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

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Individuals with mental illness are significantly over-represented in the incarcerated population. This paper studies whether increasing access to mental healthcare could reduce the likelihood that mentally ill individuals commit criminal offenses. Specifically, I study the effect of losing insurance coverage on low-income men's likelihood of incarceration using administrative data from South Carolina that is linked across state government agencies. Leveraging a discrete break in Medicaid coverage at age 19 and a difference-in-differences strategy, I find that men who lose access to Medicaid eligibility are 14% more likely to be incarcerated in the subsequent two years relative to a matched comparison group. The effects are entirely driven by men with mental health histories, suggesting that losing access to mental healthcare plays an important role in explaining the observed rise in crime. By their 21st birthdays, men with mental health histories who lost Medicaid coverage are 21% more likely to have been incarcerated than the comparison group. Cost-benefit analyses show that expanding Medicaid eligibility to low-income young men is a cost-effective policy for reducing crime, especially relative to traditional approaches like increasing the severity of criminal sanctions.

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1 Introduction

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

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

This paper revisits the role of mental healthcare in helping reduce criminal activity. 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. On any given day, over one million people with mental illness are in jail, prison, probation, or parole (Frank & McGuire 2010). Figure 1 plots the cumulative likelihood of incarceration for low-income men with and without prior mental health diagnoses using the primary data source in this paper. Low-income men *with* a mental health history were almost three times more likely to have been incarcerated by age 24 than men *without* a mental health history.

An additional reason for revisiting the relationship between healthcare and criminal behavior is the significant scientific progress that has been made over the past fifty years in understanding

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

and treating mental illness, including important advances in the development and availability of mental health medications (Frank & Glied 2006, Kendler 2019).

Despite these motivating factors, the lack of data linkages between state agencies in the U.S. has served as a perennial obstacle to assessing the efficacy of mental healthcare in deterring crime. This paper, however, is able to employ rich administrative data from South Carolina that links individual-level records across health and law enforcement agencies, thereby allowing me to identify individuals diagnosed with mental illness and measure any contact that they have with the criminal justice system.

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

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

I find that treated men who lose access to Medicaid coverage are 14% more likely to be incarcerated in the following two years. These baseline results suggest a strong, positive relationship between Medicaid disenrollment and criminal activity among low-income young men. Importantly, the rich nature of the data allows me to split the sample by men's mental health histories, and I find that the effects are entirely driven by men with mental illness. By their 21st birthdays, treated men with mental health histories are 3 percentage points (or 21%) more likely to have ever been incarcerated relative to men in the comparison group. I find increases in violent and property crimes, suggesting that losing access to health coverage impacts various types of criminal

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involvement. Finally, I find that the effects are particularly pronounced for men who were using behavioral health services right before their 19th birthdays and for those who relied on Medicaid for access to mental health medications. Together, these findings reaffirm the notion that losing access to mental healthcare plays an important role in explaining the observed rise in criminal activity.

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

The data and empirical approach used in this study are advantageous for several reasons. First, I use administrative data that links individual-level records across six state government agencies, so that I can follow the same individual across datasets and over time. Importantly, this dataset includes information on an individual's enrollment spells in the Medicaid program as well as detailed information on insurance claims, which allows me to identify individuals diagnosed with a mental health disorder. The dataset also includes records from three law enforcement agencies, thereby allowing me to measure any contact that an individual has with the criminal justice system. Furthermore, I leverage exogenous variation in Medicaid eligibility at the individual level to study the dynamic evolution of outcomes of affected individuals (relative to similar individuals in close geographic proximity who are less affected by the policy change). This study therefore does not rely on cross-state policy variation or individual enrollment choices that may be correlated with other state-level or individual-level changes, respectively.

This paper contributes to a recent and growing literature in economics studying the effect

of mental health and mental health services on various outcomes, including criminal activity (Anderson et al. 2015, Blattman et al. 2017, Bondurant et al. 2018, Busch et al. 2014, Chatterji & Meara 2010, Deza et al. 2021, Deza et al. 2022, Fletcher & Wolfe 2009, Heller et al. 2017, Marcotte & Markowitz 2011, Teplin et al. 2002).[2] By leveraging a sudden loss of Medicaid coverage, this paper quantifies the causal relationship between access to mental healthcare and contact with the criminal justice system among low-income young adults. In the same vein, this paper argues that criminal involvement is a function of health, similar to a number of papers that study the effect of cognitive health—via changes in lead exposure—on criminal behavior (Aizer & Currie 2019, Billings & Schnepel 2018, Feigenbaum & Muller 2016, Reyes 2007).

By studying changes in access to Medicaid, this paper also contributes to a recent literature studying the effects of health insurance expansions on public safety (Aslim et al. 2019, He & Barkowski 2020, Fry et al. 2020, Vogler 2020, Wen et al. 2017).[3] Unlike most of these studies, which rely on aggregate statistics to measure changes in crime rates, this paper uses individual-level health and criminal records, allowing me to investigate the importance of mental health service provision.

Finally, this paper contributes to a growing literature quantifying the social returns to Medicaid (see e.g., Arenberg et al. 2020, Boudreaux et al. 2016, Brown et al. 2020, Goodman-Bacon 2021). This study is different from previous papers in two main ways. First, it focuses on the immediate, rather than the long-term, effects of Medicaid eligibility. The findings can therefore help quantify the short-term returns to increasing Medicaid access, which are likely of interest to policymakers weighing the costs and benefits of expanding public insurance coverage.[4] Second,

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[2] Numerous other studies consider the relationship between mental health and human capital, including Angelucci & Bennett (2021), Cowan & Hao (2020), Cuellar & Dave (2016), Currie & Stabile (2006), Currie & Stabile (2007), and Fletcher & Wolfe (2008). Finally, other related papers include Biasi et al. (2019), Bütikofer et al. (2020), Chatterji et al. (2011), Chorniy & Kitashima (2016), Cuddy & Currie (2020), Cuellar & Markowitz (2006), Fletcher (2013), Ludwig et al. (2009), Maclean et al. (2019), and Ridley et al. (2020), which study the relationship between mental health and labor market outcomes, risky sexual behaviors, overall health, and mortality.

[3] By estimating the effect of health coverage on an individual's likelihood of engaging in criminal activity, this paper is also related to an expansive literature studying deterrence and desistance from crime. For detailed reviews of this literature, I refer the reader to Chalfin & McCrary (2017) and Doleac (2020). Notably, this paper complements a number of recent studies quantifying the effects of public assistance programs on criminal activity and recidivism including Deshpande & Mueller-Smith (2021), Tuttle (2019), and Yang (2017).

[4] Most state constitutions include balanced budget requirements or debt limitations that prohibit policymakers from borrowing to fund expenses with long-term returns. As such, quantifying a policy's short-term returns is likely of particular interest.

whereas previous studies typically focus on historical expansions of Medicaid for children, this study focuses on the provision of Medicaid eligibility to modern cohorts of young adults. Because young adults are relatively less likely to be insured—and are thus the group that stands most to gain from modern health insurance expansions—understanding the returns to this investment is of policy relevance. In that regard, this paper complements the findings from the Oregon Health Insurance Experiment (Baicker et al. 2014, Finkelstein et al. 2012), by focusing on a younger and more racially diverse population as well as considering incarceration as an additional outcome.[5]

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

2 Mental Health & Criminal Activity

2.1 Historical Background: Differing Views on Policy Responses to Crime

As crime rates began rising in the United States in the 1960s, contemporary observers debated the extent to which mental illness causes crime, and consequently, the degree to which the criminal justice system should be replaced with alternative, more therapeutic approaches (Murphy 1969). One prevalent perspective was that the penal system punished criminal symptoms instead of curing criminal causes. Dr. Karl Menninger, a well-respected psychiatrist at the time, published a book titled "The Crime of Punishment," arguing that "psychiatrists cannot understand why the legal profession continues to lend its support to such a system after the scientific discoveries of the past century have become common knowledge" (Menninger 1968). Individuals who held this view advocated for reforms or alternatives to the penal system, such as providing judges with psychiatric reports prior to sentencing or establishing "community safety centers" tasked with identifying and treating offenders and would-be offenders.[6]

At the same time, economist Gary Becker published his seminal work on the economics of crime, providing an alternative perspective for understanding and addressing criminal behavior.

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

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

His framework argued that criminal offenders make a rational calculation, weighing the associated costs and benefits when deciding whether to commit a crime (Becker 1968). The implications of his model were that society could deter offenders from committing crimes by either making punishments more severe or more certain (e.g., via longer prison sentences) or by raising the opportunity cost of crime (e.g., via improved employment opportunities).

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

Policymakers' reliance on harsher punishments also coincided in timing with the closure of state mental hospitals and a shift away from inpatient mental health treatment.[7] Recent research suggests 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 (Raphael & Stoll 2013a).

2.2 Prevalence of Mental Illness Among Criminal Justice Populations Today

Today, the relationship between mental illness and criminal behavior is significantly more well-established (see Frank & McGuire 2010 for a detailed review).[8] Individuals with mental illness are significantly over-represented in prisons and jails: 37% of prison inmates and 44% of jail inmates have been diagnosed with a mental disorder prior to incarceration (Bronson & Berzofsky 2017). Accordingly, the criminal justice system spends a significant share of its resources housing and treating individuals with mental illness, especially given this population's higher recidivism

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

[8] It is important to distinguish here that even though a significant portion of criminal offenders have mental health histories, it is not the case that most mentally ill individuals commit crimes (Glied & Frank 2014).

rates, longer sentences, and more expensive medical needs (Osher et al. 2012).[9]

The persistent relationship between mental illness and criminal involvement raises the question of whether improved access to health services can reduce the likelihood that mentally ill individuals come into contact with the criminal justice system. It is worth noting that when policymakers and academics were discussing the relationship between mental illness and crime in the 1960s, healthcare may not have been an effective way to reduce criminal behavior. However, in the decades that have transpired, 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 & Glied 2006, Hofmann et al. 2012, Kendler 2019, Lieberman & First 2018, Marder & Cannon 2019, Park & Zarate Jr. 2019). Acknowledging this progress, this paper revisits the potential role that mental healthcare can play in reducing criminal activity.

2.3   Role of Healthcare in Affecting Criminal Propensity

There are multiple channels through which losing access to health insurance could affect an individual's criminal behavior. This study focuses on the mental health channel: for many individuals, losing Medicaid eligibility means losing access to mental health treatments, medications, or other resources.[10,11] Loss of access to mental healthcare could result in increased criminal behavior for various reasons.

First, individuals who lose insurance coverage might begin to find criminal activities more appealing. For example, individuals who lose access to medications might begin to self-medicate via higher use of illicit drugs (Khantzian 1987, Khantzian 1997). Indeed, Busch et al. (2014) finds that following a regulatory policy that decreased antidepressant prescriptions, adolescents

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

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

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

with depression were more likely to use illegal drugs. Loss of mental healthcare could also disrupt an individual's human capital formation or labor market productivity, thereby making criminal alternatives more attractive. Biasi et al. (2019) finds that increased access to lithium—a psychiatric medication primarily used to treat mood or depressive disorders—improved the career trajectories of individuals with bipolar disorder. In addition, Currie & Stabile (2006) and Currie & Stabile (2007) find that mental health conditions can have deleterious effects on educational attainment, so to the extent that access to public health insurance can minimize these effects, then losing access could hinder an individual's academic achievement.

Finally, individuals who lose access to behavioral health services might be more prone to making errors in judgment or decision-making, and thus be more likely to engage in criminal behavior. For example, Heller et al. (2017) finds that low-income adolescents participating in cognitive behavioral therapy programming during the school year were significantly less likely to be arrested for both violent and non-violent offenses, but that these effects did not persist after the program ended. Furthermore, health insurance coverage provides individuals with access to resources (e.g., social workers, community-based services) that could help de-escalate mental health crises or treat substance abuse, thereby preventing future criminal involvement (Arora & Bencsik 2021, Bondurant et al. 2018).[12]

3 Data and Sample

This paper studies the effect of health insurance coverage on criminal behavior in the state of South Carolina. South Carolina is relatively poorer than other states in the U.S. and it also has low levels of health insurance coverage among non-elderly adults.[13]

The data source is administrative records from various state agencies that are linked at the individual level. This dataset is relatively unique in the context of U.S. administrative data in that it includes information from both health and law enforcement agencies. South Carolina's Revenue and Fiscal Affairs (RFA) Office linked data from six state government agencies for this study, so

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[12] Because police are often the first to respond to psychiatric crisis calls, individuals experiencing mental health crises without access to Medicaid resources might also face an increased likelihood of interacting with law enforcement (Lamb et al. 2002).

[13] 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).

that I can identify the same person across datatsets and time using an individual identifier. For more details on the sample and the variable construction, I refer the reader to Appendix B.

3.1 Sample

The primary sample used in this study is a disproportionately low-income group of adolescents born between 1990 and 1993. Specifically, an individual is included in the sample if he or she ever attended a high school among the poorest half of high schools in the state. One can thus think of this sample as representing the residents of the poorest half of neighborhoods in South Carolina.[14] Furthermore, because I need information on an individual's birth month (only available in the Medicaid recipient file) for the analysis, I restrict this sample to individuals who have ever been enrolled in the Medicaid program.

3.2 Medicaid Claims

Detailed information on an individual's Medicaid enrollment spells as well as insurance claims comes from data provided by South Carolina's Department of Health and Human Services. The recipient file includes demographic characteristics as well as the dates of enrollment spells. The remaining data files contain claim information from all visits to doctors and hospitals as well as pharmacy claims.

The insurance claim 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 refers to antidepressant, antianxiety, and antipsychotic medications as well as medications used to treat attention-deficit/hyperactivity disorder (ADHD). A claim is then considered a "mental health claim" if it includes a mental health diagnosis or if it prescribes a mental health medication.

3.3 Data on Criminal Behavior

To measure crime-related outcomes, I use records from the South Carolina Law Enforcement Division (SLED), the Department of Corrections (DOC), and the Department of Juvenile Justice (DJJ). Data from SLED provide information on all arrests in the state. Data from the DOC provide details on incarceration spells in state prisons. Data from DJJ contain information on all contact

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

between adolescents and the juvenile justice system. In South Carolina, individuals are legally treated as adults on their 17th birthdays.

The main outcome of interest is men's likelihood of incarceration, which peaks in prevalence in men's late teens and early twenties, and is particularly common for men in low-income communities (Freeman 1999, Lofstrom & Raphael 2016).[15] I measure this outcome by combining the SLED records—which identify individuals who were detained in an adult correctional facility after being arrested—with the DOC data, which track incarceration spells in state prisons. This outcome therefore measures the likelihood that an individual is incarcerated in any adult correctional facility.

3.4 Other Data Sources

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

4   Empirical Strategy

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

4.1   Aging out of Medicaid Eligibility

In South Carolina, children ages 0–18 with household incomes up to 208% of the federal poverty level (FPL) are insured via the Children's Health Insurance Program (CHIP), which is operated through the Medicaid program (SCDHHS 2020b). Upon reaching 19 years of age, low-income residents age out of eligibility and childless adults have limited access to Medicaid

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[15] I focus on men in this paper given the relative infrequence of women's incarceration in South Carolina. Of individuals incarcerated before age 21, 96% are men.

services.[16]

Because I do not see any other health data besides Medicaid claims, I cannot know with certainty whether individuals who lose access to Medicaid become uninsured or transition to private insurance. However, I assume that the share of this population that becomes uninsured is relatively high. Appendix Figure A1 uses data from the American Community Surveys to show that in South Carolina, the share of men who are uninsured increases rapidly once individuals reach 19 years of age.[17]

4.2 Research Design

In order to measure the impact of losing Medicaid eligibility on men's likelihood of incarceration, I employ a difference-in-differences design. Men who were likely to be enrolled in Medicaid before their 19th birthdays serve as the treated individuals in the analysis (for the remainder of the paper, I will refer to this group as the "high-enrollment" group). Individuals who were enrolled in Medicaid earlier in adolescence, but who were less likely be enrolled prior to their 19th birthdays serve as the comparison group (henceforth, the "low-enrollment" group). Importantly, because all treated individuals lose Medicaid coverage at the same point in the life cycle, it is important to compare their outcomes to those of a comparison group in order to account for age trends in crime, which are especially significant in late adolescence.[18]

A natural question is why low-enrollment men were enrolled in Medicaid earlier, but not later in adolescence. Unfortunately, there is no information available in the datasets to answer this question, but there are a few potential explanations. First, many individuals in the comparison group were likely eligible for the Medicaid program, but not enrolled because of hurdles to enrollment (e.g., long waits at the county office or needing to provide wage information from employers; Edwards & Kellenberg 2013).[19] Two other explanations could be that these individuals' families

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

[17] Moreover, in 2010, among individuals ages 19–25 living at or below 138% of the FPL, 43% were uninsured and 41% were covered by private insurance (SHADAC 2020).

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

[19] It is likely the case that a large share of these comparison individuals were uninsured. Among low-income boys in South Carolina 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).

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experienced positive income shocks that made them ineligible for the program, or that these men decided that they no longer wanted or needed access to Medicaid services. These last two possibilities suggest that on average, men in the comparison group might have higher-income parents and might be relatively healthier in late adolescence than the treated individuals. Regardless of the reason for not being enrolled, however, the key component for the research design is that the comparison group did not experience a sharp decline in Medicaid enrollment at age 19, thereby allowing me to estimate counterfactual outcome paths for the high-enrollment group around this age and disentangle the effect of the insurance loss from pure age effects.

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

4.3 Matching Procedure

To ensure that the low-enrollment individuals serve as a suitable group for estimating the high-enrollment group's counterfactual outcome paths, I implement a matching procedure that guarantees balance along observable characteristics. To be eligible for inclusion in this procedure, individuals must have been enrolled in Medicaid at some point between the ages of 10 and 18. To avoid including men who might have left the state, I also restrict the sample to those who were present in any of the data sources between ages 15–18. I then drop individuals who passed away or were incarcerated before the study's pre-period because these outcomes could mechanically determine the assignment of an individual into the treatment or comparison group. There are 22,063 eligible treatment and 8,964 eligible comparison men, which is 93% of the men in the low-income sample who are born in these cohorts and have an available birthdate.

I then match each treated person to all "counterfactual" individuals using a parsimonious

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[20] I assign men into the two groups a year before the study pre-period to guarantee that the assignment is uncorrelated with their outcomes around age 19. Specifically, men who are incarcerated do not have access to Medicaid services, and are therefore less likely to be enrolled in the program. Assigning men to a group before the start of the pre-period thus allows me to follow the natural evolution of outcomes of both groups before and after their 19th birthdays. Moreover, Medicaid enrollment is a function of household income, so I prefer to assign men into a group significantly before their 19th birthdays to ensure that any estimated effects at age 19 are coming from the loss in Medicaid enrollment, and not from contemporaneous income shocks (i.e., to avoid Ashenfelter's dip). I check the sensitivity of the main results to altering the timing of assignment into groups in Section 6.3.

set of characteristics. Treated men are matched to all comparison men based 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). This matching procedure—and the corresponding weighting scheme described below—is similar to the approach used in Smith et al. (2019). I intentionally avoid matching on outcome variables.

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

4.4   Characteristics of Matched Sample

Table 1 reports summary statistics for the full sample and various subsamples. All characteristics are measured starting at age 10 and before an individual's 19th birthday. On average, men in the matched sample (column 2) are observably different from the group of individuals who were ever enrolled in Medicaid (column 1). In particular, treated men are more likely to have a mental health history and more likely to have had prior contact with the criminal justice system. These differences in observable characteristics prior to age 19 reinforce the importance of implementing the matching procedure in order to ensure that the low-enrollment group serves as a suitable comparison group for estimating the counterfactual outcome paths of high-enrollment individuals.[21]

Appendix Table A2 shows the distribution of diagnoses for the full sample as well as for treated men. This table highlights that neurodevelopmental disorders (including ADHD) as well as behavioral disorders (i.e., conduct disorder and oppositional defiant disorder) are among the most common diagnoses in this sample. I also note that most individuals with mental health histories are diagnosed with more than one disorder throughout adolescence. Finally, this table shows that

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

a large share of individuals file Medicaid claims for psychotropic drugs, in particular for ADHD and antidepressant medications.

The matching procedure guarantees that there is balance between the high- and low-enrollment groups in terms of race, cohort, geographic location, and mental health history. However, there still might be concern that the two groups significantly differ along unobservable characteristics and would thus plausibly exhibit different trends in criminal propensity. Appendix Figure A2 plots the raw means of standardized test scores as well as juvenile justice referrals for felony offenses for the two groups earlier in adolescence, both for the full sample and separately by mental health history. These figures confirm that the treated and comparison men were relatively similar to each other in terms of their educational achievement and criminal propensity—both in levels and in trends—throughout adolescence, thereby providing additional evidence that the control units will serve as a suitable comparison group for estimating counterfactual outcome paths.

4.5 Loss of Medicaid Eligibility in Matched Sample

To confirm that high-enrollment men are indeed affected by the loss of Medicaid eligibility, Figure 3 plots the share of men enrolled in the Medicaid program before and after age 19. Given that individuals were assigned into groups prior to age $17\frac{1}{2}$, we see that a portion of men in the treatment group become naturally disenrolled before age 19 and that the share of comparison men enrolled in the pre-period is not mechanically zero (i.e., mean reversion). Nevertheless, this figure highlights that treated men experience a large decline in enrollment after their 19th birthdays, while men in the low-enrollment group are significantly less likely to be affected by the loss of eligibility.

Among treated men, the average share enrolled drops from 77% in the pre-period to 13% in the post-period, thereby confirming the presence of a large "first stage." To the extent that losing Medicaid eligibility impacts criminal activity, then we would expect to see significant changes in the outcomes of the high-enrollment group starting at age 19.

Panel (b) of Figure 3 plots the raw means separately by mental health history, showing that both groups of treated men experience a decline in Medicaid enrollment. The drop in enrollment among men with mental health histories is smaller in magnitude than the analogous drop for men without a mental health history, which would bias me against finding an effect for the former group relative to the latter. Appendix Figure A3 shows analogous figures using claims data, confirming that both groups of treated men are more than 30 percentage points less likely to file claims in the post-period. Finally, Appendix Figure A4 shows that the high-enrollment men did not experience

comparable declines in SNAP or TANF enrollment around their 19th birthdays.

5 Estimating the Effect of Losing Medicaid on Criminal Behavior

5.1 Baseline Specification

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

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

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

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

When presenting the results, I typically report the post-period average of the β_τ coefficients ($\frac{1}{8} \sum_{\tau=0}^{7} \beta_\tau$), or in other words, the average treatment effect in the post period. When considering cumulative variables (i.e., individual i has *ever* been incarcerated), I instead report the last coefficient β_7, which measures the treat-control difference in outcome Y by age 21. Finally, when interpreting the magnitudes of the estimates, I re-scale the average treatment effect in the post-period by the comparison group's average outcome Y in the post-period. When using cumulative variables, I re-scale β_7 by the comparison group's average Y in the final period of the post-period.[22]

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[22] Because criminal propensity tends to rise with time in this age range, re-scaling the estimated effect by the control group's average in the pre-period would yield larger effects. I therefore choose the more conservative approach

5.2 Identification Assumption

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

One concern with this identifying assumption might be that men in the treatment and comparison groups are fundamentally different and thus likely to have differing outcome paths. For example, if treated men are relatively lower-income than the comparison group, then we might expect treated men to be more likely to engage in criminal activity regardless of the loss in Medicaid eligibility. Matching on observable characteristics, like race and geographic location, helps alleviate this concern.[23] Moreover, even if the two groups differ in their overall levels of outcome Y, the identifying assumption relies on the two groups *trending* similarly over time in their criminal propensity. If the two groups are trending similarly and only begin to diverge when treated individuals lose access to Medicaid, then these patterns suggest that the control individuals are a suitable comparison group for estimating the counterfactual post-period outcomes of the treated men in the absence of the insurance loss. In practice, I plot the raw data and estimate non-parametric specifications in order to corroborate the plausibility of the parallel trends assumption.

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

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and re-scale the effect by the post-period average. This choice is also motivated by the identifying assumption: the comparison group is tracing out the counterfactual outcome paths of the treated units, so I re-scale the estimates by the average counterfactual outcomes.

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

19th birthdays. The differences in the timing of birthdays as well as the matching procedure therefore make it unlikely that the estimated effects are driven by treated individuals experiencing unobserved shocks unrelated to their loss in Medicaid eligibility.[24]

5.3 Triple-Differences Specification

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

Specifically, I estimate a regression of the following form:

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

(2)

where Hist_i is a variable indicating whether individual i had a mental health history prior to age 16 (i.e., one of the characteristics used in the matching procedure). This specification is similar to equation (1) in that high-enrollment men are still being compared to the matched comparison group in a given quarter τ. However, this specification allows treated individuals with mental health histories to have differing treatment effects—captured by α_τ—than treated individuals without mental health histories. In that regard, this regression is equivalent to running a triple-differences specification.

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

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[24] Other laws and policies associated with the transition into adulthood typically occur earlier in adolescence in South Carolina (e.g., the age of medical consent is 16, the school-leaving age is 17).

6 Main Results for Criminal Behavior

6.1 Full Sample

I first consider an individual's likelihood of being incarcerated in a given quarter. Panel (a) of Figure 4 plots the raw means using the full matched sample, and panel (b) plots the estimates from equation (1). These figures confirm that treated men were trending similarly to the men in the comparison group prior to age 19, thereby providing support for the parallel trends assumption. Then, starting at age 19, the two groups began to diverge such that treated men were 14% more likely to be incarcerated in any given quarter of the post-period relative to men in the comparison group. The results from this and the following subsection are summarized in Table 2.

To better understand whether the increase in incarceration is driven by men recidivating or by men being incarcerated for the first time, I then use a cumulative variable that measures whether an individual has *ever* been incarcerated.[25] Panels (c) and (d) once again show that treated and comparison men were trending similarly and only began to significantly diverge upon reaching age 19. These findings indicate that the increase in incarceration was driven by new individuals having their first serious contact with the criminal justice system. By their 21st birthdays, men in the high-enrollment group who lost access to Medicaid were roughly 2 percentage points (or 16%) more likely to have been incarcerated than men in the comparison group.[26]

A back-of-the-envelope calculation suggests that in the absence of the Medicaid loss, around 380 men in these cohorts would not have been incarcerated, implying a 10% reduction in these cohorts' likelihood of incarceration by age 21. Because this sample only represents half of the state's population, this number is almost certainly an undercount of the number of men in these cohorts who would not have been incarcerated if their Medicaid eligibility had not expired on their

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

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

19th birthdays. Moreover, to the extent that at least a portion of the men in the comparison group were affected by the loss in eligibility, then the estimates in this section would be underestimates of the effect of Medicaid disenrollment on incarceration.

Finally, it is worth noting that the treatment group's likelihood of incarceration resembled that of the comparison group's not only in its trending behavior, but also in its levels throughout the pre-period. A potential interpretation of these patterns is that having access to health services suppressed treated men's criminal propensities, keeping them comparable to those of their relatively higher-income and healthier peers.[27] Upon aging out of eligibility, treated men likely began to face constraints in accessing health services and became more likely to commit criminal offenses. One explanation for this increase in criminal activity could be that Medicaid was an important source of mental healthcare for this group of low-income adolescents, an issue I now explore in greater detail.

6.2 Heterogeneity by Mental Health History

To consider the importance of mental healthcare, I now split the sample into two groups based on an individual's mental health history prior to age 16. Figure 5 displays the results from the triple-differences specification. Panel (b) plots the treat-control differences for men without a mental health history (45% of treated men, in orange) as well as the the treat-control differences for men with a mental health history (55% of treated men, in blue). These results suggest that the increase in crime is driven entirely by men with mental health histories. Notably, despite losing Medicaid eligibility, treated men without mental health histories continue to trend similarly to their corresponding comparison group for the entire post period.[28]

I then use the dependent variable measuring men's cumulative likelihood of incarceration. The results in panels (c) and (d) show that the share of treated men who have ever been incarcerated begins diverging from the analogous share of the comparison group starting at age 19. By their

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[27] If the treated and comparison groups were identical except for their access to health services, then we might expect the criminal propensity of the comparison group to be higher than that of treated men. However, men in the comparison group likely have higher-income parents (see Appendix Figure A4) and might be relatively healthier (i.e., if they have decided that they no longer want or need health services). Both of these differences likely explain why the comparison group's incarceration rate is not higher than that of the treated group.

[28] If the effect of losing health coverage on criminal behavior was simply an income effect, then we would also expect to see an increase in incarceration for men without mental health histories who lost eligibility. The lack of an increase in incarceration for this group thus provides support for the notion that losing access to mental healthcare drives changes in criminal propensity.

21st birthdays, treated men with a mental health history are 3 percentage points (or 21%) more likely to have ever been incarcerated. These results highlight that the rise in crime is not driven by a group of men being detained for long periods of time or consistently recidivating. Instead, the termination of Medicaid eligibility results in new individuals with mental illness becoming incarcerated for the first time.

Finally, I can use these reduced-form estimates to calculate the implied effect of Medicaid enrollment on men's likelihood of incarceration. Table 3 displays the results, showing that Medicaid disenrollment increases low-income men's likelihood of incarceration by around 25%.[29] Among men with mental illness, Medicaid enrollment decreases their likelihood of incarceration by 2 percentage points, roughly a 40% reduction. To put these magnitudes into perspective, Lochner & Moretti (2004) find that one extra year of schooling reduces men's likelihood of incarceration by approximately 10%. Billings & Schnepel (2018) find that early-life interventions to reduce lead exposure decrease the likelihood of arrests by 40%. The findings in this section thus suggest that providing health coverage to low-income men with mental illness has comparable effects to those of other interventions typically associated with crime reduction.

6.3 Robustness of Main Results

6.3.1 Robustness to Clustering and Matching Procedure

In this and the next subsection, I consider the robustness of the main results displayed in Figure 5 to various other choices I could have made when constructing the baseline sample and implementing the preferred specification. Appendix Table A3 reports the results. I begin by changing the level at which I cluster the standard errors, first to the match level and then to the match and individual level. The statistical significance of the main results is preserved.

I then test the sensitivity of the baseline estimates to the matching procedure and implicit

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

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

where Post_τ is any of the eight quarters after a person's 19th birthday, and $\text{Treat}_i \times \text{Post}_\tau$ is an instrumental variable for Medicaid enrollment. The Wald estimate (i.e., β_0) is the ratio of the reduced-form and the first-stage estimates. I re-scale the estimates using changes in enrollment—rather than changes in the share of men filing claims—because I want to measure the effect of having access to mental healthcare (e.g., medications, resources) even for individuals that were not consistently using it. Finally, it is worth noting that if treated men were able to transition from Medicaid to other insurance coverage, then these magnitudes would be underestimates of the effect of losing coverage on incarceration.

weighting scheme. To verify the parallel trends assumption in these next exercises, Appendix Figure A5 displays the graphical results. First, instead of matching each treated unit to all possible comparison units, I force each treated unit to only have one randomly chosen counterfactual observation. I then return to the baseline matching procedure and drop comparison units that get disproportionate weight in the regression.[30] Next, instead of using an individual's school district as a matching characteristic, I use an individual's school so that high-enrollment men are even more likely to resemble the low-enrollment group. This change reduces the number of matched treated units, but it does not significantly alter the main findings.

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

6.3.2 Robustness to Sample & Variable Construction and to Concurrent Changes

I now test the sensitivity of the main results to the sample and variable construction. First, I alter the definition of mental illness to only be based on an individual's diagnoses. Next, I change the timing of assignment into groups, so that individuals are assigned to a group based on their Medicaid enrollment during the pre-period (as opposed to a year before the pre-period). The main results are robust to both of these changes.

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[30] As shown in Table A1, there are more eligible treated units than there are eligible comparison units. The matching procedure gives greater weight to comparison men that resemble the treated units along observable characteristics. However, this procedure could result in certain comparison units getting a disproportionate amount of weight in the regression. To address this possibility, I calculate how much total weight each comparison unit is given in the baseline sample and drop the units whose weight is in the top 1% of that distribution. I then reconstruct weights for the remaining comparison units so that there is balance on observables within the regression.

[31] Appendix Figure A6 plots the raw means for all eligible treated and control men. Panels (a) and (b) highlight that without any matching or re-weighting, the comparison group in the full sample does not exhibit parallel trends to the high-enrollment men. Once mental health history is taken into account, however, the treated men in each group do exhibit similar trends to the corresponding comparison men in the pre-period. These latter two figures thus illustrate the importance of including mental health history as a matching characteristic if the goal is to find comparison men who can serve as a suitable comparison group for tracing out the counterfactual outcome paths of treated men.

One threat to causal identification is that treated individuals might be experiencing other shocks in late adolescence that might influence their criminal propensity, such as aging out of foster care or losing Supplemental Security Income (SSI) benefits at age 18 (see e.g., Courtney et al. 2007 and Deshpande & Mueller-Smith 2021). Although the timing of these transitions differs from that of Medicaid disenrollment, I still take seriously this consideration and exclude from the sample the 13% of treated men who were in foster care or receiving SSI benefits. The main results are robust to their exclusion, suggesting that the increase in criminal activity is not driven by this group of men experiencing transitions unrelated to Medicaid enrollment during late adolescence.[32]

In the same vein, another important change that occurs in late adolescence is that individuals graduate (or leave) high school and transition into higher education or the labor market. Importantly, teens drop out or graduate from high school at different points in calendar time and there is no reason to think that the timing of these transitions would coincide with their 19th birthdays. Indeed, the age at which individuals can legally drop out of school in South Carolina is 17. Appendix Figure A7 plots the share of each group that is enrolled at a given age. These figures confirm that school enrollment begins to decline when individuals are 17 years old. Perhaps more importantly, though, these figures show that the share of treated men that was enrolled in school was comparable to the analogous share of comparison men before age 19 (both for those with and without mental health histories). These figures thus provide evidence that treated men were not differentially experiencing shocks related to educational attainment right before their 19th birthdays that might be confounding the estimated effects of Medicaid disenrollment.[33]

Finally, I consider the robustness of the main result to an alternative way of constructing the treatment and comparison groups. Specifically, I focus on men with mental health histories who were enrolled in Medicaid before age 19 and I split that sample into two groups: men filing mental

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[32] Given that youths in foster care and SSI are allowed to maintain their Medicaid eligibility past age 19, one alternative would be to use these men as a comparison group. Nevertheless, this approach is not optimal for two reasons. First, because 90% of SSI and foster care youth have a mental health history, there is not sufficient statistical power to estimate a triple-differences specification like equation (2), which investigates the importance of mental health service provision. Second, the criminal trajectories of SSI and foster care youths differ from those of other men with mental health histories at ages 17–18, indicating that the former is not a suitable comparison group for estimating the counterfactual outcomes of the latter. This difference is perhaps unsurprising given that SSI and foster care youth are experiencing various other shocks that might impact their criminal involvement prior to their 19th birthdays.

[33] Appendix Figure A4 shows that the share of men enrolled in the SNAP program was also gradually declining in late adolescence and this decline, in turn, could impact men's criminal propensity (Tuttle 2019, Yang 2017). Nevertheless, the decline is significantly more pronounced for treated men without mental health histories, which would bias me against finding an effect for those with mental health histories relative to those without.

health claims during the pre-period serve as the "treated" group and those who did not file mental health claims become the comparison units. Appendix Figure A8 shows that men filing mental health claims are 2 percentage points (or 10%) more likely to be incarcerated by age 21 relative to this alternative comparison group. This result provides additional evidence that losing access to mental healthcare seems to play an important role in explaining the observed rise in incarceration. Because both groups experienced declines in Medicaid enrollment at age 19, this figure is likely an underestimate of the effect of losing access to health services on the likelihood of incarceration.

6.3.3 Falsification Checks Around Earlier Ages

As another way to test the robustness of the main result, I conduct "placebo" checks in which I replicate the baseline empirical approach (i.e., assigning men into groups and implementing the matching procedure) around earlier ages. If the main findings are driven by the loss of Medicaid at age 19, then I should not find an increase in treated men's criminal propensities around earlier birthdays when there was not a break in Medicaid eligibility. Put differently, one concern with the empirical strategy might be that the treated and comparison group are fundamentally different and bound to have diverging criminal propensities in late adolescence. If this is the case, then I would also expect to see differences emerge between these two groups around earlier ages as these individuals are transitioning into adulthood.

I begin by shifting the approach back one year and estimating men's likelihood of incarceration around age 18. Appendix Figure A9 and Table A4 display the results from this exercise, showing a lack of an increase in the likelihood of incarceration for treated men with mental health histories after their 18th birthdays. Next, I expand the definition of incarceration to include juvenile detentions. Using this broader definition, I estimate equation (2) around ages 17, 18, and 19. The results show that an increase in incarceration only occurs around age 19, suggesting that this increase is indeed driven by the loss of insurance coverage at this later age.

6.3.4 Alternative Empirical Strategy: Regression Discontinuity

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

Medicaid coverage on criminal propensity.[34] The analysis is based on the following reduced-form regression-discontinuity model:

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

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

Appendix Figure A10 and Table A5 display the results for various subgroups, showing that men's likelihood of being arrested increases upon reaching age 19. Importantly, these findings highlight that the discontinuity is only present for men with mental health histories, and in particular for those with mental health claims in the years prior to aging out of eligibility.[35] The estimates suggest that men with recent mental health histories were 0.4 percentage points (or roughly 10%) more likely to be arrested upon reaching their 19th birthdays.

7 Heterogeneity by Offense Type, Mental Healthcare Utilization, & Diagnoses

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

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

[35] Appendix Table A5 shows that these results are robust to using a cubic polynomial when estimating equation (4). The results in panel (c) are less precisely estimated, but the point estimates suggest that men with recent mental health histories are more likely to be incarcerated upon reaching age 19, with no comparable effect present for men without recent mental health histories. I also consider the likelihood that an individual is incarcerated for the first time around age 19. Appendix Figure A11 shows a discontinuity in this likelihood only for individuals with recent mental health histories. Finally, Appendix Table A6 displays the results from analogous exercises around age 20, confirming that there is no increase in arrest or incarceration propensity for individuals with mental health histories around this later age.

7.1 Offense Types

In line with the economics of crime literature, I begin by estimating whether treated men are more likely to be incarcerated for violent and property crimes. Figure 6 and Table 4 display the results, showing that treated men with mental health histories are more likely to be incarcerated for both types of offenses relative to the comparison group. Importantly, violent and property crimes are relatively serious offenses, suggesting that the rise in incarceration is being driven by increases in serious criminal offending, rather than an increase in the likelihood of incarceration for less serious offenses.

Next, motivated by the potential channels discussed in Section 2.3, I classify the offenses that result in incarceration into (1) financially motivated offenses (e.g., burglary, robbery, drug distribution), (2) non-financial violent offenses (e.g., murder, assault), (3) drug possession, and (4) miscellaneous offenses (e.g., weapons-related offenses, resisting arrest, parole or probation violations). The findings show that by age 21, treated men with mental health histories are 2.5 percentage points (or 31%) more likely to have been incarcerated for a financially motivated offense. Notably, the increase in these crimes seems to begin soon after individuals' 19th birthdays. These results thus suggest that an important share of the rise in criminal activity is economic in nature. Nevertheless, the results also show that treated men with mental health histories are more likely to be incarcerated for non-financial violent offenses, drug possession, as well as miscellaneous offenses. Overall, these findings suggest that there are likely multiple channels through which loss of access to insurance could result in increased criminal involvement.

7.2 Differences by Mental Healthcare Utilization

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

In practice, I maintain the same matched comparison group for each treated person and use a triple-differences specification—similar to equation (2)—to estimate whether the treat-control differences for men who were filing mental health claims *more* recently are statistically different

from the treat-control differences of men filing mental health claims *less* recently. If the loss of mental health services is an important factor driving the increase in crime, then I should find a positive and statistically significant treatment effect for more-recent beneficiaries. (This result would be consistent with the robustness check in Section 6.3.2 that uses less-recent beneficiaries as an alternative comparison group.)

Figure 7 shows that men who were using behavioral health services right before their 19th birthdays (denoted by the purple circles) were significantly more likely to have been incarcerated by age 21 compared to men who were using these services *less* recently (gray markers). Table 5 shows that this difference is driven by more-recent beneficiaries committing more financially motivated offenses after losing Medicaid coverage, suggesting that the increase in criminal behavior for this group was economic in nature.

It is also worth noting that more than half of recent beneficiaries were relying on Medicaid for access to psychotropic drugs, suggesting that losing access to mental health medications might be an important channel through which losing insurance coverage results in increased criminal activity. To consider the specific role of psychotropic drugs, I split the sample of treated men with mental health histories based on their medication usage. In particular, I divide the sample into those who filed claims for mental health drugs in the years before aging out of eligibility (i.e., the 35% of treated men who had claims for psychotropic drugs between the ages of 16 and 18) and those who did not file such claims during these ages. Appendix Figure A12 shows that men relying on Medicaid for mental health medications were significantly more likely to be incarcerated than men with mental health histories who did not file claims for psychotropic drugs in late adolescence.

Furthermore, as Appendix Table A2 shows, a substantial share of men in the treated group filed claims for non-ADHD medications (namely, antidepressant, antianxiety, and antipsychotic medications). Losing access to non-ADHD medications could have more significant impacts on the likelihood of incarceration perhaps due to negative side effects like withdrawal (see e.g., Lewis et al. 2021) or deleterious effects on labor market outcomes (see e.g., Biasi et al. 2019, Ridley et al. 2020). Appendix Figure A13 therefore splits the treated men based on the type of mental health medication for which they were filing claims. Indeed, the estimates in this figure show that the incarceration effects are particularly pronounced for the group of men filing claims for non-ADHD medications in the years prior to losing eligibility.

Finally, Appendix Table A7 shows that men relying on Medicaid for access to medications were

more likely to be incarcerated for financially motivated offenses. If the self-medication channel were driving the increase in crime, we might have expected to find a larger treatment effect for offenses related to drug possession among men who relied on Medicaid for psychotropic drugs; the lack of a difference in treatment effects between the two groups in this table thus suggests that the self-medication channel is not the main mechanism underlying the results.

7.3 Differences by Mental Health Diagnoses

I also use the diagnoses codes in the Medicaid claims to study which subgroups of men are more likely to contribute to the rise in crime. Specifically, Appendix Table A8 considers the sensitivity of the baseline estimates to excluding men ever diagnosed with a particular disorder. First, it is worth noting that the baseline results are robust to excluding individuals with intellectual disabilities and other neurodevelopmental disorders, indicating that men with these diagnoses are not driving the increase in criminal activity. In contrast, panel (b) shows that the main result is sensitive to excluding men who have ever been diagnosed with conduct disorder, oppositional defiant disorder, as well as substance-related and addictive disorders.[36] The findings in this table thus suggest that men diagnosed with these disorders are those who are most likely to commit crime after losing access to Medicaid eligibility. These results are in line with findings in prior studies showing that adults diagnosed as children with conduct disorder as well as those with co-occurring substance use disorders are more likely to be violent (Glied & Frank 2014).

8 Cost Effectiveness of Providing Medicaid Eligibility

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

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

8.1 Costs of Expanding Medicaid Eligibility

First, I calculate the cost of providing a cohort of low-income young men in South Carolina with Medicaid eligibility until their 21st birthdays—as opposed to until their 19th birthdays—and conclude that this cost would amount to roughly $15 million. This cost is a function of the estimated take-up of the program (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.

8.2 Benefits of Expanding Medicaid Eligibility

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

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

This specification is almost identical to equation (1), except that it measures Medicaid's effect on serious arrests (as opposed to on a person's likelihood of incarceration) and focuses on the sample of men with mental health histories. The coefficient β_0 estimates the extent to which treated and comparison men in this sample differ in their arrest propensity in the post-period relative to the pre-period. The results are reported in Table 6.

Using these estimates and taking a relatively conservative approach, I find that the reduced social costs of crime are roughly $16 million.[37] Specifically, I use the estimates from Table 6 to estimate the number of "excess" violent, property, and drug arrests that occurred as a result of the termination in Medicaid eligibility. I then use arrest-to-victimization ratios from Heckman et al. (2010) as well as the average cost per crime using estimates from Cohen & Piquero (2009) and Miller et al. (1996) to calculate the reduction in the total social costs of victimization.

Next, I calculate the benefits from fewer incarcerations, and estimate that providing Medicaid eligibility would result in $2.9 million and $7.1 million lower fiscal and social costs, respectively. To estimate the fiscal costs, I calculate the cost of incarcerating men in South Carolina using the

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

average sentence served for various offenses as well as the daily cost per inmate. To calculate the social costs generated by prison spells, I use estimates from Mueller-Smith (2015) quantifying the impact of prison on economic outcomes (i.e., reduced employment, greater reliance on public assistance) and post-release criminal behavior.

After comparing the costs of Medicaid provision with the potential benefits generated from reduced criminal activity, I conclude that providing insurance coverage to low-income young men is a cost-effective way to reduce crime. Using moderately conservative estimates, the findings of this paper suggest that for every dollar spent on insuring low-income young men via Medicaid, society recoups $1.80 in social and fiscal costs. Table 7 summarizes the estimates from this analysis, showing that even in the most conservative approach, the estimated benefits of Medicaid provision outweigh the costs.

8.3 Marginal Value of Public Funds

Finally, I use the estimates from this section to calculate the marginal value of public funds (MVPF), which estimates the ratio of society's willingness to pay for the expansion of Medicaid eligibility to the net cost to the government of implementing this policy (Hendren & Sprung-Keyser 2020). Similar to the cost-benefit exercise above, I construct upper and lower bounds for this ratio. Using moderately conservative assumptions, I find that the MVPF of expanding Medicaid eligibility for two years is 1.8 (with a lower- and upper-bound of 1.4 and 21.1, respectively). The value of the ratio is sensitive to the average cost assigned to crime and to assumptions about which party bears the economic incidence of uncompensated care (Finkelstein et al. 2019). Nevertheless, the findings from this exercise confirm that $1 of spending on this policy delivers more than $1 to its beneficiaries.

9 Comparing Medicaid Provision to Longer Punishments

Up to this point, I have shown that Medicaid seems to be a cost-effective policy for reducing criminal behavior. But how does Medicaid's cost effectiveness compare to other crime-reduction approaches? In this section, I benchmark the cost effectiveness of providing low-income young men with Medicaid eligibility against the cost effectiveness of harsher criminal sanctions, which have been a favored approach for reducing crime for the past fifty years. One motivation for this exercise is that cost-benefit calculations, like those discussed in Section 8, can be sensitive to the average cost assigned to crime. By comparing two crime-reduction approaches, I can abstract away

from the cost assigned to crime and instead compare the cost of two policies intended to reduce crime by the same amount.

To make this comparison, I first need to calculate the extent to which sentence lengths would need to be increased for this age group in order to achieve the same reduction in crime as providing Medicaid eligibility. In other words, I need to estimate the elasticity of crime with respect to sentence lengths, $\varepsilon_{c,f}$, which represents the deterrence effect of harsher sanctions.

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

9.1 Empirical Approach for Estimating Deterrence

To estimate the deterrence effect of harsher sanctions, I combine juvenile and adult arrest records and compare men's likelihood of committing a felony before and after their 17th birthdays. Specifically, I consider male adolescents with an available birthdate and who had not committed a felony prior to age 16.[38] Any estimated discontinuity at age 17 captures the change in the likelihood of committing a first felony offense upon reaching the age of criminal majority. For more details on the sample and variable construction, I refer the reader to Appendix D.

Specifically, I follow the approach in Lee & McCrary (2017) and calculate the number of individuals arrested for a felony in a given month as a share of those who are still at risk of committing their first felony. I then summarize the hazard of a felony arrest in a given month

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[38] 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.

and the corresponding discontinuity with the following logit specification:

$$P(Y_{it} \mid D_t, X_t) = F(\alpha X_t' + D_t \theta) \tag{6}$$

$$\text{where} \quad X_t' = (1, (t - t_0), (t - t_0)^2, (t - t_0)^3)$$

$$\text{and} \quad F(z) = \frac{\exp(z)}{1 + \exp(z)}$$

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

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

9.2   No Evidence of Deterrence

Figure 8 shows the hazard rates of a felony arrest around age 17, suggesting that low-income adolescents are equally likely to commit felony offenses upon reaching the age of criminal majority. The circles show the share arrested for a felony in that month as a share of individuals who had 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. Panel (b) splits the sample by an individual's mental health history. The findings are suggestive of teens with mental health histories being even less deterred by harsher punishments than teens without mental health histories.

I then use these estimates to calculate the corresponding elasticity of crime with respect to sentence lengths (summarized in Table 8). To be conservative, I use the smallest negative effect consistent with the confidence intervals to conclude that at most, there was a 11% decline in the probability of a felony arrest. Calculations detailed in Appendix D suggest that the expected sentence length rose by approximately 1,550% (i.e., from 96 days in juvenile detention to 4.5 years in adult prison). These estimates therefore imply an elasticity of -0.007, which is equivalent to Lee & McCrary (2017)'s reduced-form elasticity. If I instead use the average time served in

state prison (2.3 years), then the corresponding elasticity is -0.014.

9.3 Cost-Comparison with Medicaid Provision

I conclude this section by using the estimated elasticity of crime with respect to sentence lengths to benchmark the cost effectiveness of Medicaid eligibility against that of longer sentence lengths. The estimates in Table 6 imply that providing Medicaid eligibility to low-income young men would reduce crime by 15% in this age group. I thus compare the cost of each policy for achieving this same reduction in crime. I summarize the estimates here, but refer the reader to Appendix E for a more detailed discussion.[39]

First, I find that the total cost of reducing crime via Medicaid totals $57 million. This cost includes the total cost of providing Medicaid coverage to low-income young men (calculated and discussed in Section 8, totaling $15 million) as well as the cost of incarcerating men who were *not* deterred from committing crime for the current length of sentences (totaling another $15 million). Two other components are included in this figure: the fiscal costs of recidivism ($4 million) as well as the social costs of victimizations arising from the fact that a share of these men will likely re-offend after serving the relatively shorter sentences ($23 million).

Using the preferred estimate of the elasticity of crime with respect to sentence lengths ($\varepsilon_{c,f} = -0.014$), I find that the total cost of reducing crime via longer sentence lengths totals $115 million, twice the cost of Medicaid provision. In order for these two policies to have the same overall cost, $\varepsilon_{c,f}$ would need to be three times as large as the preferred elasticity (i.e., -0.044). Overall, this comparison allows me to conclude that low-income young men would need to be significantly more responsive to changes in sentence lengths in order for harsher punishments to be as cost effective as healthcare provision.

10 Discussion & Conclusion

Motivated by the prevalence of mental illness among the criminal justice population, this paper studies the potential for mental healthcare to serve as a crime-reduction policy. To estimate the

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

causal effect of health insurance on crime, I leverage a discrete break in Medicaid coverage that occurs on an individual's 19th birthday and employ a matched difference-in-differences research design. Importantly, I use rich administrative data linked across various state agencies in South Carolina, which allows me to identify individuals with mental health histories and measure any contact these men have with the criminal justice system.

I find that men who lost access to Medicaid eligibility on their 19th birthdays are more likely to be incarcerated in the subsequent two years than comparable low-income men who were less affected by the termination in eligibility. I show that these effects are entirely driven by men with mental health histories. Moreover, using detailed information from insurance claims, I find that the effects are particularly pronounced for men who were filing mental health claims right before the loss in eligibility and for men who relied on Medicaid for access to mental health medications. Together, these findings suggest that losing access to mental healthcare plays an important role in explaining the observed rise in crime.

The findings of this study offer a number of takeaways and policy implications. First, this paper studies a subpopulation of individuals—low-income young men with mental health histories—for whom access to healthcare serves as an effective deterrence mechanism. Identifying the exact channels through which greater access to mental healthcare reduces criminal propensity (e.g., improved labor market prospects, human capital accumulation) is a fruitful avenue for future research. Furthermore, the cost effectiveness of Medicaid provision suggests that policymakers might consider improving access to healthcare as one of the tools in their arsenal for reducing crime and decreasing criminal justice expenditures. To the extent that mental healthcare improves an individual's decision making, then it may be the case that providing mental healthcare might make traditional, incentive-based strategies for deterring crime more effective.

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

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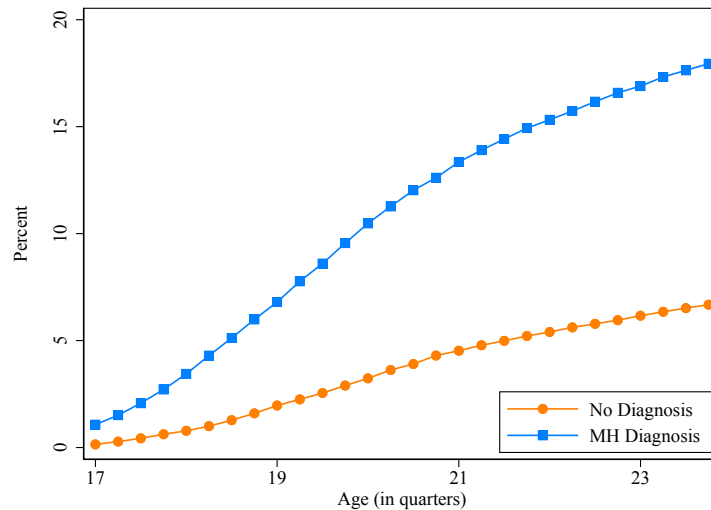
Figures and Tables

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

(a) Ever Arrested
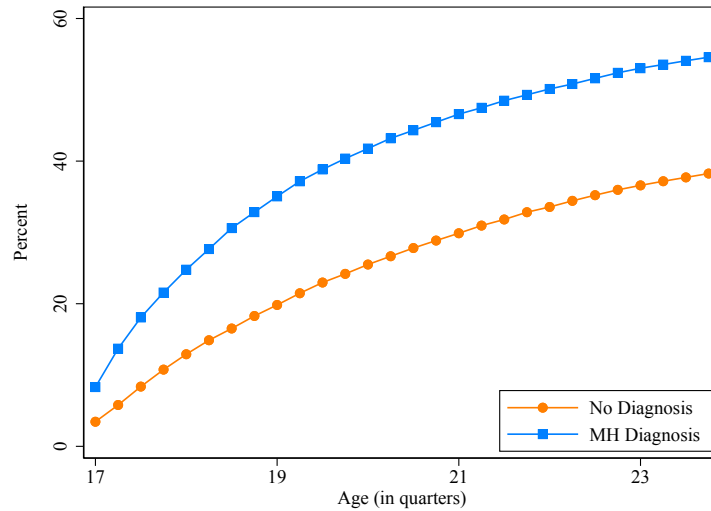
(b) Ever Incarcerated

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

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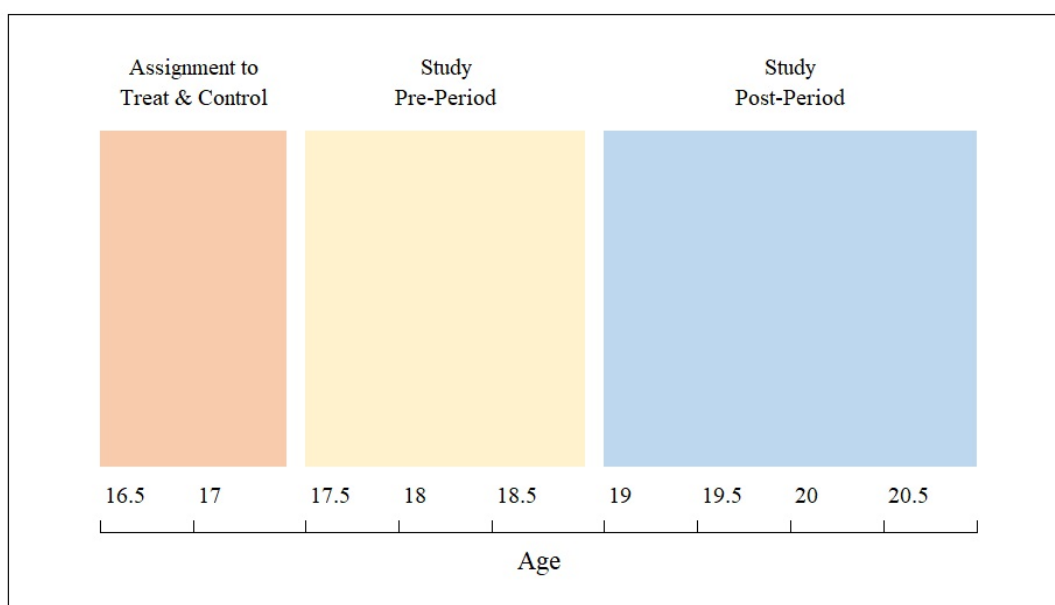
NOTE: Men are assigned to the treatment and comparison groups based on their enrollment in the Medicaid program between ages $16\frac{1}{2}$ and $17\frac{1}{2}$ (orange). Their outcomes are then allowed to evolve naturally in the study pre-period (yellow) and post-period (blue).

Figure 3: Share of Matched Treated and Comparison Men Enrolled in Medicaid (Raw Means)

(a) Full Sample
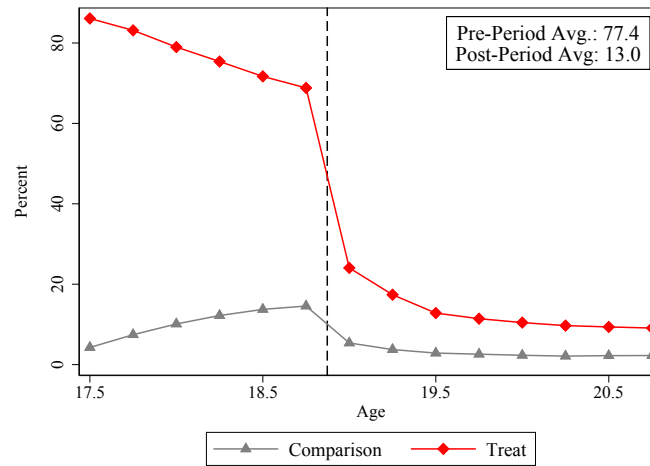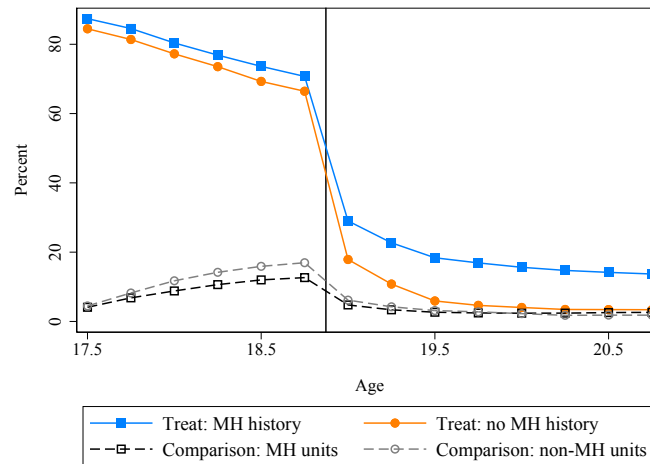
(b) By Mental Health History

NOTE: These graphs use the sample of matched treated and comparison men to plot the share of individuals enrolled in Medicaid in a given quarter. Panel (a) plots averages for the full sample and panel (b) plots averages separately by mental health history (prior to age 16). The averages reported in panel (a) correspond to the pre- and post-period shares of treated individuals.

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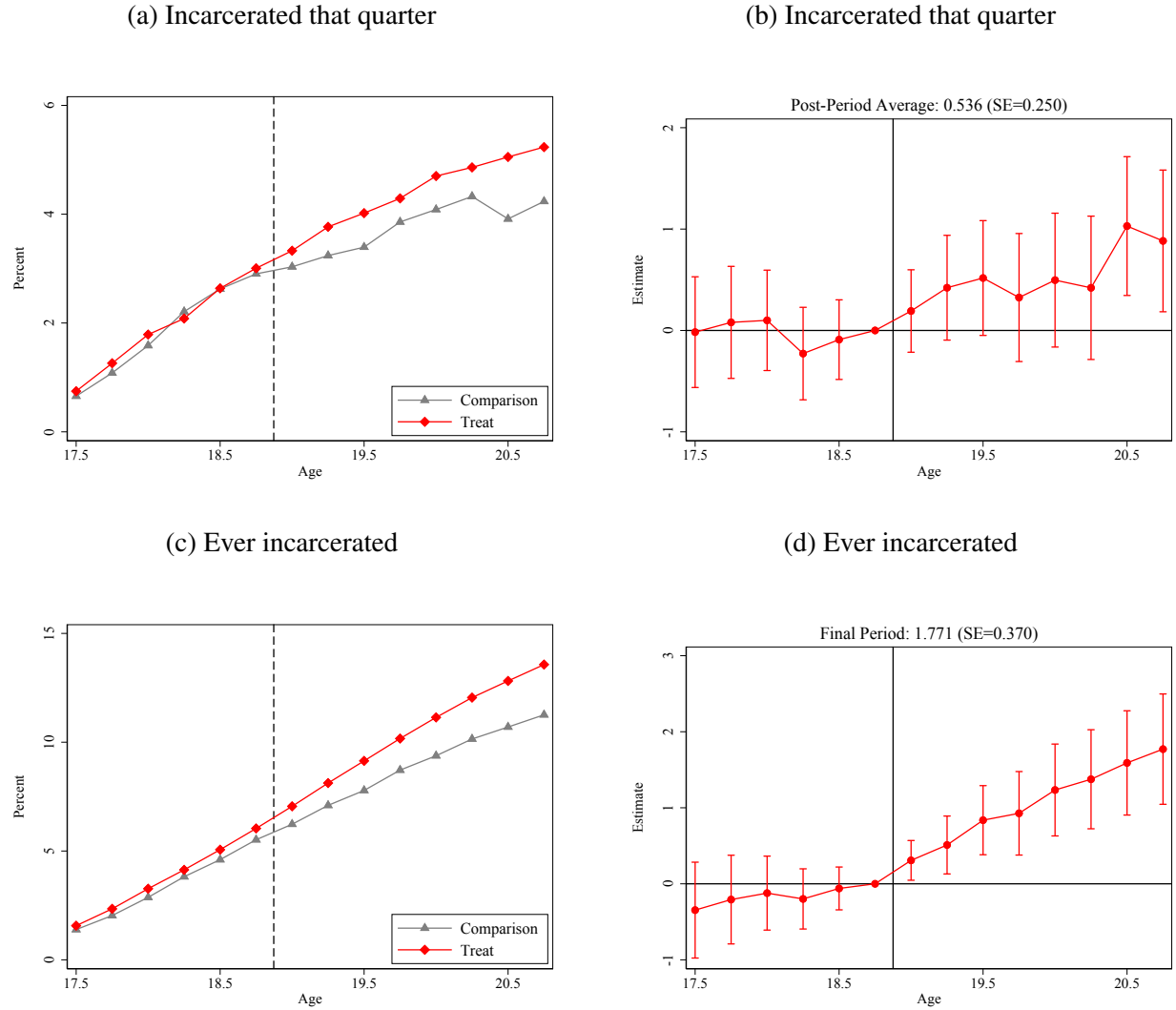
(a) Incarcerated that quarter

(b) Incarcerated that quarter

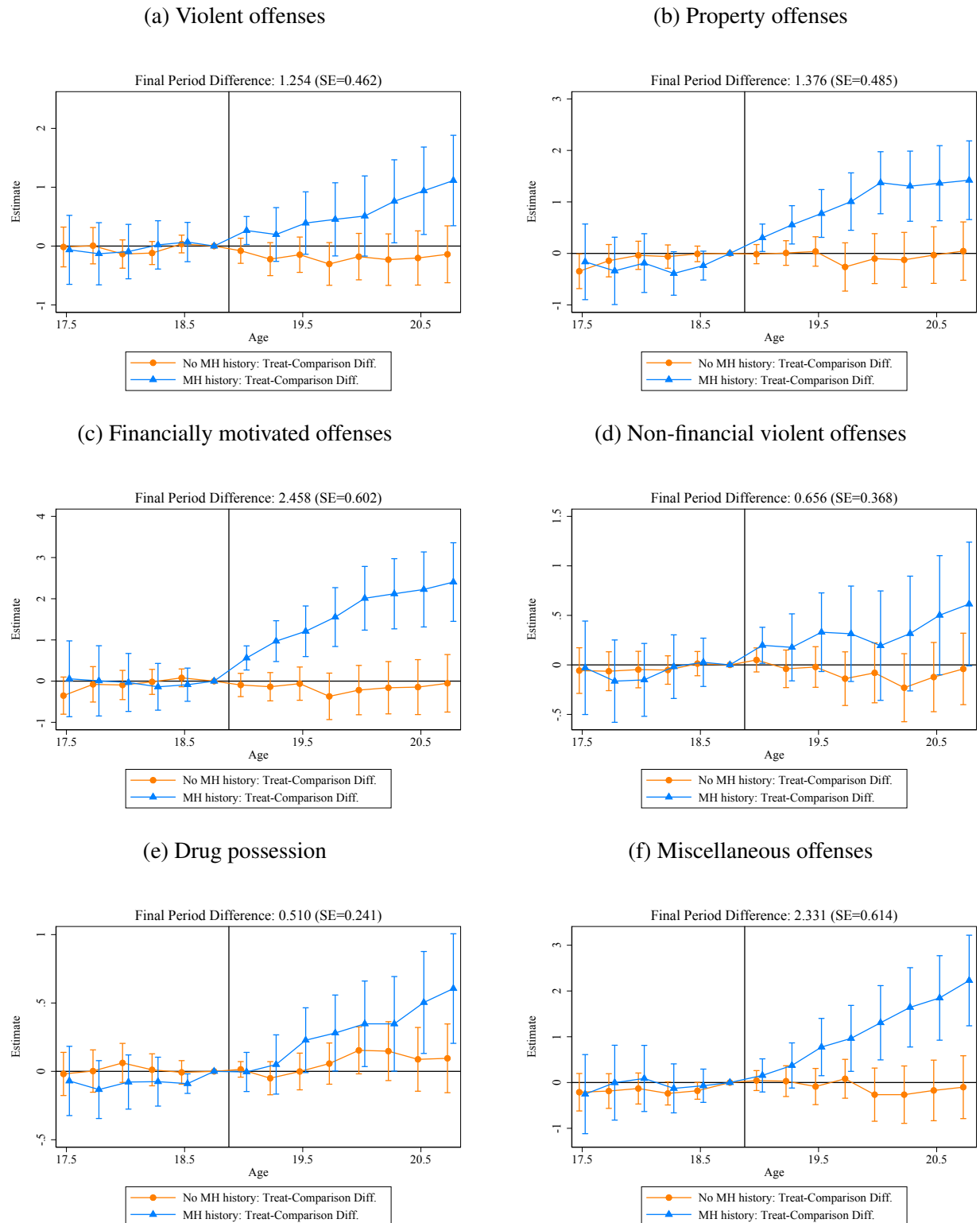(c) Ever incarcerated

(d) Ever incarcerated

NOTE: The dependent variable is a measure of whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). Panels (a) and (c) plot the raw means of the matched sample. Panels (b) and (d) plot the treat-control differences estimated using equation (1). "Post-Period Average" and "Final Period" refer to the average of the post-period β_τ coefficients and to β_7, respectively. "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. Standard errors are clustered at the individual level.

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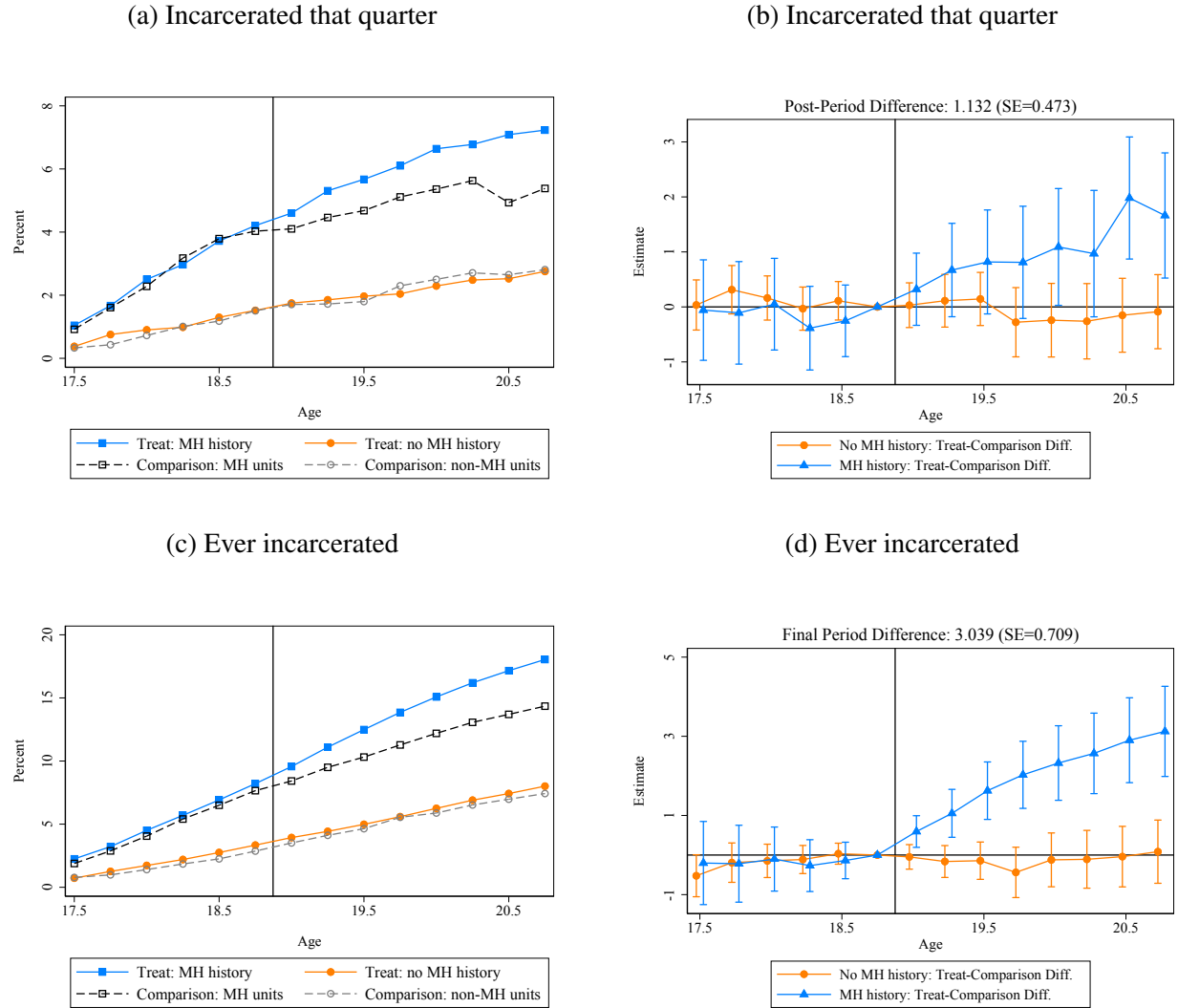
(a) Incarcerated that quarter   (b) Incarcerated that quarter

(c) Ever incarcerated   (d) Ever incarcerated

NOTE: The dependent variable is a measure of whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). Panels (a) and (c) plot the raw means of the matched sample separately by mental health history. Panels (b) and (d) plot the treat-control differences estimated using equation (2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients and to α_7, respectively. "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. Standard errors are clustered at the individual level.

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

(a) Violent offenses

(b) Property offenses

(c) Financially motivated offenses

(d) Non-financial violent offenses

(e) Drug possession

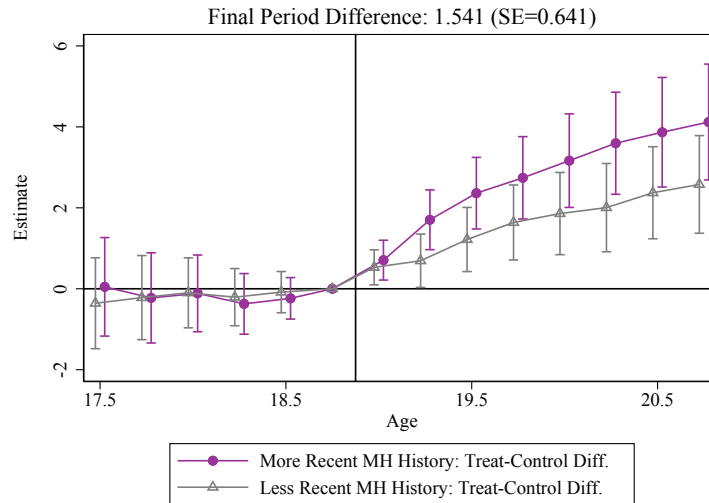
(f) Miscellaneous offenses

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

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

(a) Incarcerated that quarter
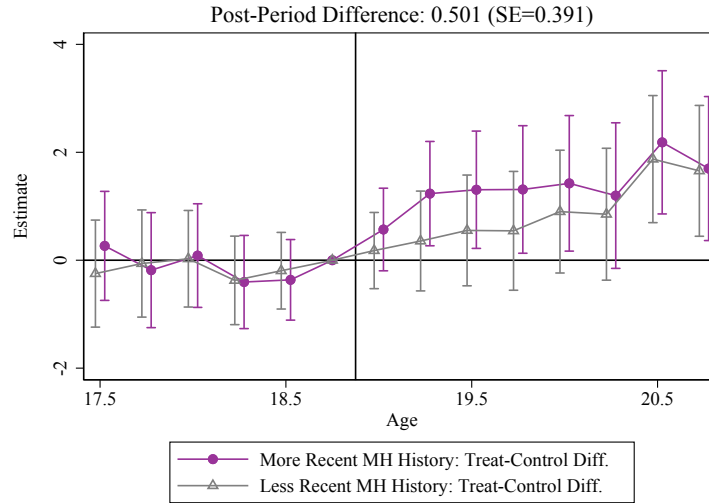

(b) Ever incarcerated

NOTE: The dependent variable is a measure of whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). These figures only consider treated men with mental health histories and their corresponding matched comparison units. The figures plot the treat-control differences for treated men who were recently filing mental health claims (in purple) and for men who were less recently using mental health services (in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively. Recent usage is defined as filing a mental health claim during the pre-period. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

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

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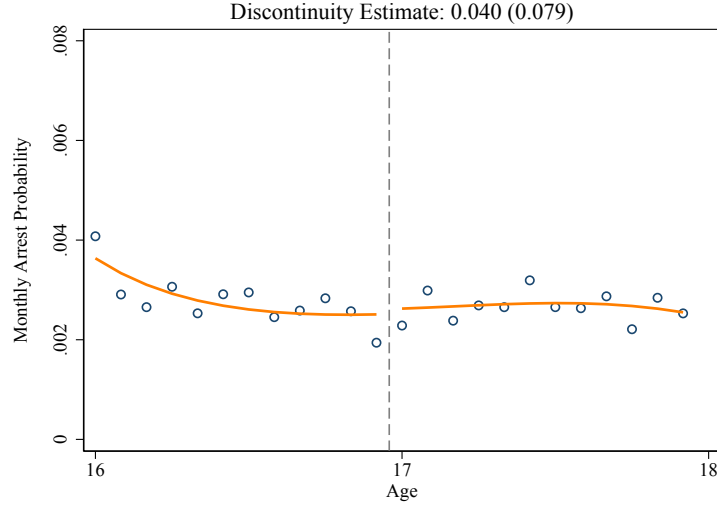
(b) By Mental Health History
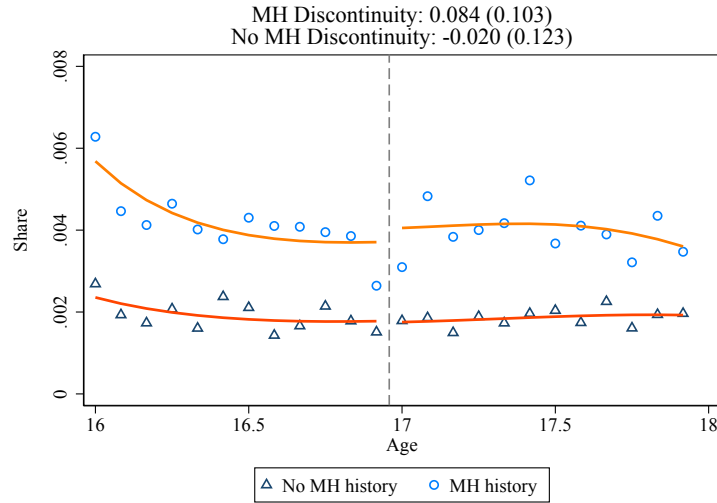
NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. The sample consists of men born between 1990–1999 who had not been arrested for a felony prior to age 16. The bottom panel splits the sample based on an individual's mental health history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

Table 1: Summary Statistics of Full and Matched Sample

	(1) All men	(2) Treated men	(3) Treated men with mental health histories
Black	0.70	0.73	0.70
Mental health history (pre 16)	0.47	0.55	1.00
Juvenile justice referral (ever)	0.37	0.41	0.52
Arrests (ever)	0.25	0.27	0.33
SNAP (ever)	0.80	0.88	0.89
TANF (ever)	0.23	0.28	0.30
Observations	33,252	21,418	11,866

NOTE: Column 1 reports means for all individuals in the 1990–1993 cohorts who were enrolled in Medicaid at some point between the ages of 10 and 18 (inclusive). Column 2 reports means for men enrolled in Medicaid at ages 16.5–17.5 (i.e., the high-enrollment group). Column 3 reports means for treated individuals with a mental health history. "Mental health history" refers to having a claim with a mental health diagnosis or for a mental health medication prior to age 16. Every other outcome is measured between the ages of 10 and 18 (inclusive). "Arrests" refers to having an arrest record in the South Carolina Law Enforcement Division data. The final two rows measure whether individuals were ever enrolled in the SNAP and TANF programs. See Table A1 for summary statistics of eligible candidates for the matching procedure.

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

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The effects in columns 1 and 2 are estimated using equation (1) and correspond to the post-period average of the β_τ coefficients and to the β_7 coefficient, respectively. The effects in columns 3 and 4 are estimated using equation (2) and correspond to the post-period average of the α_τ coefficients and to the α_7 coefficient, respectively. For the non-cumulative variables, "Control Average" refers to the average post-period incarceration rate of the matched comparison group (for the full sample in column 1 and just for those with a mental health history in column 3). For the cumulative variables, this statistic reports the incarceration rate of the comparison group measured in the last quarter of the post period (for the full sample in column 2 and for men with a mental health history in column 4). Standard errors are clustered at the individual level.

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

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

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

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

	(1) Violent	(2) Property	(3) Financially motivated	(4) Non-financial violent	(5) Drug possession	(6) Misc.
Estimated Effect	1.254***	1.376***	2.458***	0.656*	0.510**	2.331***
	[0.462]	[0.485]	[0.602]	[0.368]	[0.241]	[0.614]
Control Average	4.23	5.16	7.97	2.68	0.95	6.93
Observations	424,606	424,606	424,606	424,606	424,606	424,606

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The estimated effects correspond to the α_7 coefficient in equation (2), analogous to the estimates presented in Figured 6. "Misc." refers to offenses that were not classified in the previous three categories (columns 3–5). "Control Average" reports the share of comparison men with mental health histories who had ever been incarcerated for that offense type in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level. For more details on offense classifications, see Appendix B.

Table 5: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for Men with Mental Health Histories, by Recency of Mental Health Claims

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | | Financially | Non-financial | Drug | |
	All	Violent	Property	motivated	violent	possession	Misc.
Less recent	2.578***	1.037**	1.066***	1.968***	0.584*	0.629***	1.916***
	[0.615]	[0.423]	[0.404]	[0.515]	[0.343]	[0.223]	[0.541]
More recent	1.541**	0.216	1.005**	1.258**	0.107	-0.078	0.840
	[0.641]	[0.406]	[0.461]	[0.560]	[0.325]	[0.255]	[0.548]
Post-Period Avg.	17.95	6.89	8.63	12.79	4.36	1.94	11.81
Treated Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers treated men with mental health histories and their matched comparison units, and splits the group based on whether treated men filed mental health claims in the pre-period. "Less recent" refers to men without mental health claims in the pre-period, and the reported estimates correspond to the β_7 coefficient using equation (2). "More recent" refers to men who filed mental health claims in the pre-period, and the estimates correspond to the α_7 coefficient using the same equation. "Misc." refers to offenses that were not classified in the previous three categories (columns 4–6). "Post-Period Avg." reports the share of treated men with less-recent mental health histories who had ever been incarcerated for that offense type in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level. For more details on offense classifications, see Appendix B.

Table 6: Effect of Medicaid Disenrollment on Public Safety

	(1) All	(2) Violent	(3) Property	(4) Drug	(5) Other
Treat × Post	0.515***	0.135*	0.162*	0.234***	0.151**
	(0.145)	(0.073)	(0.084)	(0.073)	(0.075)
Control Average	1.21	0.39	0.50	0.20	0.47
Scaled Effect	208,978	208,978	208,978	208,978	208,978

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

Table 7: 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	$12.2	$16.1	$19.9
Fiscal costs	$2.6	$2.9	$3.3
Social costs	$6.1	$7.1	$8.2
Total	$20.9	$26.1	$31.4

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

Table 8: Summary of Estimated Elasticities of Crime With Respect To Sentence Lengths

	Full Sample		1997–1999 Cohorts	
	Sentence Length	Sentence Served	Sentence Length	Sentence Served
Elasticity	-0.007	-0.014	-0.009	-0.017

NOTE: This table reports the elasticities computed in Section 9, using the most negative deterrence effect consistent with the confidence intervals. The first two columns use the full sample of men, whereas the third and fourth columns focus on the three youngest birth cohorts (the group that is least likely to drop out of school at age 17). Columns 1 and 3 use the average length of prison sentences (4.5 years) to calculate the elasticity. Columns 2 and 4 use the average time served in state prison (2.3 years) to calculate the elasticity. For more details on the derivation of these estimates, I refer the reader to Appendix D.
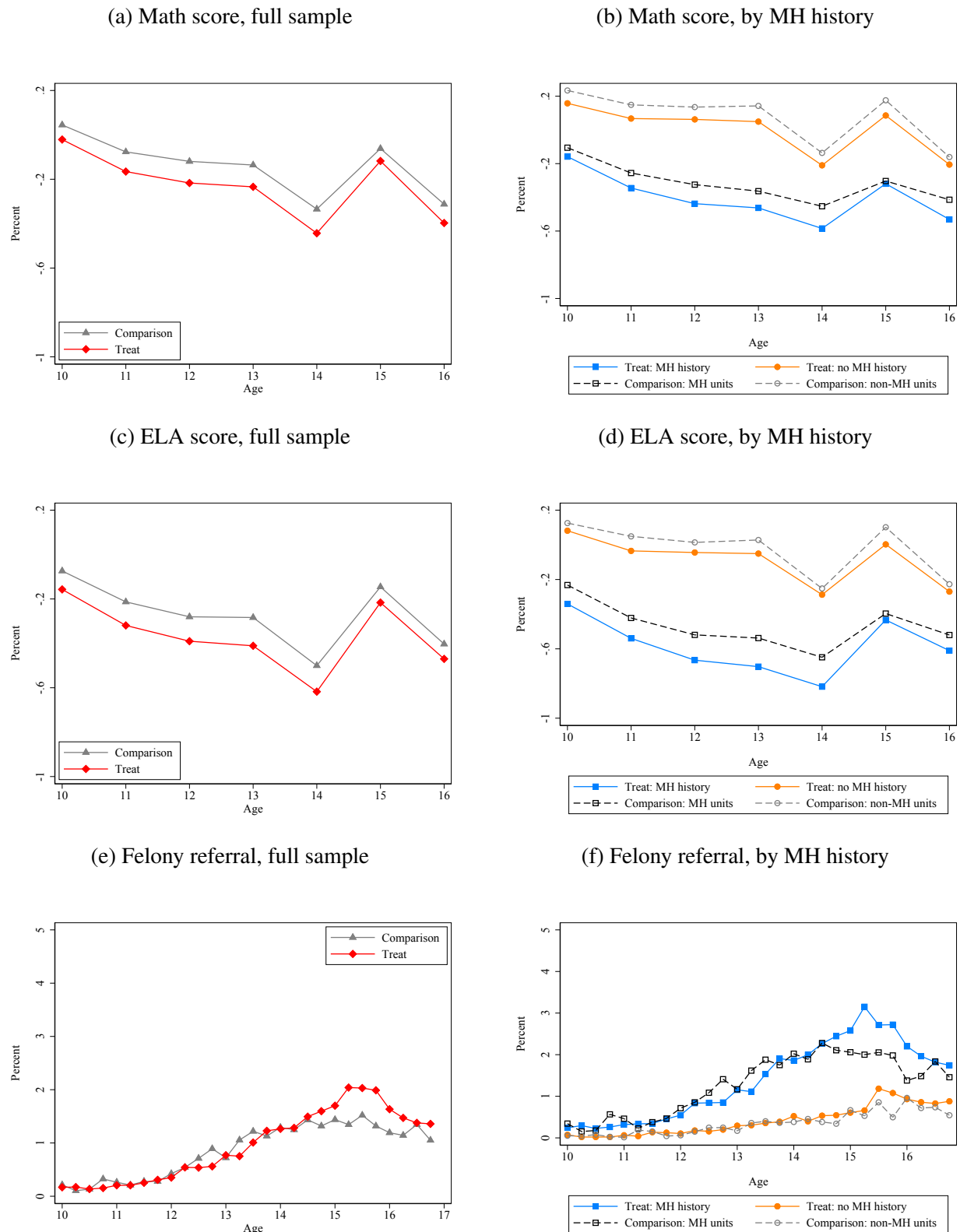
A   Appendix Figures and Tables

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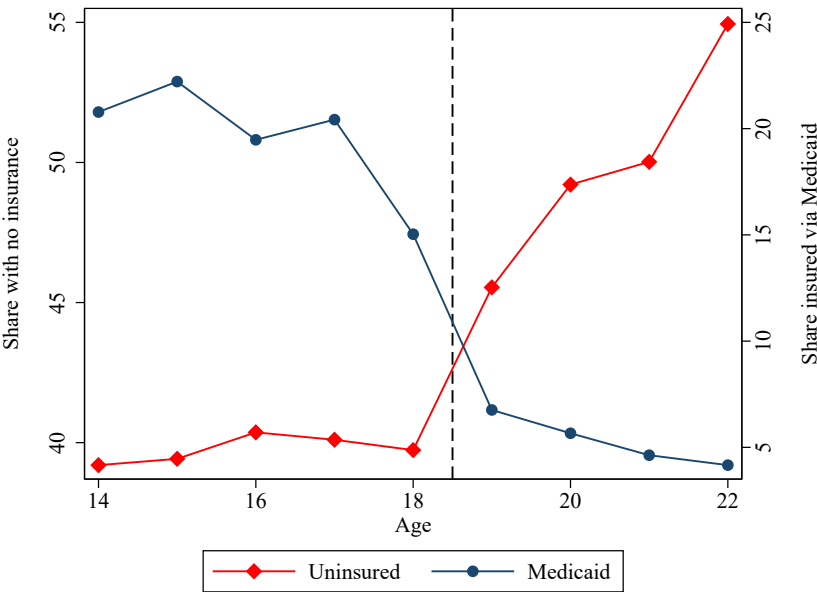

NOTE: This figure uses the 2004–2015 American Community Surveys (Ruggles et al. 2020) to plot the share of men in South Carolina who are insured via Medicaid and who do not have any health insurance coverage at a given age.

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

(a) Math score, full sample

(b) Math score, by MH history

(c) ELA score, full sample

(d) ELA score, by MH history

(e) Felony referral, full sample

(f) Felony referral, by MH history

NOTE: These graphs plot the raw means of each outcome in a given year (or quarter in the last two panels) for the full sample of treated and comparison men as well as separately by mental health history.
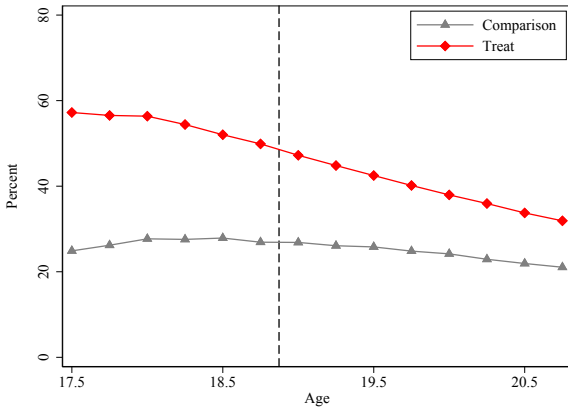
Figure A3: Share of Matched Treated and Comparison Men Filing Medicaid Claims (Raw Means)
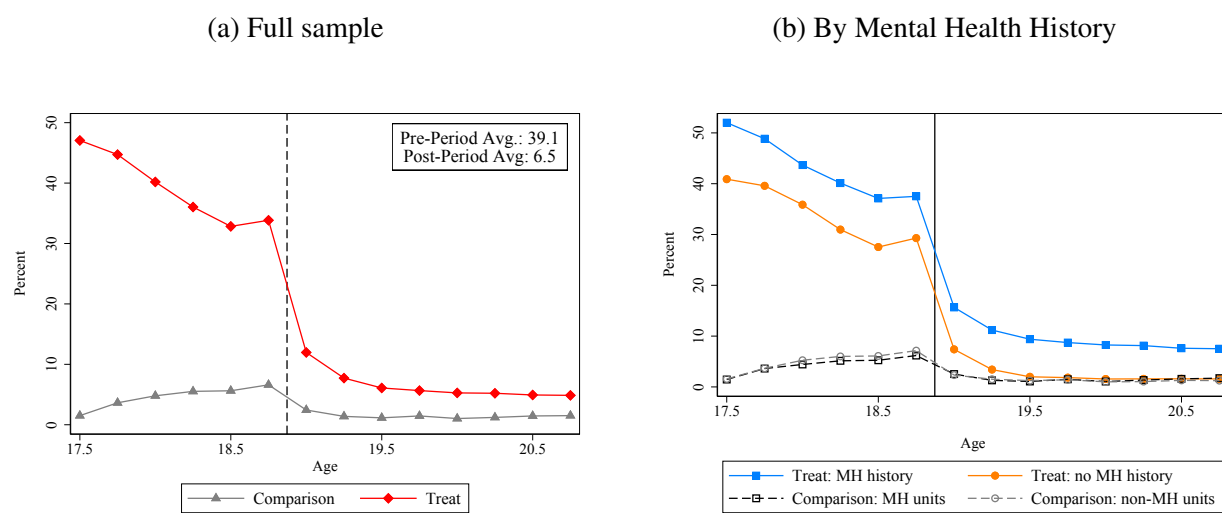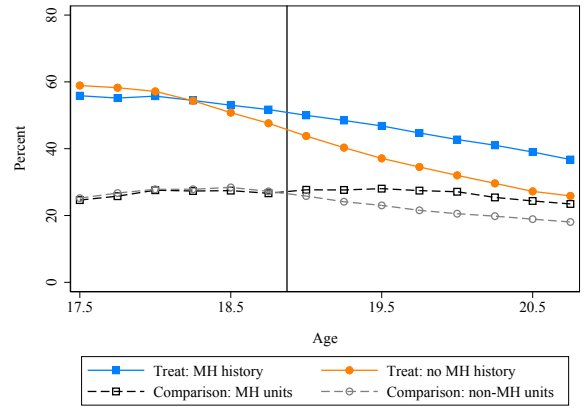
(a) Full sample

(b) By Mental Health History

NOTE: These graphs use the sample of matched treated and comparison men to plot the share of individuals filing any Medicaid claims in a given quarter. Panel (a) plots averages for the full sample and panel (b) plots averages separately by mental health history (prior to age 16). The averages reported in panel (a) correspond to the pre- and post-period shares of treated individuals.

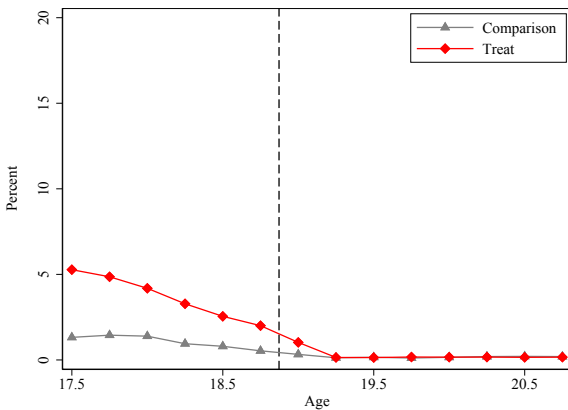# Figure A4: Enrollment in Public Assistance Programs (Raw Means)
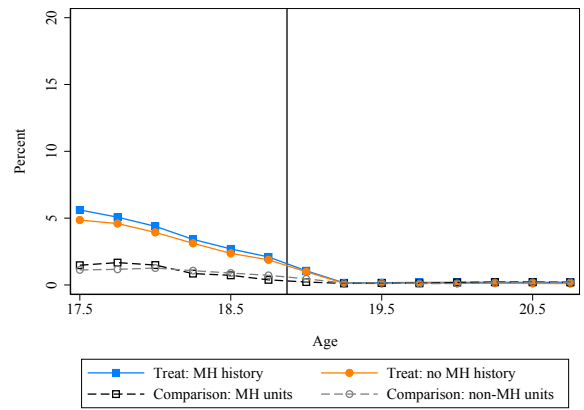
(a) SNAP, full sample

(b) SNAP, by MH history

(c) TANF, full sample

(d) TANF, by MH history

NOTE: These graphs use the sample of matched treated and comparison men to plot the share of individuals enrolled in the SNAP and TANF programs in a given quarter. "SNAP" refers to the Supplemental Nutrition Assistance Program and "TANF" refers to the Temporary Assistance for Needy Families program. Panels (a) and (c) plot averages for the full sample and panels (b) and (d) plot averages separately by mental health history (prior to age 16).

Figure A5: Robustness of Main Result to Matching, Sample, and Variable Construction
(Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)
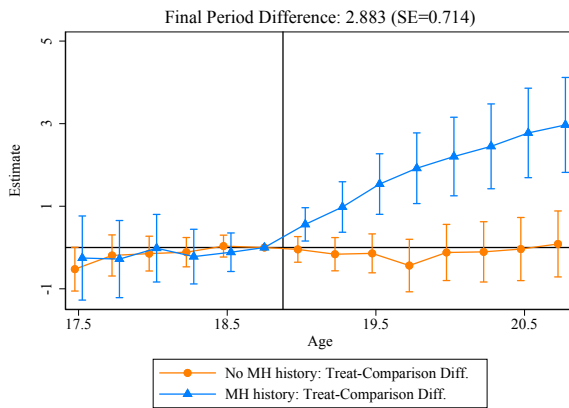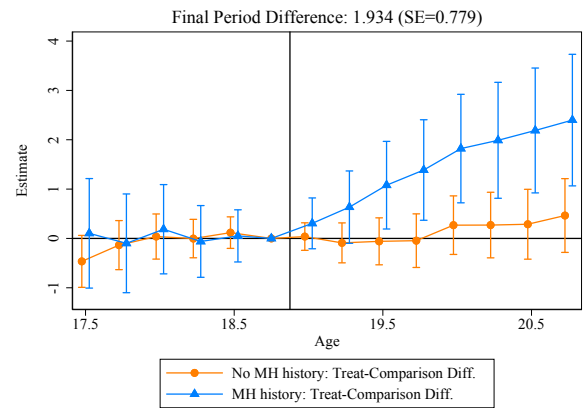
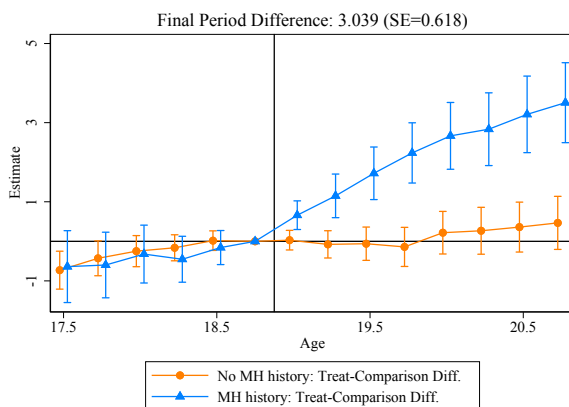(a) Baseline
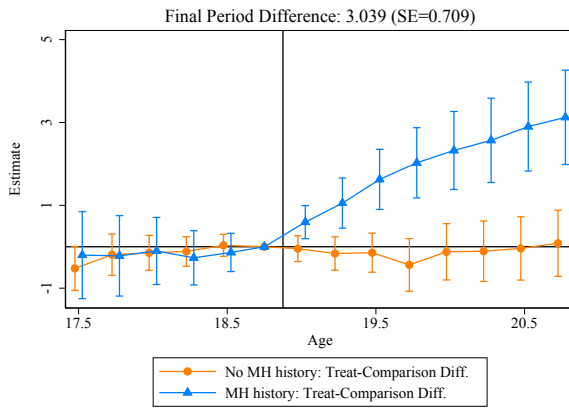
(b) Only one match per treated
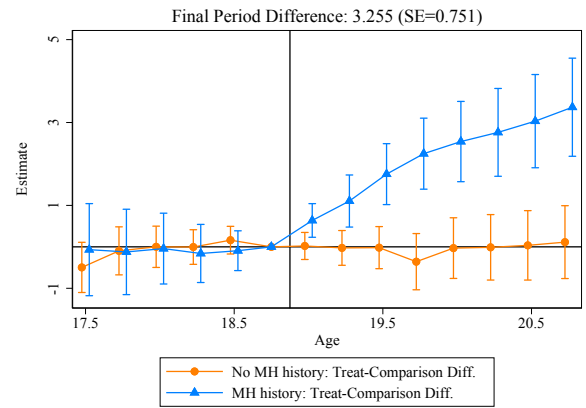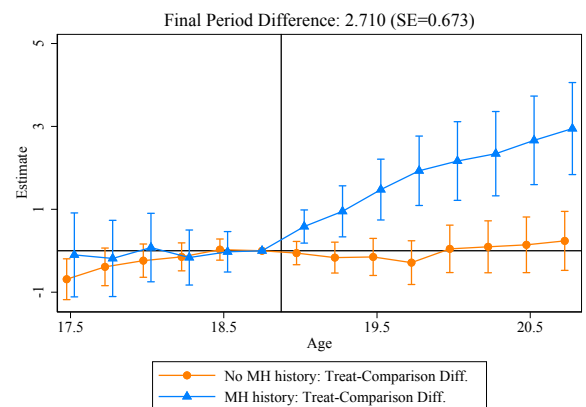

(c) Downweighting outliers
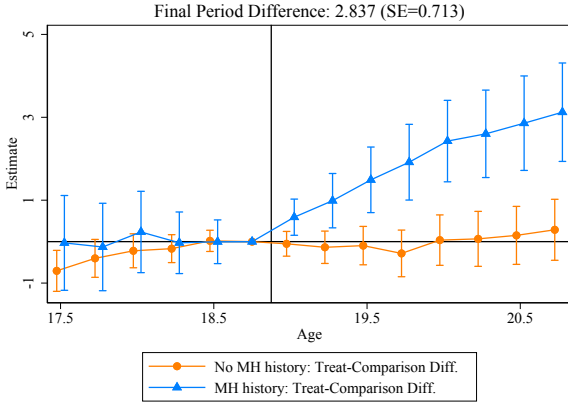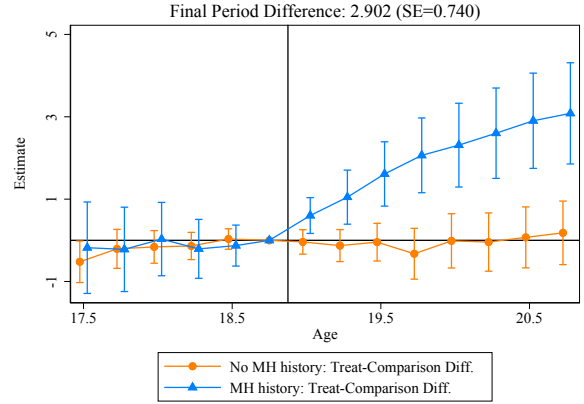
(d) Matching on school

(e) No matching

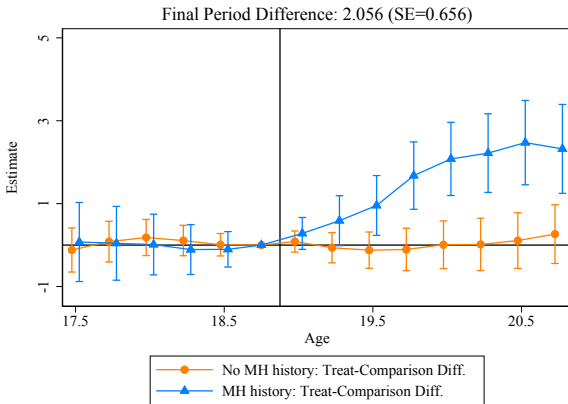(f) DFL: same matching characteristics

(g) DFL: matching on school

Final Period Difference: 2.837 (SE=0.713)

- No MH history: Treat-Comparison Diff.
- MH history: Treat-Comparison Diff.

(h) Only using diagnoses

Final Period Difference: 2.902 (SE=0.740)

- No MH history: Treat-Comparison Diff.
- MH history: Treat-Comparison Diff.

(i) Assignment in pre-period

Final Period Difference: 2.056 (SE=0.656)

- No MH history: Treat-Comparison Diff.
- MH history: Treat-Comparison Diff.

(j) Excluding foster care & SSI youth
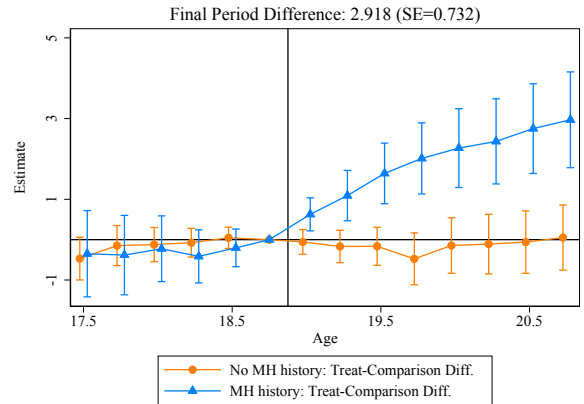
Final Period Difference: 2.918 (SE=0.732)

- No MH history: Treat-Comparison Diff.
- MH history: Treat-Comparison Diff.

NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. Each figure plots the treat-control differences estimated using equation (2) and "Final Period" refers to the α_7 coefficient. Panel (a) reproduces the baseline result from Figure 5. Panel (b) randomly assigns one comparison individual to each treated individual. Panel (c) drops comparison observations with disproportionate weight in the regression. Panel (d) matches treated and comparison men using school attended instead of district. Panel (e) runs the specification using all eligible treated and comparison individuals (i.e., with no matching). Panels (f) and (g) use the DFL re-weighting approach to re-weight the eligible comparison individuals. Panel (f) re-weights men based on the same characteristics as the baseline matching approach, and panel (g) replaces district with school. Panel (h) only uses diagnoses to identify men with mental health histories. Panel (i) assigns men to the treatment and comparison group based on their Medicaid enrollment in the pre-period. Panel (j) excludes foster care and SSI youth (and their corresponding comparison units) from the sample. Standard errors are clustered at the individual level.

Figure A6: Raw Means for All Eligible Treated & Comparison Men (No Matching)

(a) Incarcerated that quarter

(b) Ever incarcerated

(c) Incarcerated that quarter

(d) Ever incarcerated
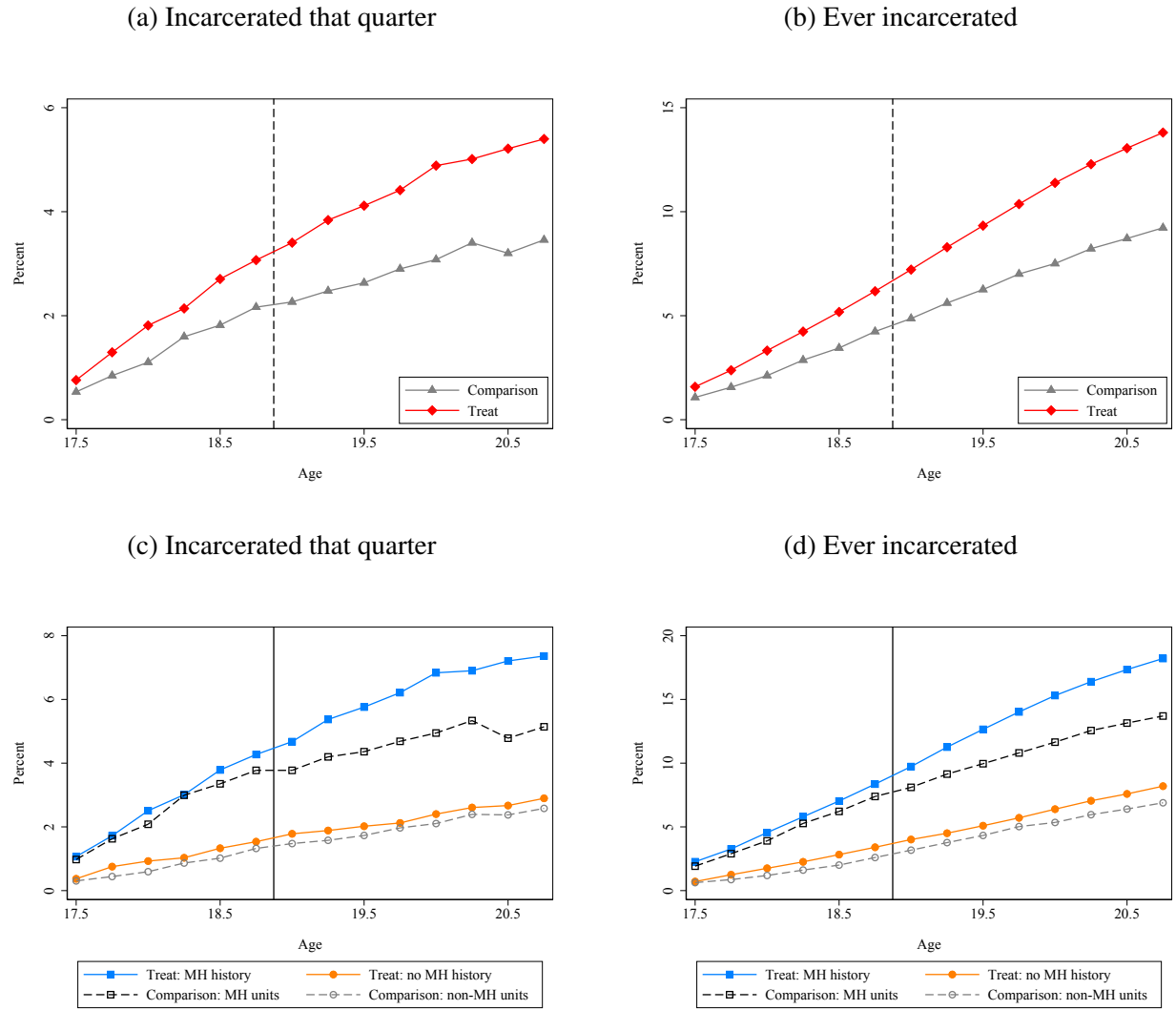
NOTE: The dependent variable is a measure of whether an individual was incarcerated. Panels (a) and (b) plot the raw means for all eligible treated and comparison individuals, and panels (c) and (d) plot these means separately by men's mental health history prior to age 16.

Figure A7: Public School Enrollment (Raw Means)

(a) Full sample
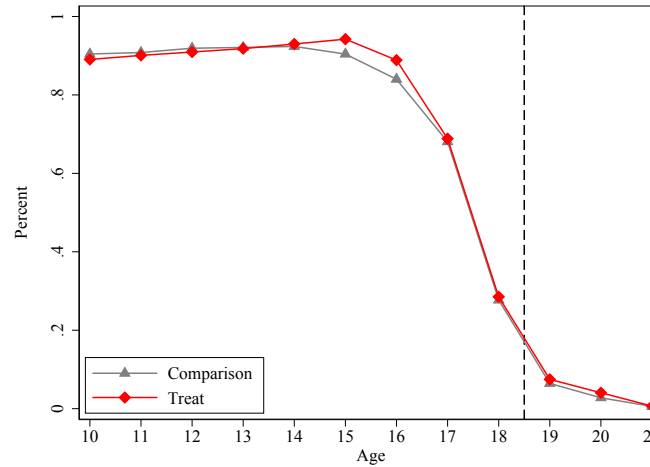
(b) By Mental Health History
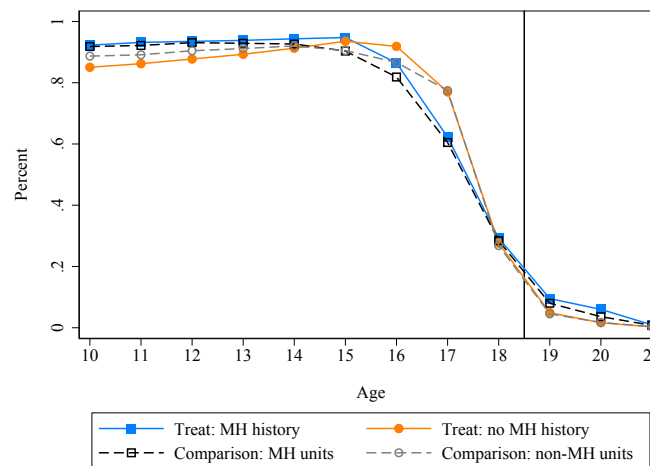
NOTE: These graphs plot the share of individuals enrolled in public school at a given age for the full sample of matched treated and comparison men as well as separately by mental health history. Data on school enrollment comes from South Carolina's Department of Education, and being enrolled refers to appearing in the administrative dataset that contains information on all public school students.

Figure A8: Likelihood of Ever Being Incarcerated for Men with Mental Health Histories Enrolled in Medicaid, by Recency of Mental Health Claims
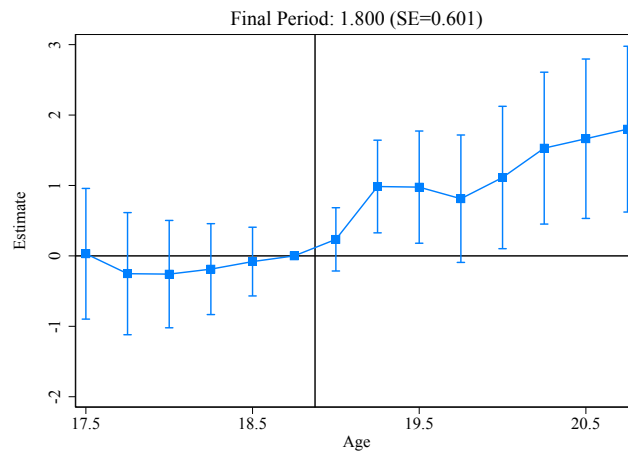
NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated. This figure considers men with mental health histories who were enrolled in Medicaid during the assignment period (see Figure 2). The figure plots the treat-control differences estimated using equation 1, in which treated men are those with mental health claims in the study pre-period and comparison units are those with no mental health claims in the pre-period. Similar to the baseline approach, I implement a matching procedure prior to estimation so that the treatment and comparison group are balanced on observable characteristics (i.e., race, year of birth, school district). Standard errors are clustered at the individual level.

Figure A9: Placebo Check: Likelihood of Ever Being Incarcerated Around
Ages 18 and 19, by Mental Health History
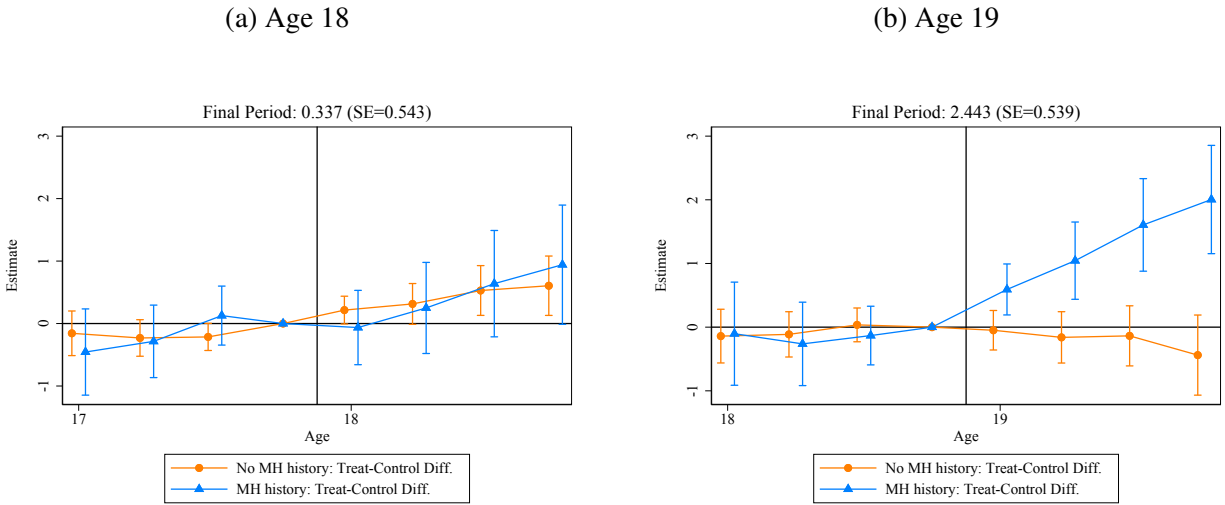
(a) Age 18

(b) Age 19

NOTE: The dependent variable is a measure of whether an individual has ever been incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). Panel (a) presents the results from the placebo check measuring the likelihood of incarceration around age 18. Men are assigned into groups based on their Medicaid enrollment at ages 15.5–16.5 (instead of 16.5–17.5) and then the matching procedure is implemented. Panel (b) presents the results from the baseline approach measuring the likelihood of incarceration around age 19. In both panels, treat-control differences are estimated using equation (2) with three and four quarters in the pre- and post-period, respectively. "Final Period" refers to the α_4 coefficient using that equation. Standard errors are clustered at the individual level.

Figure A10: Regression Discontinuity: Arrest Probability Around Age 19

(a) Enrolled in Medicaid, ages 10–18

(b) Mental health history, ages 10–18

(c) Mental health history, ages 16–18

(d) No mental health history, ages 16–18
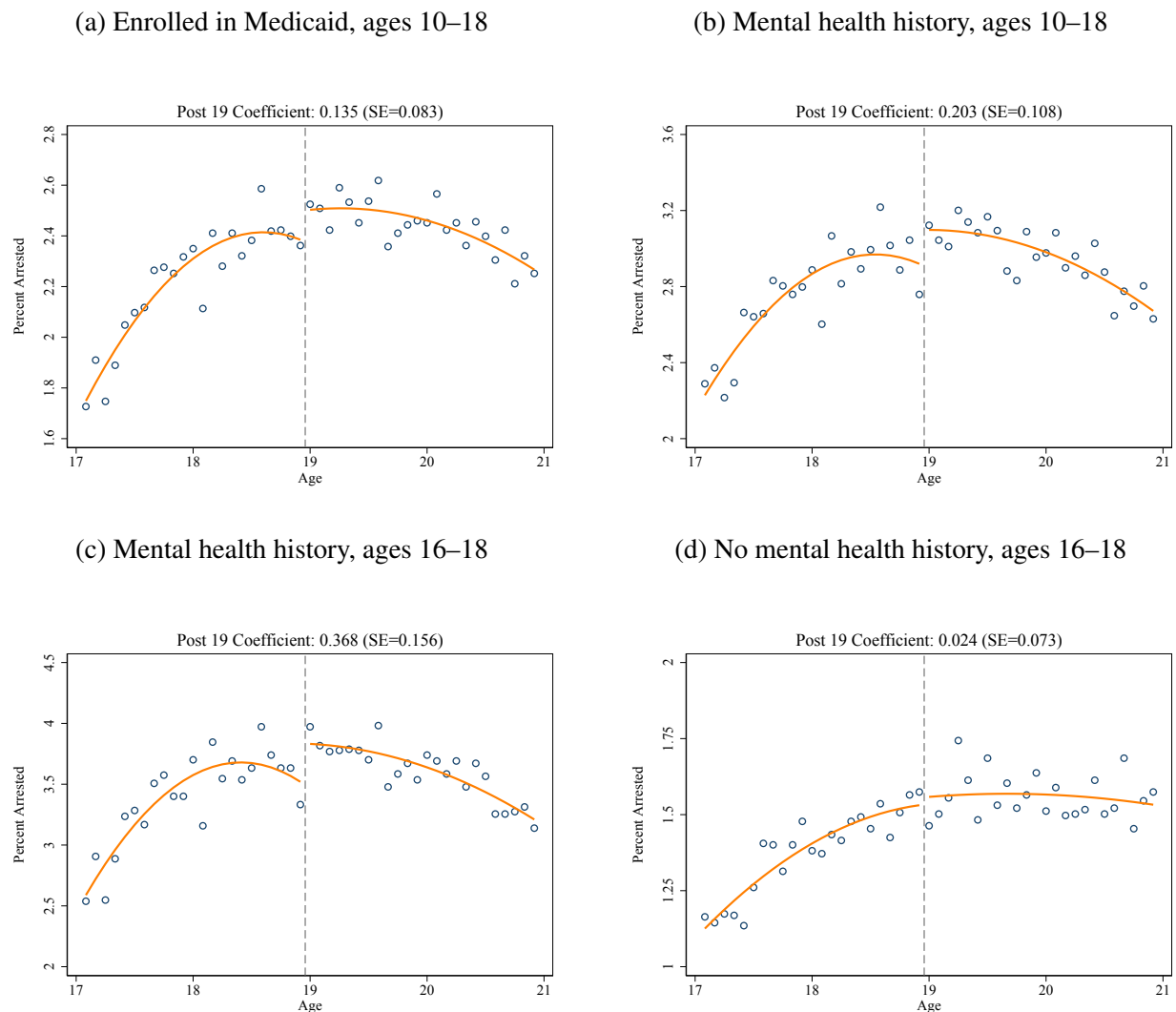
NOTE: This figure plots men's probability of being arrested around their 19th birthday using data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (4). The estimate reported above each figure is the discontinuity estimate from this equation and its standard error (clustering at the individual level). Panel (a) considers all men enrolled in Medicaid between the ages of 10 and 18 (inclusive). Panel (b) considers men with a mental health diagnosis or medication in this age range. Panel (c) considers men with a mental health diagnosis or medication between the ages of 16 and 18 (inclusive), and panel (d) considers men with no mental health diagnosis or medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on exact birthdate and the timing of the age of criminal majority.

Figure A11: Regression Discontinuity: First-Time Incarceration Propensity Around Age 19

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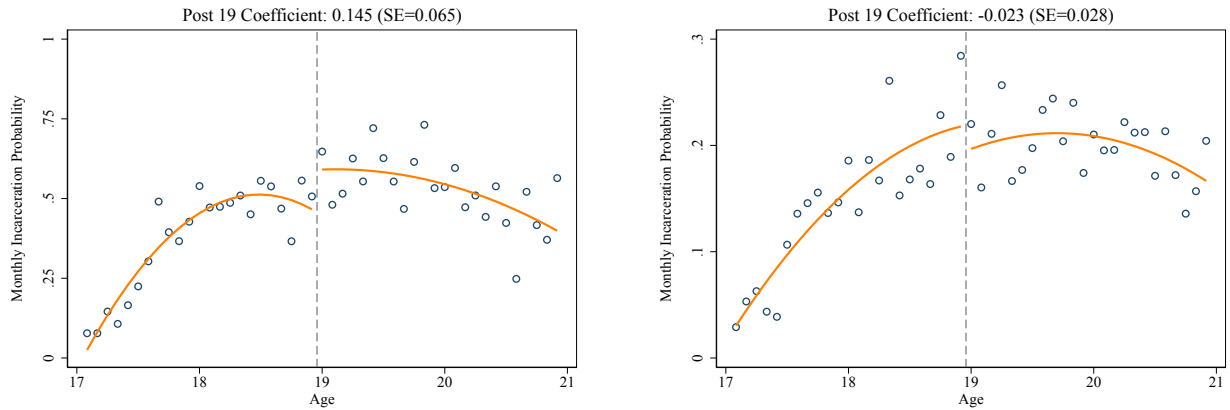
NOTE: This figure plots men's probability of being incarcerated for the first time using data on arrests that ended with an individual being taken into custody. The circles represent the share of men incarcerated for the first time in that month as a share of men who were still at risk of being incarcerated.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

Figure A12: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage

(a) Incarcerated that quarter
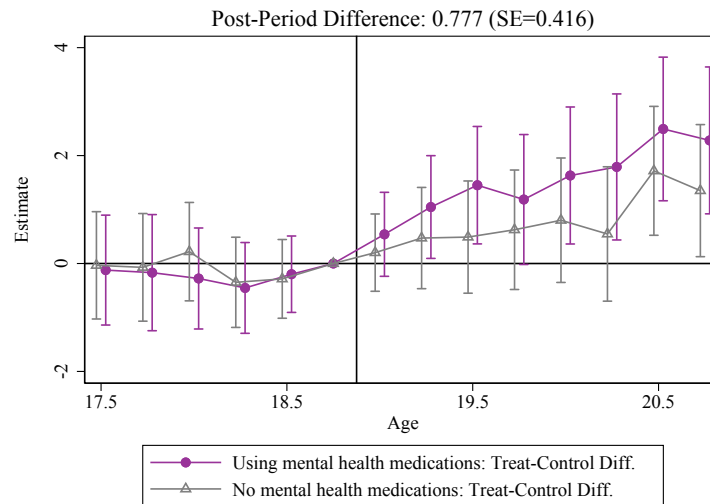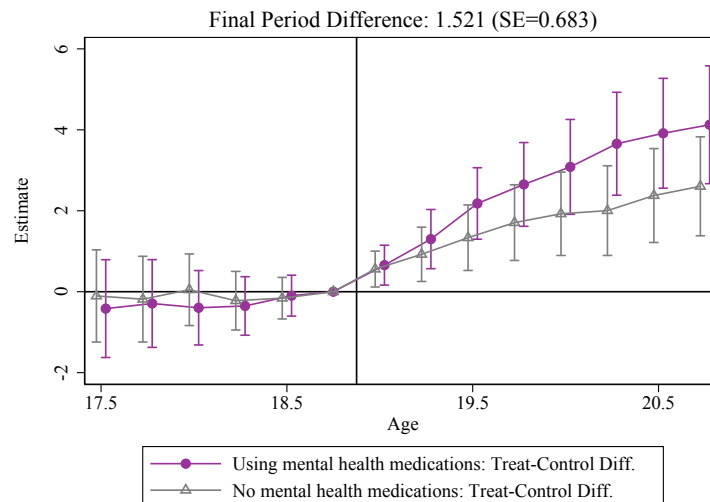
(b) Ever incarcerated

NOTE: The dependent variable is a measure of whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). These figures consider treated men with mental health histories and their matched comparison units. The figures plot the treat-control differences for treated men who were taking mental health medications (in purple) and for men who were not filing claims for medications (in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively. Medication utilization is defined as filing a claim for a psychotropic drug between the ages of 16 and 18. Standard errors are clustered at the individual level.

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

(a) Incarcerated that quarter
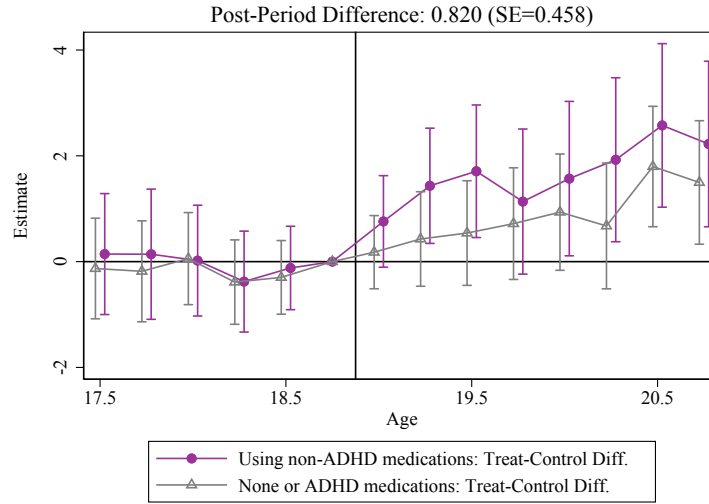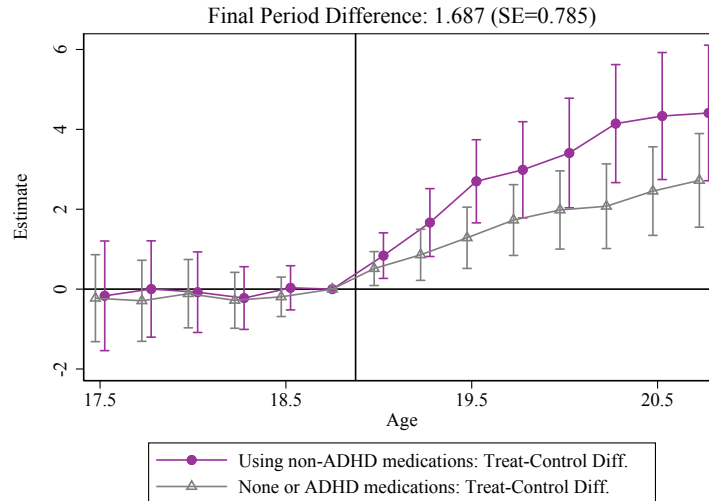
(b) Ever incarcerated

NOTE: The dependent variable is a measure of whether an individual was incarcerated (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). These figures consider treated men with mental health histories and their matched comparison units. The figures plot the treat-control differences for treated men who were taking non-ADHD mental health medications (in purple) and for men who were not filing claims for such medications (in gray) using estimates from equation (2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients and to α_7, respectively. Non-ADHD medication utilization is defined as filing a claim for antianxiety, antidepressant, or antipsychotic medications between the ages of 16 and 18. Standard errors are clustered at the individual level.

Table A1: Summary Statistics of Matching Procedure Candidates and Matches

	(1) Candidate Comparison	(2) Matched Comparison	(3) Candidate Treated	(4) Matched Treated
Black	0.67	0.73	0.72	0.73
Mental health history (pre 16)	0.34	0.55	0.56	0.55
Juvenile justice referral (ever)	0.29	0.34	0.41	0.41
Arrests (ever)	0.19	0.22	0.27	0.27
SNAP (ever)	0.71	0.73	0.88	0.88
TANF (ever)	0.14	0.16	0.28	0.28
Observations	8,964	8,911	22,063	21,418

NOTE: "Candidate" refers to individuals who were eligible for the matching procedure. "Matched" refers to individuals who were successfully matched. In column 2, individuals are weighted using the total weight that each individual gets in the matched sample. "Mental health history" refers to having a claim with a mental health diagnosis or for a mental health medication prior to age 16. Every other outcome is measured between the ages of 10 and 18 (inclusive). "Arrests" refers to having an arrest record in the South Carolina Law Enforcement Division data. The final two rows measure whether individuals were ever enrolled in the SNAP and TANF programs.

Table A2: Distribution of Mental Health Diagnoses and Medications

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

NOTE: This table reports the share of men that have an insurance claim with that diagnosis or for that mental health medication at any point between the ages of 10 and 18 (inclusive). The first column considers all men who were enrolled in Medicaid between the ages of 10 and 18 (inclusive). The second column considers the high-enrollment group (i.e., the treated men). The final column considers high-enrollment men with a mental health history prior to age 16. "Other neurodevelopmental disorder" refers to disorders in the neurodevelopmental disorders category that are not intellectual disabilities or ADHD. "Other unclassified disorder" refers to having a mental health diagnosis not captured by the previous eleven categories. For more details on the variable construction, see Appendix B.

Table A3: Robustness to Specification, Matching, & Samples for Men's Likelihood of Incarceration

	Treated Units	Incarcerated		Ever Incarcerated	
		Full Sample	MH History	Full Sample	MH History
Baseline	21,418	0.536**	1.132**	1.771***	3.039***
		(0.250)	(0.473)	(0.370)	(0.709)
Cluster: match level	21,418	0.536***	1.132***	1.771***	3.039***
		(0.114)	(0.219)	(0.187)	(0.363)
Cluster: match & ind.	21,418	0.536**	1.132**	1.771***	3.039***
		(0.250)	(0.471)	(0.368)	(0.706)
One control per treat	21,418	0.576**	0.942*	1.919***	3.255***
		(0.257)	(0.489)	(0.391)	(0.751)
Downweighting outliers	21,418	0.473*	1.024**	1.675***	2.883***
		(0.250)	(0.473)	(0.372)	(0.714)
Matching on school	19,675	0.611**	0.594	1.504***	1.934**
		(0.275)	(0.526)	(0.406)	(0.779)
No matching	22,063	0.699***	1.084***	2.617***	3.039***
		(0.182)	(0.406)	(0.292)	(0.618)
DFL re-weighting	22,063	0.462*	0.856*	1.761***	2.710***
		(0.245)	(0.455)	(0.356)	(0.673)
DFL re-weighting, school	22,063	0.617**	1.076**	1.888***	2.837***
		(0.268)	(0.498)	(0.377)	(0.713)
Using diagnoses only	21,363	0.575**	1.265***	1.680***	2.902***
		(0.249)	(0.489)	(0.376)	(0.740)
Assignment in pre-period	20,820	0.379*	0.603	1.385***	2.056***
		(0.223)	(0.426)	(0.340)	(0.656)
Excluding SSI & foster care	18,605	0.455*	1.129**	1.522***	2.918***
		(0.242)	(0.481)	(0.368)	(0.732)

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The first row reproduces the baseline estimates seen in Figure 5. The second and third rows cluster the standard errors at the match level and at the match and individual level, respectively. The fourth row forces each treated unit to have one comparison unit. The fifth row drops comparison units with disproportionate weight in the regression. The sixth row matches men using school instead of district. The seventh row uses all eligible treated and comparison men (i.e., no matching). The eighth and ninth rows use the DFL re-weighting approach to re-weight eligible comparison men. The eighth row uses the baseline matching characteristics and the ninth row uses school. The tenth row only uses diagnoses to identify men with mental health histories. The eleventh row assigns men to the treatment and comparison group based on their enrollment during the pre-period. The twelfth row excludes foster care and SSI youth (and their comparison units). Except for the second and third rows, standard errors are clustered at the individual level and are reported under their estimate in parentheses.

Table A4: Placebo Check: Likelihood of Ever Being Incarcerated Around
Different Ages, by Mental Health History

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports the results from the robustness checks conducted in Section 6.3.3 in which the baseline empirical approach is replicated and shifted back two years (for Age 17) and one year (for Age 18). All of the estimates correspond to the α_4 coefficient using equation (2) with three and four quarters in the pre- and post-period, respectively. In the first row, the dependent variable is a measure of whether an individual has ever been incarcerated in an adult correctional facility (i.e., arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data). In the second row, the outcome variable is a measure of whether an individual has ever been incarcerated in an adult correctional facility or detained in a juvenile facility. Standard errors are clustered at the individual level.

Table A5: Estimated Discontinuities in Criminal Behavior Around Age 19

(a) Arrest propensity, quadratic polynomial

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

(b) Arrest propensity, cubic polynomial

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

(c) Incarceration propensity, quadratic polynomial

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

NOTE: This table considers men's probability of being arrested and incarcerated for each month around their 19th birthdays using data from the South Carolina Law Enforcement Division. Each estimate reports the discontinuity estimate from equation (4) and its standard error (clustering at the individual level). Panels (a) and (c) use a quadratic polynomial, whereas panel (b) uses a cubic polynomial. The first column considers men enrolled in Medicaid between the ages of 10 and 18 (inclusive). The second column considers men with a mental health diagnosis or medication in this age range. The third column considers men with a mental health diagnosis or medication between ages 16–18, and the fourth column considers men with no mental health diagnosis or medication in this age range. To be conservative, estimates exclude the first month of age 17 due to lack of information on exact birthdate and the timing of the age of criminal majority.

Table A6: Estimated Discontinuities in Criminal Behavior Around Age 20

(a) Arrest propensity, quadratic polynomial

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

(b) Incarceration propensity, quadratic polynomial

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

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

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

(a) All mental health medications

	(1) All	(2) Violent	(3) Property	(4) Financially motivated	(5) Non-financial violent	(6) Drug possession	(7) Misc.
No MH medications	2.605***	0.783*	1.083***	1.898***	0.481	0.601***	1.982***
	[0.624]	[0.434]	[0.412]	[0.529]	[0.351]	[0.229]	[0.546]
MH medications	1.521**	0.962**	0.992**	1.506**	0.409	0.000	0.681
	[0.683]	[0.423]	[0.493]	[0.589]	[0.341]	[0.272]	[0.590]
Post-Period Avg.	17.76	6.66	8.23	12.45	4.26	1.97	11.50
Treated Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

(b) Non-ADHD medications

	(1) All	(2) Violent	(3) Property	(4) Financially motivated	(5) Non-financial violent	(6) Drug possession	(7) Misc.
None or ADHD meds.	2.723***	0.928**	1.244***	2.040***	0.522	0.569***	2.080***
	[0.598]	[0.411]	[0.395]	[0.503]	[0.335]	[0.216]	[0.519]
Non-ADHD meds.	1.687**	0.777	0.753	1.572**	0.415	0.133	0.566
	[0.785]	[0.475]	[0.575]	[0.679]	[0.386]	[0.315]	[0.663]
Post-Period Avg.	17.58	6.55	8.34	12.38	4.17	1.91	11.29
Treated Men	11,866	11,866	11,866	11,866	11,866	11,866	11,866

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers treated men with mental health histories and their matched comparison units. Both panels report estimates using equation (2). The estimate in the first row corresponds to the β_7 coefficient in that equation, whereas the estimate in the second row corresponds to the α_7 coefficient. In panel (a), "MH medications" and "No MH medications" refer to men who did and did not file a claim for a psychotropic drug between the ages of 16–18, respectively. "Post-Period Avg." reports the share of treated men in the latter group who had ever been incarcerated in the last quarter of the post period. In panel (b), "Non-ADHD meds." refers to men filing claims for non-ADHD medications (antidepressant, antianxiety, antipsychotic medications) and "None or ADHD meds." refers to men who did not file such claims between the ages of 16–18. "Post-Period Avg." reports the share of treated men in the latter group who had ever been incarcerated in the last quarter of the post period. "Misc." refers to offenses that were not classified in the previous three categories (columns 4–6). Standard errors are clustered at the individual level. For more details on offense and drug classifications, see Appendix B.

Table A8: Effect of Medicaid Loss on Likelihood of Ever Being Incarcerated for
Men with Mental Health Histories, Excluding Men with Specific Diagnoses

Panel (a):

	(1) Baseline	(2) Intellectual disability	(3) Other neurodev.	(4) Depression	(5) Anxiety	(6) Bipolar	(7) PTSD
Ever detained	3.129***	3.222***	3.746***	2.747***	3.047***	2.820***	3.084***
	[0.583]	[0.598]	[0.658]	[0.590]	[0.598]	[0.587]	[0.584]
Observations	2,506,252	2,078,202	1,583,190	1,977,332	2,143,106	2,341,402	2,422,770

Panel (b):

	(1) Conduct	(2) Opp. defiant	(3) Adjustment	(4) ADHD	(5) ADHD only	(6) Substance abuse	(7) Other
Ever detained	1.065*	1.475***	3.113***	1.868***	3.411***	0.807	3.259***
	[0.557]	[0.556]	[0.590]	[0.640]	[0.598]	[0.553]	[0.587]
Observations	1,588,188	1,735,314	2,094,092	1,163,582	2,324,812	1,697,192	2,211,720

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers treated men with mental health histories and their matched comparison units, and reports the results from Section 7.3 showing how the estimates change as treated men with certain diagnoses (and their comparison units) are excluded from the sample. The estimates correspond to the treat-control difference in the final quarter of the sample period (i.e., age 20, quarter 4). The estimate in column 1 of panel (a) reports the baseline treat-control difference using all men with mental health histories. The remaining columns exclude treated men with a specific diagnosis as well their corresponding comparison units. Column 5 in panel (b) excludes treated men whose only diagnosis is ADHD (and their comparison units). Standard errors are clustered at the individual level. For more details on the classification of diagnoses, see Appendix B.

B Loss in Medicaid Eligibility: Sample and Variable Construction

B.1 Selection of Sample

To select the individuals for this study, South Carolina's Revenue and Fiscal Affairs (RFA) Office used information from the Department of Education records. First, they calculated the share of students receiving free or reduced-price lunch at each high school in the state for every year between 2008 and 2014.[40] These shares were then averaged across the 2008–2014 school years and schools were ranked based on the average share. The high schools in the upper half of this distribution were chosen and if an individual ever attended one of these "low-income" high schools, he or she entered the sample. The dataset then provides information on these individuals starting at age 10.

None of the datasets provide exact information on an individual's date of birth. For individuals ever enrolled in Medicaid (roughly 70% 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 six state government agencies.

Throughout the analysis, I focus on individuals born between 1990 and 1993 because subsequent cohorts were impacted by the introduction of an automatic enrollment program for Medicaid. South Carolina implemented the Express Lane Eligibility (ELE) program in 2011, resulting in substantial increases in Medicaid enrollment during adolescence starting with the 1994 cohort. The increase in enrollment prompted by ELE reduces the number of individuals eligible for the comparison group, thereby worsening the match rate and allowing fewer individuals to have disproportionate impact on the estimated counterfactual outcome paths. Perhaps more importantly, this increase in enrollment also alters the composition of eligible treatment and comparison individuals. Prior to ELE, the comparison group included lower-income individuals who were likely eligible but not enrolled in Medicaid. However, starting with the 1994 cohort, many of these lower-income individuals were automatically enrolled into Medicaid, thereby becoming part of the treated group; the remaining comparison individuals therefore had relatively higher incomes and were potentially worse candidates for estimating counterfactual outcome paths.

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[40] Students who attend private school or home school are not included in this data.

B.2 Variable Construction

I use information on the diagnoses and medications in the Medicaid claims to identify individuals who have a mental health history. A diagnosis is classified as a mental health disorder if it belongs 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). 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. Finally, I identify foster care and SSI youth using the payment categories in the Medicaid recipient file.

I use both the South Carolina Law Enforcement Division (SLED) as well as the Department of Corrections (DOC) files in order to measure whether an individual was 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 this group into offenses 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. The only difference between violent offenses and non-financial violent offenses is the exclusion of robbery in the latter category.

I use information from the Department of Education administrative records to identify a person's main district and school. District refers to the modal school district attended between the ages of 15 and 18. If there is no modal district, I use the last school district attended before age 19. School refers to the last school attended before age 19. Finally, when considering academic achievement, I standardize math and English Language Arts (ELA) test scores at the grade, year, and test-type level to have mean zero and standard deviation one. I standardize at the test-type level because South Carolina transitioned across various standardized tests during the study's sample period.

C   Cost-Benefit Analysis: Providing Medicaid Eligibility

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

Throughout this analysis, I focus on the costs and benefits for one cohort and only consider the poorest half of South Carolina's residents who are enrolled in Medicaid at some point in adolescence (i.e., the sample of this paper), assuming that the higher-income half of the state is unaffected by changes in Medicaid eligibility. Moreover, I focus exclusively on men in this analysis because providing women with Medicaid eligibility could result in additional associated benefits that I would not be taking into account.[41] Throughout this exercise, I also assume that Medicaid only has crime-related benefits; to the extent that expanding Medicaid eligibility impacts individual's outcomes beyond their criminal involvement, then if anything, I would be understating the benefits of this policy.

C.1   Increased Costs of Expanding Eligibility

I begin by calculating the number of individuals in a cohort that would likely take-up the program if eligibility were expanded. I linearly interpolate the enrollment patterns at ages 19 and 20 using the share enrolled in each quarter prior to age 19 (i.e., the shares in Figure 3). I find that roughly 42% of the cohort would take up Medicaid at age 19 and 35% of the cohort would take up Medicaid at age 20. Multiplying these numbers by the annual per-enrollee cost for children in South Carolina in 2011 ($2,008 using Young et al. 2015), I find that that the total cost of providing this group of low-income young men with Medicaid totals $12.8 million.[42] The estimates from this and the next subsection are summarized in Table 7.

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 in these cohorts who

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

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

would choose to take up the program. If the same share of individuals who was enrolled in the program in the quarter prior to age 19 remained enrolled through ages 19 and 20 (44% of the cohort), then the cost rises to $14.6 million. Finally, I consider a higher per-enrollee cost using the nationwide annual per-enrollee cost for children ($2,492). Assuming this higher cost and the higher level of program take-up implies a total cost of $18.1 million.

Finally, is is worth mentioning that in this exercise, I consider the costs of expanding eligibility to all low-income individuals. Nevertheless, alternative policies could be considered, such as expanding eligibility only to individuals who have been diagnosed with a mental health disorder or implementing a limited benefit program that allows individuals to keep accessing behavioral health services (but are not eligible for full insurance coverage).[43] These more targeted policies would likely be less expensive than extending Medicaid eligibility to full cohorts of low-income individuals, although identifying and verifying this subset 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 6 to calculate the number of violent, property, and drug-related crimes that occurred as a result of the Medicaid loss.[44] 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), and assume that the ratio for drug offenses is the same as that for property crimes.

I then calculate the cost of the average violent, property, and drug-related incident (similar to the approach in Mello 2019) and summarize the estimates in Table C1. First, I split each category of crime into subcategories. Then, I use the prevalence of these sub-crimes in the SLED data in conjunction with the victimization-to-arrest ratios to calculate the share of victimizations that correspond to each category. Next, I use the average cost to victims (from Cohen & Piquero 2009 and Miller et al. 1996) for each subcrime in order to calculate the cost of each average violent, property, and drug offense.[45]

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

[44] I calculate these numbers by multiplying the average number of people in one cohort (2,967) × the estimated coefficients in Table 6 (divided by 100) × 8 quarters.

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

The upper-end figures use all of the costs from Cohen & Piquero (2009). To be conservative in terms of the statistical value of life, the lower-end estimates divide the cost of murder in half (similar to Heller et al. 2017). For drug-related offenses, the upper-end estimate assigns DUI offenses the average cost from drunk driving crashes from Cohen & Piquero (2009). The lower-bound estimate assigns DUI offenses the cost of drunk driving incidents without injuries from Miller et al. (1996). For the remaining drug-related crimes, I conservatively assign them a cost of $0 because these offenses are typically "victimless."[46] 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 $12.2 to $19.9 million.

C.2.2 Lower Fiscal Costs from Fewer Incarcerations

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

To be conservative, I assume that not all of the men arrested for these serious crimes are sentenced to state prison spells. Using the SLED and DOC data, I estimate that 65, 55, 41, and 44 percent of individuals who committed violent, property, drug, and miscellaneous offenses are sentenced to a prison spell, respectively.[47] For each offense, I then multiply the number of incarcerated individuals by the average sentenced served in 2009 in South Carolina and by the daily cost per inmate (roughly $45 in 2011).[48] I allow for heterogeneity in the length of sentences served by offense type: the average time served for violent, property, drug, and miscellaneous crimes is 4 years, 1.9 years, 2.2 years, and 2.3 years, respectively (Pew Center on the States 2012). Overall, I find that the fiscal cost would have been reduced by $3.3 million.

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[46] 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.

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

[48] 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`.

If I can use the marginal—as opposed to the average—cost of incarcerating an individual, then the associated institutional costs of incarcerating one individual would be lower. Owens (2009) finds that the marginal cost of incarcerating an individual for one year is around $12,675 in 2010 dollars. If I use this estimate, I find that the total fiscal cost totals $2.6 million. To get a middle-ground estimate, I take an average of the upper and lower bounds.

C.2.3 Lower Social Costs from Fewer Incarcerations

Next, I calculate the reduced social costs from fewer incarcerations. Estimates from Mueller-Smith (2015) suggest that a two-year prison term has economic impacts (in terms of employment and public assistance) of around $34,650 per person and crime impacts (in terms of post-release criminal behavior) between $14,983 and $33,297.[49] Multiplying these costs by the number of individuals in each cohort that would have been sentenced to serve a state prison spell after losing Medicaid eligibility implies a total cost between $4.0 and $5.5 million.

For the remaining individuals who were detained but did not serve time in a state prison, I use the estimates from Mueller-Smith (2015) for 6-month prison terms. I find that the social costs for these individuals range from $2.1 to $2.7 million, which puts the overall total social costs between $6.1 and $8.2 million. To get a middle-ground estimate, I take an average of the upper and lower bounds.

C.3 Marginal Value of Public Funds

In this subsection, I calculate this policy's marginal value of public funds (MVPF) (Hendren & Sprung-Keyser 2020). Specifically, the MVPF is a ratio of society's willingness to pay for this policy to the net cost of the policy to the government. Similar to the approach above, I construct upper and lower bounds for this ratio as well as a preferred middle-ground estimate based on various assumptions. Estimates are summarized in Table 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. The lower bound of the MVPF is close to the MVPFs for policies targeting adults, and the upper bound is closer to the ratios found for direct investments in low-income children's health and education. Given the age range and socioeconomic status of this population, it seems reasonable to think that this policy's MVPF would likely fall between these two categories.

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

C.3.1 Willingness to Pay

I begin by calculating the numerator of the ratio, which measures the aggregate social willingness to pay for the policy change and which includes three main components.

The primary component in this numerator is society's willingness to pay for fewer criminal victimizations v. I begin by using the estimates of the social cost of crimes averted, which are discussed above and shown in Table C1, but I make two additional adjustments to these calculations. First, one important consideration is the extent to which extending Medicaid eligibility prevents crimes altogether, or whether they are simply delayed by two years until eligibility expires. When constructing the lower- and middle-ground estimates of the MVPF, I assume the policy is able to prevent half of the incidents, whereas the other half still occur, but just two years later.[50] 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.[51] Overall, the willingness to pay for fewer victimizations ranges from \$8.3 to \$25.1 million.

Second, I consider the willingness to pay for improved labor market prospects, η, by the individuals who avoided incarceration. In other words, beneficiaries should be willing to pay for the increase in wages they experience from this policy change. To calculate this foregone income, I first use the 2009–2013 American Community Survey to calculate the employment rate and average annual income of employed men in South Carolina who were aged 19–25 and living under 200% of the federal poverty level: 50% and \$11,950, respectively (Ruggles et al. 2020).[52] I then calculate the total foregone income of affected individuals *during* incarceration, allowing for heterogeneity by crime type (given the different average sentence lengths). For the middle- and 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

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

[51] 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.

[52] 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).

foregone income in the five years after release.[53]

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

It is worth noting that these calculations ignore various other components including the insurance value of Medicaid beyond the transfer value, society's willingness to pay for improvements in health (beyond the effects on criminal activity), as well as individuals' willingness to pay to avoid being incarcerated (beyond improved labor market prospects).[55] 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

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

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

[55] 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).

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).[56]

Finally, improved labor market prospects for individuals translate to higher tax revenue for the government. Following the approach of Hendren & Sprung-Keyser (2020), I use a 20% tax rate and find that the government recoups between $132 and $322 thousand in tax revenue.[57]

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

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

Table C1: Social Costs per Crime

Offense	Percent	Ratio	Estimated Cost		
			Upper	Middle	Lower
Violent Crimes					
Murder	6.8	1.5	$4,837,696	—	$2,418,848
Sex Offenses	3.9	4.6	$141,976	—	$141,976
Robbery	31.8	5.9	$12,620	—	$12,620
Assault	57.5	4.1	$38,912	—	$38,912
Avg. Violent Crime			$142,286	$114,471	$86,656
Property Crimes					
Larceny	14.8	17.3	$473	—	$473
Burglary	72.5	15.9	$2,103	—	$2,103
MV Theft	12.7	6.7	$5,784	—	$5,784
Avg. Property Crime			$2,037	$2,037	$2,037
Drug Crimes					
DUI	2.9	—	$29,447	—	$4,074
All other	97.1	—	$0	—	$0
Avg. Drug Crime			$854	$486	$118
Total Social Cost			$19,905,051	$16,077,004	$12,248,958

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

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

	Estimated Cost		
	Upper	Middle	Lower
Willingness to Pay:			
Fewer crime victimizations, ν	25,060,637	10,813,605	8,252,337
Improved labor market prospects, η	1,609,827	873,217	661,727
Value of insurance transfer, γ	2,665,765	10,086,578	8,757,327
Aggregate willingness to pay	29,336,229	21,773,401	17,671,391
Costs to the Government:			
Cost of providing Medicaid, function of G	5,833,376	14,583,441	14,583,441
Fewer incarcerations, μ	–4,121,233	–2,170,398	–1,723,495
Foregone tax revenue, 0.2η	–321,965	–174,643	–132,345
Net Cost	1,390,178	12,238,400	12,727,600
Marginal Value of Public Funds	21.10	1.78	1.39

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 leverage 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, which I can then use to compare the cost of Medicaid provision to that of harsher punishments.

I begin by discussing the sample and variable construction for this analysis. I then discuss the main results from this exercise and associated robustness checks. Next, I calculate the increase in average sentence lengths that occurs at age 17 in South Carolina, and use these estimates to calculate the elasticity of crime with respect to sentence lengths. I conclude the section by discussing how the minimum dropout age is likely not confounding the estimates of deterrence around age 17.

D.1   Sample and Variable Construction

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

I focus on felonies throughout this analysis because the definition of felony is not a function of an individual's age. Moreover, because felonies tend to be relatively serious in nature, focusing on this group reduces the likelihood that individuals will be charged with that crime as juveniles, but not as adults (or vice versa).[58]

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

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

during an individual's 17th-birthday month when the individual was still 16). To address the potential misclassification in ages, I shift all non-technical referrals that occurred at age 17–month 1 to age 16–month 12.[59] Correspondingly, I then (randomly) shift half of non-technical referrals occurring during any month t to month $t-1$.

Next, I classify arrests in the South Carolina Law Enforcement Division (SLED) data as felonies using the South Carolina's Judicial Branch CDR codes.[60] 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.

D.2 Likelihood of Felony Arrest Around Age 17

Using equation (6), I estimate whether there is any discontinuity in the likelihood of committing a felony offense upon reaching the age of criminal majority. The estimate, shown in Figure 8, is small and statistically insignificant, suggesting that adolescents are not deterred from committing serious crimes despite facing much harsher criminal sanctions as adults. These estimates suggest that at most, there was a 11% 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 small and statistically insignificant.

Third, one concern with the baseline method for classifying felonies is that for the roughly 9% of arrests that do not have available charge information, I may be over- or under-counting the number of felonies by only using information from the disposition. For example, if between the charge and

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[59] Technical referrals refers to referrals related to probation or aftercare program violations. Individuals who violate the terms of their probation or aftercare program can be referred to DJJ even after their 17th birthdays.

[60] 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.

the disposition, many felony charges were not ruled to be felonies, then I would be undercounting the number of felony charges. To consider this potential misclassification, I use the group of arrests that have both a charge and a disposition available (53% of arrests for men ages 17 and younger) in order to gauge the magnitude of the misclassification.[61] The last column of Appendix Table D1 displays the estimate of θ after accounting for this potential misclassification, showing that it 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 6.3.4, which uses the probability of arrest rather than hazard rates. The statistically insignificant estimate from this figure confirms that individuals are equally likely to be arrested for a felony offense upon reaching the age of criminal majority.

D.3   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.[62] 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. Finally, instead of considering the average sentence length, I consider the average

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[61] In other words, I use the arrests with an available charge and disposition to calculate α, β_1, and β_2 in Appendix Figure D1. I then use these estimates to calculate the share of felonies that are incorrectly classified, γ_1, as well as the share of non-felonies that were mis-identified, γ_2. I calculate $\gamma_1 = \frac{(1-\alpha)(1-\beta_2)}{(1-\alpha)(1-\beta_2)+(\alpha)(\beta_1)}$ to represent the share of felony dispositions that were likely non-felony charges, and $\gamma_2 = \frac{(\alpha)(1-\beta_1)}{(\alpha)(1-\beta_1)+(1-\alpha)(\beta_2)}$ to represent the share of non-felony dispositions that were likely felony charges. I assume that these estimates of misclassification represent the degree of misclassification for the group of arrests with missing charge information. I then use these shares to randomly re-classify γ_1% of classified felonies in this group as non-felonies, and γ_2% of classified non-felonies as felonies.

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

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 estimates of deterrence, I calculate the elasticity of crime with respect to sentence lengths. The results are summarized in Table 8, showing an elasticity of crime to sentence lengths of -0.007 when using the average sentence length and -0.014 using the average sentence served.

D.4   Potential Confounding Effect of Minimum Dropout Age

Because the age at which adolescents can legally drop out of school coincides with the age of criminal majority, there might be concern that the deterrence estimate is combining the effect of the harsher punishments with the effect of adolescents dropping out of school upon reaching age 17. In other words, to the extent that schools have an "incapacitation" effect (i.e., they reduce the likelihood that teens commit offenses; see e.g., Anderson 2014, Berthelon & Kruger 2011, Fischer & Argyle 2018), then one concern might be that the small and statistically insignificant estimates of deterrence at age 17 are combining the negative effect of harsher criminal sanctions on crime with the positive effect of individuals dropping out of school on crime.[63] In this subsection, I conduct three exercises that together suggest that the dropout age is not confounding the estimates of deterrence.

I begin by testing for the presence of schools' incapacitation effect in this setting. Specifically, I estimate men's likelihood of being arrested for a felony during each month of the calendar year. Appendix Figure D3 displays the graphical results and Appendix Table D2 summarizes the estimates, showing that adolescents in South Carolina are *not* arrested more often during summer months for felony offenses. If anything, these teens are *more* likely to be arrested for felonies during the school year. These estimates thus indicate that at least for individuals in this sample, schools do not play a significant incapacitation role.

Second, I consider the seasonality of dropout: Appendix Figure D4 shows that teens are significantly more likely to drop out of school in the beginning of the school year. Individuals born in the first half of the calendar year are therefore less likely to drop out immediately after turning 17 than individuals born in the second half of the calendar year (Appendix Figure D5). If dropout is contaminating the

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

estimates of deterrence, then we would expect the estimate of θ for teens born in the first half of the calendar year to be less confounded (i.e., more negative) than the estimate for individuals born later in the year. Appendix Figure D6 shows that this pattern is not present in the data, providing another piece of evidence against the idea that the minimum dropout age is contaminating the deterrence estimates.

Finally, I utilize the fact that the share of students dropping out of school is declining with time in this sample in order to gauge the extent to which the deterrence estimate is confounded by dropout. In other words, if dropout is contaminating the estimate and the share of students dropping out is declining with time, then we should see θ become less confounded (i.e., more negative) with time as well. Appendix Figure D7 confirms that the share of students dropping out decreases with time: the share of teens enrolled in school at age 17 is 11 percentage points higher for the 1997–1999 birth cohorts than for the 1990–1992 birth cohorts. Appendix Table D3 shows that despite this large decline in the likelihood of dropout, the deterrence estimates do not fall monotonically with time, remaining both close to zero and statistically insignificant.

Overall, the results from these three exercises suggest that the deterrence estimates are not confounded by students dropping out of school and committing more crime upon reaching age 17. However, I still use individuals born in the 1997–1999 birth cohorts (i.e., the group that is least likely to drop out of school) to calculate the most conservative elasticities available in this sample. Using the most negative effect consistent with the confidence intervals, I estimate an elasticity of crime to sentence lengths of -0.009 (or -0.017 using the average sentence served).[64]

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

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

	Using charges	Using decisions	Using all information	Using charges, adjusted
Discontinuity at 17	0.040	0.039	0.081	0.115
	[0.079]	[0.080]	[0.079]	[0.076]
Observations	1,693,014	1,707,808	1,692,422	1,691,668

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. The final column adjusts the felony definition for potential misclassification. Standard errors are clustered at the individual level.

Figure D1: Likelihood of Felony Arrest Charge and Disposition
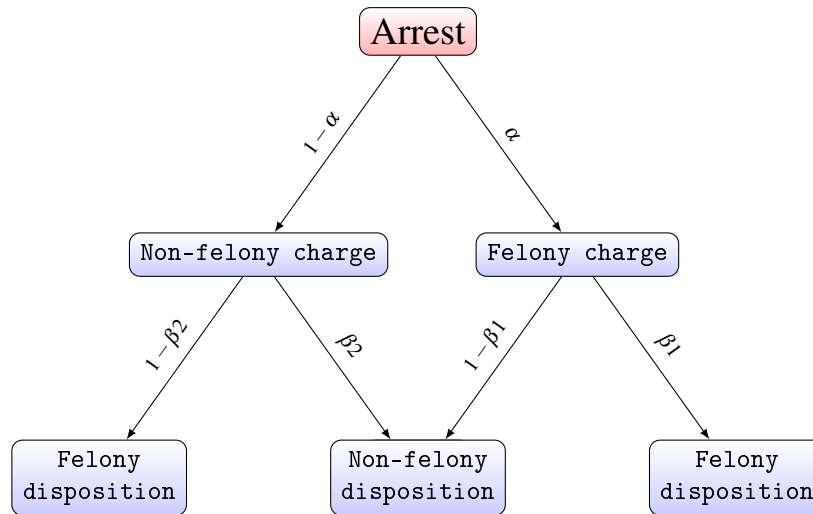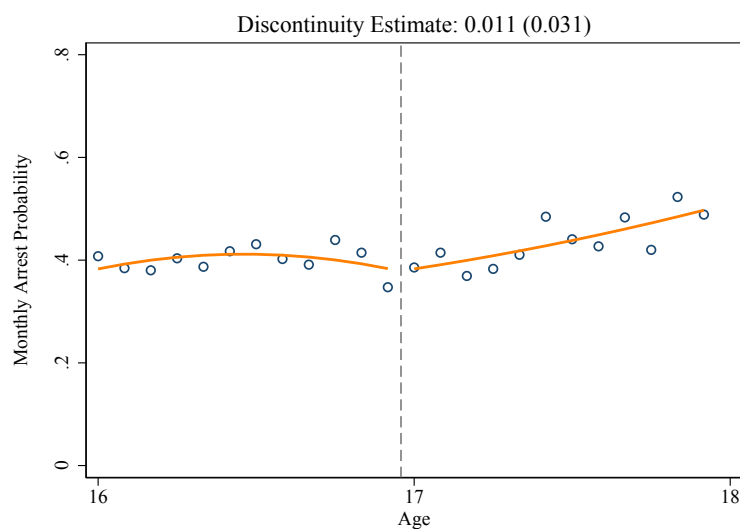
Figure D2: Share of Individuals Arrested for a Felony Around the Age of Criminal Majority

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

Figure D3: Likelihood of Felony Arrest or Death for Males Aged 12–16, by Month

(a) All felonies

(b) Non-school-related felonies

(c) Adult felonies

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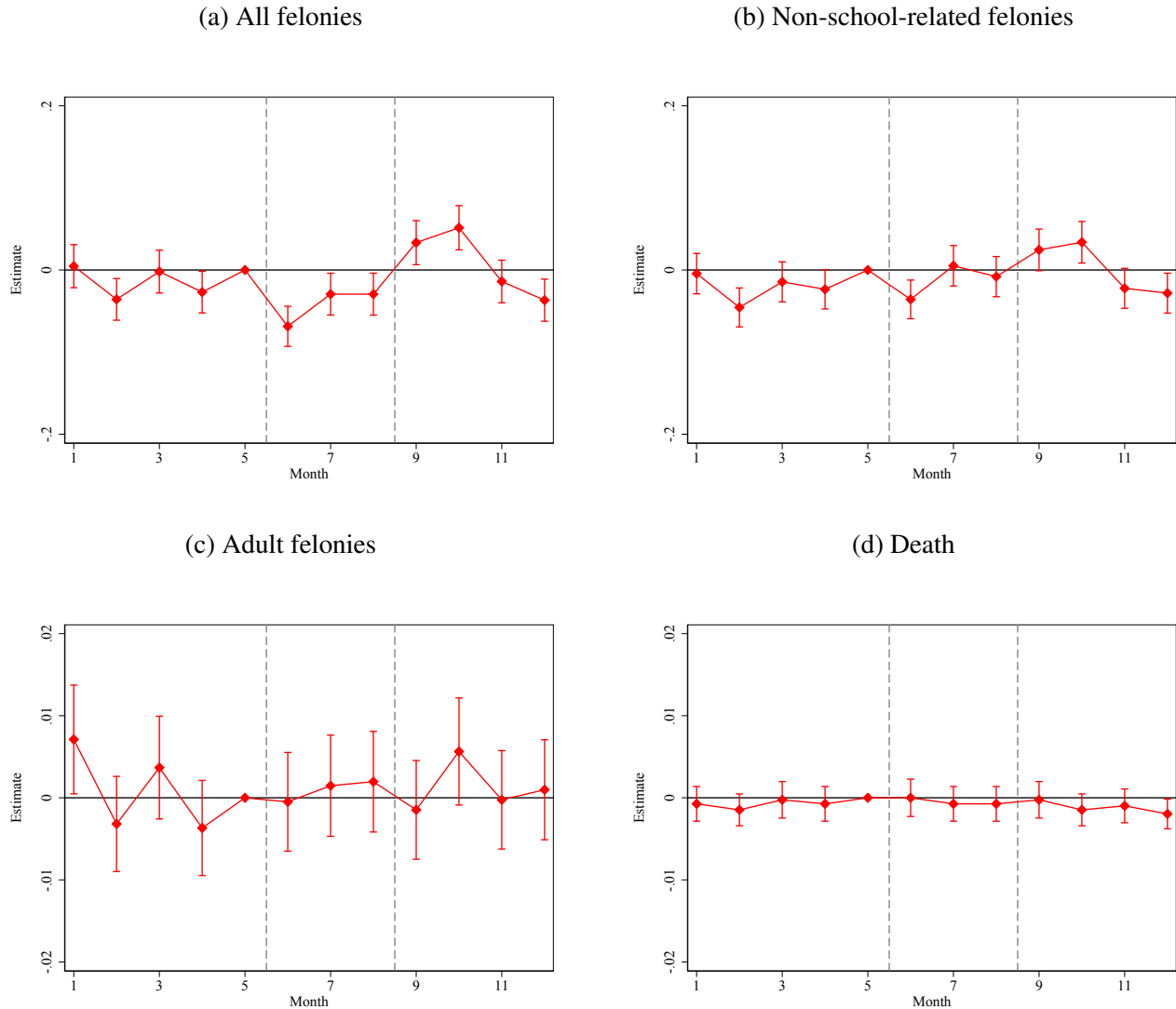
NOTE: This figure plots the likelihood that low-income male adolescents were arrested for committing a felony or passed away, by calendar month. The sample used is all male adolescents and the data is a balanced panel at the individual × age × month level for ages 12–16. "Non-school-related felonies"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

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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	-0.040*** [0.006]	-0.004 [0.006]	-0.000 [0.001]	0.000 [0.000]
Observations	4,895,400	4,895,400	4,895,400	4,895,400

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 all male adolescents and the data is a balanced panel at the individual × age × month level for ages 12–16. "Non-school-related felonies" refers to felonies that do not have the word "school" in the charge description (e.g., carrying weapons on school property). "Adult felonies" refers to felonies that appear in the adult criminal justice records despite these men being juveniles. "Death" is measured using death certificate records and is used as another proxy for adolescent risky behavior. Estimates come from regressing an indicator variable for a 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.

Figure D4: Seasonality of Dropout
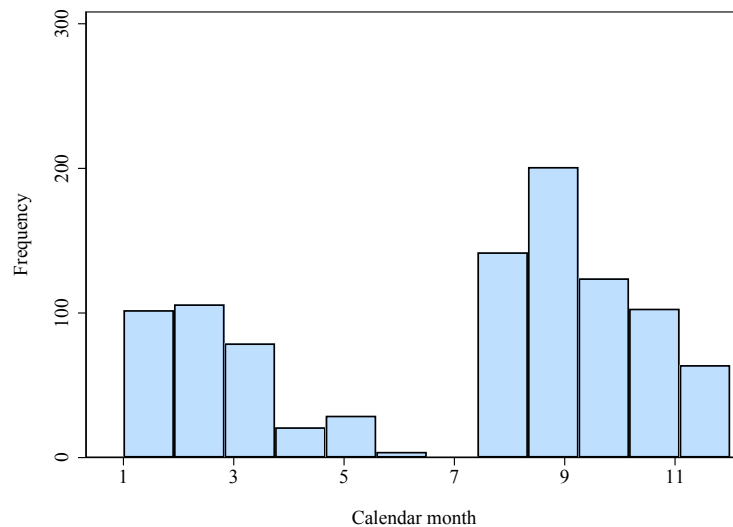
NOTE: This figure shows the dropout month recorded in the Department of Education administrative records for all individuals with an available dropout date.

Figure D5: Difference between 17th-Birthday Month and Dropout Month, by Birth Month

(a) January–June

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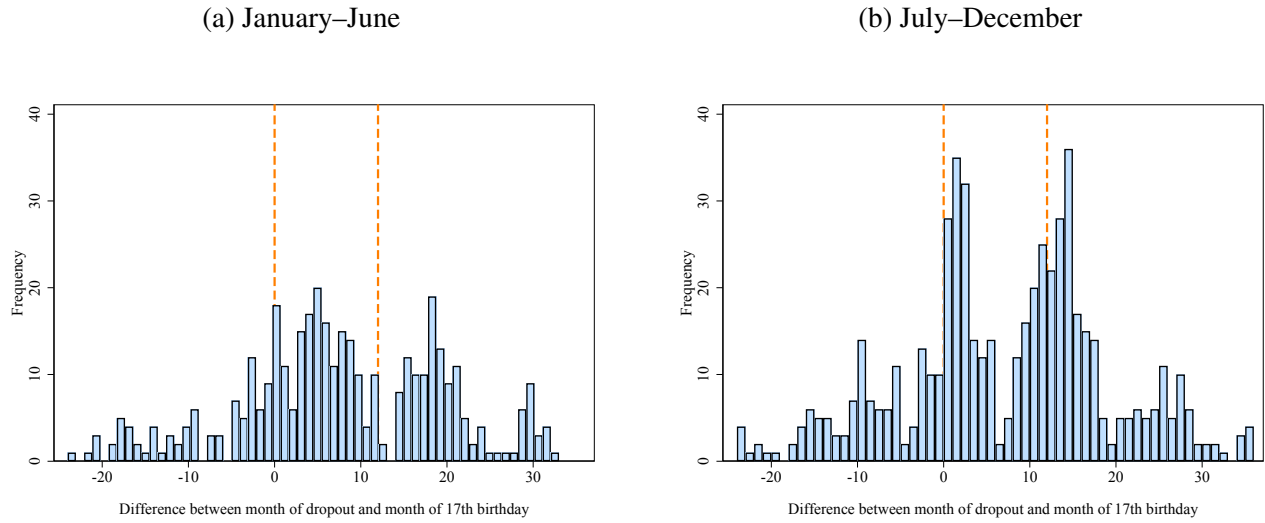
NOTE: This figure shows the distribution of time between an individual's 17th-birthday month and the month of dropout, as recorded in the Department of Education administrative records for all individuals with an available dropout date. The dashed orange lines indicate the time between the individual's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

Figure D6: Felony Propensity Estimates Around the Age of
Criminal Majority, by Birth Month
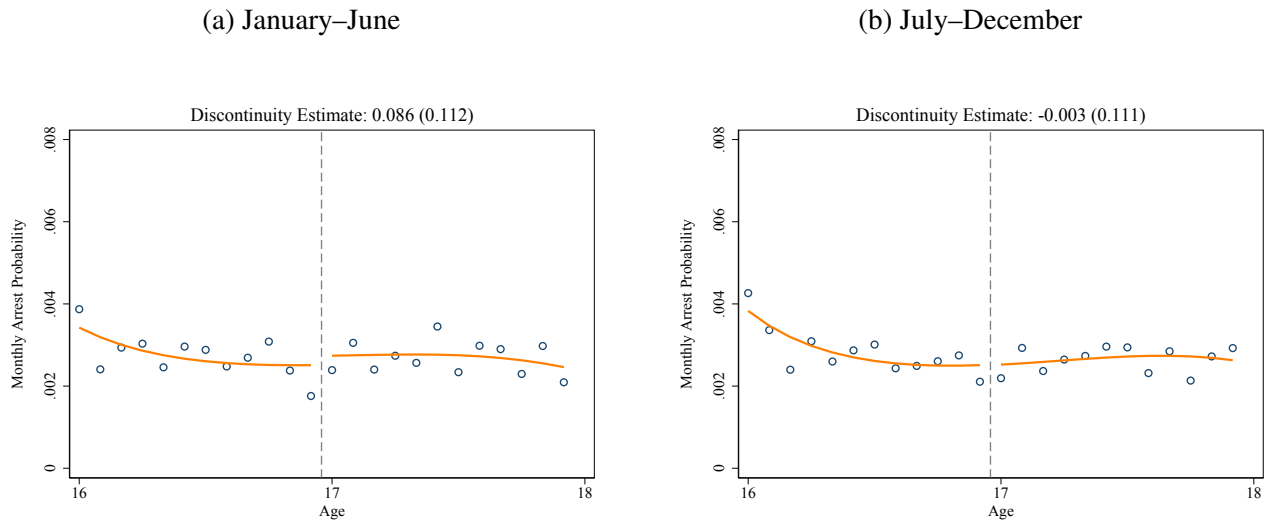
(a) January–June

(b) July–December

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

Figure D7: Likelihood of Dropout, by Cohort
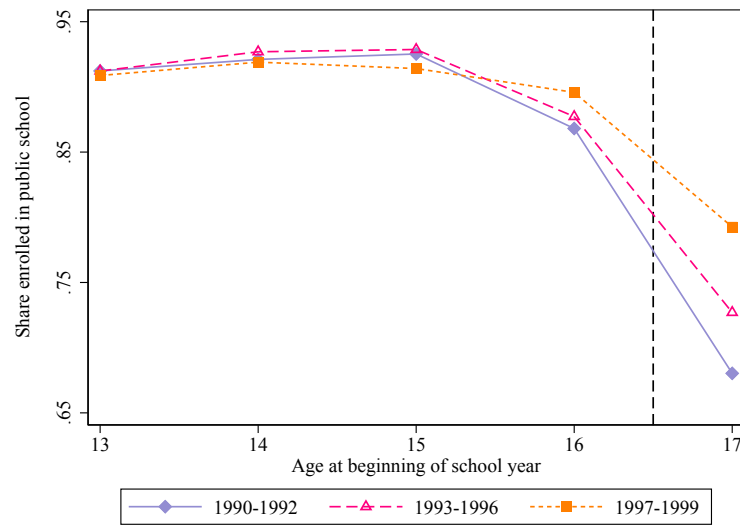
NOTE: This figure shows the share of individuals born in each cohort that appeared in the Department of Education administrative school records noting their enrollment during that school year. Age refers to the age of the individual in September of the school year.

Table D3: Estimates of Felony Propensity Around Age 17, By Cohort

	1990–1992	1993–1996	1997–1999
Discontinuity at 17	0.153	-0.141	0.144
	[0.139]	[0.128]	[0.146]
Observations	499,836	652,880	540,298

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. Each column considers different groups of birth cohorts. Standard errors are clustered at the individual level.

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

In this appendix, I compare the costs of three potential approaches for reducing crime: providing low-income young adults with access to Medicaid, increasing sentence lengths, and hiring more police officers. In particular, I consider the cost of each policy for reducing crime by 15%.[65]

E.1 Cost of Providing Medicaid Eligibility

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

The next component is the fiscal cost of incarcerating men who were *not* deterred (i.e., men who still commit crimes). In other words, even if some offenders were deterred from committing offenses, there would still be roughly 700 serious arrests per cohort. Similarly to the calculations in Appendix C, I only consider the cost of incarcerating men in state prison. I begin by calculating the number of arrests that would have still occurred for each offense type. I then use the raw data to calculate the share of serious arrests that have a corresponding state prison spell. I also use the average sentence served for each offense type as well as the daily cost per inmate in South Carolina (Pew Center on the States 2012). The total fiscal cost of incarcerating these individuals totals $15.5 million.

Importantly, because these individuals are serving relatively shorter (i.e., the status quo) sentence lengths, there is a chance that they will re-offend after serving their prison spell.[66] 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).[67] The fiscal cost of this second round of incarcerations totals $3.7 million.

When individuals re-offend, their crimes also impose social costs on victims. To calculate these costs, I use the share of serious *second* arrests that have an associated prison spell to back out the number of serious arrests that would occur. I then use the share of serious arrests by offense type

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[65] The estimate in column 1 of Table 6 suggests that among men born between 1990–1993, there were roughly 489 excess serious arrests ($11,866 \times 0.00515 \times 8$ quarters). I then use the raw data to calculate that there were 3,282 serious arrests among 19- and 20-year old men born in these cohorts, which implies that serious crime would have been 15% lower in this age group if Medicaid eligibility had not suddenly expired at age 19.

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

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

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

Summing these components together, I find that the total cost of this approach is roughly $56.9 million. Note that this approach is relatively conservative in assuming that the individuals who serve shorter prison spells do not generate additional benefits (e.g., tax revenue) after being released, and only takes into account the potential costs from re-offending.

E.1.1 Cost of Longer Prison Spells

To calculate the cost of this crime-reduction approach, I use the preferred estimate of the elasticity of crime with respect to sentence lengths from Table 8 ($\varepsilon_{c,f} = -0.014$ using the full sample and 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 1000% 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 $115.3 million, which is twice the cost of Medicaid provision.

I then do a back-of-the-envelope calculation to calculate how large $\varepsilon_{c,f}$ would need to be in order for this approach to have the same cost as the provision of Medicaid. I find that $\varepsilon_{c,f}$ would need to be around -0.044, which is three times as large as the preferred elasticity.

E.2 Providing Medicaid Eligibility versus Hiring Police Officers

Another favored crime-reduction approach for the past fifty years has been to hire more police officers. Indeed, a number of studies have estimated the effect of police presence on criminal activity (see e.g., Chalfin & McCrary 2018, Evans & Owens 2007, Mello 2019, Weisburst 2019). In this subsection, I consider the cost of reducing crime by hiring more police officers. When comparing the cost of hiring police officers to the cost of providing Medicaid, I use the short-term cost of Medicaid

provision (i.e., excluding the social and fiscal costs of recidivism): $30.1 million.[68]

Similar to the analysis investigating the cost of longer sentence lengths, I use the elasticity of crime to police (i.e., $\varepsilon_{c,p}$) to calculate how many police officers would need to be hired in order to achieve the same percent reduction in crime as Medicaid provision. Using the elasticity of crime to police officers from Evans & Owens 2007 ($\varepsilon_{c,p} = -0.34$), I find that the state would need to increase the overall size of their police force by around 45%, which implies hiring roughly 5,161 more police officers.[69,70] Assuming a marginal cost of $130,000 for hiring a police officer (Chalfin & McCrary 2018), the fiscal cost of this policy amounts to $1.3 billion. I sum this cost to the cost of incarcerating individuals who were not deterred (discussed in Section E.1).

However, hiring police officers not only reduces the criminal activity of 19- and 20-year-old men, but it has spillover effects on the criminal activity of individuals of other ages. To calculate the number of additional crimes averted in other age groups, I begin by looking at the age distribution of admitted inmates in South Carolina (focusing on individuals ages 17–40, who make up the majority of individuals committing crime). I then use the share of serious arrests that end in state prison (calculated from the raw data) to back out the number of total arrests in each age group. I use the share of arrests that are violent and property offenses in this sample to calculate the implied number of violent and property arrests. Next, I use the violent and property crime elasticities from Evans & Owens (2007) to estimate how many fewer arrests there would be given the increased number of police officers.[71] Finally, I use the victimization-to-arrest ratios from Heckman et al. (2010) to calculate how many fewer violent and property incidents there would be if more police officers were hired.

Using the upper-bound social costs of violent crime from Table C1, I find that hiring police officers would reduce the social costs of violent and property victimizations by $1.4 billion and $23.5 million, respectively. Finally, there is an additional reduction in fiscal costs of $30.0 million from fewer individuals being incarcerated (after multiplying the number of individuals in these other age groups

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

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

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

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

who would have been incarcerated by the daily inmate cost and average sentence served).

The results from this exercise suggest that the benefits of hiring more police officers outweigh the costs. However, I note that these results are sensitive to the cost assigned to violent crime—especially because the evidence from prior studies shows that violent crime is particularly responsive to police presence—as well as to the assumption that police reduces crime for individuals ages 17–40. If I use the lower-bound for the cost of violent crime in Table C1 and assume that the spillovers only affect men ages 18–30, then I find that this policy has an overall net cost of $866.5 million, which would favor Medicaid provision over hiring more police officers. Table E1 summarizes how the costs change as I alter certain assumptions, including lowering the marginal cost of hiring police officers.

Overall, the calculations from this subsection suggest that in terms of crime reduction, Medicaid provision is likely less cost effective than hiring more police officers. There are two caveats to this conclusion. First, this analysis does not take into account the social costs of policing (e.g., the costs of police brutality, excessive force, or over-policing; see e.g., Ang 2021). Second, the calculations ignore the non-crime-related benefits that health insurance might provide to individuals (e.g., in terms of financial stability or earnings and tax revenue; Gallagher et al. 2019, Gross & Notowidigdo 2011, Hu et al. 2018). Consequently, even though I am making a relatively parallel comparison for these policies—estimating the cost of a 15% crime reduction—it is likely the case that these calculations are underestimating the benefits of providing low-income young adults with access to Medicaid.

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

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

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