided incarceration. In other words, beneficiaries should be willing to pay for the increase in wages they experience from this policy change. To calculate this forgone 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).[44] I then calculate the total forgone income of affected individuals *during* incarceration, allowing for heterogeneity by crime type (given the different average sentence lengths). For the middle- and 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 forgone income in the five years after release.[45]

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).[46] 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

[42] 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).

[43] 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.

[44] 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).

[45] I allow the post-release employment rates to differ by crime category (i.e., individuals who served time for violent offenses have lower post-release employment rates than individuals who served time for property or drug offenses). I calculate post-release forgone income for 5 years, following Mueller-Smith (2015)'s approach, which estimates effects using 5 years of post-charge data.

[46] 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 (Table 7, https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/NationalHealthExpendData/Age-and-Gender).

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). 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). 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 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).[47] Finally, improved labor market prospects for individuals translate to higher tax revenue for the government. Following the approach of Hendren and Sprung-Keyser (2020), I use a 20% tax rate.[48]

[47] I assume that 30% of individuals recidivate and that they serve an average sentence of 28 months.

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

Table C1: Social Costs per Crime

Offense	Percent	Ratio	Estimated Cost Upper	Estimated Cost Middle	Estimated Cost Lower
A. 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
B. 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
C. Drug Crimes					
DUI	2.9	—	$29,447	—	$4,074
All other	97.1	—	$0	—	$0
Avg. Drug Crime			$854	$486	$118

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 and Piquero (2009) (victim costs in Table 5, inflated to 2010 dollars) and Miller et al. (1996) (Table 2, inflated to 2010 dollars).

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

	Estimated Cost		
	Upper	Middle	Lower
Willingness to Pay:			
Fewer crime victimizations, ν	30,317,018	13,118,966	10,058,220
Improved labor market prospects, η	1,937,751	1,051,093	796,561
Value of insurance transfer, γ	2,665,765	10,086,578	8,757,327
Aggregate willingness to pay	34,920,533	24,256,637	19,612,109
Costs to the Government:			
Cost of providing Medicaid, function of G	5,833,376	14,583,441	14,583,441
Fewer incarcerations, μ	–4,967,718	–2,616,009	–2,077,315
Foregone tax revenue, 0.2η	–387,550	–210,219	–159,312
Net Cost	478,108	11,757,213	12,346,814
Marginal Value of Public Funds	73.04	2.06	1.59

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 mental healthcare provision via the Medicaid program to the cost of harsher punishments in Appendix E.

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 a felony offense 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).[49]

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 potential misclassification in ages, I shifted all non-technical referrals that occurred at age 17–month 1 to age 16–month 12.[50] 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

[49] To illustrate this logic, consider the case of assaults: 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.

[50] Technical referrals are 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.

felonies using South Carolina's Judicial Branch CDR codes.[51] 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 and 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 logit specification:

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

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 and 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

[51] 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.

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).

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 5.3 (i.e., using 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.[52] 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 and McCrary (2017)'s reduced-form elasticity. If I instead use the average time served, then the corresponding elasticity is -0.017.

D.5 Potential Confounding Effect of Minimum Dropout Age

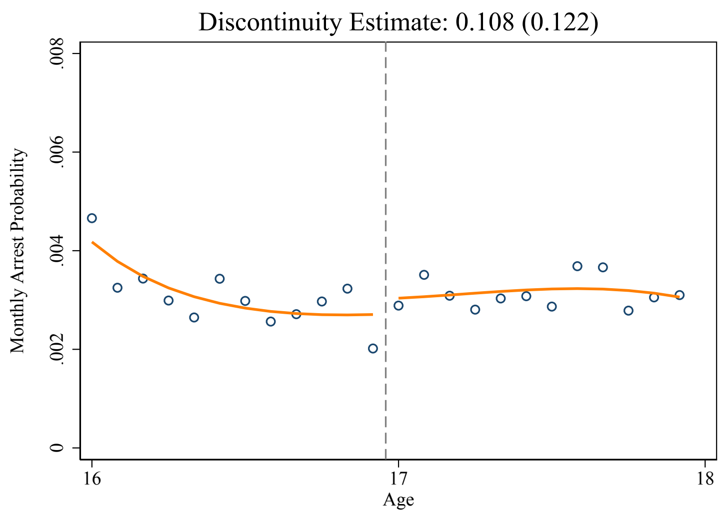
Because the age at which adolescents can legally drop out of school coincides with the age of criminal majority, there might be concerns that the deterrence estimate is combining the effect of

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

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 (see, e.g., Anderson, 2014), 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 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



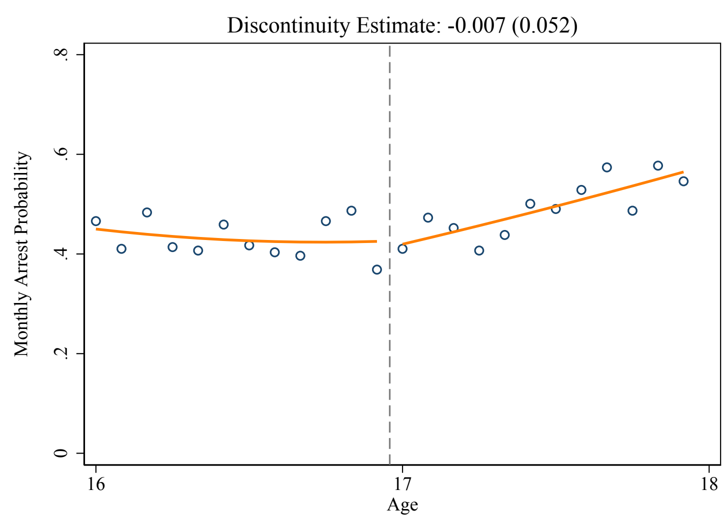
Note: This figure plots the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. The sample consists of men who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (6). The reported estimate corresponds to the discontinuity estimate from this equation and its standard error 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: This figure plots the monthly estimates of the likelihood of a felony arrest around an individual's 17th birthday. The sample consists of men 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 (5). The estimate above the figure reports the discontinuity estimate from this equation and its standard error clustering 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 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 of Medicaid vs. More 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 mental healthcare via Medicaid, increasing sentence lengths, and hiring more police officers. Across these policies, I consider the cost of each policy for reducing crime by the same amount (18% based on the estimates from Table 4).[53]

E.1 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 committed crimes. In other words, even if some offenders were deterred from committing offenses, there would still be a significant number of 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).

Importantly, because these individuals are serving relatively shorter sentence lengths (i.e., the status quo length of sentences), there is a chance that they will re-offend after serving their prison spell.[54] 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).[55]

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 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.

Summing these components together, I find that the total cost of this approach is roughly $55.3 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

[53] The estimate in column 1 of Table 4 suggests that among men born in these cohorts, there were roughly 594 excess serious arrests (11,866 × 0.00626 × 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 18% lower in this age group if Medicaid eligibility had not suddenly expired at age 19.

[54] 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 given that criminal behavior declines with age.

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

released and only takes into account the potential costs from re-offending.

E.2 Comparison to Longer Prison Spells

To calculate the cost of this crime-reduction approach, I use the preferred estimate from Appendix D for 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 roughly 1,000% 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 $113 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 -0.05, which is more than 3 times as large as the preferred elasticity.

E.3 Comparison to Hiring Police Officers

Another favored crime-reduction approach for the past fifty years has been to hire more police officers, and a number of studies have estimated the effect of police presence on criminal activity (e.g., Chalfin and McCrary, 2018; Evans and Owens, 2007). 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 million.[56]

Similar to the analysis investigating the cost of longer sentence lengths, I use the elasticity of crime to police, $\varepsilon_{c,p}$, 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 and Owens (2007) ($\varepsilon_{c,p} = -0.34$)[57], I find that the state would need to increase the overall size of its police force by around 50%.[58] Assuming a marginal cost of $130,000 for hiring a police officer (Chalfin and McCrary, 2018), the fiscal cost of this policy amounts to $1.6 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

[56] When comparing Medicaid provision to longer sentence lengths, possible recidivism from using shorter sentence lengths needs to be accounted for to yield 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.

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

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

(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 and Owens (2007) to estimate how many fewer arrests there would be given the increased number of police officers.[59] 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 $1.7 billion and $29 million, respectively. Finally, there is an additional reduction in fiscal costs of $36 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).

These results suggest that the benefits of hiring more police officers may 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 reduce 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 $700 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 excessive force; see Ang, 2021; Tebes and Fagan, 2022). 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; see Gross and Notowidigdo, 2011). Consequently, even though I am making a relatively parallel comparison for these policies—estimating the cost of a 18% crime reduction—it is likely the case that these calculations are underestimating the benefits of providing low-income young adults with access to Medicaid.

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

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	\$549.5	−\$97.3
Ages 17–30	\$1,049.3	\$703.8
B. Marginal cost: $73,000		
Ages 17–40	−\$155.2	−\$802.1
Ages 17–30	\$344.6	−\$905.2

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 and McCrary (2018) and Evans and 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.