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2021

Essays on the Economics of Immigration and Crime

Jacome, Elisa

Princeton, NJ : Princeton University

http://arks.princeton.edu/ark:/88435/dsp01q524jr84m

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Essays on the Economics of Immigration and Crime

Elisa Jácome

A Dissertation
Presented to the Faculty
of Princeton University
in Candidacy for the Degree
of Doctor of Philosophy

Recommended for Acceptance
by the Department of
Economics
Advisers: Leah Boustan and Ilyana Kuziemko

May 2021

Abstract

This dissertation consists of three chapters in public and labor economics. A theme throughout these chapters is using empirical analysis to study the outcomes of low-income and immigrant individuals in the United States as well as the effects of public policies on these communities.

Chapter 1 explores whether access to mental healthcare can reduce criminal activity. Specifically, I study the effect of losing insurance coverage on low-income men's likelihood of incarceration using administrative data from South Carolina. Leveraging a discrete break in Medicaid coverage at age 19, I find that men who lose access to Medicaid eligibility are 15% more likely to be incarcerated in the subsequent two years relative to a matched comparison group. The effects are 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. 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.

Chapter 2 documents that immigration policies affect an individual's willingness to report crime. I analyze the 2015 Priority Enforcement Program (PEP), which focused immigration enforcement on individuals convicted of serious crimes and shifted resources away from immigration-related offenses. I use data from the Dallas Police Department that include a complainant's ethnicity to show that Hispanic-reported incidents increased by 8% after the introduction of PEP. These results suggest that reducing enforcement of individuals who do not pose a threat to public safety can potentially improve trust between immigrant communities and the police.

Finally, in Chapter 3, Ran Abramitzky, Leah Boustan, Santiago Pérez, and I find that children of immigrants from nearly every sending country have higher rates of upward mobility than children of the US-born. Using millions of father-son pairs spanning more than 100

years of US history, we show that immigrants' advantage is similar historically and today despite dramatic shifts in sending countries and US immigration policy. Immigrants achieve this advantage in part by choosing to settle in locations that offer better prospects for their children.

Acknowledgements

I am eternally grateful to my advisers for their unrelenting support, guidance, mentorship, and friendship. I thank Leah Boustan for insightful and practical advising centered around finding historical and contemporary data to answer meaningful economic questions. Leah's discipline and clear mind have taught me to push myself to understand all aspects of a question and to convey findings transparently. I thank Ilyana Kuziemko for enthusiastic and incisive advising, with an emphasis on combining careful empirics with policy-relevant questions. Ilyana's rigor and intellectual curiosity have inspired me to use my economics training to improve our collective understanding of poverty and inequality. Both Leah and Ilyana have been constant sources of strength and positivity throughout the PhD, and I am humbled by their generosity and their dedication to my success. I feel incredibly lucky to have had two brilliant female economists as my role models and advisers.

I also benefitted from the advice of many other members of the Princeton faculty, particularly Janet Currie, Hank Farber, Bo Honoré, David Lee, Christopher Neilson, and Owen Zidar. I am especially grateful to Alex Mas for his mentorship and advice, and for welcoming me into the Industrial Relations Section. I thank Laura Hedden and Gianluca Violante, who patiently answered my questions and guided me through the first-ever virtual job market.

The Industrial Relations Section and the Princeton Program in Public Finance have been supportive and stimulating academic homes for the past four years. I am grateful for the support of Valerie Ching, Ellen Graf, Jeannie Moore, and Patti Tracey. I am also grateful for the financial support provided by the Graduate School, the Industrial Relations Section, the Princeton Program in Public Finance, and the Fellowship of SPIA Scholars. I also thank Melissa Kearney, Ben Harris, Brad Hershbein, and Arik Levinson, who encouraged me to apply to graduate school and pursue a career in economics.

I am grateful for the companionship and support of my classmates, both past and present, especially Rachel Anderson, David Arnold, Reyhan Ayas, Emily Battaglia, Jessica Brown,

Jiwon Choi, Matt Cocci, Felipe Goncalves, Stephanie Kestelman, Elena Marchetti-Bowick, Steve Mello, Mathilde Le Moigne, Vivek Sampathkumar, and Alex Wise. I especially thank Mathilde for her unbeatable friendship, Matt for his excited willingness to talk econometrics, Steph for constantly inspiring me to push onward, and Alex for brightening up countless dining hall meals with his quick wit. I am also very grateful to Steve Mello, my unofficial third adviser, for his patience and friendship during the transition into research.

A special thank you to Elizabeth, Ida, Michelle, and Morgan for their fierce loyalty and unparalleled ability to make me laugh; to Amy, Anne, Emily, Jessie, Julie, Lily, Lucie, and Madeline for their thoughtfulness and for consistently raising my spirits; to Claire and Delaney for inspiring me to connect economics to the real world; and to Dusty, Greg, Maysa, and Michael for helping me persevere through the PhD in the midst of a global pandemic. These friendships have helped me complete the emotional journey that is the PhD.

Finally, I am grateful to my family for their unconditional love and encouragement. I thank my mother, Maria Soledad, for her unwavering belief in me and for inspiring me to study important questions. I thank my father, Luis, not only for passing on his love of economics and public policy to me, but for being my number one cheerleader throughout the PhD. I thank my brother Juan Ignacio for his affection and sense humor, which always bring me enormous joy; my brother Felipe for helping me keep economics and research in perspective; and Tía Cristina for the plentiful hugs from afar. I am highly indebted to my future parents-in-law, Sandra and Andrew, for welcoming me into their home—not only every summer of the PhD, but for the past fourteen months—and for their tremendous generosity and support during the virtual job market. An obvious special thank you to my officemate and emotional support: Asheroo. But most of all, I am forever in debt and in awe of my Ben: his inquisitive nature and careful listening made these chapters better; his companionship as well as his patience, cooking, and sense of humor have sustained me; and his love and kindness have been an unending source of comfort and joy.

Para Tito y Chichi.

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Chapter 1

Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility[1]

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

[1]I am grateful to Leah Boustan and Ilyana Kuziemko for their constant guidance and support. I also thank Rachel Anderson, David Arnold, Reyhan Ayas, Emily Battaglia, Jesse Bruhn, Anna Chorniy, Matt Cocci, Janet Currie, Manasi Deshpande, Ben Eskin, Hank Farber, Felipe Goncalves, Sara Heller, Bo Honore, Steph Kestelman, David Lee, Mathilde Le Moigne, Elena Marchetti-Bowick, Alex Mas, Steve Mello, Mike Mueller-Smith, Suresh Naidu, Christopher Neilson, Vivek Sampathkumar, Emily Weisburst, Owen Zidar and seminar participants at Princeton University for helpful comments. A big thank you to Sarah Crawford, Muhammad Salaam, and Kaowao Strickland for helping me fulfill numerous data requests and for their invaluable patience and help with accessing and working with the data. I am grateful to Dr. Ida Ahmadizadeh, Dr. Rebecca Berger, Tricia Brooks, Dr. Daniel Eden, Delaney Ozmun, Claire Sherburne, and Dr. Julia Shuster for several helpful conversations. Finally, thank you to the Industrial Relations Section as well as the Fellowship of SPIA Scholars at Princeton University for generous financial support. All errors are my own.

1.1 Introduction

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

In the decades that followed Becker's pioneering work, crime rates rose in many cities across the country and policymakers embraced the punitive implications of Becker's model (Donohue 2007). The severity of punishments increased at both the state and federal level, contributing to rapid growth in the incarcerated population (Raphael and Stoll 2013b). Today, incarceration is a common occurrence for low-income and minority men.[2] Recent research has documented the adverse effects of incarceration—including increased barriers to employment and greater reliance on public assistance—in addition to the onerous fiscal costs (see e.g., Dobbie et al. 2018, Kearney et al. 2014, Mueller-Smith 2015). These high economic and social costs have called into question the cost effectiveness of the modern criminal justice system and have forced policymakers to consider alternative policies for deterring criminal behavior.

Contemporary debates about the the determinants of criminal activity have overlooked the disproportionate representation of mentally ill individuals in the criminal justice population. On any given day, over one million people with mental illness are in jail, prison, probation, or parole (Frank and McGuire 2010). Figure 1.1 plots the cumulative likelihood

[2]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 and Western 2004, Western and Pettit 2010).

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 3 times more likely to have been incarcerated by age 24 than men *without* a mental health history. The prevalence of mental illness among incarcerated individuals raises the natural question: is increasing access to health services a cost-effective way to reduce criminal activity?

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

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

Specifically, I employ a matched difference-in-differences approach, in which I study the evolution of outcomes for men who were impacted by the termination in eligibility. To control for age trends in crime, these "treated" men are compared to otherwise similar low-income men who were likely eligible, but not enrolled in Medicaid prior to their 19th birthdays, and who were therefore less affected by the loss in eligibility. The assumption underlying

this approach is that, in the absence of the Medicaid loss, treated men would have trended similarly to the comparison group in their propensity to commit crime. I provide support for this assumption by showing that the two groups were trending similarly prior to age 19, and only began to diverge when treated men lost access to Medicaid.

I find that treated men who lose access to Medicaid coverage are 15 percent more likely to be incarcerated in the following two years. These baseline results suggest a strong, positive relationship between Medicaid disenrollment and criminal activity among low-income young men. Importantly, the rich nature of the data allows me to split the sample by men's mental health histories, and I find that the effects are entirely driven by men with mental illness. By their 21st birthdays, treated men with mental health histories are 22 percent more likely to have ever been incarcerated relative to men in the matched comparison group. I find increases in violent, drug, and property crimes, suggesting that losing access to health coverage impacts all types of criminal involvement. Finally, I find that the effects are particularly pronounced for men who were using behavioral health services right before their 19th birthdays and for men who relied on Medicaid for access to mental health medications. These findings reaffirm the notion that losing access to mental health services plays an important role in explaining the observed rise in criminal activity.

In the last part of the paper, I use the estimates quantifying the effect of Medicaid eligibility on criminal activity to conduct a series of cost-benefit analyses. First, I show that the benefits of providing low-income young men with Medicaid eligibility—in terms of reduced fiscal and social costs—outweigh the program costs. Next, I compare the cost effectiveness of providing Medicaid eligibility to that of longer punishments, which has been a favored crime-reduction policy for the past fifty years. To make this comparison, I first replicate the approach of prior studies and show that low-income adolescents in my sample are relatively undeterred from engaging in criminal behavior when faced with harsher criminal sanctions (i.e., upon reaching the age of criminal majority) (Hjalmarsson 2009, Lee and

McCrary 2017). Using these estimates of deterrence, I show that if the goal is to deter young adults from engaging in crime, then providing Medicaid eligibility is significantly more cost effective than increasing sentence lengths. These results suggest that policymakers should consider improving access to healthcare as an approach for reducing crime and lowering criminal justice expenditures.

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

This paper contributes to a recent and growing literature in economics studying the effect of mental health and mental health services on various outcomes, including criminal activity (Anderson et al. 2015, Bondurant et al. 2018, Busch et al. 2014, Chatterji and Meara 2010, Deza et al. 2020, Fletcher and Wolfe 2009, Heller et al. 2017, Marcotte and Markowitz 2011, Teplin et al. 2002).[3] By leveraging a sudden loss of Medicaid coverage, this paper adds to

[3]Numerous other studies consider the relationship between mental health and human capital, including Cowan and Hao (2020), Cuellar and Dave (2016), Currie and Stabile (2006), Currie and Stabile (2007), and Fletcher and Wolfe (2008). Finally, other related papers include Biasi et al. (2019), Bütikofer et al. (2020), Chatterji et al. (2011), Chorniy and Kitashima (2016), Cuddy and Currie (2020), Cuellar and Markowitz

our understanding of the causal relationship between mental health service provision and contact with the criminal justice system among low-income young adults. Moreover, this paper argues that criminal involvement is a function of health, similar to a number of papers that study the effect of developmental health—via changes in lead exposure—on criminal behavior (Aizer and Currie 2019, Billings and Schnepel 2018, Feigenbaum and Muller 2016, Reyes 2007).

Finally, by studying changes in access to Medicaid, this paper contributes to a recent literature studying the effects of health insurance expansions on public safety (Aslim et al. 2019, He and Barkowski 2020, Vogler 2017, Wen et al. 2017).[4] Unlike most of these studies, which rely on aggregate crime statistics in order to measure changes in public safety, this paper uses individual-level records, allowing me to identify and study offender traits (e.g., mental health history, first-time offenders). More broadly, this paper contributes to a growing literature quantifying the social returns to Medicaid (see e.g., Arenberg et al. 2020, Boudreaux et al. 2016, Brown et al. 2020, Goodman-Bacon 2016).[5] 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. The findings of this paper may also help improve our understanding of the mechanisms underlying the documented long-term effects of Medicaid access. Second, whereas previous studies typically focus on historical Medicaid expansions for children, this study focuses on the provision of Medicaid eligibility to modern cohorts of adolescents and young adults. Because young

(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.

[4]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 and McCrary (2017) and Doleac (2020).

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

adults are relatively less likely to be insured—and are thus the group who stands most to gain from modern health insurance expansions—understanding the returns to this investment is of policy relevance. In that regard, this paper complements the findings from the Oregon Health Insurance Experiment, by focusing on a younger and more racially diverse population as well as considering additional outcomes (Baicker et al. 2014, Finkelstein et al. 2012).[6]

The remainder of the paper is organized as follows. Section 1.2 provides a brief discussion about the relationship between mental illness and criminal activity. In Section 1.3, I describe the data and the sample. Sections 1.4 and 1.5 outline the research design and discuss the estimation strategy. Section 1.6 presents the main results and corresponding robustness checks, and Section 1.7 explores heterogeneous effects. Sections 1.8 and 1.9 conduct a series of cost-benefit analyses. Section 1.10 concludes.

1.2 Mental Health & Criminal Activity

1.2.1 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 knowl-

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

edge" (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.[7]

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

For the remainder of the 20th century, policymakers reduced Becker's framework to its punitive implications and used it as an intellectual justification for adopting harsher criminal sanctions (Donohue 2007). Policymakers at all levels of government increased the length of punishments as well as the likelihood of sending convicted offenders to prison, two policies which contributed to a nearly fivefold increase in the incarceration rate (Pfaff 2017, Neal and Rick 2016, Raphael and Stoll 2013b).[8] 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.[9] Recent

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

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

[9]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

research suggests that around five percent of incarcerated individuals with mental illness in the 1980s–2000s would have been institutionalized in state mental hospitals, rather than in prisons, in prior decades (Raphael and Stoll 2013a).

1.2.2 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 and McGuire 2010 for a detailed review).[10] Individuals with mental illness are significantly over-represented in prisons and jails: 37 percent of prison inmates and 44 percent of jail inmates have been diagnosed with a mental disorder prior to incarceration (Bronson and Berzofsky 2017). Accordingly, the criminal justice system spends a significant share of its resources housing and treating individuals with mental illness, especially given this population's higher recidivism rates, longer sentences, and more expensive medical needs (Osher et al. 2012).[11]

The persistent relationship between mental illness and criminal involvement raises the question of whether improved access to behavioral health services can reduce the likelihood that mentally ill individuals commit crime. It is worth noting that when policymakers and academics were discussing the relationship between mental illness and crime in the 1960s, healthcare may not have been an effective way to reduce criminal behavior. However, in the decades that have transpired, significant scientific progress has been made in understanding and treating mental illness, including important developments and improvements

Court decision limiting the reasons for which an individual could be involuntarily committed (Raphael and Stoll 2013a).

[10] 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.

[11] 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 https://www.theatlantic.com/politics/archive/2015/06/americas-largest-mental-hospital-is-a-jail/395012/"America's Largest Mental Hospital Is a Jail" or NPR's segment https://www.npr.org/2011/09/04/140167676/nations-jails-struggle-with-mentally-ill-prisoners"Nation's Jails Struggle With Mentally Ill Prisoners."

in psychotropic drugs (e.g., antidepressants, mood stabilizers) as well as alternative modes of psychotherapy (e.g., cognitive behavioral therapy) (see e.g., Frank and Glied 2006, Hofmann et al. 2012, Kendler 2019, Lieberman and First 2018, Marder and Cannon 2019, Park and Zarate Jr. 2019). Acknowledging this progress, this paper revisits the potential role that mental healthcare can play in reducing criminal activity.

1.2.3 Role of Healthcare in Affecting Criminal Propensity

There are multiple channels through which losing access to health insurance can affect an individual's criminal behavior. This study focuses on the mental health channel: for many individuals, losing Medicaid eligibility means losing access to mental health treatments or medications.[12,13] This loss of access to mental healthcare could result in increased criminal behavior for various reasons.

First, individuals who lose insurance coverage might begin to find criminal activities more appealing. For example, individuals who lose access to medications might begin to self-medicate via higher use of illicit drugs (Khantzian 1987, Khantzian 1997). Indeed, Busch et al. (2014) finds that following a regulatory policy that decreased antidepressant prescriptions, adolescents with depression were more likely to use illegal drugs. Loss of mental healthcare could also disrupt an individual's human capital formation or labor market productivity, thereby making criminal alternatives more attractive. Biasi et al. (2019) finds that increased access to lithium—a psychiatric medication primarily used to treat mood or depressive disorders—improved the career trajectories of individuals suffering from bipolar

[12]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).

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

disorder. In addition, Currie and Stabile (2006) and Currie and 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.[14]

Finally, individuals who lose access to behavioral health services might be more prone to making errors in judgment or decision-making, and thus be more likely to engage in criminal behavior. For example, Heller et al. (2017) finds that low-income adolescents participating in cognitive behavioral therapy (CBT) programming during the school year were significantly less likely to be arrested for both violent and non-violent offenses, but that these effects did not persist after the program ended. Furthermore, health insurance coverage provides individuals with access to resources (e.g., social workers, community-based services) that could help de-escalate mental health crises or treat substance abuse, thereby preventing future criminal involvement (Bondurant et al. 2018).[15]

1.3 Data and Sample

This paper studies the effect of health insurance on crime in the state of South Carolina. South Carolina is relatively poorer than other states in the U.S. and it also has low levels of health insurance coverage among non-elderly adults.[16]

The data source used in this study is individual-level administrative data from various state agencies, which have been linked by South Carolina's Revenue and Fiscal Affairs (RFA)

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

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

[16]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).

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

1.3.1 Sample

The primary sample used in this study is a disproportionately low-income group of individuals born between 1990 and 1999. One can think of this sample as representing the residents of the poorest half of neighborhoods in South Carolina.[17] Specifically, an individual is included in the sample if he or she ever attended a high school among the poorest half of high schools in the state. The dataset provides information on 210,443 individuals starting at age ten. For more details on the sample and the variable construction, I refer the reader to Appendix B.

1.3.2 Medicaid Claims

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

[17] 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]).

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

1.3.3 Data on Criminal Behavior

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

1.3.4 Other Data Sources

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

program. Finally, I make use of death certificate records from the Department of Health and Environmental Control.

1.4 Empirical Strategy

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

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

1.4.1 Research Design

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

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

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

therefore measures the likelihood that an individual is incarcerated in any adult correctional facility.

In order to measure the impact of losing Medicaid eligibility on men's likelihood of incarceration, I employ a difference-in-differences design. Men who were likely to be enrolled in Medicaid before their 19th birthday serve as the treated individuals in the analysis (for the remainder of the paper, I will refer to this group as the "high-enrollment" group). Individuals who were enrolled in Medicaid earlier in adolescence, but who were less likely be enrolled prior to their 19th birthday serve as the comparison group (henceforth, the "low-enrollment" group). Importantly, because all treated individuals lose Medicaid coverage at the same point in the life cycle, it is important to compare their outcomes to those of a comparison group in order to estimate and account for age trends in crime.[20] It is not possible to discern from the data why the low-enrollment group was enrolled in Medicaid earlier, but not later in adolescence. However, possible explanations include large hurdles to enrolling (e.g., long waits at the county office) or no longer wanting or needing access to Medicaid services. Regardless of the reason for not being enrolled, the comparison group helps me estimate counterfactual outcome paths for the treated individuals and thus allows me to disentangle the effect of the insurance loss from pure age effects.

Specifically, I assign men into the high- and low-enrollment groups based on their enrollment in Medicaid between the ages of $16\frac{1}{2}$ and $17\frac{1}{2}$. I then follow the natural evolution of each group's outcomes for three and a half years: the first year and a half serves as the study's pre-period (ages 17.5–19) and the latter two years, which begin on the individual's 19th birthday, serve as the study's post-period (ages 19–21). Figure 1.2 offers a graphical timeline of the approach.[21]

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

[21]I assign individuals into the treatment and control groups a year before the study pre-period in order to guarantee that the assignment is uncorrelated with the outcomes in the pre-period. For example, men who are incarcerated before age 19 might be less likely to be enrolled in Medicaid. Assigning individuals

1.4.2 Matching Procedure

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

I then match each treated person to all "counterfactual" individuals using a parsimonious set of characteristics. Treatment men are matched to all control men based on year of birth, race (measured as Black or non-Black), school district (measured as the last school district attended prior to age 19), and mental health history prior to age 16 (i.e., measured as having a Medicaid mental health claim between the ages of 10 and 15). This matching procedure—and the corresponding weighting scheme described below—is similar to the approach used in Smith et al. (2019a). 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 1.1 illustrates that men with prior mental health histories are

to a group before the start of the study pre-period therefore allows me to follow the natural evolution of outcomes both before and after their 19th birthdays. I check the sensitivity of the main results to this choice in Section 1.6.3.

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

on significantly different criminal trajectories than men without any diagnosed mental disorders. Matching on mental health history before age 16, when both treated and control individuals were enrolled in Medicaid, is therefore useful for matching treated individuals to counterfactual men with similar criminal propensities (as opposed to explicitly matching on criminal histories, which would entail matching on an outcome variable). I illustrate the importance of including mental health history as a matching characteristic in Section 1.6.3.

1.4.3 Characteristics of Matched Sample

Table 1.1 reports summary statistics for the full sample and various subsamples. All characteristics are measured before an individual's 19th birthday (i.e., between the ages of 10 and 18). On average, individuals in the matched sample (column 3) are more likely to be Black and more likely to have a mental health history than individuals in the full low-income sample (column 1). Importantly, men in the matched sample are also observably different from the group of individuals who were ever enrolled in Medicaid (column 2). In particular, treated men are more likely to have a mental health history and slightly more likely to have had prior contact with the criminal justice system. These differences in observable characteristics prior to age 19 reinforce the importance of implementing the matching procedure in order to ensure that the low-enrollment group serves as a suitable comparison group for estimating the counterfactual outcome paths of high-enrollment individuals.[23]

Appendix Table 1A.2 shows the distribution of diagnoses for the full sample as well as for treated men. This table highlights that hyperkinetic syndrome of childhood (i.e., attention-deficit/hyperactivity disorder [ADHD]), developmental delay, and disturbances of conduct are among the most common diagnoses in this sample. Importantly, I also note that the shares in this table do not sum to 100 because most teens have more than one diagnosis

[23]Table 1A.1 compares means for the matching candidates (i.e., the individuals who met the criteria for being eligible for the matching procedure) as well as the successfully matched individuals. One takeaway from this table is that around 97% of eligible treated individuals were successfully matched to at least one control individual.

in their medical records (more than 60 percent of treated men with mental health histories have at least two different diagnoses in their claims). Finally, Appendix Table 1A.3 shows that individuals utilizing behavioral health services most often file claims for psychotropic drugs as well as case management services.

The matching procedure guarantees that there is balance between the high- and low-enrollment groups in terms of race, cohorts, geographic location, and mental health history. However, there still might be concern that the two groups significantly differ along unobservable characteristics and would thus plausibly exhibit different trends in criminal propensity. Appendix Figure 1A.1 plots the raw means of standardized test scores as well as juvenile justice referrals for felony offenses for the two groups earlier in adolescence. These figures show that the treated and control men in the matched sample were relatively similar to each other in terms of their educational achievement and criminal propensity, both in levels and in trends, throughout their adolescence.

1.4.4 Loss of Medicaid Eligibility in Matched Sample

To confirm that high-enrollment men are indeed affected by the loss of Medicaid eligibility at age 19, I use the Medicaid enrollment and claims data to plot the share of men enrolled in the program and filing claims. The first two panels of Figure 1.3 show that treated men experience a decline in Medicaid enrollment and in their likelihood of filing claims after their 19th birthday. It is worth noting that because individuals are assigned to the treatment and control groups prior to age $17\frac{1}{2}$, the share of control men enrolled and filing claims in the pre-period is not mechanically zero; however, men in this low-enrollment group are significantly less likely to be affected by the loss of Medicaid eligibility at age 19.

Among treated men, the average share enrolled drops from 78 percent in the pre-period to 14 percent in the post-period, thereby confirming the presence of a large "first stage." Appendix Figure 1A.2 shows that the high-enrollment men did not experience comparable

drops when considering enrollment in other public assistance programs (i.e., SNAP and TANF).

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

1.5 Estimating the Effect of Losing Medicaid on Criminal Behavior

1.5.1 Baseline Specification

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

$$Y_{it} = \sum_{\tau=-6}^{\tau=8} \Big[\beta_\tau(\text{Treat}_i \times \gamma_\tau) + \theta_\tau \gamma_\tau\Big] + \mu \text{Treat}_i + \delta_t + \epsilon_{it} \tag{1.1}$$

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

weight each control observation by one over the number of control units matched to the corresponding treated individual. All standard errors are clustered at the individual level.

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

When presenting the results, I typically report the post-period average of the β_τ coefficients (i.e., $\frac{1}{8}\sum_{\tau=1}^{8}\beta_\tau$), or in other words, the average treatment effect in the post period. When considering cumulative variables (e.g., individual i has *ever* been incarcerated), I instead report the last coefficient β_8, which measures the treat-control difference in outcome Y by age 21. Finally, when interpreting the magnitudes of the estimates, I re-scale the average treatment effect in the post-period by the control group's average outcome Y in the post-period. When using cumulative variables, I re-scale β_8 by the comparison group's average Y in the final period of the post-period.[24]

1.5.2 Identification Assumption

I interpret the β_τ coefficients as the causal effect of losing Medicaid eligibility on outcome Y. Identification of the causal effect comes from differences in outcomes between high-enrollment and low-enrollment men over time. Even though the loss of Medicaid eligibility occurs at different calendar times for each treated individual, the presence of the comparison group ensures that the effects are estimated using the differences between treated and control units (as opposed to using the differences in timing to identify the treatment effect, like in an

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

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

One concern with this identifying assumption might be that men in the treatment and control groups are fundamentally different and thus likely to have differing outcome paths. For example, if treated men are relatively lower-income than the comparison group, then we might expect treated men to be more likely to engage in criminal activity regardless of the loss in Medicaid eligibility. Matching on observable characteristics, like race and geographic location, helps alleviate this concern.[25] 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 control individuals turn 19 at similar times (by construction) and all individuals have their 19th birthdays at different points in calendar time. For unobserved shocks to confound the estimates, it would have to be the case that these shocks only affect high-enrollment men—and not observably similar individuals living in close proximity—and that the timing of these shocks coincided with these individuals' 19th birthdays. The differences in the timing of birthdays as well as the matching procedure therefore make it unlikely that the

[25]The robustness checks in Section 1.6.3 also addresses this concern by matching on school rather than district.

estimated effects are driven by treated individuals experiencing unobserved shocks unrelated to their loss in Medicaid eligibility.

1.5.3 Heterogeneity

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

$$Y_{it} = \sum_{\tau=-6}^{\tau=8} \Big[\alpha_\tau(\text{Treat}_i \times \text{Hist}_i \times \gamma_\tau) + \nu_\tau(\text{Hist}_i \times \gamma_\tau) + \beta_\tau(\text{Treat}_i \times \gamma_\tau) + \theta_\tau\gamma_\tau\Big] + \text{Treat}_i + \delta_t + \epsilon_{it} \tag{1.2}$$

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 (1.1) in that high-enrollment men are still being compared to the matched comparison group over time. However, this specification allows treated individuals with mental health histories to have differing treatment effects—captured by α_τ—than treated individuals without mental health histories. When reporting results, I report the post-period average of the α_τ coefficients (or α_8 for cumulative variables), which measures whether treated individuals *with* a mental health history had a different average treatment effect than individuals *without* a mental health history. In that regard, this regression is equivalent to running a triple-differences specification.[26]

[26] I could have also split the sample by mental health history and run two separate double-difference regressions using equation (1.1). The benefits of using equation (1.2) are that I can more easily test whether

1.6 Main Results for Criminal Behavior

1.6.1 Full Sample

I first consider an individual's likelihood of being incarcerated in a given quarter. Panel (a) of Figure 1.4 plots the raw means using the full matched sample, and panel (b) plots the estimates from equation (1.1).[27] These figures confirm that treated men were trending similarly to the men in the comparison group prior to age 19, thereby providing support for the parallel trends assumption. It is also worth highlighting that the treatment group's likelihood of incarceration resembled that of the comparison group's not only in its trending behavior, but also in its levels during the entire pre-period. The estimates from panel (b) show that treated men were 15 percent more likely to be incarcerated in any given quarter of the post-period than the men in the comparison group. The results from this and the following subsection are summarized in Table 1.2.

In an effort to understand whether the estimates are driven by the same or different individuals being incarcerated over time, I then use a cumulative variable, which captures the extent to which *new* individuals are being incarcerated.[28] Panels (c) and (d) once again show that treatment and control individuals were trending similarly prior to age 19, and began to significantly diverge after age 19. By their 21st birthdays, men in the high-enrollment group who lost access to Medicaid were 2 percentage points (or 18 percent) more likely to have been incarcerated than men in the matched comparison group.

the differences are statistically different from each other and I can use the full sample to estimate the calendar time fixed effects.

[27] Analogous plots of the raw means and treat-control differences using alternative measures of criminal behavior are displayed in Appendix Figures 1A.3 and 1A.4, and summarized in Appendix Table 1A.4.

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

1.6.2 Heterogeneity by Mental Health History

To see whether this rise in criminal activity is driven by men with mental illness, I split the sample into two groups based on men's mental health history prior to age 16. Figure 1.5 displays the results. Panel (b) plots the treat-control differences for men without a mental health history (44 percent of treated men, in orange) as well as the the treat-control differences for men with a mental health history (56 percent of treated men, in blue). These results suggest that the increase in crime is driven entirely by men with a mental health history. Treated men without mental health histories continue to trend similarly to their corresponding comparison group throughout the post period.[29]

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

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

Finally, I can use these reduced-form estimates to estimate the implied effect of Medicaid enrollment on men's likelihood of incarceration. Table 1.3 displays the results, showing that

[29] Appendix Figure 1A.5 plots these same treat-control differences by race, indicating that the higher likelihood of incarceration is more pronounced for Black men (who represent 70% of high-enrollment men with a mental health history).

Medicaid enrollment decreases low-income men's likelihood of incarceration by around 25 percent.[30] Among men with mental illness, Medicaid enrollment decreases their likelihood of incarceration by 2 percentage points, a 40 percent reduction. To put these magnitudes into perspective, Lochner and Moretti (2004) find that one extra year of schooling reduces men's likelihood of incarceration by approximately 10 percent. These findings thus suggest that providing health coverage to individuals with mental illness could be one way to significantly reduce their criminal involvement.

1.6.3 Robustness of Main Results

Robustness to Clustering and Matching

In this section, I consider the robustness of the main results displayed in Figure 1.5 to various other choices I could have made when constructing the baseline sample and implementing the preferred specification. Appendix Table 1A.5 reports the results from this subsection. The first row reproduces the main results using the baseline sample. In the second and third rows, I cluster the standard errors at different levels: at the match level and at the match and individual level, respectively. The statistical significance of the main results is preserved.

I then test the sensitivity of the estimates to the matching procedure and implicit weighting scheme. To verify the parallel trends assumption in these robustness checks, Appendix Figure 1A.6 displays the graphical results. First, instead of matching each treated unit to all possible counterfactual control units, I force each treated unit to only have one randomly chosen counterfactual observation (fourth row of Appendix Table 1A.5). In the fifth row, I return to the baseline matching procedure, but I drop control units that get disproportionate

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

$$Y_{it} = \beta_0 \text{Enrolled}_{i\tau} + \beta_1 \text{Post}_{\tau} + \beta_2 \text{Treat}_i + \delta_t + \epsilon_{it} \tag{1.3}$$

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.

weight in the regression.[31] In the next row of the table, I alter the timing of assignment into the treatment and control groups, so that individuals are assigned to a group based on their Medicaid enrollment during the study's pre-period (as opposed to a year before the study's pre-period). Finally, instead of using an individual's school district as a matching characteristic, I use an individual's school (measured as the last school attended before age 19) so that high-enrollment individuals are even more likely to resemble the low-enrollment group. This change reduces the number of matched treated units, but it does not significantly alter the main findings (seventh row).

Next, I consider the robustness of the main results to alternative ways of constructing the comparison group. First, I abstain from any matching and re-weighting, and instead run the specification with all eligible treated and control men.[32] The main results by mental health history (eighth row) are very similar to those using the baseline matched sample. Finally, instead of relying on coarsened exact matching to construct a suitable comparison group, I instead re-weight the eligible control men using the DFL re-weighting approach (DiNardo et al. 1995, Fortin et al. 2011). I estimate two potential weights using this method: one based on the baseline set of matching characteristics and one that replaces school district with school. I find that the results are robust to this alternative approach for re-weighting the eligible control individuals.

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

[32] Appendix Figure 1A.7 plots the raw means for all eligible treated and control men. Panels (a) and (b) highlight that without any matching or re-weighting, the comparison group in the full sample does not exhibit parallel trends to the high-enrollment men. Once mental health history is taken into account, however, the control men in each group do exhibit similar trends to the corresponding treated men in the pre-period. These latter two figures thus illustrate the importance of including mental health history as a matching characteristic if the goal is to find control men who can serve as a suitable comparison group for tracing out the counterfactual outcome paths of treated men.

As a final way to test the robustness of the main result using the matched difference-in-differences approach, I conduct a "placebo" check, in which I shift the entire empirical approach back one year and estimate men's likelihood of incarceration around age 18, rather than around age 19.[33] If the main findings are driven by the loss of Medicaid at age 19, then I should not find an immediate increase in treated men's criminal propensities after their 18th birthday when there was not a break in Medicaid eligibility. Figure 1A.8 displays the results from this exercise, which suggest that the immediate increase in the cumulative likelihood of incarceration among treated men with mental health histories is indeed driven by this group's loss of Medicaid eligibility at age 19.

Alternative Empirical Strategy: Regression Discontinuity

As an alternative way to check the robustness of the main result—that men with mental health histories who lose Medicaid eligibility at age 19 are more likely to be incarcerated in the following two years—I abstract away from assigning men to treatment and control groups, and instead utilize a regression discontinuity approach, similar to that in Card et al. (2009) and Lee and 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:

$$Y_{it} = f(a_i) + \beta_1 \text{Post19}_t + \beta_2[\text{Post19}_t \times f(a_i)] + \epsilon_{it} \tag{1.4}$$

[33] I assign individuals to treatment and control groups based on their Medicaid enrollment at ages 15.5–16.5 (instead of 16.5–17.5). I then implement the matching procedure and estimate the treat-control differences around men's 18th birthdays. I run the same specification as equation (1.2) with six and eight quarters in the pre- and post-period, respectively, but only report the coefficients for the first four quarters of the post-period in order to assess whether treated individuals are more likely to commit crimes between their 18th and 19th birthdays.

[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 SLED arrest records (as opposed to using incarceration records from the Department of Corrections, which might reflect longer-term judicial decisions).

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

Appendix Figures 1A.9 and 1A.10 display the results for various subgroups, showing that men's likelihood of being arrested increases upon reaching age 19. Importantly, panels (c) and (d) show that these discontinuities are particularly salient for men with mental health histories, and especially for men who were using Medicaid's behavioral health services right before their 19th birthdays. These findings suggest that men with mental health histories were 0.25 percentage points (or roughly 10 percent) more likely to be arrested upon reaching their 19th birthdays.

1.7 Heterogeneity in Offenses and in Behavioral Healthcare Utilization

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

1.7.1 Offense Types

In order to gain insight into why men with mental health histories are more likely to be incarcerated, I study whether these men are more likely to be incarcerated for any particular types of crimes. In particular, I classify the offenses that result in incarceration into (1) violent crimes (e.g., murder, assault, robbery), (2) drug- and alcohol-related crimes, (3) property crimes (e.g., burglary, larceny, motor vehicle theft), and (4) miscellaneous offenses (e.g., weapons offenses, parole or probation violations).

Figure 1.6 and Table 1.4 display the results, showing that treated men with mental illness are more likely to be incarcerated for violent, drug-related, and property offenses relative to their matched comparison group. The largest effect comes from drug-related offenses (50 percent increase), suggesting that self-medication might be a particularly relevant channel through which the loss of insurance results in increased criminal activity. However, Appendix Figure 1A.11 shows that the rise in drug-related offenses comes from increases in both possession-related and distribution-related charges (66 percent and 54 percent increases, respectively), thus indicating that at least part of the rise in criminal activity is likely economically motivated. Overall, these findings suggest that there is not a single channel through which loss of access to insurance results in increased criminal involvement.

1.7.2 Differences by Behavioral Healthcare Utilization

Because losing insurance coverage means losing access to mental health treatment or medications, we would expect the treatment effects to be larger in magnitude for men who were using behavioral health services right before turning 19. To test this hypothesis, I focus on men with mental health histories and split this group based on how recently each treated individual was filing claims for Medicaid's behavioral health services. Specifically, I designate treated men with mental health histories as recent beneficiaries if they had a mental health

claim in the year and a half before turning 19 (i.e., the study pre-period). Among men with mental health histories, 37 percent of them filed a behavioral health claim during this period. Within this group, 56 percent were filing claims for mental health medications, 8 percent for alcohol and substance abuse treatment services, and 38 percent for psychotherapy or case management services.

In practice, I maintain the same matched control individuals for each treated person, and run triple-difference specifications—similar to equation (1.2)—in order to estimate whether the treat-control differences for men who were using behavioral health services *more* recently are statistically different from the treat-control differences of individuals using services *less* recently. If the loss of mental health services is an important factor driving the increase in criminal activity, then I should find a positive and statistically significant average treatment effect for more-recent beneficiaries.

Figure 1.7 shows that men who were using behavioral health services right before their 19th birthdays (denoted by the purple circles) are indeed more likely to be incarcerated after losing access to Medicaid, when compared to men with mental health histories who were using these services *less* recently (gray markers). Overall, men with mental health histories who were using behavioral health services are roughly 10 percent more likely to be incarcerated than less-recent users of behavioral health services. Table 1.5 and Appendix Figure 1A.12 show that this difference is mainly driven by more-recent beneficiaries committing more property crimes after losing Medicaid coverage, suggesting that these crimes were likely economically motivated.

Finally, it is worth noting that around half of these men were relying on Medicaid for access to psychotropic drugs, suggesting that losing access to mental health medications might be an important channel through which losing insurance coverage results in increased criminal activity. To consider the specific role of psychotropic drugs, I instead split the sample of treated men with mental health histories based on mental health medication usage. In

particular, I split the sample into those who filed claims for mental health drugs between the ages of 14–18 (36 percent of treated men with mental health histories) and those who did not file such claims during these ages.[35]

Appendix Figure 1A.13 shows that in every quarter after their 19th birthdays, the men who had previously filed claims for psychotropic drugs were significantly more likely to be incarcerated than men with mental health histories who did not rely on Medicaid for access to mental health medications. Appendix Figure 1A.14 classifies incarcerations by offense type and shows that men relying on Medicaid for medications were more likely to commit both violent and property offenses than men who did not file claims for psychotropic drugs. If the self-medication channel were driving the observed increase in crime, we might have expected to find a larger treatment effect for drug-related crimes among men who relied on Medicaid for psychotropic drugs; the lack of a difference in treatment effects between the two groups in this figure therefore suggests that the self-medication channel is not driving the results.

1.7.3 Differences by Mental Health Diagnoses

Next, I use the diagnoses codes in the Medicaid claims to study which subgroups of men are more likely to contribute to the rise in crime. I first divide treated men with mental health histories into two groups, motivated by the groupings in the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV). Specifically, I divide men into those with a diagnosed personality disorder (which includes intellectual disabilities and represents 18 percent of men

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

with mental illness) and those without a diagnosed personality disorder. Appendix Figure 1A.15 shows that both groups are contributing to the rise in crime.[36]

To further explore which subgroups might be driving the results, Appendix Figure 1A.16 reproduces the main result in Figure 1.5 for men with a mental health history, dropping one diagnosis at a time (i.e., dropping all men ever diagnosed with a particular disorder). The results in this figure indicate that the main result is most sensitive to dropping men who have ever been diagnosed with conduct disorders, disturbances of emotions (e.g., oppositional defiant disorder), non-dependent drug abuse, and depressive disorders. These results thus indicate that men ever diagnosed with these disorders are those who are most likely to commit crime after losing access to Medicaid eligibility.[37] In other words, these findings suggest that Medicaid access was able to suppress these men's criminal propensities and thus was serving as as important safety net for men with these diagnoses.

Lastly, noting the importance of substance abuse in driving the main results, I split the treated men with mental illness into two groups based on whether they have ever been diagnosed with alcohol or drug dependence or abuse (28 percent of men with mental health histories). I then consider the effect of the Medicaid loss on drug-related offenses. Appendix Figure 1A.17 shows that men with mental health histories who have been diagnosed with substance abuse are more likely to be incarcerated for drug-related offenses after losing Medicaid eligibility than men with mental illness who were never diagnosed with a substance abuse disorder.

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

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

1.8 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.[38] In this section, I summarize the approach and estimates, but I refer the reader to Appendix C for a more-detailed discussion.

1.8.1 Costs of Expanding Medicaid Eligibility

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

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

1.8.2 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:

$$Y_{it} = \beta_0(\text{Treat}_i \times \text{Post}_\tau) + \beta_1 \text{Post}_\tau + \beta_2 \text{Treat}_i + \delta_t + \epsilon_{it} \tag{1.5}$$

This specification is almost identical to equation (1.1), except that it measures Medicaid's effect on serious arrests (as opposed to on a person's likelihood of being incarcerated). The coefficient β_0 estimates the extent to which treatment and control individuals differ in their arrest propensity in the post-period relative to the pre-period. The results are reported in Table 1.6.

Using these estimates and taking a relatively conservative approach, I find that the reduced social costs of crime are roughly $22 million. Specifically, I use the estimates from Table 1.6 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 and Piquero (2009) and Miller et al. (1996) to calculate the reduction in the total social costs of victimization. Appendix C discusses the sensitivity of these estimates to the average cost assigned to certain offenses (i.e., murder and drug-related offenses).

Next, I calculate the benefits from fewer incarcerations, and estimate that providing Medicaid eligibility would result in $2.4 million and $5.1 million lower fiscal and social costs, respectively. To estimate the fiscal costs, I calculate the cost of incarcerating men in South Carolina using the average sentence served for various offenses as well as the daily cost per inmate. Finally, I use the estimates from Mueller-Smith (2015) to calculate the social costs

generated by prison spells in terms of economic impact (i.e., reduced employment, greater reliance on public assistance) and post-release criminal behavior.

After comparing the costs of Medicaid provision with the potential benefits generated from reduced criminal activity, I conclude that providing insurance coverage to low-income young men is a cost-effective way to reduce crime. Using moderately conservative estimates, the findings of this paper suggest that for every dollar spent on insuring low-income young men via Medicaid, society recoups $2 in social and fiscal costs. Table 1.7 summarizes the estimates from this analysis, showing that even in the most conservative approach, the estimated benefits of Medicaid provision outweigh the costs.

1.8.3 Marginal Value of Public Funds

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

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

these two categories. Overall, though, the findings from this exercise confirm that $1 of spending on this policy delivers more than $1 in benefits to its beneficiaries.

1.9 Comparing Medicaid Provision to Longer Punishments

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

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

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

1.9.1 Empirical Approach for Estimating Deterrence

In order to estimate the deterrence effect of harsher sanctions, I compare men's likelihood of committing a felony before and after their 17th birthdays using data from both the

Department of Juvenile Justice and the South Carolina Law Enforcement Division. I estimate this likelihood for all of the men in the low-income sample for whom I have an available birth month (i.e., ever enrolled in Medicaid) and who had not committed a felony prior to age 16 (66 percent of the full sample, $N = 72,939$).[39] Any estimated discontinuity at age 17 captures the change in the likelihood of committing a first felony offense upon reaching the age of criminal majority. For more details on the sample and variable construction, I refer the reader to Appendix D.

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

$$
\begin{aligned}
P(Y_{it} \mid D_t, X_t) &= F(\alpha X_t' + D_t\theta) \qquad (1.6)\\
\text{where} \quad X_t' &= (1, (t-t_0), (t-t_0)^2, (t-t_0)^3)\\
\text{and} \quad F(z) &= \frac{\exp(z)}{1+\exp(z)}
\end{aligned}
$$

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

The parameter θ represents the discontinuous change in the log-odds of committing a felony offense upon reaching the age of criminal majority. In order to interpret θ as the

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

deterrence effect of harsher sanctions, the main assumption is that other determinants of criminal propensity were not changing discontinuously at age 17. Unlike Lee and McCrary (2017), the age of criminal majority in South Carolina coincides with the age at which individuals can legally drop out of school. After presenting the main results, I discuss this potential confounder and provide evidence showing that the minimum dropout age is most likely not confounding the estimate of θ.

1.9.2 No Evidence of Deterrence

Figure 1.8 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 based on estimates using equation (1.6). The estimated discontinuity θ is small and statistically insignificant, showing little indication of a systemic drop in felony arrests upon reaching age 17.[40] The bottom panel of this figure displays the same deterrence estimates after splitting the sample by an individual's mental health history. The findings are suggestive of teens with mental health histories being even less deterred by harsher punishments than teens without mental health histories.

I then use these estimates to calculate the corresponding elasticity of criminal behavior to sentence lengths (summarized in Table 1.8). Specifically, I use the discontinuity estimate from the full sample and the smallest negative effect consistent with the confidence intervals to conclude that at most, there was a 14 percent decline in the probability of a felony arrest (after dividing the most negative marginal effect estimate by the age-16 average). With respect to the percent change in sentence lengths, calculations detailed in Appendix D

[40] Appendix Figure 1A.18 shows the likelihood of a felony arrest around age 17 using the approach outlined in Section 1.6.3 (i.e., using shares instead of hazard rates). The statistically insignificant estimate from this figure confirms that individuals are equally likely to commit a felony offense upon reaching the age of criminal majority.

suggest that the expected sentence length rose by approximately 1,550 percent (i.e., from 96 days in juvenile detention to 4.5 years in adult prison). These estimates therefore imply an elasticity of -0.009, which is very close to Lee and McCrary (2017)'s reduced-form elasticity of -0.007. If I use the average time served in state prison (2.3 years), then the corresponding elasticity is -0.018.

1.9.3 Minimum Dropout Age is not Confounding Deterrence Estimates

Because the age at which adolescents can legally drop out of school coincides with the age of criminal majority, there might be concern that the deterrence estimate is combining the negative effect of the harsher punishments with the effect of adolescents dropping out of school upon reaching age 17. In principle, one might think that dropping out of school has a positive effect on the likelihood that an adolescent engages in crime, primarily because schools "incapacitate" would-be offenders (see e.g., Anderson 2014, Berthelon and Kruger 2011, Fischer and Argyle 2018).[41]

To test for the presence of this "incapacitation" effect in this setting, I first estimate men's likelihood of being arrested for a felony during each month of the calendar year. Appendix Figure 1A.19 displays the graphical results and Appendix Table 1A.6 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.

[41] Increased educational attainment has also been shown to reduce an individual's likelihood of incarceration and arrest in the long run (Lochner and Moretti 2004). Cook and 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).

Second, I leverage the seasonality of dropout: Appendix Figure 1A.20 shows that teens are significantly more likely to drop out of school in the beginning of the school year. Individuals born in the first half of the year are therefore less likely to drop out immediately after turning 17 than individuals born in the second half of the school year (Appendix Figure 1A.21). If dropout is contaminating the estimates of deterrence, then we would expect the estimate of θ for teens born in the second half of the calendar year to be more confounded (i.e., less negative) than the estimate for individuals born earlier in the year. Appendix Figure 1A.22 shows that this pattern is not present in the data, providing another piece of evidence against the idea that the minimum dropout age is contaminating the deterrence estimates.

Finally, I utilize the fact that the share of students dropping out of school is declining with time in this sample in order to gauge the extent to which the deterrence estimate is confounded by dropout. In other words, if dropout is contaminating the estimate and the share of students dropping out is declining with time, then we should see θ become less confounded (i.e., more negative) with time as well. The first panel of Appendix Figure 1A.23 illustrates that the share of students dropping out decreases with time: the share of teens enrolled in school at age 17 is 12 percentage points higher for the 1997–1999 birth cohorts than for the 1990–1992 birth cohorts. The remaining panels illustrate that despite this large decline in the likelihood of dropout, the deterrence estimates do not fall monotonically with time, remaining both close to zero and statistically insignificant.

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

estimate an elasticity of crime to sentence lengths of -0.019 (or -0.037 using the average sentence served).[42]

1.9.4 Cost-Comparison for 10% Crime Reduction

I conclude this section by using the estimated elasticities of crime with respect to sentence lengths to compare the cost effectiveness of two crime-reduction approaches: providing Medicaid eligibility and increasing sentence lengths. The estimates in Table 1.6 imply that providing Medicaid eligibility to low-income young men would reduce crime by 10 percent. I thus compare the cost of each policy for achieving this same reduction in crime.[43] I summarize the estimates here, but refer the reader to Appendix E for a more-detailed discussion.[44]

First, I find that the total cost of reducing crime by 10 percent via Medicaid totals $77 million. This cost includes the total cost of providing Medicaid coverage to low-income young men (which I calculated and discussed in Section 1.8 and totals $15 million) as well as the cost of incarcerating men who were *not* deterred from committing crime for the current length of sentences (totaling $20.6 million). Two other components are included in this figure: the fiscal costs of recidivism ($4.6 million) as well as the social costs of victimizations ($36.6 million) arising from the fact that a share of these men will likely re-offend after serving the relatively shorter sentences.

Alternatively, I find that the total cost of reducing crime by 10 percent through longer sentence lengths totals $109.6 million, which is roughly 45 percent higher than the cost

[42]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.

[43]The estimate in column 1 of Table 1.6 suggests that among men born between 1990–1993, there were roughly 403 excess serious arrests (21,455 $\times$ 0.00235 $\times$ 8 quarters). I then use the raw data to calculate that there were 3,918 serious arrests among 19- and 20-year olds born in these cohorts, which implies that serious crime would have been 10 percent lower if Medicaid eligibility had not suddenly expired at age 19.

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

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

1.10 Discussion & Conclusion

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

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

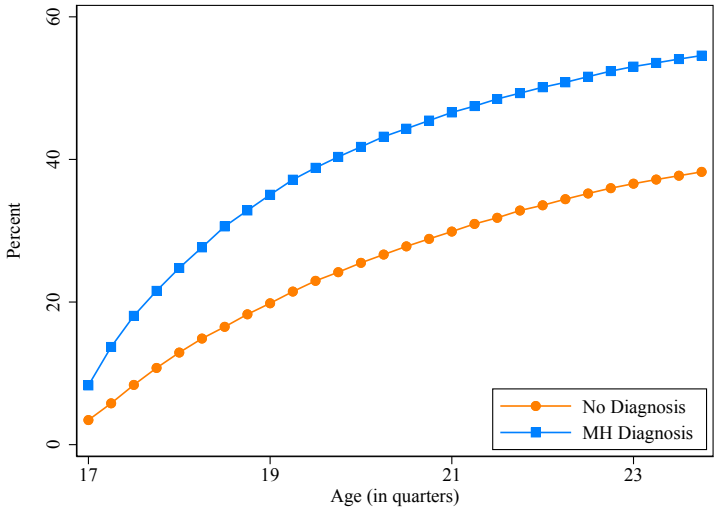
The findings of this study offer a number of takeaways and policy implications. First, this paper studies a subpopulation of offenders—low-income young men with mental health histories—for whom access to healthcare serves as an effective deterrence mechanism. The cost effectiveness of Medicaid provision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

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

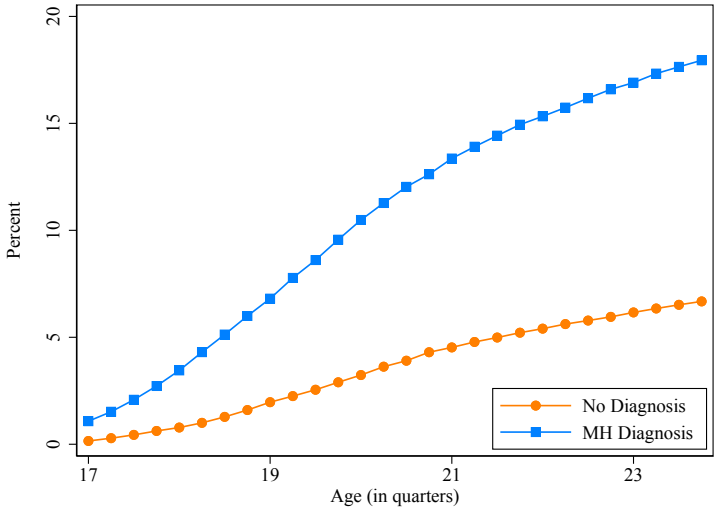
Figures and Tables

Figure 1.1: Criminal Activity, by Mental Health Diagnosis Earlier in Adolescence

(a) Ever Arrested

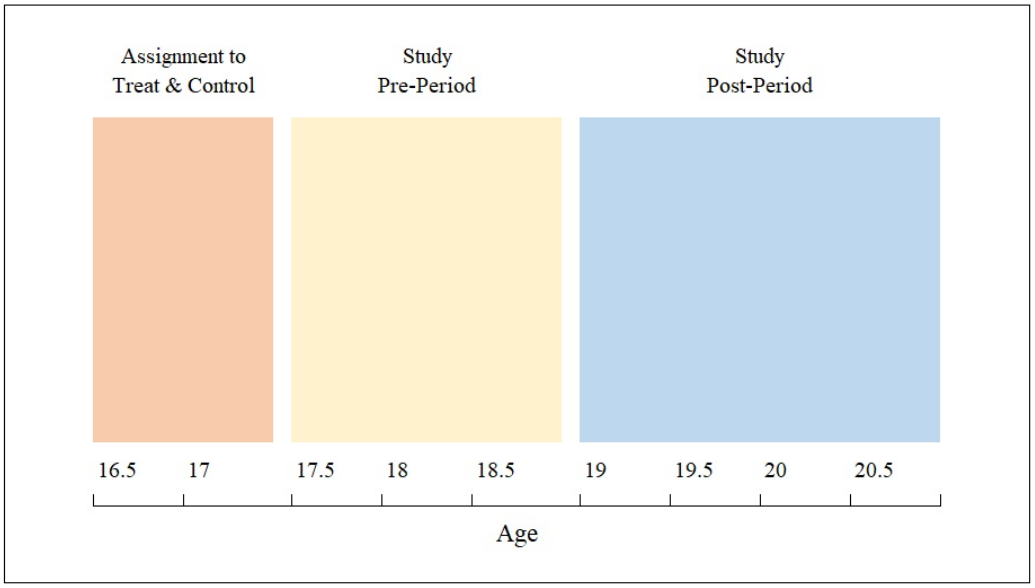


(b) Ever Incarcerated



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

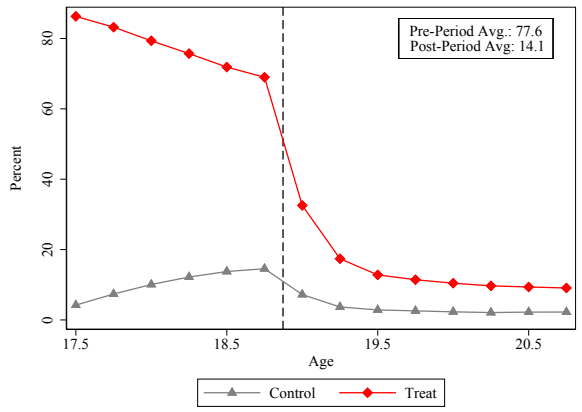
Figure 1.2: Timeline of Empirical Strategy



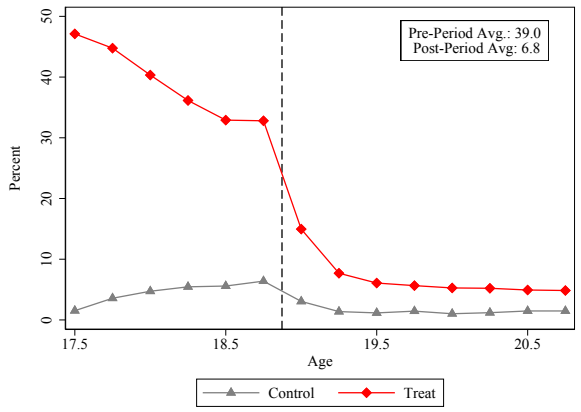
NOTE: Individuals are assigned to the treatment and control groups based on their enrollment in the Medicaid program between ages $16\frac{1}{2}$ and $17\frac{1}{2}$ (orange). Their outcomes are then allowed to evolve naturally in the study pre-period (yellow) and post-period (blue).

Figure 1.3: Share of Matched Treated and Control Individuals Enrolled in Medicaid and Filing Claims (Raw Means)

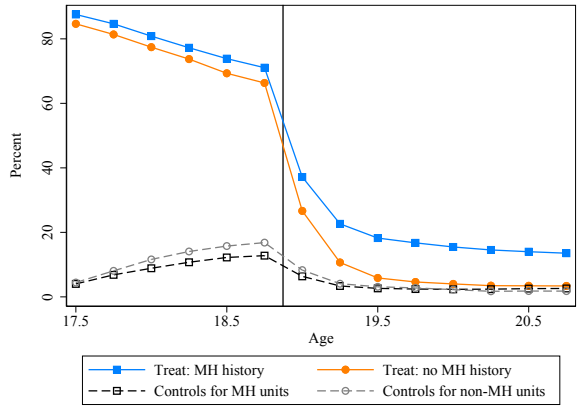
(a) Enrolled



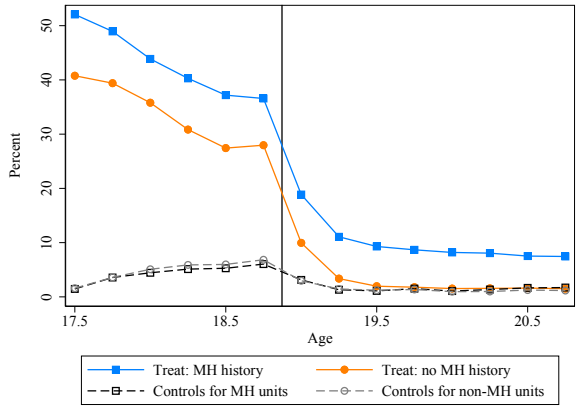
(b) Filing any claims



(c) Enrolled



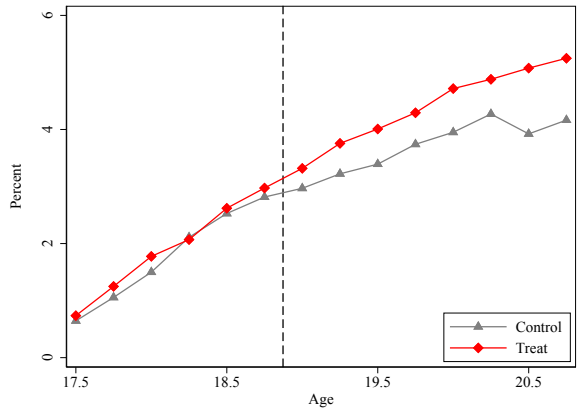
(d) Filing any claims



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

Figure 1.4: Likelihood of Incarceration in Full Sample (Raw Means & Treat-Control Differences)

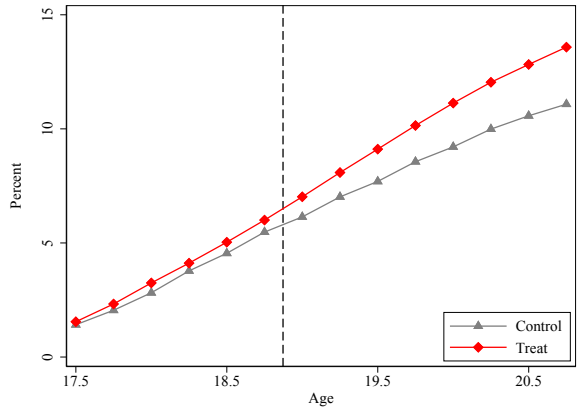
(a) Incarcerated that quarter



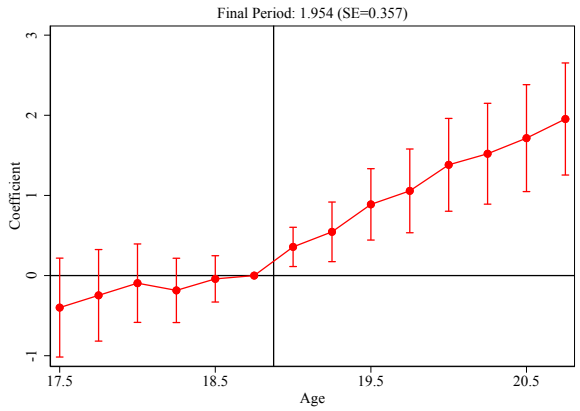
(b) Incarcerated that quarter



(c) Ever incarcerated



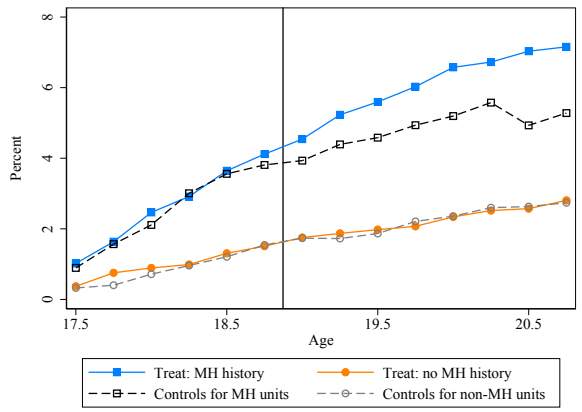
(d) Ever incarcerated



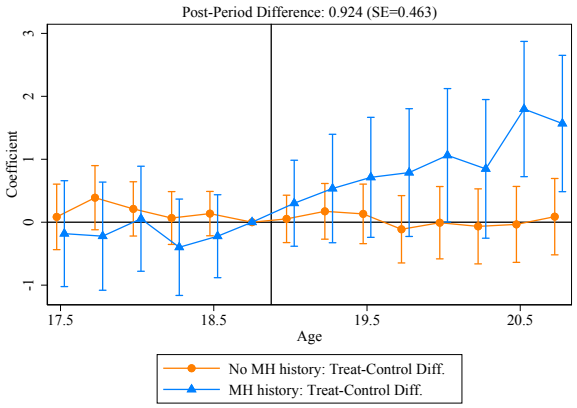
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the raw means of the matched sample. Panels (b) and (d) plot the treat-control differences estimated using equation (1.1). "Post-Period Average" and "Final Period" refer to the average of the post-period β_τ coefficients or to β_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 1.5: Likelihood of Incarceration, by Mental Health History (Raw Means & Treat-Control Differences)

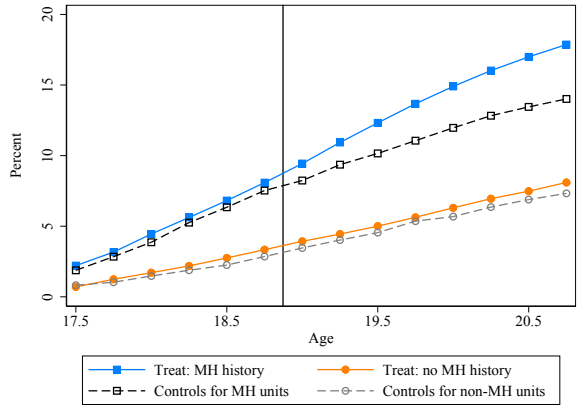
(a) Incarcerated that quarter



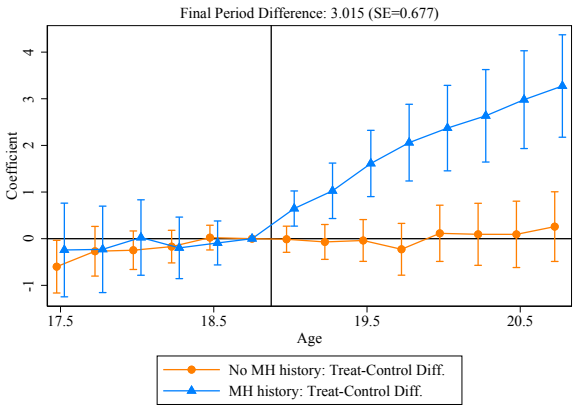
(b) Incarcerated that quarter



(c) Ever incarcerated



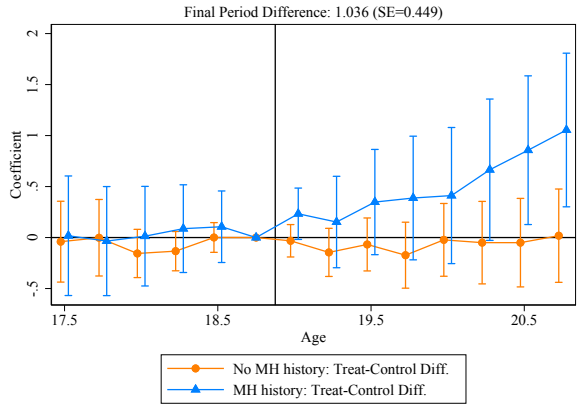
(d) Ever incarcerated



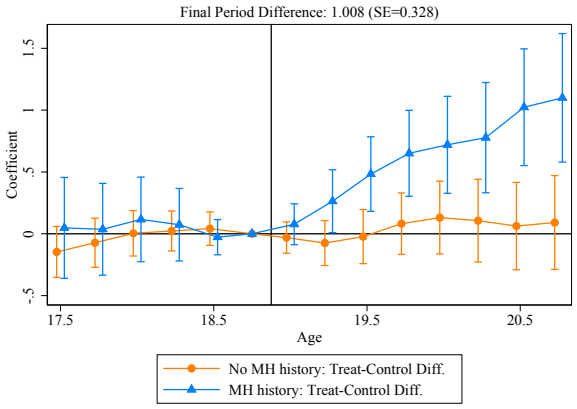
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the raw means of the matched sample separately by mental health history. Panels (b) and (d) plot the treat-control differences estimated using equation (1.2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 1.6: Likelihood of Ever Being Incarcerated, by Mental Health History and Offense Type

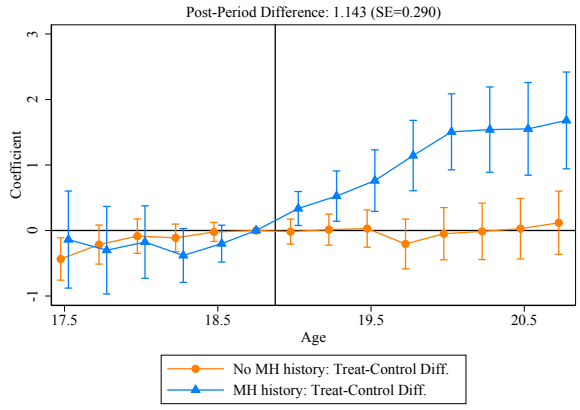
(a) Violent Offenses



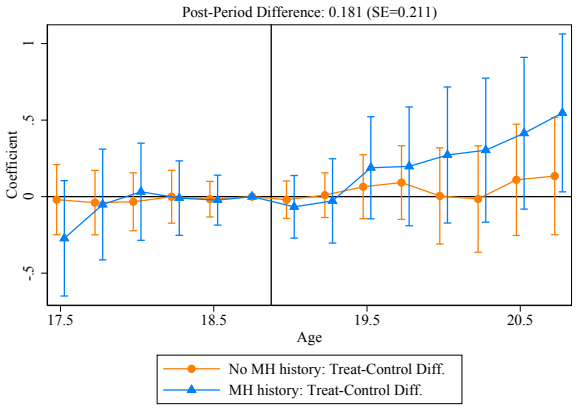
(b) Drug-Related Offenses



(c) Property Offenses



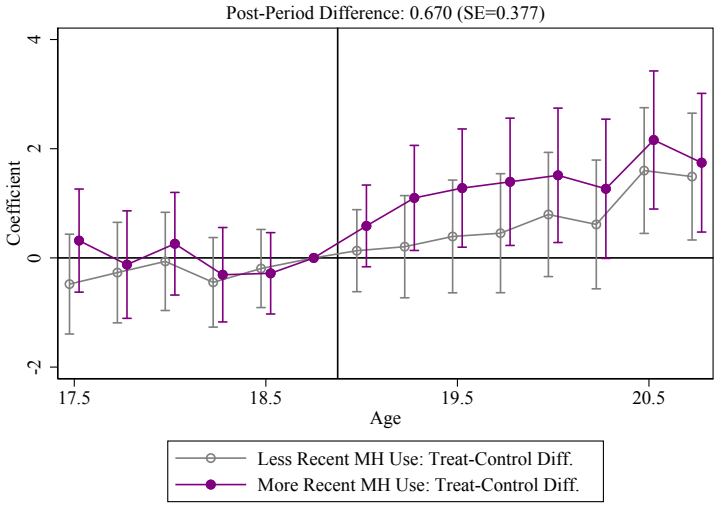
(d) Miscellaneous Offenses



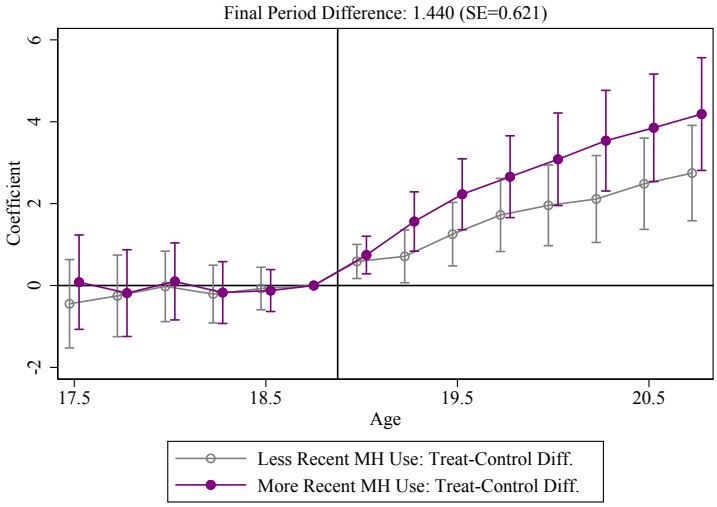
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures plot the treat-control differences estimated using equation (1.2). "Final Period" refers to the α_8 estimate in that specification. Standard errors are clustered at the individual level.

Figure 1.7: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization

(a) Incarcerated that quarter



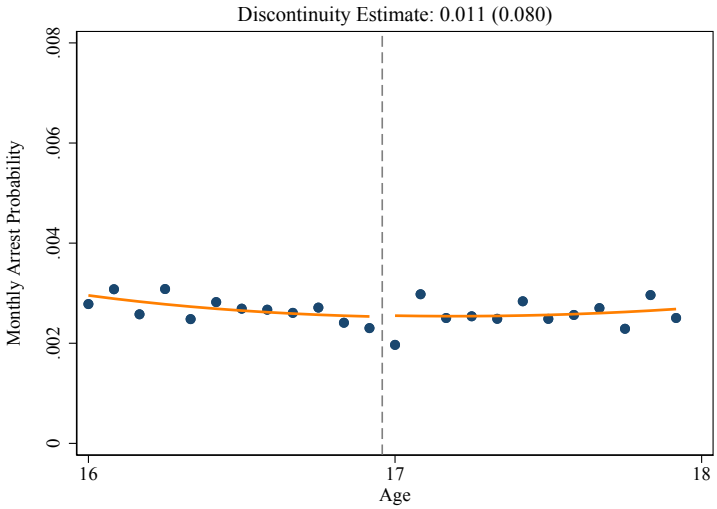
(b) Ever incarcerated



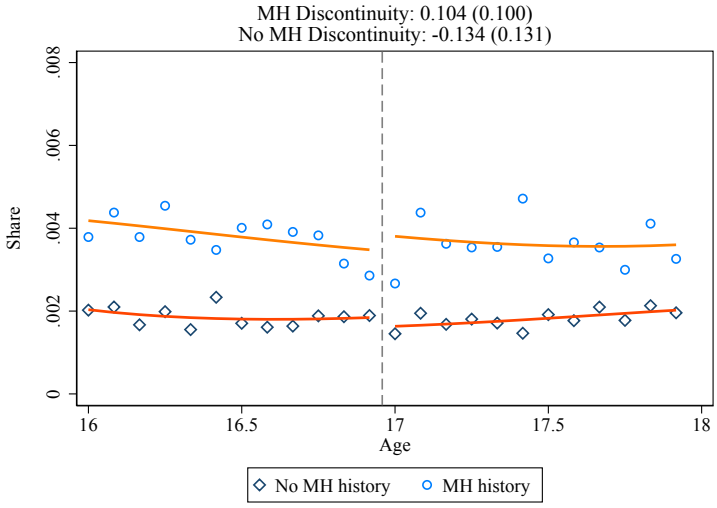
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who were recently using mental health services (in purple) and for men who were less recently using mental health services (in gray) using estimates from equation (1.2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. Recent usage is defined as filing a mental health claim during the study pre-period. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 1.8: Felony Propensity Estimates Around the Age of Criminal Majority

(a) Full Sample



(b) By Mental Health History



NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The bottom panel splits the sample based on an individual's mental health history prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (1.6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Table 1.1: Summary Statistics of Full and Matched Sample

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

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

Table 1.2: Effect of Medicaid Loss on Men's Likelihood of Incarceration

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. For the full sample, the estimated effects in columns 1 and 2 correspond to the post-period average of the β_τ coefficients and to the β_8 coefficient, respectively, estimated using equation (1.1). For the mental health sample, the estimated effects in columns 3 and 4 correspond to the post-period average of the α_τ coefficients and to the α_8 coefficient, respectively, estimated using equation (1.2). Standard errors are clustered at the individual level. For the non-cumulative variables, "Post-period Control Average" refers to the post-period average for the matched control group (for the full sample in column 1 and just for those with a mental health history in column 3). For the cumulative variables, this statistic reports the value measured in the last quarter of the post period (age 20, quarter 4) for the full sample in column 1 and just for those with a mental health history in column 3.

Table 1.3: 2SLS Effect of Medicaid Enrollment on Men's Likelihood of Incarceration

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports the two-stage least squares coefficient using equation (1.3), in which Medicaid enrollment is instrumented with a $\text{Treat}_i \times \text{Post}_\tau$ indicator variable. The first column considers the full matched sample, whereas the second column focuses on individuals with mental health histories prior to age 16. "Post-period Average" refers to the average post-period incarceration rate of the full sample in column 1 and to the average post-period incarceration rate of men with a mental health history in column 2. Standard errors are clustered at the individual level.

Table 1.4: Effect of Medicaid Loss on Men's Likelihood of Ever Being Incarcerated, by Mental Health History & Offense Type

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. The estimated effects correspond to the α_8 coefficient in equation (1.2), analogous to the estimates presented in Figured 1.6. "Misc." refers to miscellaneous offenses. "Post-Period Avg." refers to the average incarceration rate of men with mental health histories in the matched comparison group, measured in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level.

Table 1.5: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table only considers treated men with mental health histories (and their corresponding matched control units) and splits the group based on treated men's behavioral healtchcare utilization in the study pre-period. The estimated effect in column 1 corresponds to the post-period average of the α_τ coefficients using equation (1.2). The estimated effects in columns 2–6 correspond to the α_8 coefficient using equation (1.2). "Misc." refers to miscellaneous offenses. For column 1, the post-period average reports the average incarceration rate in the post-period for treated men with a mental health history who did not use behavioral health services in the pre-period. For the cumulative variables, this statistic reports the value for this same group measured in the last quarter of the post period (age 20, quarter 4). Standard errors are clustered at the individual level.

Table 1.6: Effects of Medicaid Enrollment on Public Safety

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table considers the full sample and measures the effect of the loss in Medicaid eligibility on serious crime, as measured by arrests in the South Carolina Law Enforcement Division data that end with an individual being taken into custody. This table reports the Treat × Post coefficient from equation (1.5). "Control Average" refers to the post-period average of the control group. "Scaled Effect" is the quotient of the reduced-form estimate and the control group's post-period average. "Non-violent" refers to all drug-related, property, and miscellaneous offenses. Standard errors are clustered at the individual level.

Table 1.7: Summary of Costs: Providing Medicaid & Reduced Criminal Activity (In Millions of $2010)

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

NOTE: This table reports the estimates from the cost-benefit analysis in Section 1.8. "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 1.8: Summary of (Most Negative) Estimated Elasticities of Crime to Sentence Lengths

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

NOTE: This table reports the elasticities computed in Section 1.9, using the most negative deterrence effect consistent with the confidence intervals. The first two columns use the full sample ($N = 72,939$), whereas the third and fourth columns focus on the three youngest birth cohorts ($N = 23,194$). Columns 1 and 3 use the average length of prison sentences (4.5 years) to calculate the elasticity. Columns 2 and 4 use the average time served in state prison (2.3 years) to estimate the elasticity. For a full derivation of these estimates, I refer the reader to Appendix D.

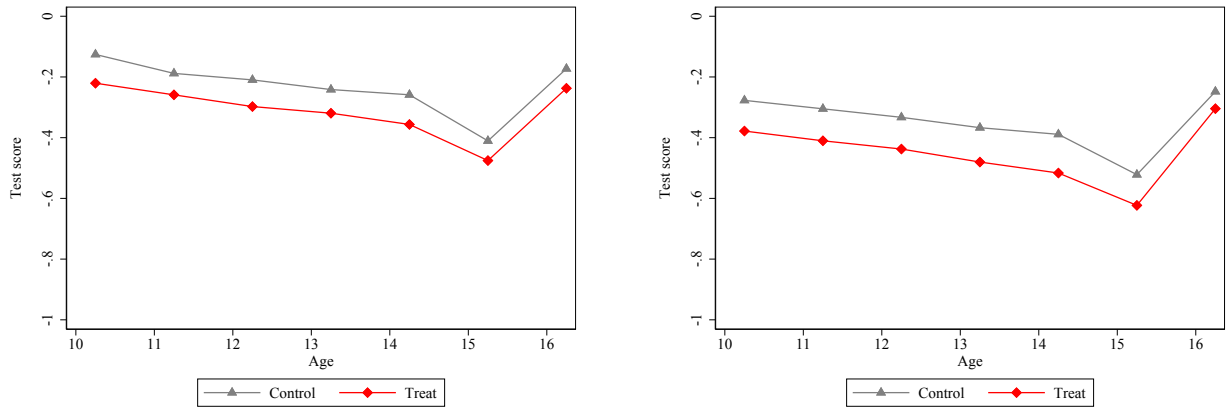
Appendices

A Appendix Figures and Tables

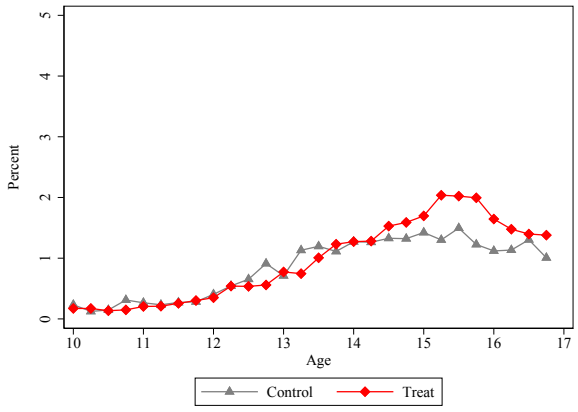
Figure 1A.1: Standardized Test Scores and Juvenile Justice Felony Referrals (Raw Means) For Full Sample

(a) Math score

(b) ELA score



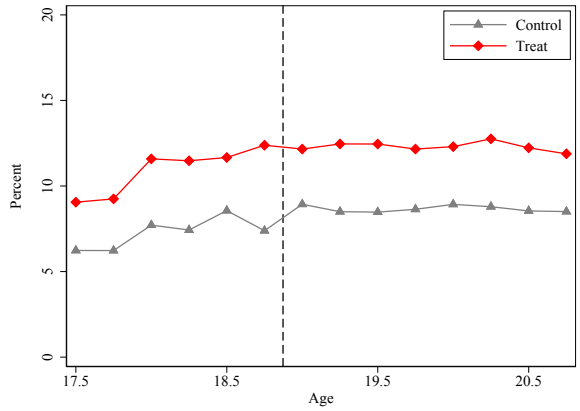
(c) DJJ referral for felony offense



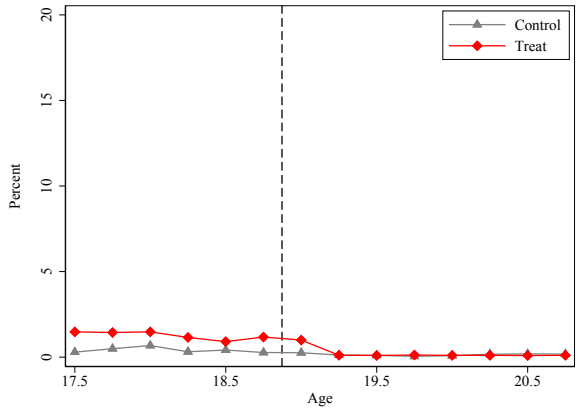
NOTE: These graphs plot the raw means of each outcome in a given year (or quarter in the third panel) for the full sample of matched treated and control men. Data on test scores comes from South Carolina's Department of Education. "ELA score" refers to test scores on English Language Arts standardized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

Figure 1A.2: Enrollment in Public Assistance, Full Sample (Raw Means)

(a) SNAP enrollment



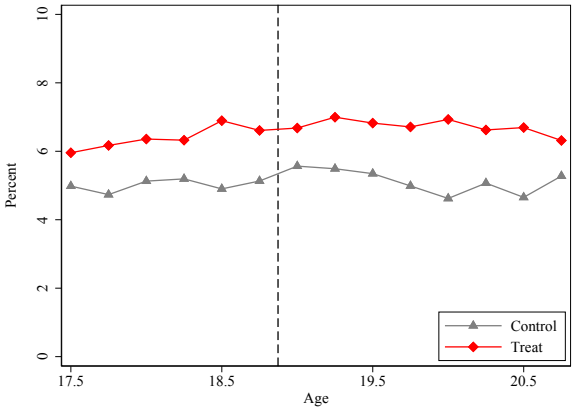
(b) TANF enrollment



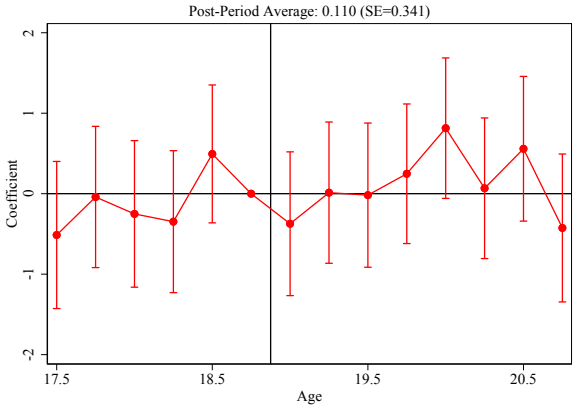
NOTE: These graphs plot the raw means of each outcome in a given quarter for the full sample of matched treated and control men. Data on program enrollment comes from South Carolina's Department Social Services. "SNAP" refers to the Supplemental Nutrition Assistance Program and "TANF" refers to the Temporary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

Figure 1A.3: Criminal Activity For Full Sample (Raw Means & Treat-Control Differences)

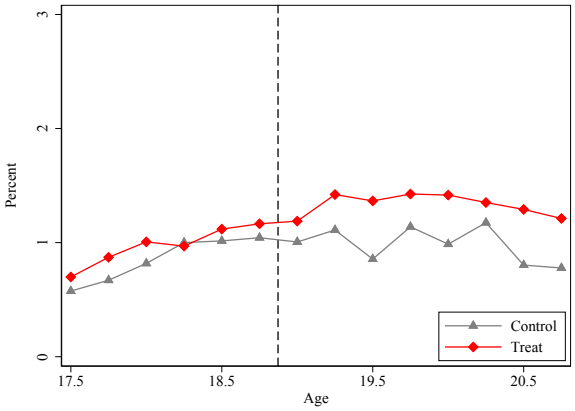
(a) All Arrests



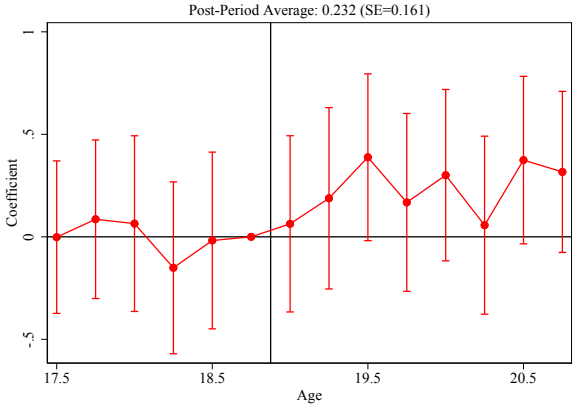
(b) All Arrests



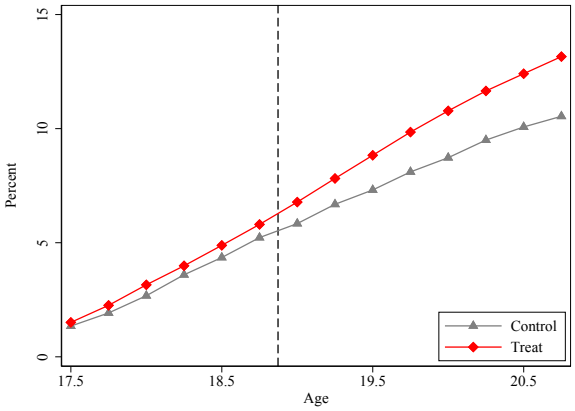
(c) Custody Arrests



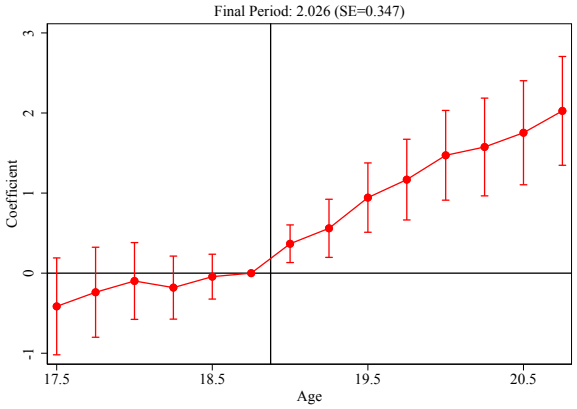
(d) Custody Arrests



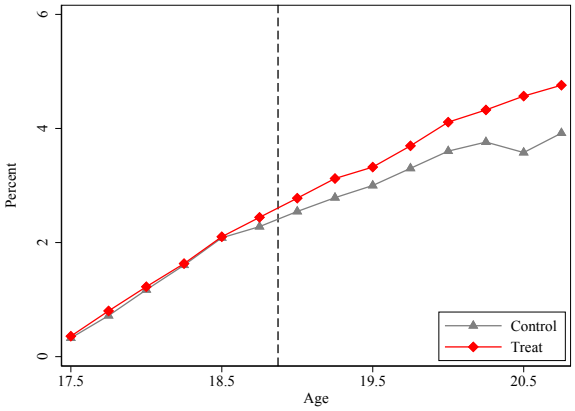
(e) Ever Taken into Custody



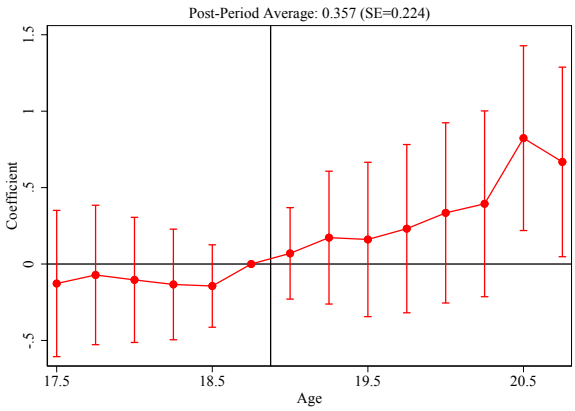
(f) Ever Taken into Custody



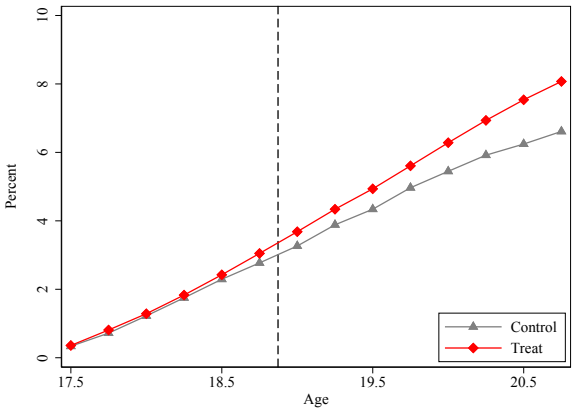
(g) Incarcerated in State Prison



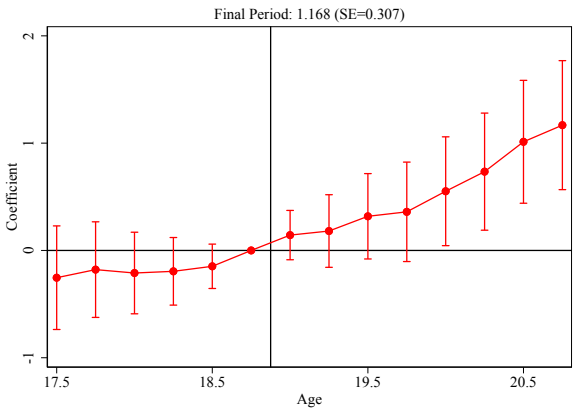
(h) Incarcerated in State Prison



(i) Ever Incarcerated, State Prison



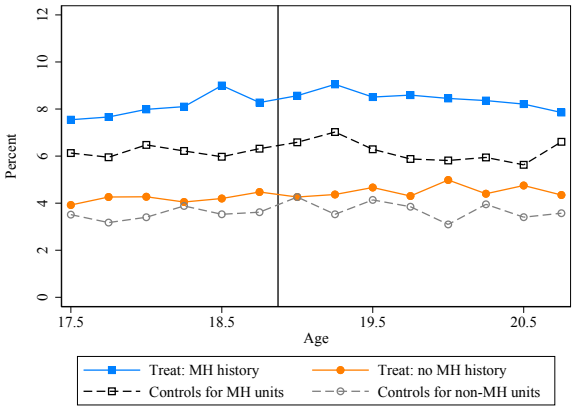
(j) Ever Incarcerated, State Prison



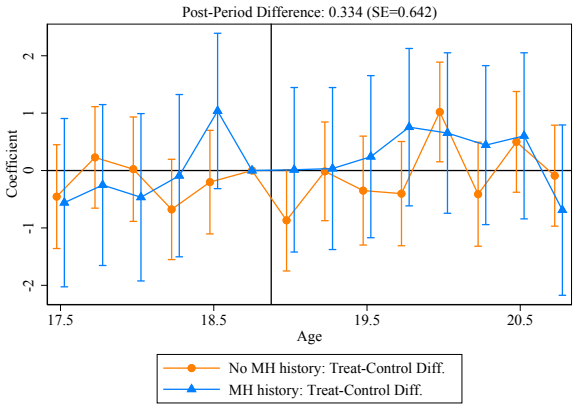
NOTE: Data on arrests and incarcerations come from the Law Enforcement Division and the Department of Corrections, respectively. The graphs on the left plot the raw means of each criminal outcome in a given quarter, and the graphs on the right plot the corresponding treat-control differences using equation (1.1) for the full sample. "Post-Period Average" and "Final Period" refer to the post-period average of the β_τ coefficients or to β_8, respectively. "Custody arrests" refers to arrests in which an individual was arrested and taken into custody. Standard errors are clustered at the individual level.

Figure 1A.4: Criminal Activity, by Mental Health History (Raw Means & Treat-Control Differences)

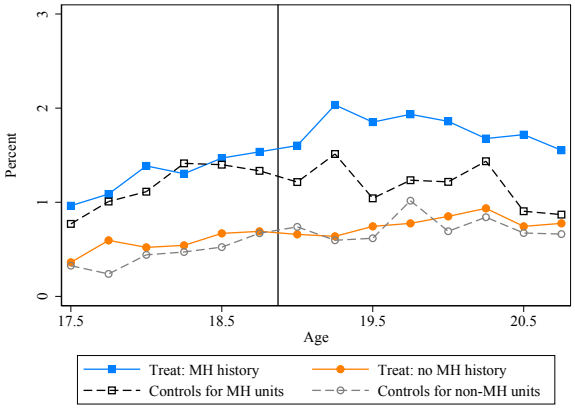
(a) All Arrests



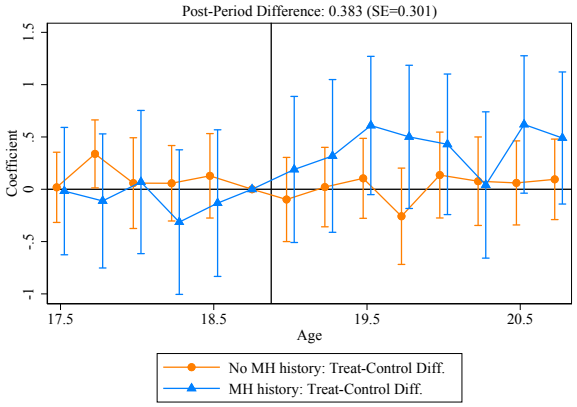
(b) All Arrests



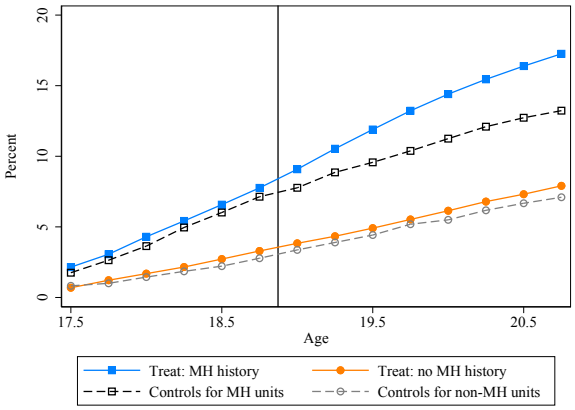
(c) Custody Arrests



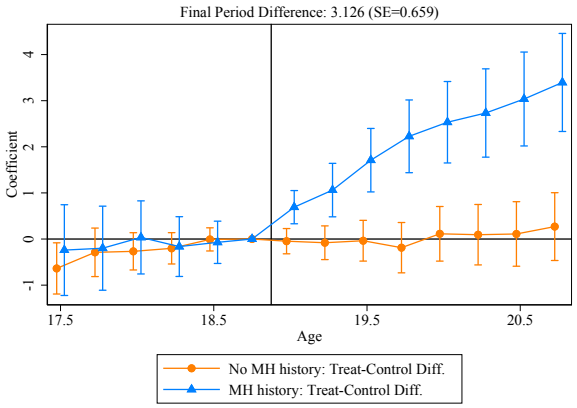
(d) Custody Arrests



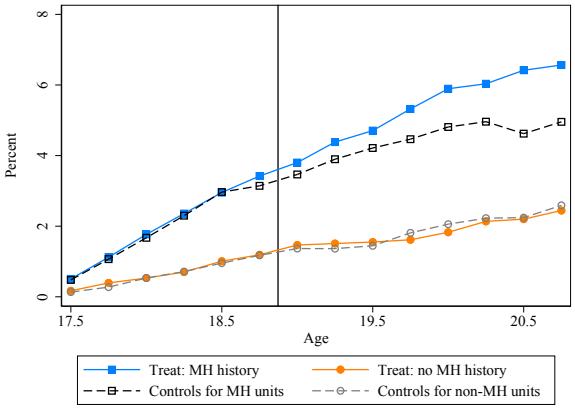
(e) Ever Taken into Custody



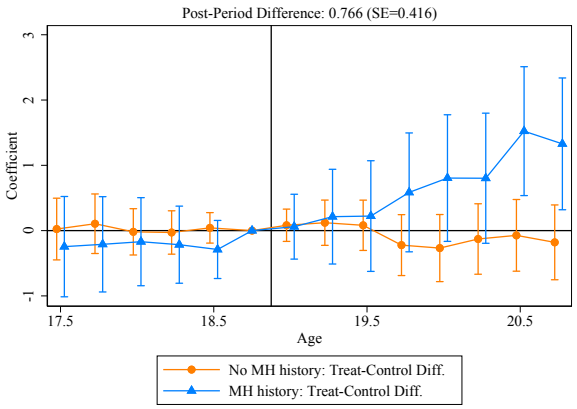
(f) Ever Taken into Custody



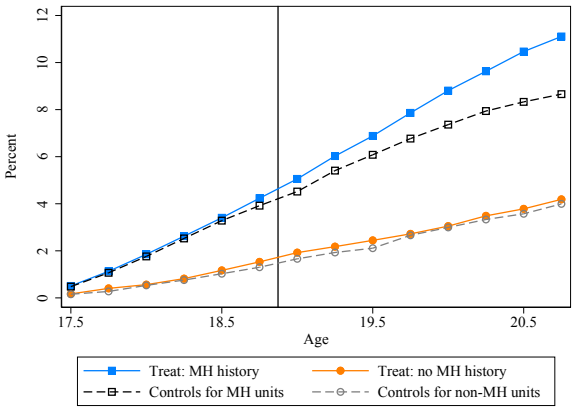
(g) Incarcerated in State Prison



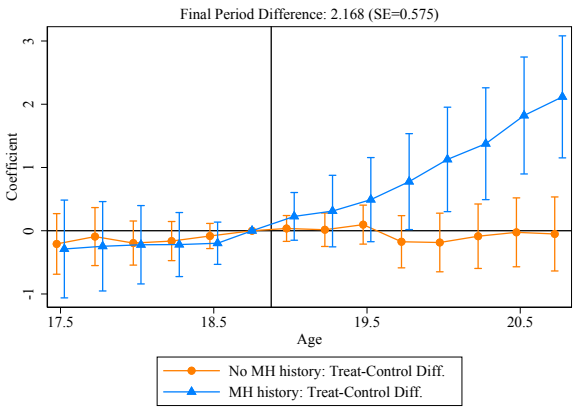
(h) Incarcerated in State Prison



(i) Ever Incarcerated, State Prison



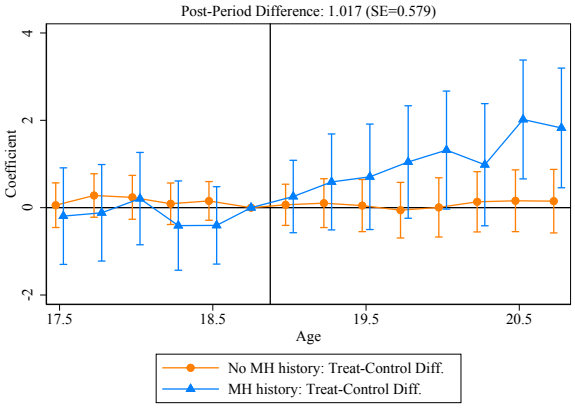
(j) Ever Incarcerated, State Prison



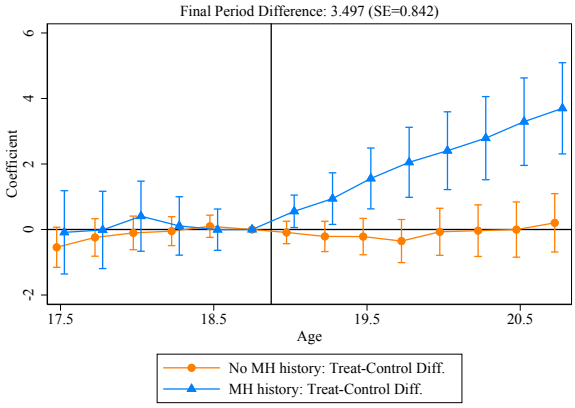
NOTE: Data on arrests and incarcerations come from the Law Enforcement Division and the Department of Corrections, respectively. The graphs on the left plot the raw means of each criminal outcome in a given quarter, and the graphs on the right plot the corresponding treat-control differences using equation (1.2). "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Custody arrests" refers to arrests in which an individual was arrested and taken into custody. Standard errors are clustered at the individual level.

Figure 1A.5: Likelihood of Incarceration, by Mental Health History and Race

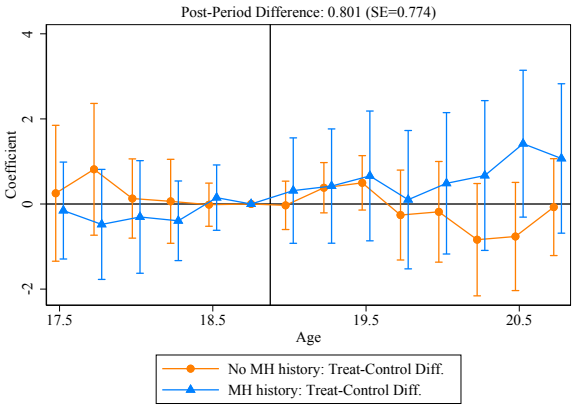
(a) Black: Incarcerated that quarter



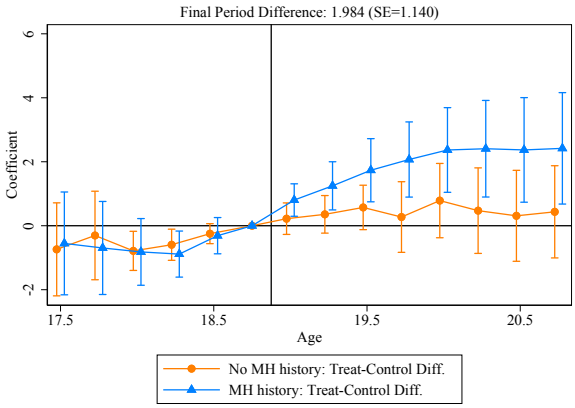
(b) Black: Ever incarcerated



(c) White: Incarcerated that quarter



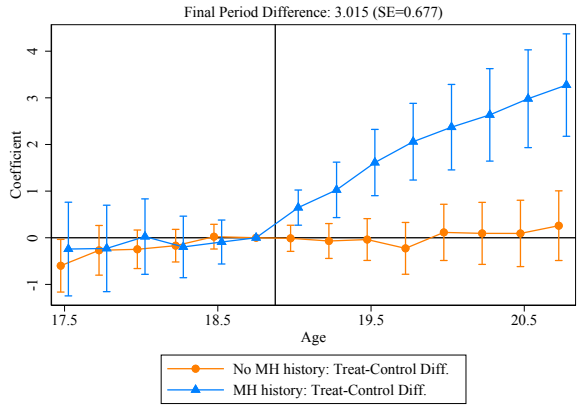
(d) White: Ever incarcerated



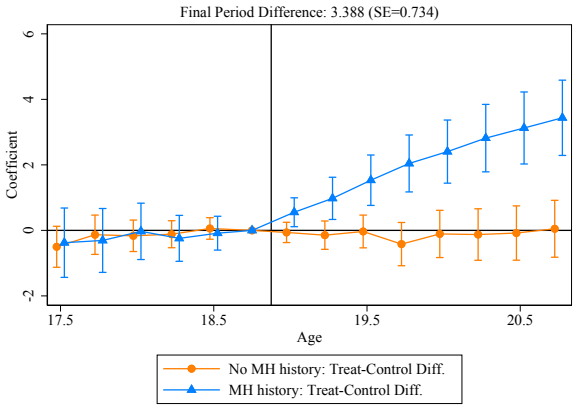
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (c) plot the treat-control differences of the likelihood of being incarcerated that quarter. Panels (b) and (d) plot the treat-control differences in the cumulative likelihood of having ever been incarcerated. "Post-Period Average" and "Final Period" refer to the post-period average of the α_τ coefficients or to α_8, respectively, using equation (1.2). "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 1A.6: Robustness of Main Result to Sample and Matching Choices (Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)

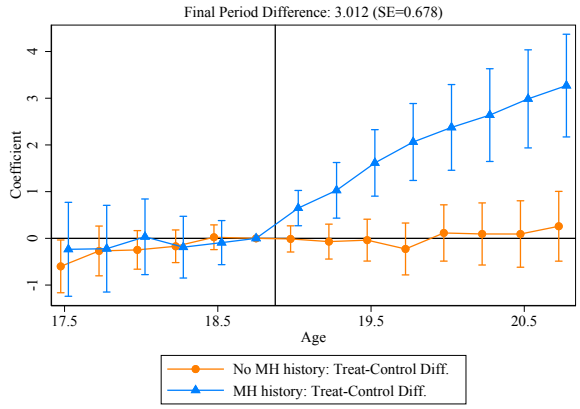
(a) Baseline



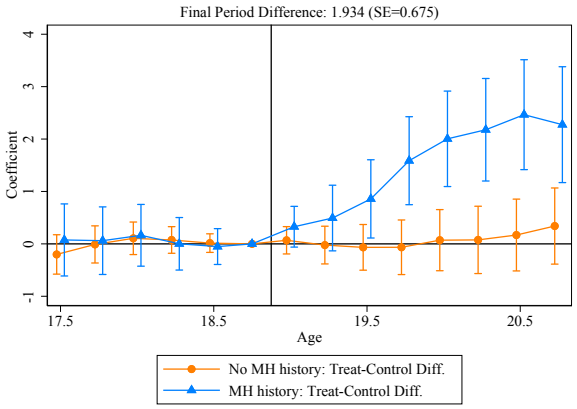
(b) Only one match per treated



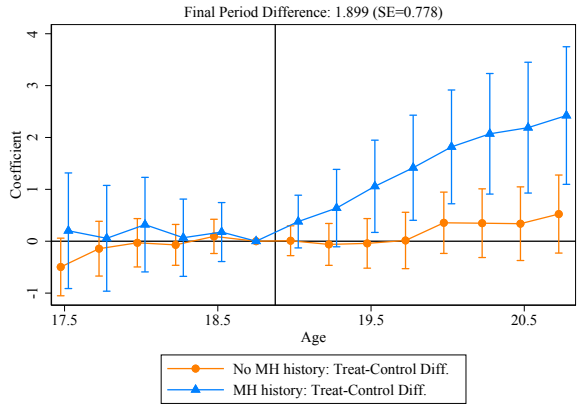
(c) Down-weight outliers



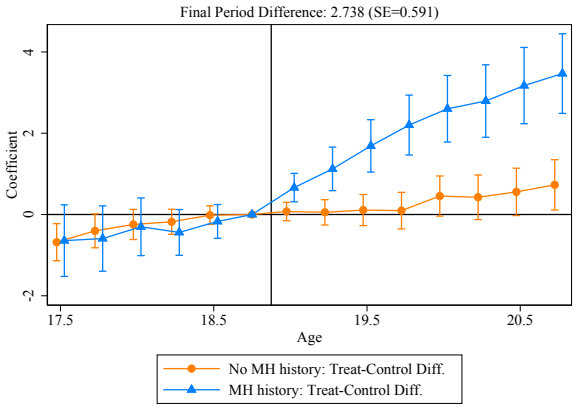
(d) Pre-period assignment



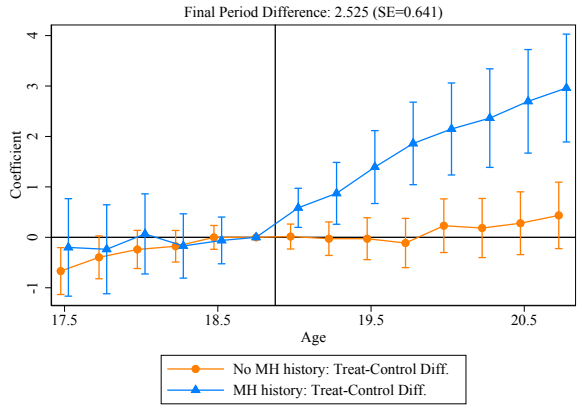
(e) Match on school



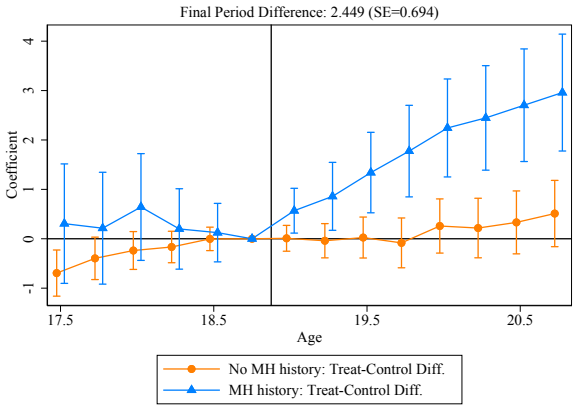
(f) No matching



(g) DFL: same matching characteristics



(h) DFL: matching on school

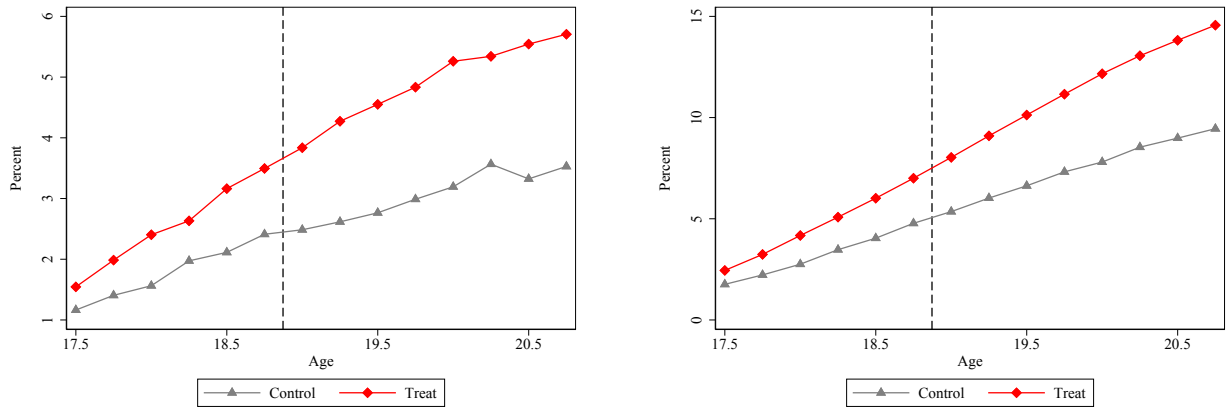


NOTE: The dependent variable is a measure of whether an individual was ever arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panel (a) plots the baseline result from Figure 1.5. Panel (b) randomly assigns one control individual to each treated individual. Panel (c) drops control observations with disproportionate weight in the regression. Panel (d) assigns individuals to the treatment and control period based on their Medicaid enrollment in the study pre-period. Panel (e) matches treatment and control men using school attended instead of district. Panel (f) runs the specification for all eligible treated and control individuals (i.e., with no matching). Panels (g) and (h) use the DFL re-weighting approach (instead of the coarsened matching procedure) to re-weight the eligible control individuals. Panel (g) re-weights men based on race, year of birth, school district, and mental health history, and panel (h) replaces district with school. "Final Period" refers to the α_8 coefficient using equation (1.2). Standard errors are clustered at the individual level.

Figure 1A.7: Raw Means for All Eligible Treat & Control Individuals (No Matching)

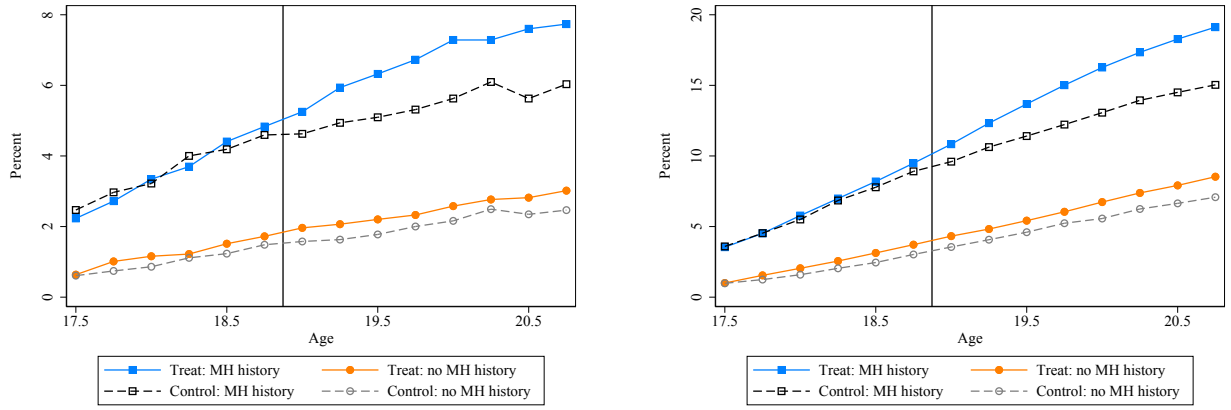
(a) Incarcerated that quarter

(b) Ever incarcerated



(c) Incarcerated that quarter

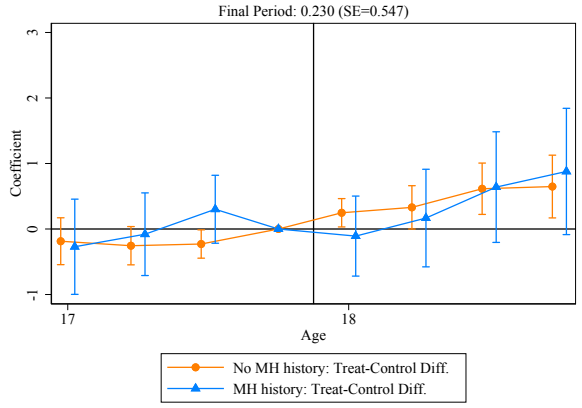
(d) Ever incarcerated



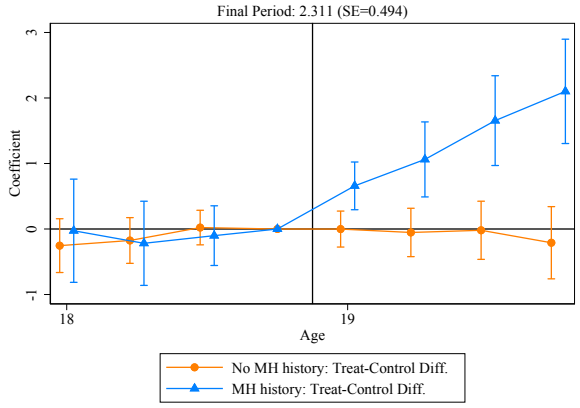
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panels (a) and (b) plots the raw means for all treatment and control individuals, and panels (c) and (d) separate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

Figure 1A.8: Placebo Check: Moving Treatment Age to 18 (Measuring the Likelihood of Ever Being Incarcerated by Mental Health History)

(a) Age 18



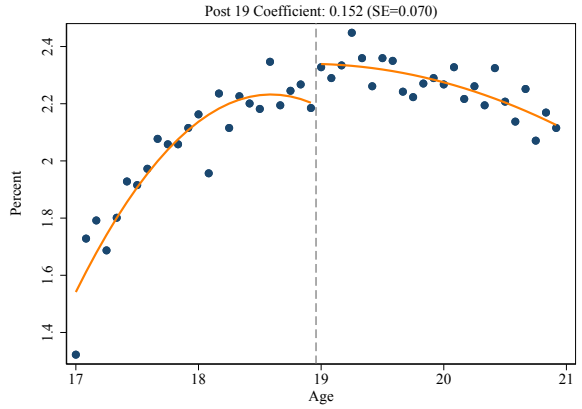
(b) Age 19



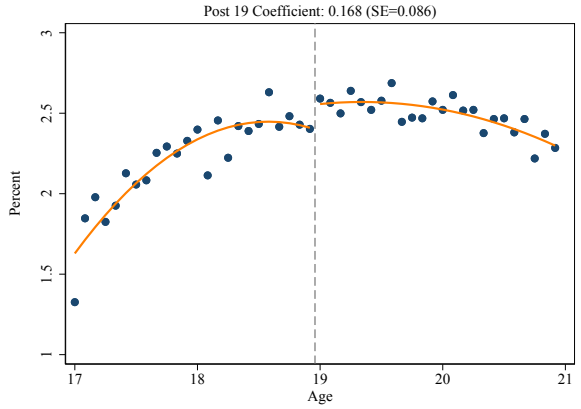
NOTE: The dependent variable is a measure of whether an individual was ever arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. Panel (a) presents the results from the placebo check (i.e., measuring the likelihood of incarceration around age 18). Panel (b) presents the main results from Figure 1.5 zoomed in around ages 18 and 19. "Final Period" refers to the α_8 coefficient using equation (1.2). Standard errors are clustered at the individual level.

Figure 1A.9: Regression Discontinuity: Arrest Probability Around Age 19

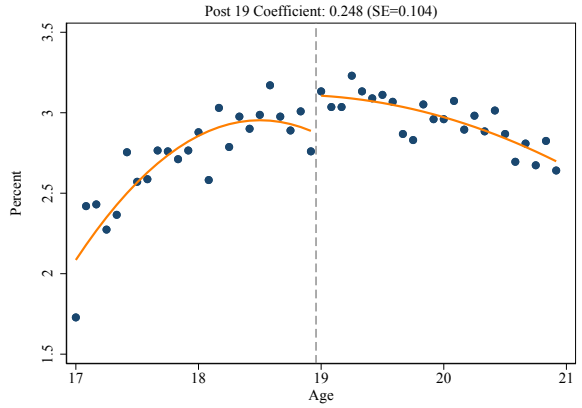
(a) Enrolled in Medicaid, Ages 10–18



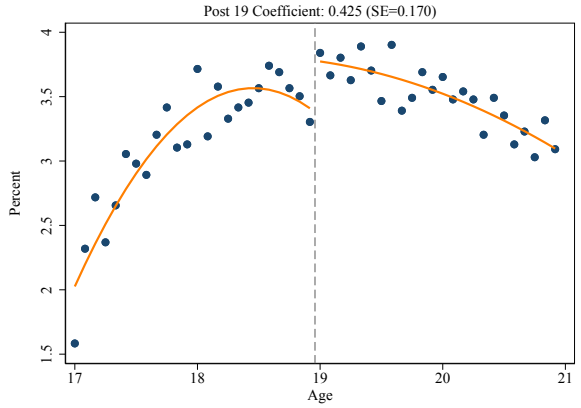
(b) Enrolled in Medicaid, Ages 17–18



(c) Mental Health History, Ages 10–18



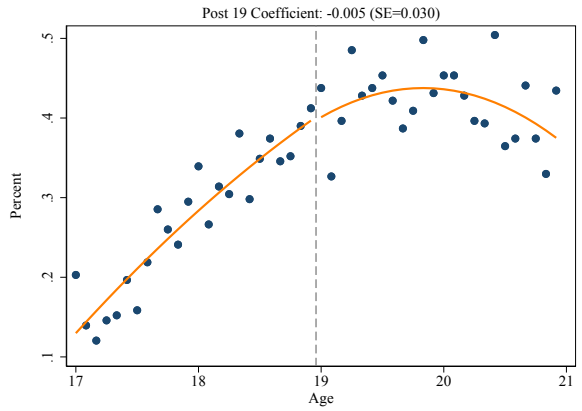
(d) Using Behavioral Health Services, Ages 17–18



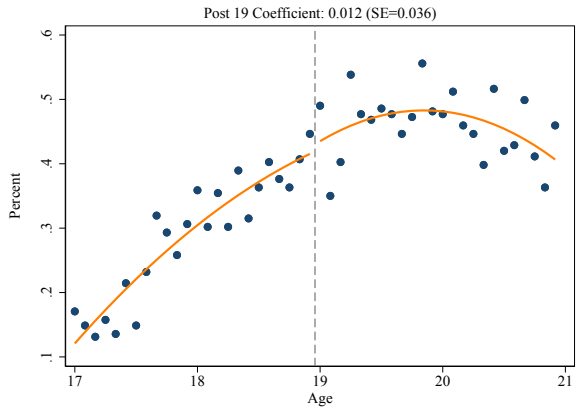
NOTE: This figure plots men's probability of being arrested for each month around their 19th birthday, using arrest data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (1.4). The estimate reported above each figure reports the discontinuity estimate from this equation and its standard errors (clustering at the individual level). Panels (a) and (b) consider all men enrolled in Medicaid between ages 10–18 and 17–18, respectively. Panel (c) considers all individuals with a Medicaid mental health claim between ages 10 and 18 (inclusive), whereas panel (d) only considers individuals with a mental health claim right before the loss in eligibility (i.e., at ages 17 or 18).

Figure 1A.10: Regression Discontinuity: Probability of Being Taken into Custody Around Age 19

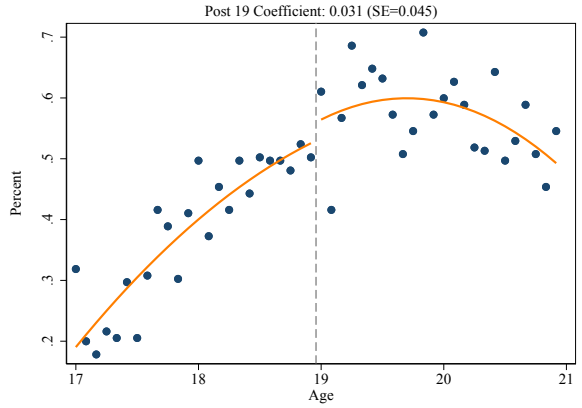
(a) Enrolled in Medicaid, Ages 10–18



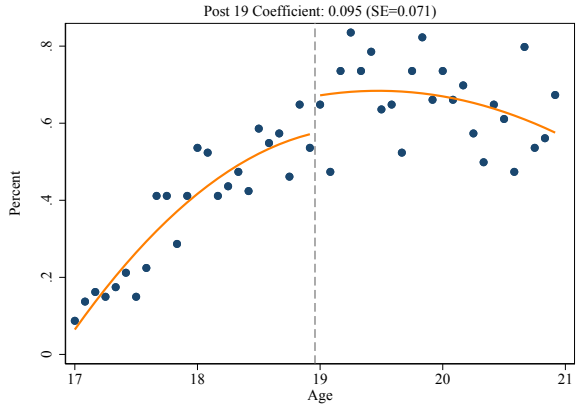
(b) Enrolled in Medicaid, Ages 17–18



(c) Mental Health History, Ages 10–18



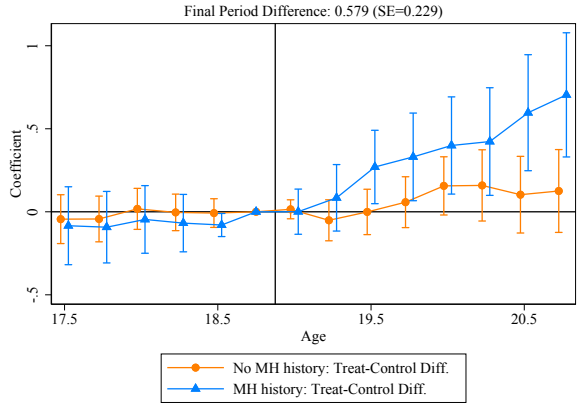
(d) Using Behavioral Health Services, Ages 17–18



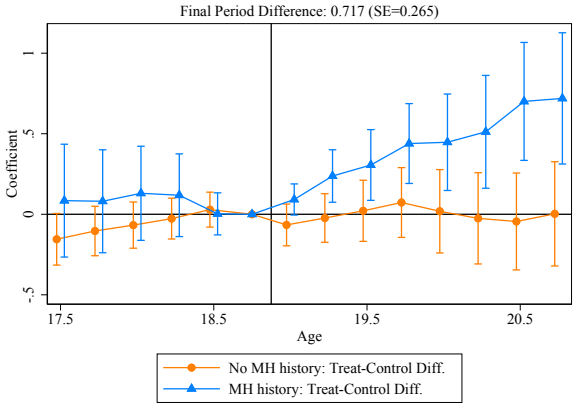
NOTE: This figure plots men's probability of being arrested and taken into custody for each month around their 19th birthday, using arrest data from the South Carolina Law Enforcement Division. The circles represent the share of individuals arrested in that month. The solid line represents the estimate derived using equation (1.4). The estimate reported above each figure reports the discontinuity estimate from this equation and its standard errors (clustering at the individual level). Panels (a) and (b) consider all men enrolled in Medicaid between ages 10–18 and 17–18, respectively. Panel (c) considers all individuals with a Medicaid mental health claim between ages 10 and 18 (inclusive), whereas panel (d) only considers individuals with a mental health claim right before the loss in eligibility (i.e., at ages 17 or 18).

Figure 1A.11: Likelihood of Ever Being Incarcerated for Drug-Related Offense, by Mental Health History and Offense Type

(a) Possession-Related Charge



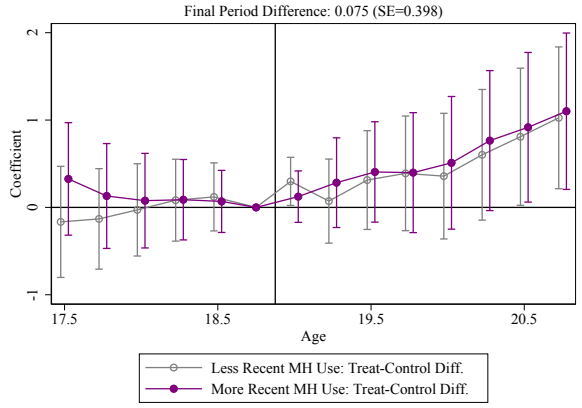
(b) Distribution-Related Charge



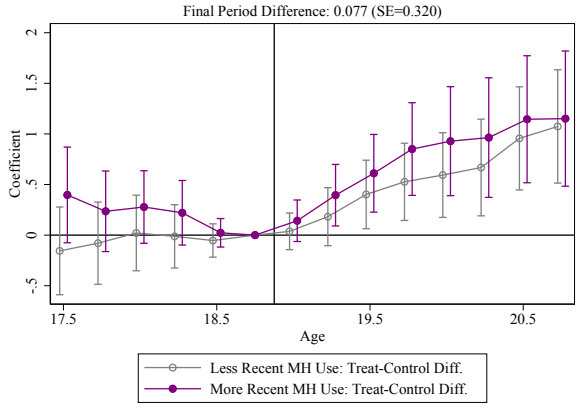
NOTE: The dependent variable is a measure of whether an individual has ever been arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data for a drug-related offense. These figures plot the treat-control differences estimated using equation (1.2). "Final Period" refers to α_8 in that equation. Standard errors are clustered at the individual level.

Figure 1A.12: Likelihood of Incarceration for Men with a Mental Health History, by Recency of Behavioral Healthcare Utilization & Offense Type

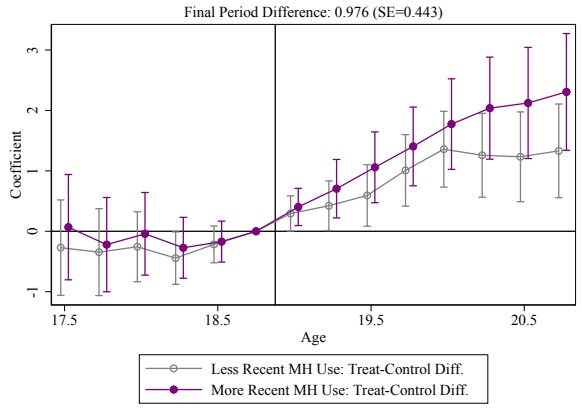
(a) Violent Offenses



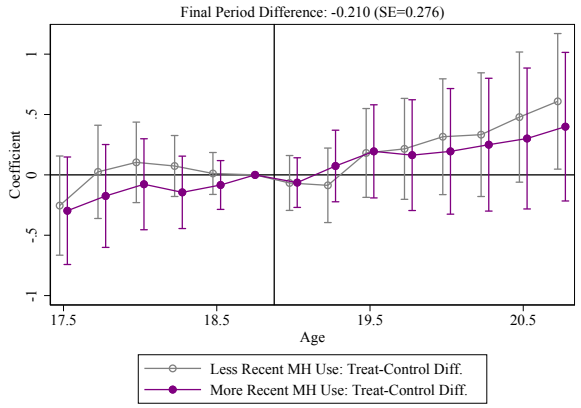
(b) Drug-Related Offenses



(c) Property Offenses



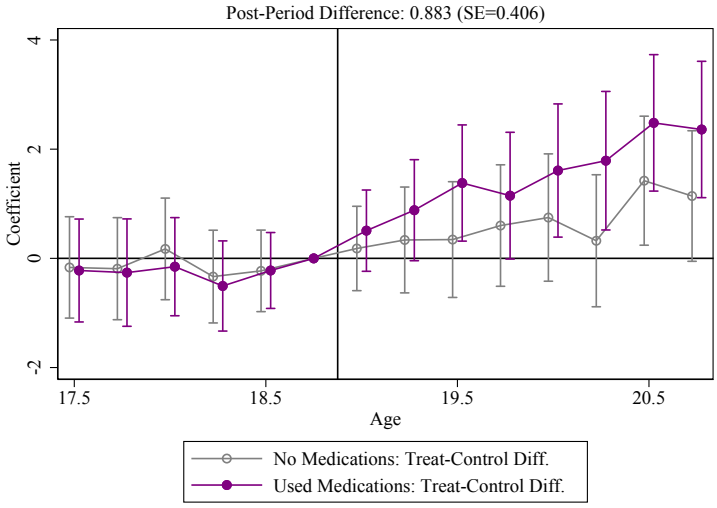
(d) Miscellaneous Offenses



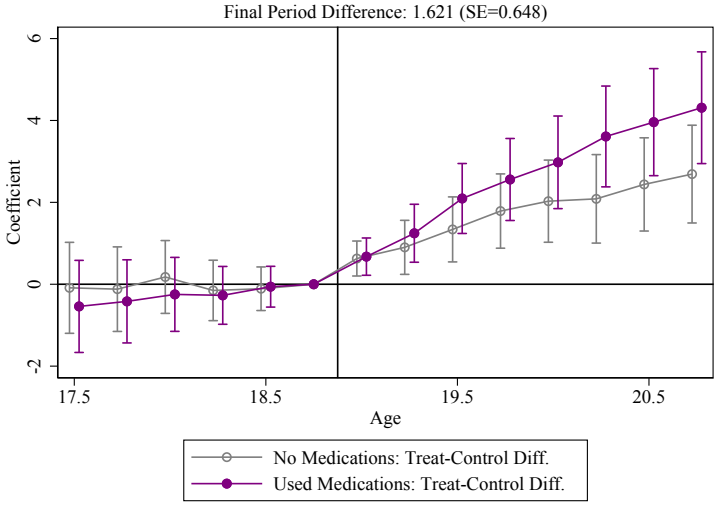
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who were recently using mental health services (in purple) and for men who were less recently using mental health services (in gray) using estimates from equation (1.2). "Final Period Difference" refers to the α_8 coefficient using that equation. Recent usage is defined as filing a mental health claim during the study pre-period. Standard errors are clustered at the individual level.

Figure 1A.13: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage

(a) Incarcerated that quarter



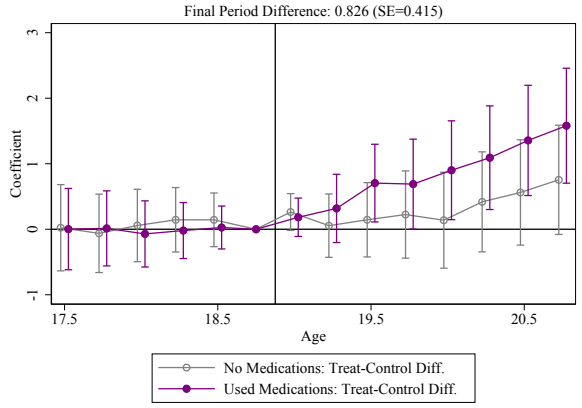
(b) Ever incarcerated



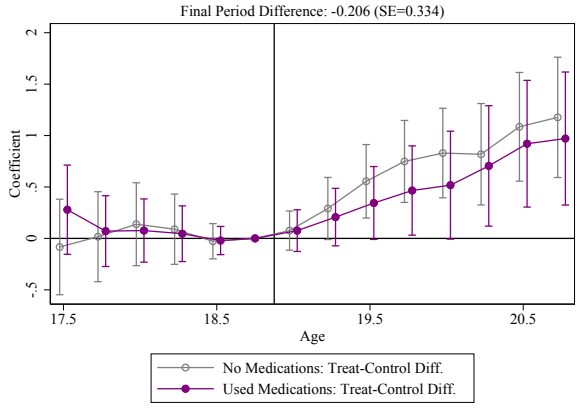
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who filed claims for psychotropic drugs between ages 14–18 (in purple) and for men who did not file claims for medications during these ages (in gray) using estimates from equation (1.2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before 7.4. Standard errors are clustered at the individual level.

Figure 1A.14: Likelihood of Incarceration for Men with a Mental Health History, by Mental Health Medication Usage & Offense Type

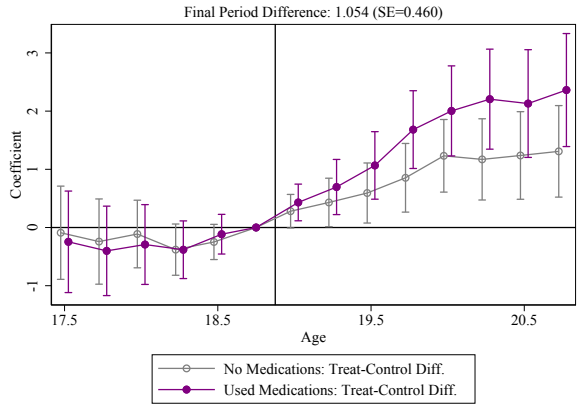
(a) Violent Offenses



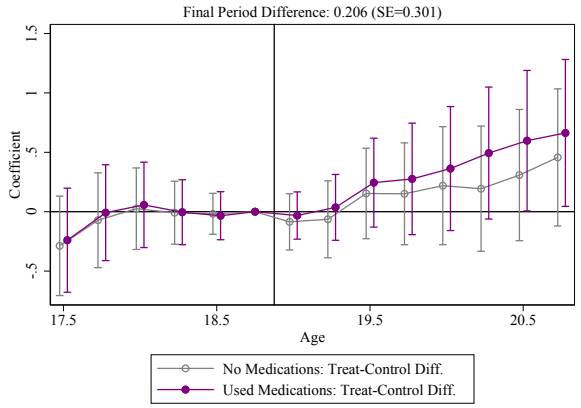
(b) Drug-Related Offenses



(c) Property Offenses



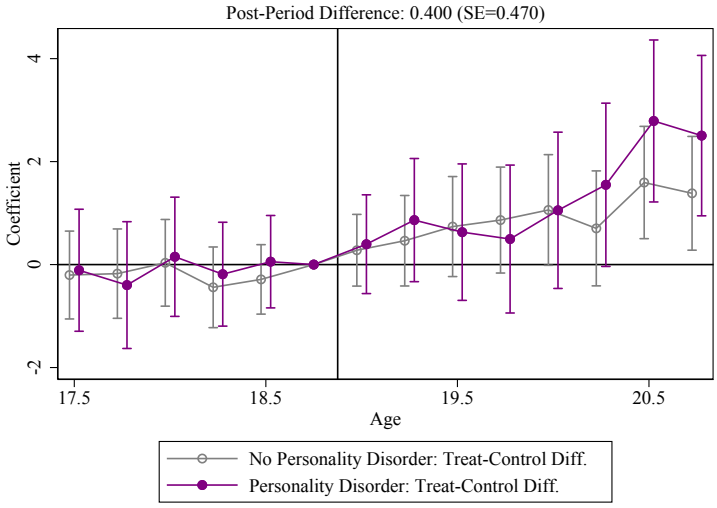
(d) Miscellaneous Offenses



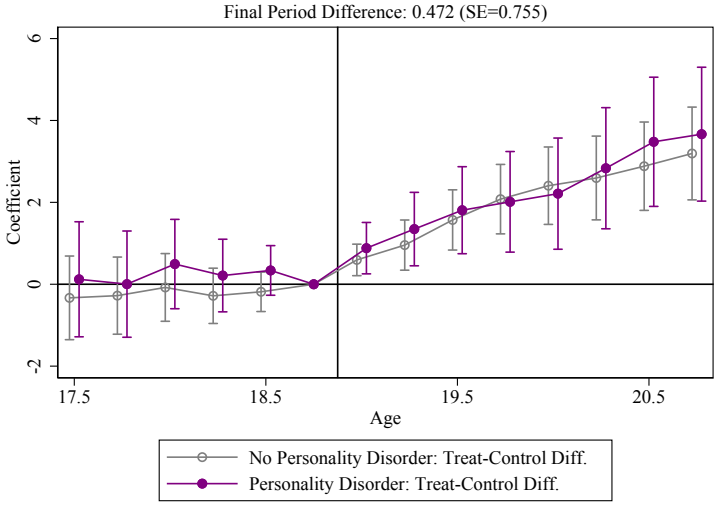
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men who filed claims for psychotropic drugs between ages 14–18 (in purple) and for men who did not file claims for medications during these ages (in gray) using estimates from equation (1.2). "Final Period Difference" refers to the α_8 coefficient using that equation. Standard errors are clustered at the individual level.

Figure 1A.15: Likelihood of Incarceration for Men with a Mental Health History, by Diagnosis

(a) Incarcerated that quarter



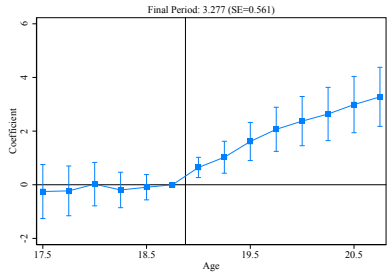
(b) Ever incarcerated



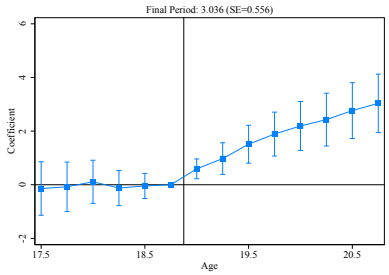
NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men ever diagnosed with a personality disorder (in purple) and for men without a personality disorder (i.e., those with a clinical disorder only, in gray) using estimates from equation (1.2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter. "Ever incarcerated" refers to having been detained in a correctional facility at least once before. Standard errors are clustered at the individual level.

Figure 1A.16: Likelihood of Incarceration for Men with a Mental Health History, Excluding One Diagnosis At a Time

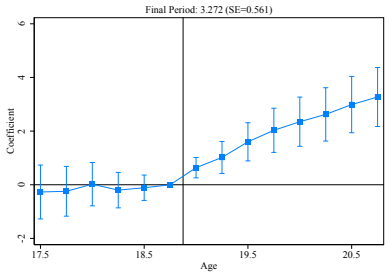
(a) Baseline



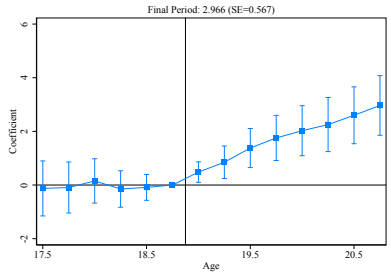
(b) Org. psychotic conditions



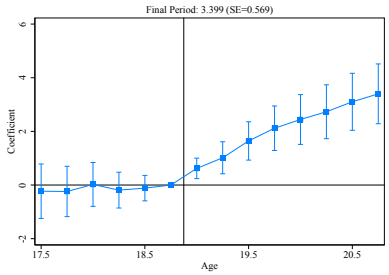
(c) Schizophrenic disorders



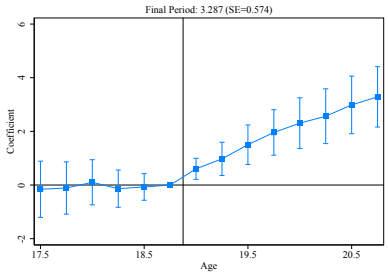
(d) Episodic mood disorders



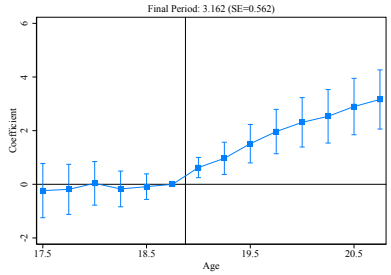
(e) Other psychoses disorders



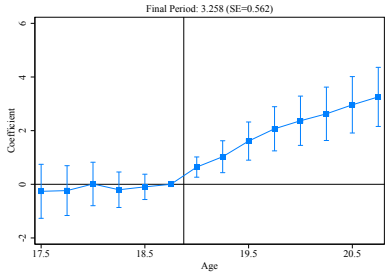
(f) Neurotic disorders



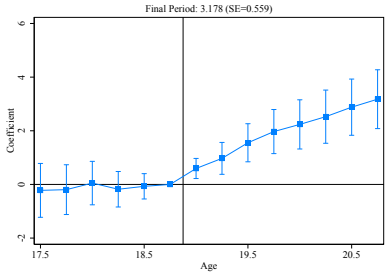
(g) Personality disorders



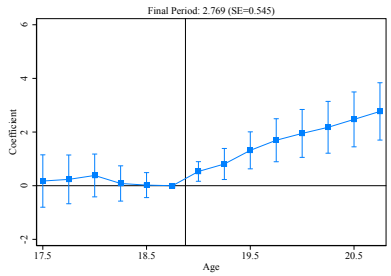
(h) Sexual disorders



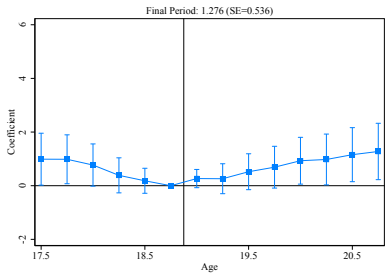
(i) Alcohol dependence syndrome



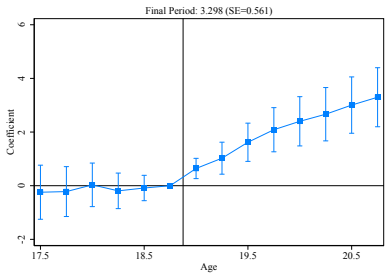
(j) Drug dependence



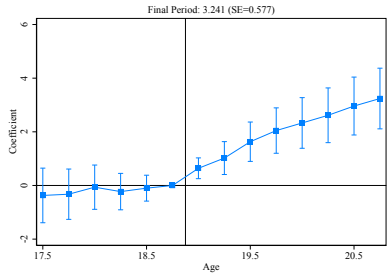
(k) Non-dependent drug abuse



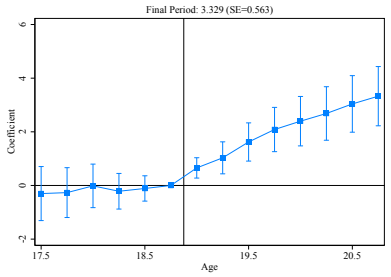
(l) Physiological malfunction



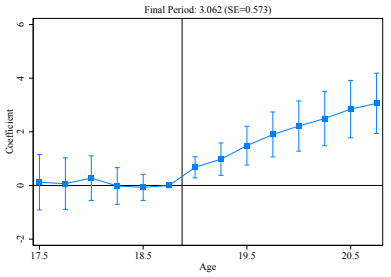
(m) Special syndromes



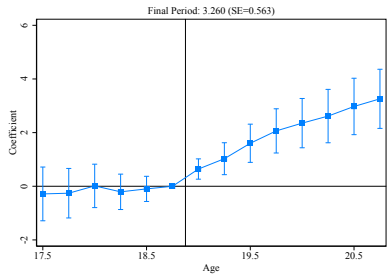
(n) Acute reaction to stress



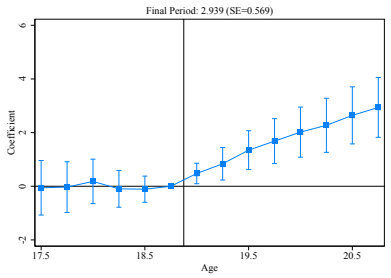
(o) Adjustment reaction



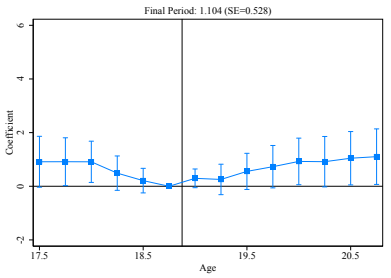
(p) Specific disorders



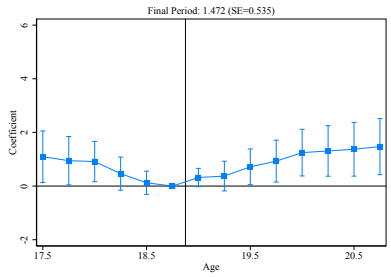
(q) Depressive disorder



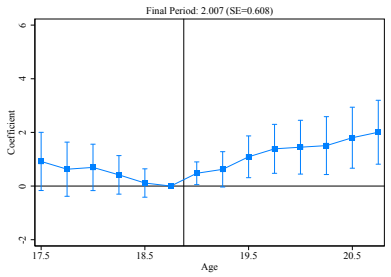
(r) Disturbance of conduct



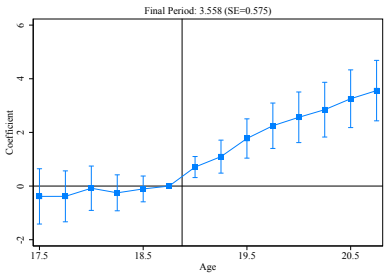
(s) Disturbance of emotions



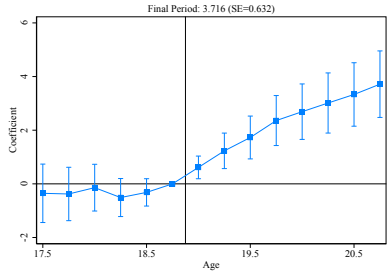
(t) Hyperkinetic syndrome



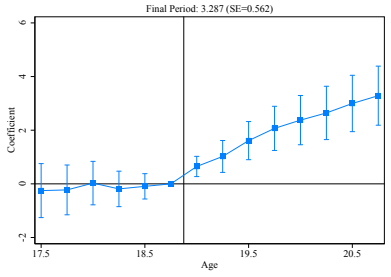
(u) Hyperkinetic syndrome only



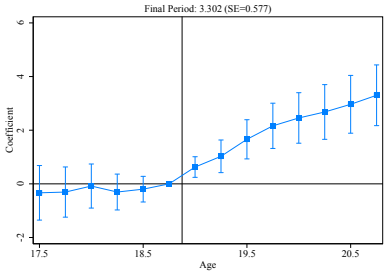
(v) Developmental delay



(w) Other psychic factors



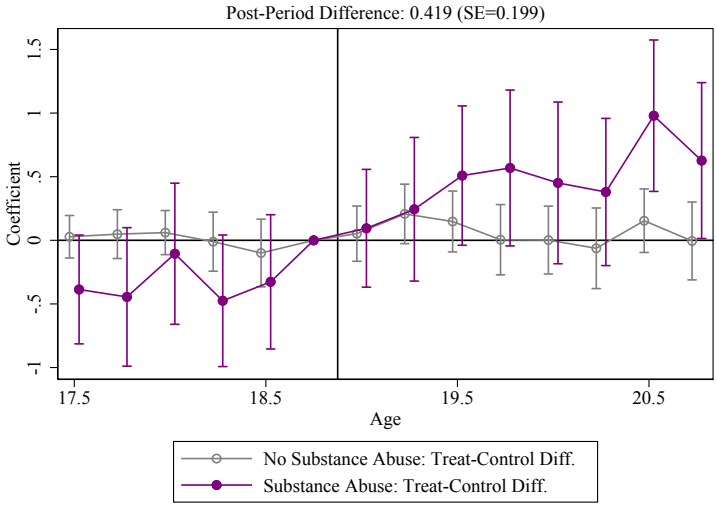
(x) Intellectual disabilities



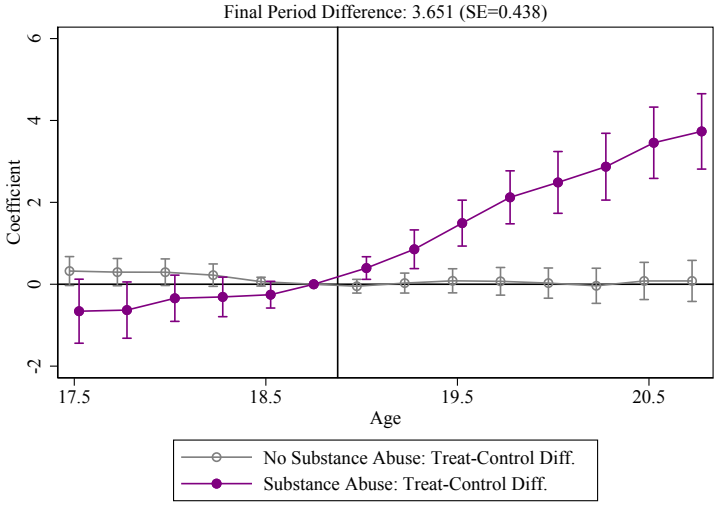
NOTE: The figures reproduce the main estimates from Figure 1.5, plotting the treat-control differences for men with a mental health history after dropping men ever diagnosed with a specific ICD-9 code. "Final Period" refers to α_8 in equation (1.2). Standard errors are clustered at the individual level. Panel (u) drops men whose sole diagnosis is hyperkinetic syndromes.

Figure 1A.17: Likelihood of Drug-Related Incarceration for Men with a Mental Health History, by Substance Abuse Diagnosis

(a) Incarcerated that quarter

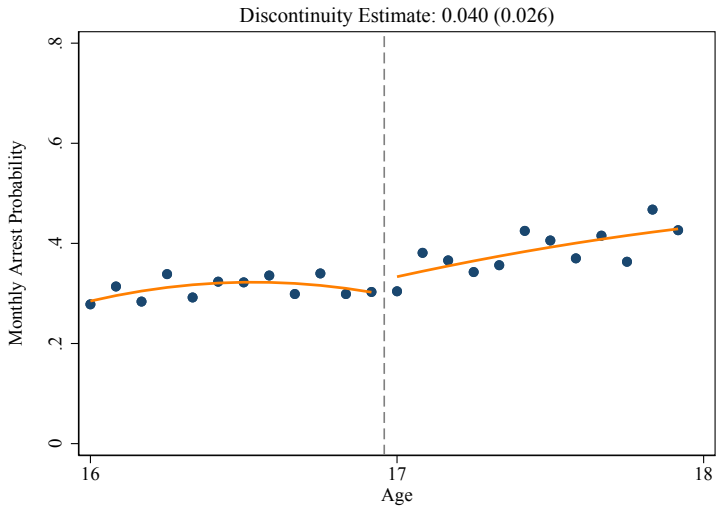


(b) Ever incarcerated



NOTE: The dependent variable is a measure of whether an individual was arrested and taken into custody in the Law Enforcement Division data or incarcerated in the Department of Corrections data for a drug-related offense. These figures only consider treated men with mental health histories and their corresponding matched control units. The figures plot the treat-control differences for treated men ever diagnosed with a substance abuse disorder (in purple) and for men without a substance abuse disorder(in gray) using estimates from equation (1.2). "Post-Period Difference" and "Final Period Difference" refer to the post-period average of the α_τ coefficients or to α_8, respectively. "Incarcerated that quarter" refers to being detained in any adult correctional facility in that quarter for a drug-related offense. "Ever incarcerated" refers to having been detained in a correctional facility at least once before for a drug-related offense. Standard errors are clustered at the individual level.

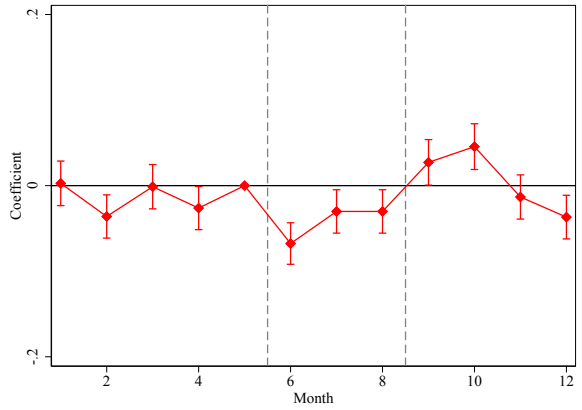
Figure 1A.18: Share of Individuals Arrested for a Felony Around the Age of Criminal Majority



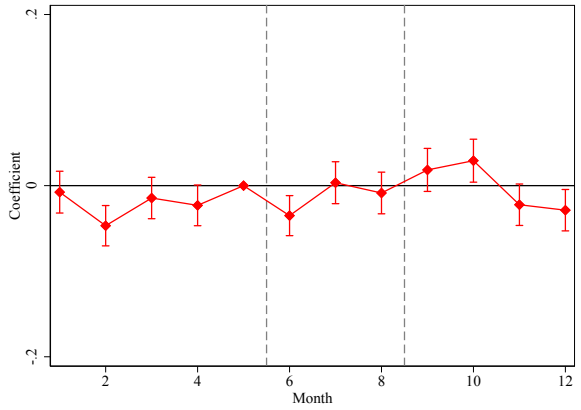
NOTE: These figures plot the monthly estimates of the likelihood of a felony arrest around an individual's 17th birthday. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (1.4). The estimate above the figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Figure 1A.19: Likelihood of Felony Arrest or Death for Males Aged 12–16, by Month

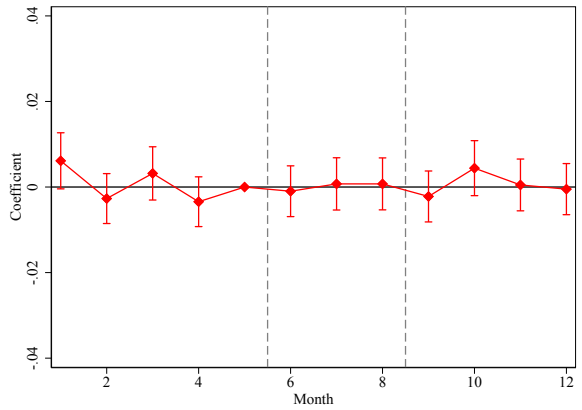
(a) All felonies



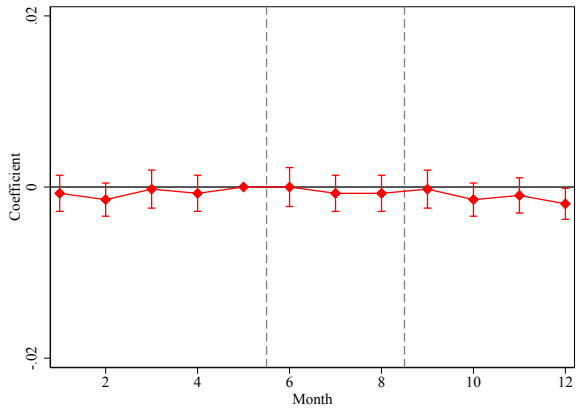
(b) Non-school-related felonies



(c) Adult felonies

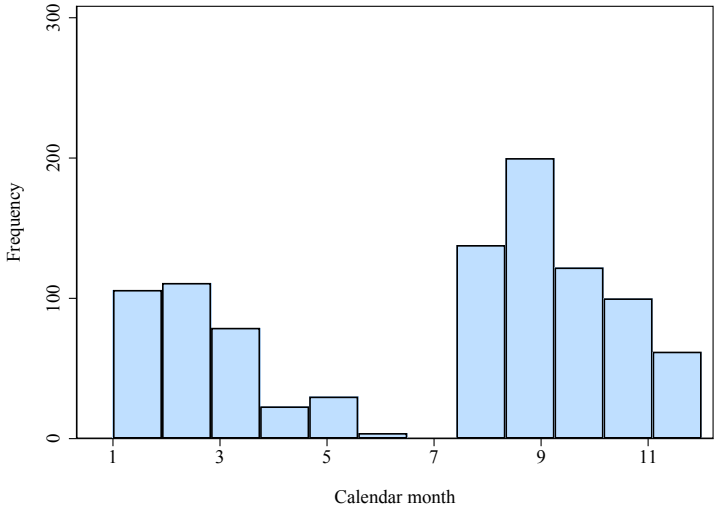


(d) Death



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

Figure 1A.20: Seasonality of Dropout

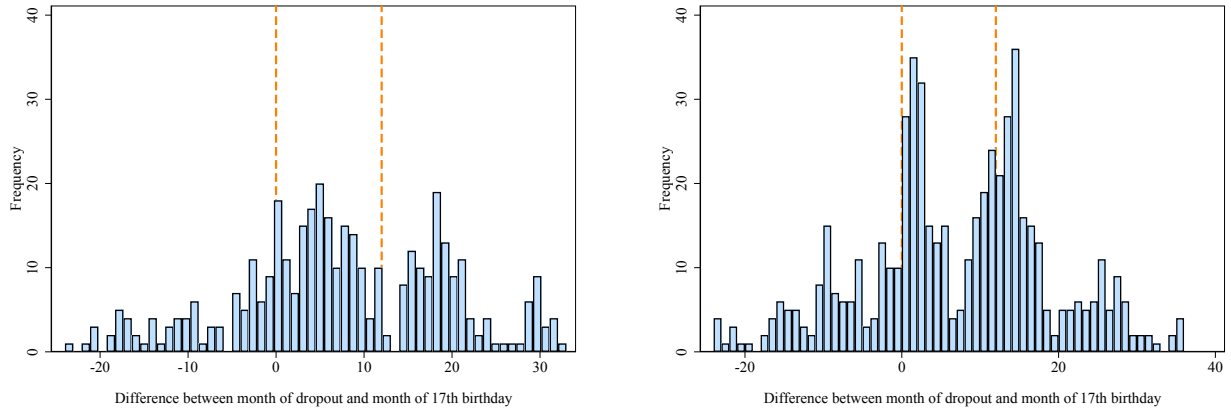


NOTE: This figure shows the dropout month recorded in the Department of Education administrative records for all individuals with an available dropout date.

Figure 1A.21: Difference between 17th-Birthday Month and Dropout Month, by Birth Month

(a) January–June

(b) July–December

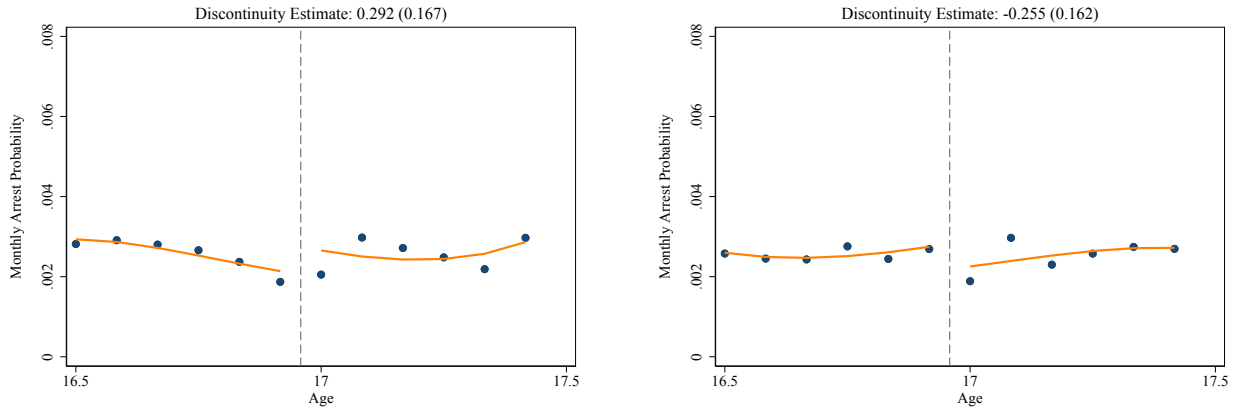


NOTE: This figure shows the difference between an individual's 17th-birthday month and dropout month, as recorded in the Department of Education administrative records for all individuals with an available dropout date. The dashed orange lines indicate the time between the individual's 17th-birthday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

Figure 1A.22: Felony Propensity Estimates Around the Age of Criminal Majority, by Birth Month

(a) January–June

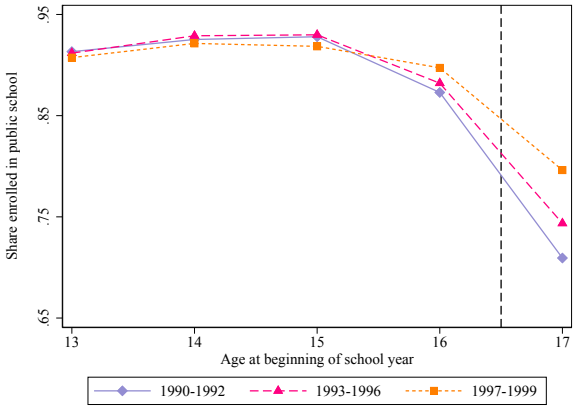
(b) July–December



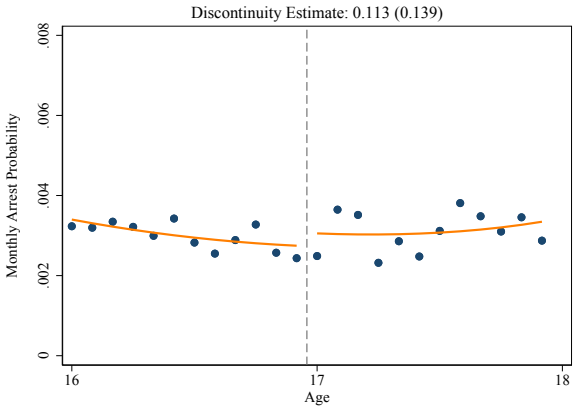
NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday, separately by birth month. The figures consider all men born between 1990 and 1999 who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (1.6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Figure 1A.23: Likelihood of Dropout and Deterrence Estimates, by Cohort

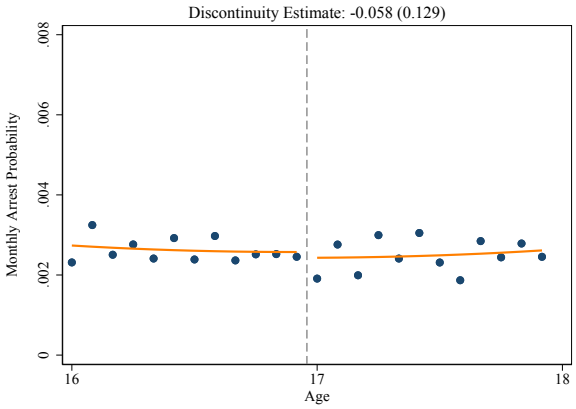
(a) Share enrolled by age



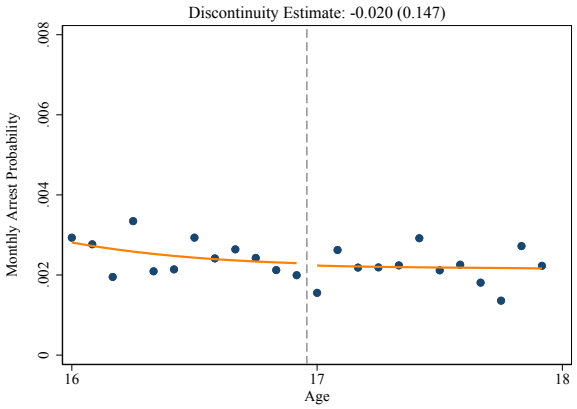
(b) 1990–1992 Cohorts



(c) 1993–1996 Cohorts



(d) 1997–1999 Cohorts



NOTE: The first panel shows the share of individuals born in that cohort who appeared at that age in the Department of Education administrative school records noting enrollment at the beginning of the school year. The remaining figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday, separately by birth cohort. The figures consider all men born who had not been arrested for a felony prior to age 16. The blue markers represent the share of individuals arrested for a felony in that month as a share of individuals who were still at risk of being arrested for a felony. The solid line represents the estimates based on equation (1.6). The estimate above each figure reports the discontinuity estimate from this equation and its standard error (clustering at the individual level).

Table 1A.1: Summary Statistics of Matching Candidates and Matches

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

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

Table 1A.2: Distribution of Mental Health Diagnoses

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

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

Table 1A.3: Common Mental Health Services

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

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

Table 1A.4: Effect of Medicaid Loss on Men's Criminal Activity

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

NOTE: Data on arrests come from the South Carolina Law Enforcement Division and data on incarcerations in state prison come from the South Carolina Department of Corrections. The average in column 1 reports the post-period average for the matched control observations in the full sample. The average in column 3 reports this same statistic for control observations with a mental health history. For the two cumulative variables, the averages correspond to the value measured in the last quarter of the post period (age 20, quarter 4). The estimated effects in column 2 correspond to the post-period average of the β_τ coefficients (or β_8 for cumulative variables) estimated using equation (1.1). The estimated effects in column 4 correspond to the post-period average of the α_τ coefficients (or α_8 for cumulative variables) estimated using equation (1.2). Standard errors are clustered at the individual level and reported under corresponding estimates in parentheses.

Table 1A.5: Robustness to Specification, Matching, & Samples for Men's Likelihood of Incarceration

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

NOTE: Stars report statistical significance: *** = p-value < 0.01, ** = p-value < 0.05, * = p-value < 0.1. This table reports the results from the robustness checks conducted in Section 1.6.3. The first row reproduces the baseline estimates seen in Figure 1.5. The second and third rows cluster the standard errors at the match level and at the match and individual level, respectively. The fourth row forces each treated unit to have one control unit. The fifth row re-weights control that have disproportionate weight in the baseline specification. The sixth row assigns individuals to the treatment and control group based on their Medicaid enrollment in the sample pre-period. The seventh row matches individuals using school instead of district. The eighth row uses all eligible treated and control individuals (i.e., no matching). The ninth and tenth rows use the DFL re-weighting approach to re-weight eligible control individuals. The ninth row uses the same baseline matching characteristics and the tenth row uses school instead of district. Except for the second and third rows, standard errors are clustered at the individual level and reported under their corresponding estimate in parentheses.

Table 1A.6: Likelihood of Felony Arrest or Death for Males Aged 12–16 in Summer Months

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

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

B Medicaid Loss: Sample and Variable Construction

Selection of Sample

To select the individuals for this study, South Carolina's Revenue and Fiscal Affairs (RFA) Office used the characteristics of individuals and schools in the Department of Education records. First, they calculated the share of students receiving free or reduced-price lunch at each high school in the state, for every year between 2008 and 2014.[45] These shares were then averaged across the 2008–2014 school years and schools were ranked based on the average share. The high schools in the upper half of this distribution were chosen and if an individual ever attended one of these "low-income" high schools, he or she entered the sample.

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

Throughout the analysis, I focus on individuals born between 1990 and 1993 because subsequent cohorts were affected by the introduction of an automatic enrollment program for Medicaid. Specifically, South Carolina implemented the Express Lane Eligibility (ELE) program in 2011–2012, resulting in increased Medicaid enrollment starting with the 1994 cohort. Among men born between 1990–1993, 29 percent were eligible control individuals, whereas among men born between 1994–1997, only 16 percent were eligible control units. The increase in enrollment prompted by ELE reduces the number of individuals eligible for

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

the control group, thereby worsening the match rate and allowing fewer individuals to have a disproportionate impact on the estimated counterfactual outcome paths. Perhaps more importantly, this increase in enrollment also alters the composition of eligible treatment and control individuals. Prior to ELE, the control group included lower-income individuals who were likely eligible but not enrolled in Medicaid. However, starting with the 1994 cohort, many of these lower-income individuals were automatically enrolled into Medicaid, thereby becoming part of the high-enrollment group; the remaining control individuals therefore had relatively higher incomes and were potentially worse candidates for estimating counterfactual outcome paths. For these reasons, the analysis focuses on individuals born between 1990 and 1993.

Variable Construction

I use the Medicaid insurance claims to classify medical visits or medications as mental healthcare. Specifically, I classify insurance claims as mental healthcare if any of the diagnoses codes or any of the pharmacologic-therapeutic drug classifications are related to behavioral health. Mental health diagnoses are identified using ICD-9 codes 290–319 and ICD-10 codes F01–F99. In Section 1.7, I split the sample into two groups using the ICD-9 diagnosis codes available in their insurance claims. I identify individuals as having a personality disorder if they ever filed a mental health claim with an ICD-9 code of 301, 317, 318, or 319. Individuals with a substance abuse disorder are those who ever filed a claim with ICD-9 codes 303, 304, or 305. Finally, drugs are labeled as mental health medications if their code is 281604–282800, 281208, 281292, 283292, 284000, or 289200.

I also use the billing codes in the mental health claims to identify the types of services used. Alcohol or substance abuse services refers to mental health claims that have alcohol or substance abuse services in the billing codes. Psychotherapy refers to any mental health

claims that list psychiatric services or other therapy in the billing codes. Case management refers to mental health claims that have case management listed in the billing codes.

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

I classify the offenses in the SLED and DOC data into four categories: violent offenses, drug-related offenses, property offenses, and miscellaneous offenses. Violent-crime incarcerations in the DOC data are incarcerations for murder, assault, robbery, or sex offenses. Violent-crime incarcerations in the SLED data are those whose arrest dispositions are for murder, assault (excluding simple assault, but including domestic violence as well as kidnapping charges), robbery, and sex offenses (excluding sex offender violations).[46] Drug offenses are those related to the possession, distribution, or manufacturing of drugs or alcohol (including DUIs). Property crimes are incarcerations for burglary, larceny/theft, or motor vehicle theft. Finally, miscellaneous offenses are all remaining offenses that do not fit into any of these categories (e.g., weapons offenses, parole or probation violations).

Finally, when considering academic achievement, I standardize the test scores at the grade, year, and test-type level to have mean zero and standard deviation one. I standardize at the test-type level because South Carolina transitioned across various standardized tests

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

during the study's sample period: the Palmetto Achievement Challenge Test (PACT), the Palmetto Assessment of State Standards (PASS), the High School Assessment Program (HSAP), the ACT Aspire test, and the South Carolina College- and Career-Ready (SC READY) Assessment.

C Cost-Benefit Analysis: Providing Medicaid Eligibility

Increased Costs of Expanding Eligibility

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

Overall, there are roughly 11,748 men in each cohort. I linearly interpolate the enrollment patterns of this group using the share enrolled in each quarter prior to age 19 (i.e., the shares in Figure 1.3). I find that roughly 30 percent of the cohort would take up Medicaid at age 19 and 24 percent of the cohort would take up Medicaid at age 20. Multiplying these numbers by the annual per-enrollee cost for children in South Carolina in 2011 ($2,008), I find that that the total cost of providing this group of low-income young men with Medicaid totals $12.5 million (Young et al. 2015).[47]

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

I also consider more-conservative possibilities in terms of the share of men in these cohorts who would choose to take up the program. If the same share of individuals who was enrolled in the program in the quarter prior to age 19 remained enrolled through ages 19 and 20 (31

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

percent of the cohort), then the cost rises to $14.6 million. Finally, I consider a higher per-enrollee cost using the nationwide annual per-enrollee cost for children ($2,492). Assuming this higher cost *and* the higher level of program take-up implies a total cost of $18.2 million.

Benefits of Expanding Eligibility

Lower Social Costs from Fewer Victimizations

To calculate the reduced social costs from fewer victimizations, I begin by using the estimates from Table 1.6 to calculate the number of violent, property, and drug-related crimes that occurred as a result of the Medicaid loss. I find that on average for one cohort, there were 35, 37, and 40 additional violent, property, and drug-related crimes, respectively.[48] I then use the victimization-to-arrest ratios calculated in Heckman et al. (2010) to estimate the average number of additional *incidents* that likely occurred as a result of the Medicaid loss. Specifically, I use the violent and property crime ratios from this source (4.0 and 15.4), and assume that the ratio for drug offenses is the same as that for property crimes.[49] These ratios imply that 139 violent incidents, 577 property crimes, and 623 drug-related offenses occurred after the loss of Medicaid eligibility.

I then calculate the cost of the average violent, property, and drug-related incident (similar to the approach in Mello 2019). First, I split each category of crime into subcategories. Then, I use the prevalence of these sub-crimes in the SLED data in conjunction with the victimization-to-arrest ratios to calculate the share of victimizations that fall into each category. Next, I use the average cost to victims (from Cohen and Piquero 2009 and Miller et al. 1996) for each subcrime in order to calculate the cost of each average violent, property, and drug offense (summarized in Table 1C.1).[50] The upper-end figures use all of the

[48] I calculate these numbers by multiplying the average number of people in one cohort (5,364) by the estimated coefficients in Table 1.6 (divided by 100) and by 8 quarters.

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

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

costs from Cohen and Piquero (2009). To be conservative in terms of the statistical value of life, the lower-end estimates divide the cost of murder in half (Heller et al. 2017). For drug-related offenses, the upper-end figures assign DUI offenses the average cost from drunk driving crashes from Cohen and Piquero (2009). The lower-bound estimate assigns DUI offenses the cost of drunk driving incidents without injuries from Miller et al. (1996). For the remaining drug-related crimes, I conservatively assign them a cost of $0 because most of these offenses tend to be "victimless."[51] For both violent and drug offenses, the moderately conservative estimate takes an average of the upper and lower bounds. Overall, the total social cost of crime that was averted ranges from $16.4 to $28.3 million.

Lower Fiscal Costs from Fewer Incarcerations

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

To be conservative, I assume that not all of the men arrested for these serious crimes are sentenced to state prison spells. Using the full sample of low-income men born between 1990 and 1993, I estimate that 67 percent, 54 percent, and 42 percent of violent, property, and drug arrestees are sentenced to a prison spell.[52] For each offense, I then multiply the number of incarcerated individuals in state prisons by the average sentenced served in 2009 in South Carolina and by the daily cost per inmate (roughly $45 in 2011).[53] I allow for

[51]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.

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

[53]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.

heterogeneity in the length of sentences served by type of offense: the average time served for violent, property, and drug crimes is 4 years, 1.9 years, and 2.2 years, respectively (Pew Center on the States 2012).[54] Overall, I find that the fiscal cost would have been reduced by $2.6 million.

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

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.[55] Taking the lower-bound and upper-bound of these estimates suggests that a two-year prison spell has total social costs of $49,633 and $67,947, respectively. Prior calculations suggest that roughly 61 individuals in each cohort would have been sentenced to serve a state prison spell after losing Medicaid eligibility, implying a total cost between $3.0 and $4.1 million.

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

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

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

Marginal Value of Public Funds

In this subsection, I use the causal estimates to form this policy's marginal value of public funds (MVPF) (Finkelstein and Hendren 2020, Hendren and Sprung-Keyser 2020). Specifically, the MVPF is a ratio of society's willingness to pay for this policy to the net cost of the policy to the government. Similar to the approach above, I construct both an upper and lower bound for this ratio based on different assumptions. Estimates are summarized in Table 1C.2.

Willingness to Pay

I begin by calculating the numerator of the ratio, which measures the aggregate social willingness to pay for the policy change (i.e., extending Medicaid eligibility for two years). The primary component in this numerator is society's willingness to pay for fewer criminal victimizations ν. I use the upper and lower bounds of the social cost of crimes averted outlined above and shown in Table 1C.1. In constructing the upper bound of the MVPF, I also include the second round of criminal victimizations that would be averted (i.e., from men not recidivating).[56] Overall, the willingness to pay for fewer victimization ranges from \$16.4 to \$33.0 million.

Next, I consider the willingness to pay for improved labor market prospects, η, by the individuals who avoided incarceration. In other words, beneficiaries should be willing to pay for the increase in wages they experience from this policy change. To calculate this foregone income, I first use the 2009–2013 American Community Survey to calculate the employment rate and average annual income of employed men in South Carolina who were aged 19–25 and living under 200 percent of the FPL: 50 percent and \$11,950, respectively (Ruggles

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

et al. 2020).[57] I then calculate the total foregone income of affected individuals *during* incarceration: roughly $973,100.[58] For the upper bound of the MVPF, I also include the losses in income that follow the incarceration spell. I estimate the post-release employment rate of offenders (using Mueller-Smith 2015, Table 7) and use this figure to calculate foregone income in the five years after release: roughly $313,300.[59]

Second, the overall willingness to pay for this policy change includes the value of the public insurance transfer γ. In the upper bound MVPF estimate, I assume that beneficiaries would be willing to pay the average out-of-pocket spending amount for individuals ages 19–34: $370 dollars (Peterson-KFF 2020).[60] In the lower-bound estimate, I conservatively assume that beneficiaries would only be willing to pay $1 for this transfer. However, I note that the total value of the transfer γ also depends on who bears the cost of uncompensated care (Finkelstein et al. 2019). I therefore relegate the final calculation of this parameter to the following subsection.

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

Net Cost to the Government

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

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

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

[60] 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).

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 Appendix C, so that G is approximately \$14.6 million. In determining the net cost to the government of this policy, however, it is important to consider the ultimate economic incidence of the transfers to external parties. Finkelstein et al. (2019) finds that 60 percent of Medicaid spending is a transfer to providers of uncompensated care for the low-income uninsured. If the government bears the cost of uncompensated care, then the total cost of this policy will only be $0.4G$. If individuals bear the cost of uncompensated care, then the total cost of this policy is G. I thus assume that the government and individuals bear the cost of uncompensated care when estimating the upper and lower bound of the MVPF, respectively. Finally, I note that in the scenario in which individuals bear the cost of uncompensated care (i.e., the lower bound of the MVPF), then society would also incorporate this component in their willingness to pay for the public insurance transfer (Finkelstein et al. 2019). In other words, γ would also include $0.6G$.

In calculating the total cost to the government, I also factor in the reduced fiscal cost from fewer incarcerations, μ, which ranges from \$2.1 to \$2.6 million (see Appendix C). For the upper bound of the MVPF, I also incorporate the fiscal cost of a second round of incarcerations (i.e., from recidivism), which amounts to roughly \$578,000.[61]

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

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

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 1C.1: Social Costs per Crime

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

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

Table 1C.2: Marginal Value of Public Funds (MVPF), Upper and Lower Bounds

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

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

D Measuring Deterrence Around Age of Criminal Majority

Sample and Variable Construction

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

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

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

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

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

I then classify arrests in the South Carolina Law Enforcement Division data as felonies. First, I create a crosswalk between arrest charges and felony classifications; I use all possible arrest charge codes in the SLED files and classify that charge as a felony if a majority of those charges were labeled as felonies. I then use this crosswalk to classify the primary charge for each arrest as a felony.[64] For the 10 percent of arrests that do not have available charge information, I use the information that describes the disposition of the arrest to classify that arrest as a felony. Specifically, I create a second crosswalk between arrest (literal) descriptions and felony classifications. I then use this second crosswalk to classify arrest dispositions as felonies.[65]

One concern with this classification method is that for the 10 percent of arrests that do not have available charge information, I may be over- or under-counting the number of felonies. For example, if between the charge and the disposition, many felony charges were not ruled to be felonies, then I would be undercounting the number of felony charges. To correct for potential misclassification, I use all of the arrests that have both a charge and a disposition available (52 percent of arrests for men ages 17 and younger) in order to gauge the magnitude of the misclassification. In other words, I use the arrests with an available charge and disposition to calculate α, β_1, and β_2 in Figure 1D.1.

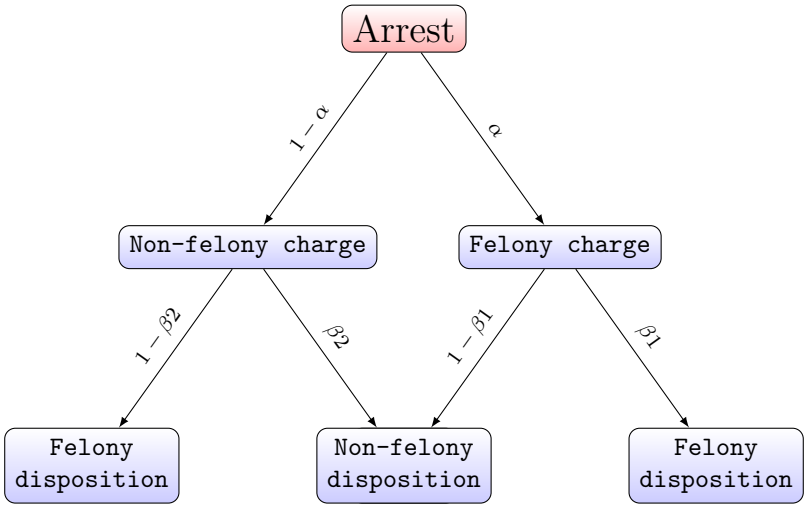
I then use these estimates to calculate the share of felonies that are incorrectly classified (i.e, they were likely non-felony charges) as well as the share of non-felonies that were misidentified (i.e., they were likely felony charges):

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

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

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

Figure 1D.1: Likelihood of Felony Arrest Charge and Disposition



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

I assume that these estimates of misclassification represent the degree of misclassification for the 10 percent of arrests with missing charge information. I then use these shares to randomly re-classify $\gamma_1\%$ of classified felonies in this group as non-felonies, and $\gamma_2\%$ of classified non-felonies as felonies. In the end, I have two measures of felony charges in the SLED data: one which uses the disposition for the missing-charge group, and one which corrects for the potential misclassification in the process.

Finally, in order to construct the figures I combine the two data sources, so that individual i is labeled as committing a felony at time t if he had a felony referral in the DJJ data *or* a felony arrest in the SLED data. I then restrict the sample one more time to individuals who had not committed a felony prior to age 16 (90 percent of individuals ever-enrolled in Medicaid). The baseline measure used in Figure 1.8 uses the felony classification that has been corrected for potential misclassification. I check the robustness of this result to the usage of the original felony classification in Figure 1D.2. The results are hardly changed.

Elasticity Construction: Length of Sentences

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

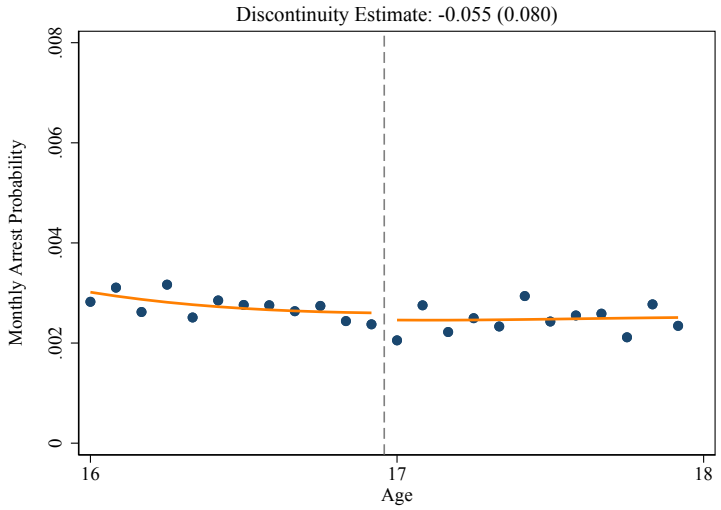
I then use the admissions data from the Department of Corrections files to calculate that the average incarceration sentence for men admitted at age 17 is between 4.5–5 years. I confirm this estimate using the DOC's statistical reports, which show that the average sentence lengths for inmates admitted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

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

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

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

Figure 1D.2: Felony Propensity Estimates Around the Age of Criminal Majority



NOTE: These figures plot the monthly estimates of the hazard for felony arrests around an individual's 17th birthday. This figure considers all men born between 1990 and 1999 who had not been arrested by the DJJ for a felony prior to age 16 ($N = 72,939$). The circles represent the share of individuals arrested for a felony in that month as a share of individuals who were at risk of being arrested for a felony. The solid line represents the estimate based on equation (1.6). The corresponding discontinuity estimate from this equation and its standard error (estimated by clustering at the individual level) are reported above the figure.

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

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

Cost of Providing Medicaid Eligibility

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

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

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

Finally, because these individuals are serving relatively shorter (i.e., the status quo) sentence lengths, there is a chance that they will re-offend after serving their prison spell.[69] To calculate the fiscal cost, I assume that 30 percent of the men who were incarcerated recidivate within five years and I use the average length of sentences served in South Carolina for all crimes (2.3 years).[70] The fiscal cost of this second round of incarcerations totals $4.6 million.

When individuals re-offend, their crimes also impose social costs on victims. To calculate these costs, I use the share of serious *second* arrests that have an associated prison spell to back out the number of serious crimes committed. I then use the share of serious arrests by offense type and the victimization-to-arrest ratios to calculate the implied number of incidents. I use the upper-bound (i.e., the least conservative) estimate for violent crimes from Table 1C.1 to estimate the total social costs of this second round of victimizations: $36.6 million.[71]

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

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 1.8—$\varepsilon_{c,f} = -0.018$ using the full sample and the average sentence served—to estimate the degree to which sentence lengths would need to be extended to achieve a 10 percent reduction in crime. I find that sentences would need to be roughly 570 percent longer. I assume that this elasticity applies

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

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

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

to all offense types uniformly and calculate the new average sentence length served for each type of crime. Finally, I follow the same approach as above for calculating the total fiscal cost, multiplying the number of incarcerations for each offense type by the longer sentence length and by the cost per inmate. I find that the total fiscal cost amounts to $109.6 million.

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

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 and McCrary 2018, Evans and Owens 2007, Mello 2019, Weisburst 2019). In this subsection, I consider the cost of reducing crime by 10 percent by hiring more police officers. When comparing the cost of hiring police officers to the cost of providing Medicaid, I use the short-term cost of Medicaid provision (i.e., excluding the social and fiscal costs of recidivism): $36.1 million.[72]

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

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

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

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

However, hiring police officers does not only reduce the criminal activity of 19- and 20-year olds, but it has spillover effects on the criminal activity of individuals of other ages. To calculate the number of additional crimes averted in other age groups, I begin by looking at the age distribution of admitted inmates in South Carolina (focusing on individuals ages 17 through 40, who make up the majority of individuals committing crime). I then use the share of serious arrests that end in state prison (calculated from the raw data) to back out the number of total arrests in this age group. Next, I use the share of arrests that are violent and property offenses in this sample to calculate the implied number of violent and property arrests for this age group. I then use the violent and property crime elasticities from Evans and Owens (2007) to estimate how many fewer arrests there would be given the increased number of police officers.[75] Finally, I use the victimization-to-arrest ratios from Heckman et al. (2010) to calculate how many fewer violent and property incidents there would be if more police officers were hired. Using the upper-bound social costs of violent crime from Table 1C.1, I find that hiring police officers would reduce the social costs of violent and property victimizations by $1.3 billion and $18.6 million, respectively. Finally, there is an additional reduction in fiscal costs of $21.8 million from fewer individuals being incarcerated (after multiplying the number of individuals in these other age groups who would have likely been incarcerated by the daily inmate cost and average sentence served).

The results from this exercise suggest that the benefits of hiring more police officers outweigh the costs. However, I note that these results are sensitive to the cost of violent crimes—especially because the evidence from prior studies shows that violent crime is particularly responsive to police presence—as well as to the assumption that police reduces crime uniformly for individuals ages 17 through 40. If I use the lower-bound for the cost of violent crime in Table 1C.1 and assume that the spillovers only affect men ages 18–30, then

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

Table 1E.1: Summary of Estimated Net Cost of Hiring Police Officers (In Millions)

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

NOTE: This table reports the estimated net costs of hiring police officers (i.e., negative costs imply that the benefits outweigh the costs). The top and bottom panels use a higher and lower marginal cost of hiring police officers, respectively (Chalfin and McCrary 2018, Evans and Owens 2007). The two columns use the lower- and upper-bound costs of violent crimes from Table 1C.1. The age range indicates the extent to which hiring police officers is assumed to deter criminal activity among individuals in other age groups.

I find that this policy has an overall net cost of $509.8 million, which would favor Medicaid provision over hiring more police officers. Table 1E.1 summarizes how the estimated costs change as I alter certain assumptions, including lowering the marginal cost of hiring police officers.

Overall, the calculations from this subsection suggest that in terms of crime reduction, Medicaid provision is likely less cost effective than hiring more police officers. There are two caveats to this conclusion. First, this analysis does not take into account the social costs of policing (for example, the social costs of police brutality, excessive force, or over-policing). Second, the calculations ignore the non-crime-related benefits that health insurance might provide to individuals (e.g., in terms of financial stability, earnings and tax revenue) (Gallagher et al. 2019, Gross and Notowidigdo 2011, Hu et al. 2018). Consequently, even though I am making a relatively comparable comparison for these policies—estimating the cost of a 10 percent crime reduction—it is almost certain that these calculations are underestimating the associated benefits of providing low-income young adults with Medicaid access.

Chapter 2

The Effect of Immigration Enforcement on Crime Reporting: Evidence from Dallas[1]

2.1 Introduction

The ability of law enforcement officials to detect and sanction criminal behavior depends on an individual's willingness to report crime. However, unauthorized immigrants or people living near unauthorized immigrants might fear that contact with the police could result in law enforcement learning about an individual's immigration status (see for example, Lake et al. 2013). Under-reporting of crime can make it more difficult for police departments to

[1]I am grateful to Leah Boustan and Ilyana Kuziemko for guidance and support throughout this project. I also thank Jessica Brown, Jiwon Choi, Matt Cocci, Will Dobbie, Ben Eskin, Hank Farber, Felipe Goncalves, Stephanie Kestelman, David Lee, Elena Marchetti-Bowick, Alex Mas, Steve Mello, Whitney Rosenbaum, Owen Zidar, and participants of the Princeton Industrial Relations Section Labor Lunch, Public Finance Working Group, and the Fellowship of Woodrow Wilson Scholars for helpful comments. Thank you also to Nicole Jackson and Lieutenant Phyllis Nobles for providing me access to Dallas Police Department data; Sue Long and the Transactional Records Access Clearinghouse for access to data on ICE detainer requests; Chris Moriarty for a primer on ICE and PEP; Krsna Avila for sharing information on cooperation between ICE and local jurisdictions after PEP; and Soila Monroy, Sergeant Robert Muñoz, and Gloria Renteria for taking the time to talk to me about police-community relations in Dallas. Finally, thank you to Princeton University for generous financial support. All errors are my own.

prevent and solve crimes, leading to a misallocation of resources and inhibiting investigation or prosecution of offenders (Langton et al. 2012). Yet, a majority of crimes—even serious violent crimes—often go unreported.[2]

Immigrants' hesitancy to contact the police likely stems from federal immigration programs (i.e., the 287(g) and Secure Communities programs) that established cooperation between local law enforcement and the Department of Homeland Security (DHS)'s Immigration and Customs Enforcement (ICE) agency.[3] Indeed, as the number of detentions and deportations rose between 2008 and 2014 as a result of Secure Communities, a number of police departments expressed that worsening relations with the Hispanic community was making them less effective.[4]

As one illustration of this concern, former Chief of the Los Angeles Police Department William J. Bratton remarked:

> Keeping America's neighborhoods safe requires our police forces to have the trust and help of everyone in our communities. My nearly 40 years in law enforcement and my experience as Police Commissioner in Boston and New York City and as Chief in Los Angeles has taught me this. Yet every day our effectiveness is diminished because immigrants living and working in our communities are afraid to have any contact with the police. [...] My officers can't prevent or solve crimes if victims or witnesses are unwilling to talk to us because of the fear of being deported. [...] We can't solve crimes that aren't reported because the victims are afraid to come forward to the police.

[2]The National Crime Victimization Survey found that in 2016, only 44 percent of 5.4 million violent crimes and 35 percent of 15.8 million property crimes were reported to the police (Morgan and Kena 2018).

[3]For more background and details on the strengthening of the 287(g) program in 2006 and the launch of Secure Communities in 2008, I refer the reader to Alsan and Yang (2018), Miles and Cox (2014), Wang and Kaushal (2019), and Watson (2013), among others.

[4]Well over 90 percent of ICE detentions during Secure Communities were of Hispanic individuals (Alsan and Yang 2018).

Many local jurisdictions began limiting their cooperation with ICE, which eventually convinced DHS to suspend the Secure Communities program in November of 2014.

This paper studies whether the Priority Enforcement Program (PEP)—a federal program that was launched in 2015 to rebuild trust between law enforcement and immigrant communities—changed the degree to which Hispanic individuals reported incidents to the police. Under PEP, the agency no longer sought to detain individuals with immigration offenses alone, and instead only focused on detaining individuals convicted of significant criminal offenses. In reducing the number of circumstances under which it could detain individuals, ICE attempted to re-establish the cooperation that it lost during the height of Secure Communities with state and local jurisdictions.

By redefining ICE's priorities to only focus on individuals who posed a threat to public safety, PEP reduced the likelihood that an unauthorized immigrant who had not committed a crime would be detained or deported, thereby reducing the cost of reporting an incident to the police. However, PEP did not lower the probability that an immigrant who was convicted of a serious crime would be detained. For these reasons, PEP—unlike most other immigration policies—directly increased the incentives for Hispanic victims to report incidents, without also increasing the incentives to commit a serious offense.

To explore the impact of this program on the share of crimes reported by Hispanic complainants, I use administrative data from the Dallas Police Department (DPD). This incident-level dataset from the DPD is relatively unique in that it includes information about a complainant's race and Hispanic ethnicity as well as his or her full name.[5] Using this information, I employ a difference-in-differences strategy, in which I estimate how Hispanic complainants' reporting behavior changed relative to reporting by non-Hispanic complainants in the same neighborhood (i.e., Census tract) after the introduction of PEP. In addition to the richness of its policing data, Dallas is a useful setting for evaluating this

[5]Most other police departments are either unwilling to release this level of information or have inaccurate data on complainants' race and ethnicity.

policy because slightly more than 40 percent of the population is Hispanic and the vast majority of neighborhoods are at least 20 percent Hispanic.[6] Moreover, roughly 20–25 percent of Dallas County's Hispanic population is unauthorized immigrants (U.S. Census Bureau 2019b; Migration Policy Institute 2018).

Using this identification strategy, I find that after PEP was launched, the number of incidents reported to the police by Hispanics increased by around 8 percent (relative to the mean of 35 incidents in a Census tract per quarter). I also find that the reporting of violent and property crimes rose by around 4 percent (relative to the mean of 15 crimes in a tract per quarter). A rough estimate suggests that during the year and a half that PEP was in effect, the DPD was notified of around 6,000 and 1,200 more total incidents and serious crimes, respectively, than it would have been otherwise.[7] These results are robust to the definition of neighborhood used as well as to the identification of Hispanic complainants using names (as opposed to using the race/ethnicity recorded by the DPD).

I consider two alternative explanations for the increase in reported incidents by Hispanic individuals following the implementation of PEP: a rise in the underlying crime rates of Hispanic offenders (who often commit crimes against other Hispanic individuals) and growth of the Hispanic population.[8] I use arrest data from the DPD to show that the share of arrestees who were Hispanic stayed relatively constant over this time period. These results suggest that the increase in reported crime was likely not driven by Hispanic individuals committing more crimes, which is consistent with the program's goal of prioritizing the detainment of serious criminal offenders. I then use the American Community Survey as well as data on school enrollment to show that there was not a sudden influx of Hispanic individuals into Dallas that could explain the increase in complaints.

[6]The median Census tract in Dallas in 2010 was 30 percent Hispanic (TIGER/Line Shapefiles 2017).

[7]For the remainder of the paper, "serious crimes" will refer to crimes classified by the FBI's Uniform Crime Reporting program as violent crimes (e.g., assault and robbery) or property crimes (e.g., burglary and theft).

[8]As an example, between 2012 and 2015, 40 percent of violent victimizations against Hispanic individuals were committed by Hispanic offenders (Morgan 2017).

This paper contributes to a number of literatures. First, I add to a growing literature in economics about how immigration laws and policies can influence the choices and behavior of immigrants. A number of studies show that immigration policies that instill fear can affect the physical health, mental health, and economic outcomes of immigrants, and can have a "chilling" effect on their willingness to participate in safety-net programs (Alsan and Yang 2018; Amuedo-Dorantes et al. 2018; East et al. 2018; Wang and Kaushal 2019; Watson 2014). Previous studies have also shown that immigration policies affect immigrants' educational attainment as well as their willingness to file for permanent residency (Amuedo-Dorantes and Arenas-Arroyo 2021; Kuka et al. 2020; Liscow and Woolston 2018). The most closely related study to this one is Comino et al. (2016), which shows that the 1986 Immigration Reform and Control Act increased amnesty recipients' likelihood of reporting victimizations. To my knowledge, this study is one of the first in this literature to consider the effect of modern immigration policies on individuals' willingness to report a crime to the police, a key input into public safety.

Second, this paper is related to a number of studies, mainly in criminology and sociology, that use surveys to study the relationship between immigrant communities and law enforcement (see for example, Kirk et al. 2012 and Tyler et al. 2010). This paper is the first in this literature to use administrative data to estimate the causal effect of immigration enforcement on the number of interactions between immigrants and the police. Moreover, by showing how crime reporting increases when the enforcement environment changes, this study also contributes to a relatively small literature on the underreporting of crime as well as on policies that encourage individuals to report crime (Carr and Doleac 2018; Miller and Segal 2019). Indeed, despite the millions of crimes that go unreported, little is known about the effectiveness of policies to increase an individual's willingness to report a crime.

Finally, this paper complements a large body of literature that estimates the effect of immigration enforcement on crime rates and police effectiveness (Bohn et al. 2015; Chalfin

and Deza 2018; Ciancio 2017; Cox and Miles 2015; Freedman et al. 2018; Hines and Peri 2019; Miles and Cox 2014). Within this body of literature, an obstacle in interpreting results is differentiating whether changes in crime rates are driven by changes in underlying criminal behavior or changes in crime reporting. By studying Dallas' implementation of the Priority Enforcement Program—a program that reduced the cost of reporting incidents, without lowering the punishment for serious criminal offenses—this study is one of the first in this literature to be able to directly estimate changes in crime reporting.

The remainder of the paper is organized as follows. Section 2.2 provides a brief background on the implementation of PEP nationwide and in Dallas. Section 2.3 discusses the theoretical predictions of PEP's effects. Section 2.4 outlines the data utilized, presents summary statistics, and introduces the methodological framework. Section 2.5 presents the results and conducts robustness checks. Section 2.6 discusses and rules out alternative explanations. Section 2.7 discusses the potential role of local enforcement and Section 2.8 concludes.

2.2 Background on the Priority Enforcement Program

2.2.1 Development and Passage of PEP

The 2008 Secure Communities program increased coordination between local law enforcement and federal immigration authorities in order to detain and deport non-citizen immigrants who had committed crimes. Secure Communities ensured that an arrested person's fingerprints were not only sent to the FBI, but were also forwarded to the Department of Homeland Security so that ICE could determine whether there was probable cause for deportation. Importantly, under this program, ICE could seek the transfer of non-citizen immigrants in state or local custody (i.e., issue a "detainer request") for a broad number of

reasons, including immigration-related offenses.[9] After the start of Secure Communities, the number of individuals detained and deported increased nationwide: between 2008 and 2013, more than 2.3 million individuals were deported.[10]

A few years after its introduction, a number of state and local jurisdictions became wary of the Secure Communities program and began limiting their cooperation with ICE, citing reduced trust between immigrant communities and the police as a primary reason.[11] Moreover, some local jurisdictions were hesitant to cooperate with ICE after federal courts found that parts of the Secure Communities program were unconstitutional. By 2014, more than 100 localities—not including Dallas—had limited or ended their cooperation with ICE.[12]

In November of that year, Secretary of Homeland Security Jeh Johnson issued a memo suspending Secure Communities, citing the reduced cooperation at the local level as well as the federal court decisions as the main reasons (Department of Homeland Security 2014). He then announced the program that would soon take its place: the Priority Enforcement Program. The program was officially launched in July of 2015 after ICE released a brochure on PEP with the new enforcement guidelines as well as the new forms for law enforcement officials and ICE officers.[13]

[9] A detainer request is a written notice from ICE requesting local jails or law enforcement to detain an individual while ICE decides whether he or she will be taken into federal custody for removal purposes.

[10] To better assess this magnitude, roughly 239,000 individuals were deported each year between 2005 and 2007, compared to roughly 388,000 individuals between 2008 and 2013, implying a 62 percent increase in the annual deportation levels (TRAC 2010; TRAC 2014).

[11] For example, in 2012 the Los Angeles Police Department announced that it would no longer honor ICE detainer requests for unauthorized immigrants arrested for nonviolent offenses (e.g., driving without a license) unless they were part of a gang or had a criminal record. Police Chief Charlie Beck said that Secure Communities had hampered efforts to keep the city safe by eroding trust between communities and the police. He announced, "Community trust is extremely important to effective policing. So it's my intent, by issuing this change in procedures, that we gain this trust back." See: "Los Angeles to Cease Transferring Some Immigrants." *The New York Times*. October 4, 2012.

[12] "Why cities are rebelling against the Obama administration's deportation policies." *Vox*. June 6, 2014.

[13] The six-month period between the suspension of Secure Communities and the official launch of PEP was a period of relative uncertainty. In January of 2015, the U.S. House of Representatives tried to pass a bill to reinstate Secure Communities, but it was blocked by the Senate. ICE slowly began implementing the new program after its announcement, but it was not until June 12, 2015 that ICE distributed an official brochure on PEP as well as the new forms.

The main goal of the program was to target resources toward detaining and deporting individuals convicted of *significant criminal offenses*. PEP's guidelines directed ICE to focus on detaining individuals who posed a threat to public safety (e.g., participating in gang or terrorist activity, being convicted of a felony), and to no longer seek the transfer of individuals with solely civil immigration offenses or those who had not been convicted of a criminal offense.[14]

Unlike most other immigration policies that affect both victims and offenders, PEP is unique in that by design, it increases the incentives for individuals to report incidents, but does not lower the punishment for serious criminal behavior. Indeed, in his original memo announcing PEP, Secretary Johnson motivated the changes by highlighting the need to support community policing and to maintain the trust of individuals in working with local law enforcement. Throughout 2015, ICE (and DHS more broadly) "conducted a nationwide effort to implement PEP and promote collaboration, reaching out to thousands of local law enforcement agencies and government officials" (Department of Homeland Security 2015). The Department of Homeland Security announced in their FY2015 report that most law enforcement agencies were back to cooperating with ICE via PEP. This program remained in effect until January of 2017 with the start of the new administration.

2.2.2 Implementation of PEP in Dallas

As mentioned above, after announcing PEP, ICE tried to engage with state and local governments as well as law enforcement officials in order to introduce the program and ensure cooperation. Even though Dallas did not stop cooperating with ICE during Secure Communities, the director of ICE still visited the city in May of 2015 to introduce the agency's new

[14]More precisely, the prioritized enforcement included: participating in an organized criminal gang or terrorism; constituting a threat to national security; or being convicted of a felony or aggravated felony, three or more misdemeanor offenses, or one "significant" misdemeanor (e.g., burglary, domestic violence, DUI). The de-prioritized enforcement included individuals with civil immigration offenses alone or individuals who had been charged, but not convicted of criminal offenses (U.S. Immigration and Customs Enforcement 2015).

priorities.[15] She remarked at one meeting that "People think of us as deporting women and children and adult males willy nilly. It is my job to ensure that each of our 26 field officers are actually enforcing the priorities which focuses on criminals." ICE representatives then met with Dallas County officials in July and August of 2015 to further discuss PEP, and Dallas County Sheriff Lupe Valdez formally agreed to participate in the program.

In late August, Sheriff Valdez also introduced a local policy to ensure the enforcement of these new priorities.[16] Dallas County officials began reviewing ICE's requests prior to honoring them—with the vetting guidelines being very similar to the PEP priorities—thereby ensuring that only individuals who posed a threat to public safety were transferred to ICE's custody. Given the similarity between the federal and the local policies, the Sheriff's office did not decline any of ICE's requests following the launch of PEP. In a spreadsheet tracking PEP's reception by local jurisdictions, ICE officials write about Dallas County (DALCO): "DALCO detainer vetting is similar to our PEP priorities. DALCO provided [ICE] with the points-of-contact for detainer vetting and all detainers have been approved since the inception of the process."[17]

2.2.3 ICE Requests Before and After PEP

Following the announcement and implementation of these redefined enforcement priorities, the number of ICE detainer requests issued—and thus the number of individuals held in county jails with detainer requests—began to fall. Figure 2.1 uses reports written for the Texas Commission on Jail Standards to plot the number of inmates (i.e., the stock of individuals) held in Dallas County jails with an immigration detainer. The figure shows a slight

[15] "ICE chief says immigration removals focus on convicted criminals." *The Dallas Morning News.* May 20, 2015.

[16] The motivation behind the sheriff's new policy was not clear, but it was perhaps the result of growing controversy and legal challenges to immigration holds and inmate deaths in Dallas County earlier that year.

[17] I am grateful to the Immigrant Legal Resource Center for obtaining and sharing information about whether particular jurisdictions complied with PEP as well as about meetings between ICE officers and local officials.

decline in the number of inmates following the announcement of PEP (dashed green line), and a continued fall after its launch (solid green line).[18]

As a result of the changing enforcement priorities, the composition of individuals with immigration detainers also changed with PEP. Using data on ICE detainer requests from the Transactional Records Access Clearinghouse (TRAC), Figure 2.2 shows the number of immigration detainers issued in Dallas County each month between 2013 and 2015 as well as broken down by an individual's most serious criminal conviction.[19] The drop in the total number of detainers after the announcement of PEP in panel (a) corresponds with the lower number of inmates held with immigration detainers in 2015 in Figure 2.1. Importantly, panel (b) of Figure 2.2 highlights that PEP seems to have achieved its enforcement goals in Dallas County: it hardly changed the number of immigration detainers issued for individuals convicted of a felony or aggravated felony, but it lowered the number of detainers issued for individuals convicted of misdemeanors and for those with no conviction. A back-of-the-envelope calculation suggests that if the number of detainers for these latter two groups stayed at their 2014 levels, ICE would have detained nearly 1,150 more individuals in Dallas County by the end of 2015.

2.3 Theoretical Predictions for PEP's effects

In this section, I discuss the effect that PEP likely had on the share of incidents reported to the police as well as on the amount of crime committed. PEP lowered the average cost of reporting an incident, and we therefore expect the share of crimes reported by Hispanic

[18]It is not clear from these monthly counts why the number of inmates sometimes rises (for example, in 2014 or 2016), but it is worth reiterating that these data measure the stock, not the flow, of inmates. So, if any incident occurred that resulted in a large number of immigration detainer requests in Dallas County (e.g., targeted raids, spikes in gang activity), the number of inmates would remain at those higher levels for the subsequent months. Since inmates are held in jails for several weeks or months, the numbers in Figure 2.2 will not add up to the numbers in Figure 2.1, but will contribute to the total stock of inmates.

[19]Unfortunately, ICE has refused to release data that include individuals' criminal convictions for any month after November of 2015.

complainants to increase following the program's introduction. Criminal offenders likely responded to this increased probability of reporting, and we therefore expect the number of serious crimes committed—particularly against Hispanic victims—to unambiguously fall.

2.3.1 Effect on Reporting

Consider a simple model of crime reporting in which victim i chooses whether to report an incident ($R = 1$) or not ($R = 0$) based on the utility derived from each alternative:

$$U_{R=1} = f(B_i) - g(C_i, \pi_i) + \alpha_i$$
$$U_{R=0} = 0$$

Individual i's decision to report a crime is a function of the perceived benefits of reporting B_i, which include benefits to the individual (e.g., personal safety, the potential to recover a stolen good) as well as benefits to the community (e.g., improved public safety from finding the offender). Analogously, the decision to report a crime is also dependent on the costs to the individual C_i (e.g., time or psychic costs).[20]

As noted above, unauthorized immigrants might fear that contact with the police could result in law enforcement learning about their immigration status. Anecdotal and empirical evidence also suggests that individuals living with or near unauthorized immigrants alter their behavior out of fear of revealing their non-citizen contacts.[21] An individual's decision to report a crime is therefore also a function of the immigration-related cost of reporting a

[20]The costs and benefits of reporting can differ across incident types k and time periods t. The corresponding subscripts are suppressed for simplicity.

[21]For example, in a 2012 survey of over 2,000 Latinos around the United States, 44 percent of respondents reported that they were less likely to contact the police if they had been the victim of a crime because they feared that law enforcement officials would use the interaction as an opportunity to inquire into their immigration status or that of people they know (Lake et al. 2013).

crime, π_i; the cost π_i is an increasing function of the individual's likelihood of being deported, P_i, as well as of the probability that the individual's contacts are deported, P_n.[22]

Finally, the utility obtained from reporting an incident is also a function of α_i, which is individual i's relative idiosyncratic preference for reporting (where utility from not reporting has been normalized to 0). Individual i will choose to report an incident if and only if the utility obtained from reporting the crime exceeds the utility derived from not reporting the crime:

$$\alpha_i > g(C_i, \pi_i) - f(B_i).$$

By de-prioritizing immigration-related offenses, PEP reduced the likelihood that non-citizen immigrants *without* a criminal history would be deported, thereby lowering their P_i and thus their cost π_i of reporting an incident to the police. Similarly, PEP reduced P_n for individuals with non-criminal non-citizen contacts, thereby also decreasing these individuals' cost of reporting an incident. Because PEP's goal was to continue detaining criminals, the program likely did not lower P_i for unauthorized victims with criminal histories, nor did it lower P_n for individuals with unauthorized contacts with criminal histories. Overall, though, by lowering π_i for *some* individuals, PEP on average decreased the cost of reporting an offense, and we therefore expect to see the share of incidents reported to increase following the introduction of the program.[23]

[22]For individuals who are citizens and who do not have non-citizen contacts, $\pi_i = 0$ and this term does not alter their decision to report a crime.

[23]It is also plausible that a victim i might consider the immigrant status of offender j in deciding whether to report a crime, so that π_i is also increasing in P_j (see, for example, Iyengar 2009). For these individuals, we should still expect their likelihood of reporting to increase with PEP, although likely by a smaller magnitude than for individuals who do not factor P_j into their decision. Regardless of whether P_j enters into π_i, however, PEP incentivizes immigrant victims and their contacts to report crime by lowering P_i or P_n, and we should therefore expect, on average, an increase in the share of incidents reported.

2.3.2 Endogenous Behavioral Response

By increasing the likelihood of reporting a crime, PEP also in principle affected the number of crimes committed. Here, I consider how PEP's priorities affected the likelihood that an offender would victimize individual ν, where $\nu \in \{H, N\}$ represents Hispanic and non-Hispanic victims.

PEP increased Hispanic victims' likelihood of reporting, and we therefore expect the overall number of crimes committed against Hispanic victims to decrease with the introduction of PEP; γ therefore represents offenders' substitution away from Hispanic to non-Hispanic victims.[24] We also expect an overall decline in the number of crimes in which the offender does not know the victim type (e.g., auto theft of a stranger's car) because the likelihood of getting caught has risen due to increased reporting; this change is represented by β. Finally, there potentially could be additional deterrence effects δ from improved allocation of police resources (as a result of increased crime reporting).

	Change in victimizations
Hispanic victims	$-\gamma - \beta - \delta$
Non-Hispanic victims	$\gamma - \beta - \delta$

Combined, these changes yield two main predictions.[25] First, the change in the victimization rate of Hispanic individuals is unambiguously negative. Second, the change in the victimization rate of Hispanic individuals is unambiguously *more* negative than the change in the victimization rate of non-Hispanic individuals.

In Section 2.5 below, I discuss how these predictions might differ for low-level offenses (e.g., disorderly conduct or vandalism). For a more detailed discussion of these predictions, I refer the reader to Appendix B.

[24] It is also possible that offenders will be deterred from crime altogether, as opposed to switching to non-Hispanic victims. The two main predictions from this subsection are unchanged in this scenario.

[25] Note that the predictions are written as changes in the number of victimizations from a pre-period baseline level.

Finally, it is worth reiterating that ICE's treatment of serious criminal offenders did not change with PEP, and the policy therefore did *not* directly alter the incentives for offenders to commit serious crimes. In this way, PEP differs from other immigration policies—which change the incentives for both victims and offenders—and I conclude that PEP only affected the amount of serious crime committed through its effect on reporting.

2.3.3 Summary of Predictions

The primary goal of PEP was to increase trust between immigrant communities and law enforcement officials. The new priorities lowered the cost of reporting, and I thus expect the share of incidents reported by Hispanic complainants to increase following the launch of the program. By increasing individuals' likelihood of reporting, I also expect that PEP affected the number of crimes committed; overall, I expect the number of crimes committed against Hispanic victims to decrease and for this decline to be more negative than the change in the number of crimes committed against non-Hispanic victims.[26]

2.4 Data and Methodology

2.4.1 Data on Police Incidents

In order to estimate whether PEP had an effect on a Hispanic individual's willingness to report crime to the police, I utilize data on police incidents in Dallas; these data come from the Crime Analysis Unit of the Dallas Police Department and report information on every incident report written by a DPD officer. These data not only include information about the incident (e.g., the offense code and the location's spatial coordinates), but they also include details about the complainant (e.g., his or her full name as well as race/ethnicity). I restrict

[26]These predictions are similar to those in Comino et al. (2016), which considers the effect of granting amnesty to unauthorized immigrants. Their model predicts that the 1986 policy change increased immigrants' likelihood of reporting crime and reduced the number of immigrants who were victims of crime.

the sample to include all reported incidents in which the complainant was categorized as white, black, or Hispanic, and restrict the sample period to the years 2014 through 2016.[27]

Table 2.1 shows the share of police incidents reported by Hispanics and non-Hispanics in Dallas.[28] This table highlights that only 31 percent of incidents are reported by Hispanics, even though roughly 40 percent of Dallas is Hispanic (U.S. Census Bureau 2019a). In contrast, 38 percent of incidents are reported by black complainants, even though only 23 percent of Dallas is black. These differences could arise from differences in crime rates, reporting rates, or a combination of both. Table 2.1 also shows the breakdown of reported incidents into violent crimes (e.g., assault and robbery), property crimes (e.g., burglary and theft), low-level offenses (e.g., criminal mischief, disorderly conduct), and non-criminal incidents (e.g., motor vehicle accidents). Roughly 40 percent of the incidents in these data are classified as violent or property crimes, and another 40 percent are classified as non-criminal incidents. The distribution of crimes is generally similar for both Hispanic and non-Hispanic complainants, although a higher share of reported incidents with non-Hispanic complainants are non-criminal events.

To get a better sense of the spatial distribution of Hispanic residents across Dallas, panel (a) in Appendix Figure 2A.1 shows the share of incidents that have a Hispanic complainant for each tract. Panel (b) uses data from the 2010 Census to display the share of each Census tract's population that is Hispanic. These maps suggests that even though Hispanic communities are concentrated in the southwest and southeast parts of Dallas, most neighborhoods

[27]Individuals with missing race or ethnicity (0.5 percent of the sample) are excluded from the sample. Individuals categorized as belonging to any other group (roughly 3 percent of the sample) are also dropped since it is not clear if or how they were affected by the new enforcement priorities. With regard to the sample period, the number of ICE detainer requests was often spiking in 2012 and 2013 during Secure Communities, suggesting that 2012 and 2013 are not suitable years for the pre-period since the incentives for crime reporting were likely changing then (TRAC 2019). As noted above, PEP was suspended in January of 2017, so I focus on the time period during which the program was active. Doing a similar analysis to the one below with 2017 data shows that the reporting behavior of non-Hispanic individuals seems to change in 2017 (perhaps also because of the new administration), so that non-Hispanic individuals no longer seem like a suitable comparison group in that year.

[28]For the remainder of this paper, "non-Hispanic" refers to complainants categorized as white or black by the DPD.

are at least 15–20 percent Hispanic. Moreover, the fact that these two maps are so similar in representing where Hispanic communities reside suggests that the DPD data are relatively accurate in identifying whether a complainant is Hispanic.

2.4.2 Empirical Strategy

In order to estimate the effect of PEP on the degree to which Hispanic individuals in Dallas reported incidents, I employ a difference-in-differences strategy. The spirit of the analysis is that the immigration policy change only affected Hispanic individuals' incentives to report crime, while leaving the incentives unchanged for white and black individuals. I therefore compare the change in the number of police incidents reported by Hispanic individuals (i.e., the treatment group) with the change for non-Hispanic individuals (i.e., the control group) in the *same* neighborhood. The main assumption underlying this approach is that the reporting behavior of Hispanic and non-Hispanic individuals would have continued to trend similarly in the absence of PEP. This analysis also assumes that unauthorized immigrants as well as individuals with non-citizen contacts in Dallas are more likely to be Hispanic.[29]

Specifically, I estimate the following equation:

$$\ln(\text{Incidents})_{nht} = \beta_0 + \beta_1 \text{Hispanic}_h + \sum_{t=-6}^{6} \theta_t (\text{Hispanic}_h \times \text{Quarter}_t) + \lambda_t + \omega_n + \epsilon_{nht} \quad (2.1)$$

where n, h, and t index neighborhoods, Hispanic ethnicity, and time, respectively. The dependent variable is the logged number of incidents in neighborhood n, in time period t, reported by ethnicity h.[30] Equation (2.1) estimates a coefficient θ_t for each quarter in the

[29] In the 2012–2016 American Community Survey, 80 percent of non-citizen individuals residing in Dallas County Public Use Microdata Areas (PUMAs) were Hispanic (Ruggles et al. 2019).

[30] The level of observation is ethnicity × neighborhood × quarter × year. Some neighborhoods have zero incidents reported by either Hispanics or non-Hispanics in a quarter × year. In order to incorporate these observations into the analysis, I add one to all of the counts and then apply the log transformation. I choose to use the log transformation in order to reduce the influence of outliers and to estimate the average percent change in crime reporting (since there is wide heterogeneity in crime reporting across neighborhoods).

sample, so that I can plot changes in reporting over the entire time period (with the difference in reporting centered at zero in the second quarter of 2015 before PEP is launched).

I also group pre- and post-PEP quarters and quantify the average effects from equation (2.1) by fitting simplified models of the following form:

$$\ln(\text{Incidents})_{nht} = \beta_0 + \beta_1 \text{Hispanic}_h + \beta_2(\text{Hispanic}_h \times \text{Post}_t) + \lambda_t + \omega_n + \epsilon_{nht} \tag{2.2}$$

β_2 is a difference-in-differences estimate, capturing the extent to which changes in reporting before and after PEP differ for Hispanics relative to non-Hispanics.

The specifications include neighborhood fixed effects to absorb level differences in crime across neighborhoods. Both models also include quarter $\times$ year fixed effects to account for time-variant factors, such as the seasonality of crime, that influence the number of police incidents.[31] Standard errors are clustered at the neighborhood level. In all of the main results, a neighborhood is synonymous to a Census tract (of which there are approximately 340 in Dallas, with an average population of 4,000 individuals), but I check the robustness of results to the definition of neighborhood in Section 2.5.

2.4.3 Interpreting the Difference-in-Differences Estimate

In the empirical strategy, β_2 is the coefficient of interest, estimating how the difference in the number of incidents reported by Hispanics and non-Hispanics changes before and after the policy. I now discuss how a positive coefficient implies that Hispanic reporting increased and how β_2 is likely underestimating the increase in crime reporting.

[31] One might think that the desired specification would include neighborhood $\times$ time fixed effects in order to flexibly control for idiosyncratic shocks within a neighborhood and quarter. However, each neighborhood $\times$ time pair in the data has exactly one treatment and one control observation. So by construction, the empirical approach already compares the treatment and control units within a neighborhood and quarter $\times$ year. Including neighborhood $\times$ time fixed effects does not change the treatment-control differences, and therefore mechanically does not affect the difference-in-differences coefficient.

The number of incidents in the data is a function of both the total amount of crime that occurred as well as the share of incidents that were reported:

$$\hat{N}_{ht} = N_{ht} \times P_{ht} \tag{2.3}$$

where $t \in \{0, 1\}$ represents the time before and after the policy change, and $h \in \{H, N\}$ represents Hispanic and non-Hispanic complainants. N_{ht} is therefore the number of crimes committed against ethnicity h in time period t, and P_{ht} is the share of incidents reported by ethnicity h in time period t (or in other words, ethnicity h's probability of reporting an incident to the police).

Empirically, equation (2.2) estimates the difference-in-differences coefficient as follows:

$$\beta_2 = \mathbb{E}[(\ln(\hat{N}_{H,1}) - \ln(\hat{N}_{H,0})) - (\ln(\hat{N}_{N,1}) - \ln(\hat{N}_{N,0}))] \tag{2.4}$$

Plugging in equation (2.3) into equation (2.4) and assuming that $P_{N,0} = P_{N,1}$ (i.e., non-Hispanic complainants' propensity to report crime did not change with PEP), we see that:

$$\beta_2 = \underbrace{\mathbb{E}[\ln(P_{H,1}) - \ln(P_{H,0})]}_{\text{(a): Hispanic reporting effect}} + \underbrace{\mathbb{E}[\ln(N_{H,1}) - \ln(N_{H,0})]}_{\text{(b): Hispanic victimization effect}} - \underbrace{\mathbb{E}[\ln(N_{N,1}) - \ln(N_{N,0})]}_{\text{(c): Non-Hispanic victimization effect}} \tag{2.5}$$

The first term of this expression is the program's effect on Hispanic individuals' propensity to report incidents to the police (i.e., the main parameter of interest). Given the program's goals, we expect this term to be positive. The second and third terms represent the changes in the number of crimes committed against Hispanic and non-Hispanic individuals, respectively. Based on the theoretical predictions in Section 2.3.2, we expect the second and third terms to be negative due to increased reporting and for the change in (b) to be greater in magnitude than the change in (c). These predictions imply that if β_2 is positive, it must be the case that Hispanic individuals' propensity to report incidents increased.

Importantly, the empirical strategy allows us to compare the change in the number of victimizations in the *same* neighborhood. Following the predictions outlined above, if Hispanic individuals are now victimized less often than non-Hispanic individuals in the same neighborhood (i.e., (b) minus (c) is negative), then β_2 will be an underestimate of the increase in the share of incidents that are reported by Hispanic individuals. Alternatively, if the number of victimizations declined equally for Hispanic and non-Hispanic individuals in the same neighborhood (i.e., (b) and (c) are each negative but essentially equal to each other), then we can simply interpret β_2 as the increase in Hispanic complainants' propensity to report incidents in the average neighborhood.

2.5 Results

2.5.1 Increase in Total Number of Reported Incidents

I first plot the raw data to see how the number of incidents reported by Hispanic and non-Hispanic complainants trended over time. Figure 2.3 shows that reporting by Hispanics and non-Hispanics mirror each other from 2014 through the second quarter of 2015. In the third quarter of 2015 however (at the time PEP is launched), reporting by Hispanics begins to increase and diverges from the reporting patterns of non-Hispanics.

To visualize the reporting behavior of Hispanics and non-Hispanics over time after taking into account neighborhood and time fixed effects, panel (a) of Figure 2.4 plots the reporting behavior of the two groups separately. This figure confirms that the reporting behavior of the treatment and control group trended similarly (providing support for the parallel trends assumption), and then deviated with the launch of PEP. Panel (b) of Figure 2.4 plots the θ_t coefficients from equation (2.1), showing the difference in reporting between Hispanic and non-Hispanic complainants over time. The figure shows that after the launch of PEP, the Dallas Police Department saw an increase in the number of police reports with a Hispanic

complainant. The corresponding result in column 1 of Table 2.2 suggests that reporting of incidents by Hispanic complainants increased by around 8 percent (on a baseline of 35 incidents per tract per quarter) following the introduction of PEP.

The 8 percent increase implies that on average, the police was notified of three more incidents in each neighborhood per quarter following the introduction of PEP. A back-of-the-envelope calculation suggests that in the six quarters that PEP was in effect, the police was notified of around 6,000 more incidents than they would have been if the enforcement priorities had not changed.[32] Another way to gauge the magnitude of this estimate is to assume that PEP only affects the behavior of unauthorized immigrants (and not of individuals in their networks); if roughly 25 percent of the Hispanic population in Dallas is unauthorized, then the 8 percent result suggests an implied effect of the policy of 32 percent for unauthorized immigrants.[33]

In terms of comparing this magnitude to those found in other studies, Comino et al. (2016) finds that the 1986 Immigration Reform and Control Act, which provided legal status to millions of immigrants, increased the reporting rate of unauthorized individuals by 20 percentage points.[34] Nevertheless, it is not straightforward to compare PEP's effect to that of other policies since an elasticity of reporting cannot be calculated; although PEP had straightforward goals, it is not obvious who responded to the policy change (i.e., unauthorized immigrants, their families, their networks) and to what degree the cost of reporting a crime fell (i.e., by how much the likelihood of deportation changed with the policy). However, to the extent that building trust is more difficult than perpetuating fear, we would expect

[32]There were roughly 150,000 total incidents in Dallas while PEP was in effect, and roughly 45,000 of those were reported by Hispanic individuals.

[33]The unauthorized share comes from the author's calculations using U.S. Census Bureau 2019b and Migration Policy Institute 2018.

[34]As another point of comparison, Miller and Segal (2019) find that a 7.4 percentage point increase in the female police officer share (corresponding to the increase in average female officer share between 1979 and 1990) increases the reporting of female assaults by 7 percentage points; this estimate corresponds to a 14 percent increase in women's likelihood of reporting an assault.

the findings of this study to be smaller in magnitude than those found in studies about immigration policies' "chilling" effects.[35]

Finally, it is worth noting the timing of the response. The theoretical framework outlined above suggests that individuals will alter their reporting behavior following a change in the immigration-related cost of reporting a crime. Figure 2.4 shows that the number of Hispanic complainants increases following PEP's introduction, even though the enforcement environment began changing with the program's announcement (as seen in Figures 2.1 and 2.2). The timing of the response is thus consistent with individuals' perceived probability of deportation changing with the official adoption of PEP, thereby suggesting that the formality of an immigration policy change might be an important factor for improving trust between immigrant communities and the police.

2.5.2 Changes in Reporting by Crime Type

The incident-level data include all types of police incidents such as assaults, burglaries, drug offenses, and accidents. In this subsection, I classify the incidents into three categories, estimate the policy's effect on reporting for each crime type, and discuss the corresponding estimates and interpretations.

Serious Crimes

I first restrict the sample to incidents that are classified as either violent or property crimes using the Uniform Crime Report (UCR) offense code (roughly 40 percent of all incidents).[36] As noted earlier, since PEP prioritized the removal of offenders of these serious crimes—and

[35] Alsan and Yang (2018) finds that the implementation of Secure Communities resulted in a 10 and 30 percent decline in the take up of food stamps and Supplemental Security Income, respectively, among Hispanic citizen households.

[36] The Dallas Police Department changed its crime classification methods in June of 2014; for the remaining specifications in this and the following two subsections, I begin the sample period with the third quarter of 2014 (although the main findings are relatively unchanged if I maintain the full sample period). If I restrict the sample period in this way for all incidents, the difference-in-differences coefficient in column 2 of Table 2.2 also suggests an 8 percent increase in the reporting of incidents.

thus did not incentivize offenders to commit more of these offenses—a positive β_2 coefficient implies that the program increased the propensity of Hispanic individuals to report serious crimes.

Column 3 of Table 2.2 displays the results; I find an increase of roughly 4 percent in the reporting of these more serious incidents by Hispanic complainants. Moreover, as noted in Section 2.4.3, if we assume that Hispanic individuals were victimized less often than non-Hispanic individuals after PEP—i.e., (b) minus (c) is negative in equation (2.5)—then this 4 percent estimate is likely a lower-bound for the increase in the share of crimes reported by Hispanic individuals. A similar back-of-the-envelope calculation as the one above suggests that the police was notified of around 1,200 more violent or property crimes than they would have been in the absence of the program.

As a placebo check, I consider murders, since we would not expect to see a change in the number of victimizations or in the amount of reporting (because they typically already have high reporting rates) following the introduction of the program. Indeed, column 4 of Table 2.2 shows that the policy has a precisely estimated zero effect on the reporting of murders.

Non-Criminal Incidents

I now consider incidents that are not criminal in nature (another 40 percent of the total incidents in the sample). More specifically, residents of Dallas report incidents to the police that do not represent criminal activity, such as motor vehicle accidents, animal bites, missing persons, and other miscellaneous incidents. Because these events do not typically involve perpetrators, it is straightforward to estimate PEP's effect on individuals' willingness to contact the police. In other words, if we assume that the number of these incidents did not change with PEP, then we can interpret the β_2 estimate as solely reflecting the increased propensity of Hispanic individuals to report non-criminal incidents to the police.[37]

[37] One caveat is that it is possible that this estimate might be a combination of increased reporting and increased human activity (e.g., unauthorized individuals leaving the house more freely), so that (a) and (b)

Column 5 of Table 2.2 shows an increase of 8 percent in the reporting of these non-criminal events. These findings imply that PEP not only incentivized crime reporting, but also incentivized Hispanic individuals, who might have previously been hesitant to interact with law enforcement over non-criminal matters, to report these incidents to the police.

Low-Level Offenses

Finally, I consider low-level offenses (e.g., drunk and disorderly conduct, vandalism), which are roughly 20 percent of total incidents. Given PEP's focus on detaining serious criminals, it is plausible that the new priorities might have decreased the likelihood of deportation for offenders of these less serious incidents, thereby incentivizing unauthorized individuals to commit more low-level offenses. If so, then the β_2 estimate might reflect changes in reporting as well as changes in low-level offending. In the parlance of Section 2.4.3, if both of the victimization terms in equation (2.5) are positive, and if the change in the number of victimizations is larger for Hispanic individuals than for non-Hispanic individuals (e.g., if unauthorized offenders tend to victimize Hispanic individuals), then the β_2 coefficient might reflect both a reporting as well as a victimization effect.[38]

Table 2.2 shows a 5 percent increase in the number of these offenses reported by Hispanic complainants. Interpreting this coefficient—and comparing it to the coefficient for serious offenses—is not straightforward. On the one hand, there might be heterogeneity by crime type in the policy's effect on reporting, so that the difference in the coefficients could partially be capturing this heterogeneity.[39] On the other hand, if we assume homogeneous effects by crime type, then we could conclude that Hispanic complainants' overall propensity to report criminal activity rose by 4 percent and that the program increased Hispanic offenders'

in equation 2.5 would both be positive. If so, then β_2 is an upper-bound on the increase in the probability of reporting non-criminal incidents.

[38]For a lengthier discussion, I refer the reader to Appendix B.

[39]For example, if individuals were already reporting serious crimes to the police before PEP, but were on the margin of reporting less serious incidents, then we would expect to see a larger effect for this latter group.

propensity to commit lower-level offenses by roughly 1 percent. Overall, though, this estimate suggests that the police was notified of around 700 more low-level offenses after the launch of PEP, likely driven by increased reporting but potentially also by increased offending.

2.5.3 Alternate Definitions of Neighborhoods and Hispanic Ethnicity

There are alternative ways to divide Dallas into neighborhoods besides using Census tracts. To check the robustness of the main results, I run the same specification but use bigger and smaller neighborhoods: namely, the Dallas Police Department's police beats (of which there are roughly 235) and reporting areas (of which there are roughly 1,150). The corresponding results in Table 2.3 and Appendix Figure 2A.2 confirm the robustness of the main result, and suggest that the total number of incidents reported by Hispanics increased by approximately 6–10 percent after PEP.

Up to this point, I have been identifying Hispanic complainants using the race/ethnicity recorded by the police. If Hispanic people were more willing to identify themselves as Hispanic to law enforcement or if police officers were more willing to write down that a complainant was Hispanic after PEP, then I would see an increase in the number of Hispanic complainants that was unrelated to an increased willingness to contact the police. To rule out these explanations, I use the last name of the complainant to identify Hispanic ethnicity. Following Arnold et al. (2018), I match the surnames in the DPD data to the 2010 Census genealogical records of surnames. If the probability that a given surname is Hispanic is greater than 80 percent, I identify this complainant as Hispanic.[40] The Hispanic identification written down by the DPD is highly correlated to the Hispanic identification using surnames for all years in the sample period (overall correlation of 0.88), implying that the DPD data are

[40]I am able to match 91 percent of last names to the 2010 record of surnames. For individuals with more than one surname, I match each surname individually and jointly. If any of the probabilities are greater than 80 percent, then I label this individual as Hispanic.

relatively accurate in identifying Hispanic complainants. Table 2.3 and Appendix Figure 2A.3 show that the main results are hardly changed when using surnames, which leads me to conclude that the increase in complaints is indeed driven by a greater number of Hispanic complainants and not by different patterns in identifying Hispanic ethnicity over time.[41]

Additional robustness checks are included in Appendix Tables 2A.1 and 2A.2. In the former, I confirm the robustness of the results by crime type to these alternative definitions of neighborhood and Hispanic ethnicity. The latter table confirms that the findings are robust to only counting each incident once and to using reporting rates as the dependent variable instead of the logged number of incidents.[42]

2.5.4 No Effect on Clearance Rates

As discussed above, PEP's primary goal was to re-establish trust between immigrant communities and the police. In addition to studying changes in the number of incidents reported, I also consider a different component of community trust in the police: changes in cooperative behavior, as proxied by changes in the clearance rate of crimes (i.e., the share of incidents that were "closed" or "cleared by arrest").

Ex-ante, it is not clear if or how we should expect the clearance rate to change with the new policy's priorities. On the one hand, the program might have significantly increased community cooperation, thereby making it easier for the police to solve crimes. On the other

[41] An alternative way to consider this possibility is to see whether the rates of missing ethnicity and names declines after PEP. I find that the share of observations with missing ethnicity or name does not fall after the introduction of the program, thereby suggesting that the results are not driven by improved recording of Hispanic complainants in the incident reports.

[42] One might wonder whether the 8 percent increase actually resulted in reporting *more* crime to the police, or whether these complainants reported crimes that the police would have known about anyway (for example, if non-Hispanic individuals would have also reported the same crime). To test this possibility, column 2 runs the same specification as above, but this time only counting each incident *one* time (in contrast to counting each complainant-incident pair). For incidents with multiple complainants, I assume the incident was reported by a non-Hispanic individual if any of the complainants were non-Hispanic. The main result hardly changes in this specification, suggesting that the increase in Hispanic complaints was likely not driven by double reporting of incidents that the police would have seen anyway.

hand, the program might have alerted the police to more crimes or changed the composition of crimes committed, without necessarily improving the police's ability to solve them.

Panel (a) of Figure 2.5 plots the raw data, showing that the clearance rate for crimes reported by Hispanic and non-Hispanic complainants seem to mirror each other over time. Panel (b) shows the results from an empirical estimation, similar to that of equation (2.1) but using the clearance rate as the dependent variable. This figure illustrates that the clearance rate of incidents reported by Hispanics trended similarly to the clearance rate of incidents reported by non-Hispanics. These graphs therefore indicate that even though the DPD was informed of more incidents by Hispanic complainants after the launch of PEP, it was still able to clear a similar share of them in order to keep the clearance rates across ethnicities comparable. This result mirrors the findings in Cox and Miles (2015) and Hines and Peri (2019), showing that the introduction of the Secure Communities program did not affect police's clearance rates.

Finally, it is worth noting that the lack of a decrease in the clearance rate, together with a higher number of incidents reported, suggest that the DPD was able to clear a larger number of incidents after PEP's introduction. Appendix Table 2A.3 shows that the number of "cleared" incidents increased by 5 percent after the introduction of PEP.

2.6 Ruling Out Alternative Explanations

So far, I have found that the number of incidents reported by Hispanic individuals increased by roughly 4–8 percent after the introduction of PEP. However, if something besides individuals' willingness to report crime changed at the same time that PEP was introduced, then I could be incorrectly attributing this rise in the number of complaints to increased willingness to report crime. In this section, I consider two other possible explanations—increased criminal behavior and population growth—that could result in more Hispanic individuals reporting incidents to the police.

2.6.1 Increasing Criminal Behavior

As noted previously, the reason that PEP is a particularly useful policy for studying an individual's willingness to report crime is that it increases the incentive to report crime, without reducing the punishment for serious criminal offenses. Moreover, the empirical strategy allows me to see how reporting by Hispanic complainants changes relative to reporting by non-Hispanic complainants in the *same* neighborhood, thereby holding overall victimization levels relatively constant in the comparison. Nevertheless, I still consider the possibility that Hispanic offenders decided to commit more crimes at the time of PEP's introduction, so that part of the increase in the number of Hispanic complaints I find is driven by this increase in criminal behavior (because Hispanic offenders often commit crimes against other Hispanic individuals).

To test for this scenario, I use arrest data from the Dallas Police Department to see whether Hispanic individuals seem to be committing more crime after PEP. I first consider the arrests that can be linked to specific incident reports, and focus only on the incidents reported by non-Hispanic complainants.[43] By focusing on white and black individuals whose reporting incentives did not change with PEP, I can isolate changes in the criminal behavior of Hispanic individuals. If the share of arrested individuals who are Hispanic increases after PEP, then this increase would suggest that Hispanic individuals were indeed committing more crime. Figure 2.6(a) shows that the share of arrestees who are Hispanic seems to stay relatively constant after PEP.

Because focusing on crimes with non-Hispanic complainants might reduce the types of crimes considered to select cases, I also consider all arrests (i.e., not just the arrests that can be linked to specific incident reports). The analogous time series is shown in panel (b) of Figure 2.6. Like with the previous graph, the lack of an increase in the share of arrestees

[43]Because most incidents are not immediately resolved with an arrest, only a small share of the incidents in the data can be linked to arrests.

who are Hispanic implies that Hispanic individuals in Dallas did not suddenly commit more crimes following the introduction of PEP relative to non-Hispanic offenders.[44]

Appendix Figure 2A.5 illustrates this point in a different way, plotting the number of Hispanic and non-Hispanic arrestees over time as well as the corresponding difference-in-differences estimates. Panel (b) plots the estimates from the baseline specification (i.e., equation (2.1)) but using the logged number of arrestees of ethnicity h, in neighborhood n, and in time period t as the dependent variable. Because we might be particularly worried that PEP incentivized individuals to commit more low-level offenses (or to shift from serious crimes to low-level offenses), I also show the corresponding difference-in-differences estimate for arrestees charged with misdemeanors (panel (c)). All of these figures confirm that the number and percent of Hispanic arrestees does not increase relative to non-Hispanic arrestees after the introduction of PEP.

It is worth acknowledging that since most criminal offenders are not arrested, any analysis that relies on arrest data is not going to tell a full picture about who is committing crime. However, if the change in policy did incentivize Hispanic offenders to commit more crime, then we should see a corresponding increase in Hispanic arrestees as long as law enforcement officials investigate incidents reported by all ethnicities equally, both before and after the policy change. The fact that we do not see an increase in arrestees—together with the fact that the DPD did not alter its behavior, as evidenced by the lack of a change in clearance rates—lead me to conclude that the increase in reported crime that I find is not driven by Hispanic offenders' increased propensity to commit crime.[45] These findings therefore suggest

[44]I can do this same exercise with data on suspects. Unlike with the arrest-level data, many more incidents have suspects, so the sample size used for this figure is larger. Appendix Figure 2A.4 shows that the share of suspects who are Hispanic—for incidents reported by non-Hispanic complainants and for all incidents—also seems to stay relatively unchanged over this time period.

[45]In other words, if the DPD de-prioritized the investigation of crimes with Hispanic complainants, then we might see an increase in criminal behavior as well as a lack of an increase in Hispanic arrestees. However, the previous result showing that clearance rates did not change for incidents with Hispanic complainants rules out this possibility. Moreover, the null clearance rate result also helps rule out the possibility that Hispanic offenders are only committing crimes for which the clearance rate is nearly zero (thus explaining

that at least in Dallas, PEP achieved its intended goal of increasing trust in law enforcement without adversely impacting crime rates.[46]

2.6.2 Population Growth and Other Economic Factors

The main result of the analysis shows that Hispanic individuals reported more incidents to the police following the introduction of PEP. However, if there was a large influx of Hispanic people into Dallas around this time, then part of the increase in the number of complaints could be driven by growth in the Hispanic population. Previous papers in the literature have indeed shown that migration choices can be responsive to immigration policy. For example, Bohn et al. (2014) show that Arizona's immigrant population declined quickly and significantly after the passage of the 2007 Legal Arizona Workers Act.[47] In the case of Dallas in 2015, sudden growth in the Hispanic population would have had to be the result of the Dallas County Sheriff's local enforcement of the policy (given that PEP was a federal policy).

To test whether there was an influx of Hispanic people into the Dallas area around the time of PEP's introduction, I use the American Community Survey (ACS) for 2012–2016 (Ruggles et al. 2019). Panel (a) of Appendix Figure 2A.6 shows the share of the population that is Hispanic in the Dallas-Forth Worth-Arlington (DFW) metropolitan area and in Dallas County. Panel (b) shows the same time series, but for the share of the population that is Hispanic and not a citizen. The graphs illustrate that the share of Hispanic individuals, and of Hispanic non-citizens in particular, seems to stay relatively unchanged between 2012

the lack of an increase in Hispanic arrestees); in that scenario, we would also see a drop in the clearance rate of incidents with a Hispanic complainant.

[46] I can do a similar analysis using aggregate statistics from the National Crime Victimization Survey (NCVS), which reflects the "true" trends in victimizations (BJS 2021a; BJS 2021b). Even though the sample size of Hispanic individuals reporting an incident is relatively small, the trends in the data show that neither violent nor property victimizations of Hispanic people outpaced those of non-Hispanics in the months following PEP's introduction, both nationally or in the South. These results thus indicate that the increase in reported crime I see in Dallas was likely not driven by Hispanic individuals being victimized more often after the launch of PEP.

[47] Similarly, Watson (2013) shows that foreign-born individuals, especially non-citizens, relocated within the United States following the local adoption of 287(g) agreements.

and 2016. Importantly, it does not appear that there was a significant influx of Hispanic individuals into the DFW metropolitan area or into Dallas County around the time of PEP's launch, which suggests that the increase in Hispanic complainants was likely not driven by population growth.

I confirm this finding by using student enrollment reports from the Texas Education Agency's Public Education Information Management System (PEIMS). Appendix Figure 2A.7 shows that the share of students who are Hispanic in Dallas County and in the Dallas Independent School District stayed relatively constant at 55 percent and 70 percent, respectively, between 2011 and 2017.

Finally, a similar threat to the identification of PEP's effect could be that "treatment" (i.e., being Hispanic) was correlated with other shocks occurring around the same time as PEP's introduction. To explore this possibility, Appendix Figure 2A.8 shows the aggregate unemployment rate in the DFW metropolitan area as well as the annual employment and poverty rates by Hispanic ethnicity in Dallas County.[48] The results confirm that economic outcomes did not deviate differentially for Hispanic and non-Hispanic individuals around the launch of PEP.

2.7 Discussion

The empirical findings of this paper imply that more-targeted immigration policies can incentivize Hispanic individuals to report more incidents to the police. In terms of generalizing these results to other states and localities, it is important to acknowledge that even though PEP was a federal program, it was enforced at the local level by the Dallas County Sheriff's Office. Indeed, shortly after the introduction of PEP, Sheriff Lupe Valdez implemented a

[48]Since 40 percent of Dallas' population is Hispanic, if the unemployment rate of the Hispanic population suddenly increased while that of non-Hispanics stayed constant, then we should expect the aggregate unemployment rate to increase as well.

local policy, in which the Sheriff's Office reviewed each of ICE's detainer requests to ensure that only serious criminals would be detained.[49]

Even though the local policy mirrored PEP's priorities, Sheriff Valdez received significant media attention after introducing these guidelines.[50] Texas Governor Greg Abbott publicly criticized the Sheriff's policy and wrote a letter to Sheriff Valdez demanding that Dallas County unconditionally honor all ICE requests.[51] Sheriff Valdez responded to Governor Abbott's criticism by assuring him that she had not denied any of their requests in the first two months of the policy.[52]

Given the amount of media attention surrounding the clash between the Sheriff and the Governor, it is possible that Hispanic individuals in Dallas were particularly well-informed about the change in the immigration enforcement environment. It is thus plausible that some Hispanic individuals were more willing to report incidents because of the Sheriff's new policy, rather than because of the federal program (even though on paper the two policies were essentially the same).[53] If so, then we might expect increases in reporting to

[49]The Dallas Morning News reported that the Sheriff introduced this policy after meeting with ICE officials as well as activists from the Texas Organizing Project, and that the policy was well-received by the activist group. See: "Activist group commends Dallas County Sheriff's Department for new policy." *The Dallas Morning News*. September 19, 2015.

[50]The Dallas Morning News published at least ten articles between mid-September and mid-December, detailing the widespread support for and criticism of the Sheriff's new policy (articles accessed via Newsbank's archives of *The Dallas Morning News*, 1984–2016).

[51]Following the Governor's letter, support for Sheriff Valdez grew with the hashtag #StandWithLupe. Both the Texas Democratic Party and its collegiate arm, the Texas College Democrats, released statements of support for the Sheriff. The latter group claimed that they "support [Sheriff Valdez] in her efforts to build a strong relationship between the Sheriff's department and the residents of Dallas County."

[52]The sheriff's spokesman noted, "ICE is familiar with our agreement and doesn't submit a detainer unless it falls into the upper two categories that are designated so in effect we have declined zero. See: "Dallas sheriff responds to Texas governor: All ICE detainers honored this year." *The Dallas Morning News*. October 26, 2015.

[53]The Dallas director of the Texas Organizing Project claimed that the Sheriff's policy had "strengthened trust and safety in the community." It is also worth noting that Dallas residents might have been particularly well informed (relative to other localities) because of the Dallas Police Department's Latino Community Outreach program, which provides Hispanic communities with resources and information; each Division Patrol holds quarterly meetings throughout the year and brings in speakers (for example, individuals from Catholic Charities or the Mexican consulate) to talk about immigration laws and other relevant topics with community leaders and other attendees.

perhaps be of different magnitudes in other cities, in accordance with the local salience of the immigration policy among their Hispanic population.

Without similar administrative data from other cities, it is difficult to come to a definitive conclusion about the extent to which the local enforcement of the new priorities contributed to the surge in crime reporting.[54] Nevertheless, the main take-away from the increase we see in Dallas seems to be that introducing immigration policies that shift enforcement attention away from individuals who do not pose a threat to public safety can serve as a way to improve trust between immigrants and law enforcement officials. Improving our understanding of how a policy's local salience can affect the magnitude of the subsequent community response is a fruitful avenue for future research.

2.8 Conclusion

In this paper, I study the launch of the Priority Enforcement Program and estimate the degree to which the program increased Hispanic individuals' willingness to report incidents to the police. PEP was introduced to replace Secure Communities, and thus to re-establish cooperation between local law enforcement and ICE. In order to do so, PEP restricted ICE's enforcement priorities to focus only on individuals who posed a threat to public safety, and not on individuals who had only committed immigration offenses. These narrower priorities therefore reduced the cost of reporting incidents for immigrant victims, without lowering the punishment for serious criminal offenses. Unlike other immigration policies that typically

[54] In theory, one could do a similar analysis with data from the National Incident-Based Reporting System (NIBRS). Unfortunately, the variable for Hispanic ethnicity is often missing in this data and many states with large Hispanic populations (e.g., California, Florida) do not report incidents to this system. Since the difference-in-differences is contingent on accurate and consistent identification of Hispanic individuals over time, administrative data from police departments (that includes a reliable Hispanic identifier as well as names and locations in order to verify the accuracy of the Hispanic identifier) is far better for this analysis. Moreover, the NIBRS data is collected at the agency level, which would not allow me to compare individuals residing in neighborhoods with similar crime levels.

affect both victims and offenders, the introduction of this program allows me to directly estimate the effect of immigration enforcement on crime reporting.

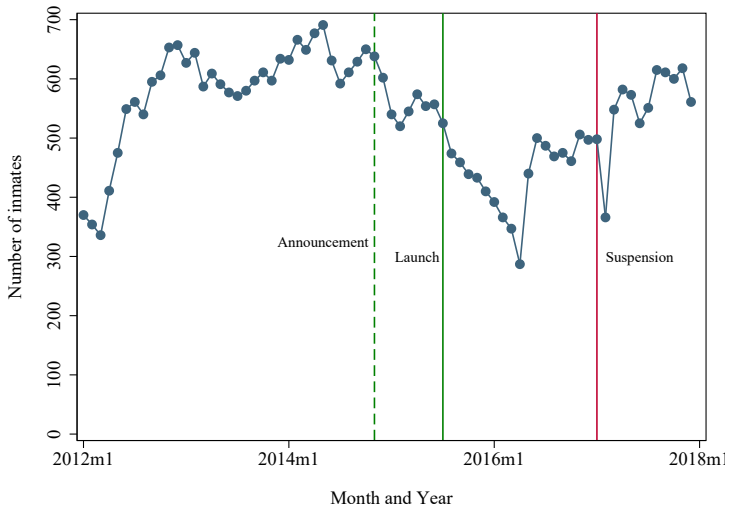
Using incident-level data from the Dallas Police Department that contains the ethnicity of the complainant, I employ a difference-in-differences strategy to estimate the degree to which reporting by Hispanic complainants changed relative to reporting by non-Hispanic complainants in the same neighborhood. I find that after PEP was introduced, the total number of incidents reported by Hispanic individuals increased by around 8 percent. Disaggregating by crime type, I find that PEP increased Hispanic victims' likelihood of reporting violent and property crimes by roughly 4 percent. The results are robust to a number of checks (including using the surname of the complainant to identify Hispanic ethnicity and alternative definitions of neighborhood), and I also rule out alternative explanations for this increase in reporting (namely, increased criminal behavior and population growth). The estimates imply that the Dallas Police Department was notified of around 6,000 more incidents than it would have been otherwise, including 1,200 more violent and property crimes. Moreover, since some criminal offenders might have been dissuaded from committing crime because of the increased reporting, these numbers are likely conservative estimates. Finally, I find that the clearance rate remained unchanged following the introduction of the policy, but because more incidents were reported to the police, the DPD was able to "clear" a larger number of incidents.

Overall, the findings of this paper suggest that lessening immigration enforcement of individuals who do not pose a threat to public safety (i.e., crime victims) can enhance levels of trust between immigrant communities and law enforcement, and thus increase immigrants' propensity to report incidents to the police. Quantifying the program's effect on underlying crime rates is beyond the scope of this paper and an important topic for future research. However, to the extent that increased reporting deterred criminals or helped law enforcement

officials do their job more effectively, this paper provides evidence that tailored immigration policies can play a role in improving public safety and the effectiveness of police departments.

2.9 Figures and Tables

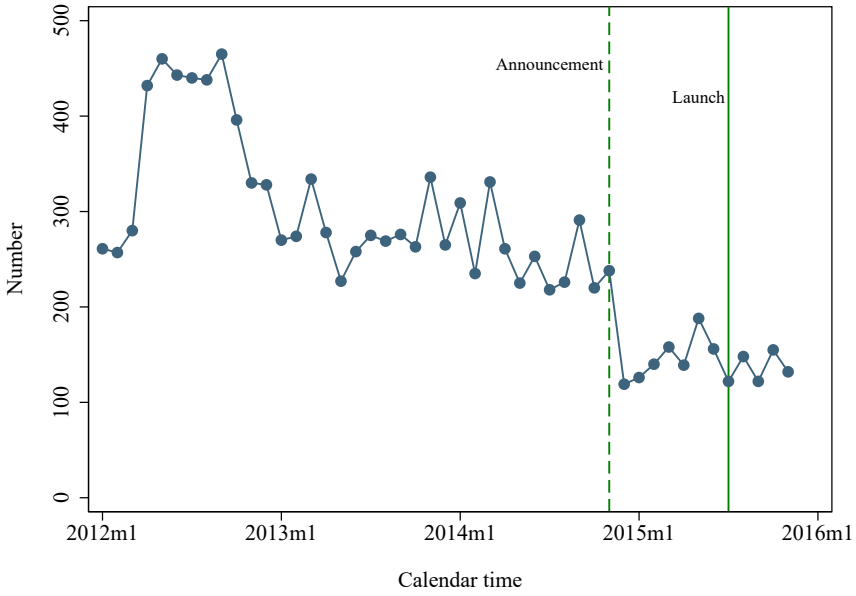
Figure 2.1: Inmates in Dallas County with Immigration Detainers, 2012–2017



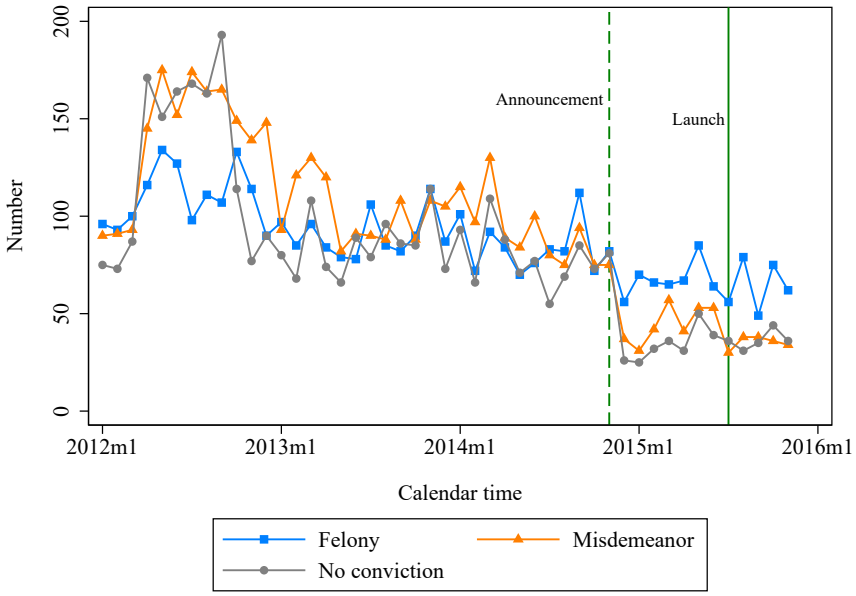
NOTE: This figure plots the number of inmates in Dallas County with immigration detainers (i.e., individuals being held in jail or prison at ICE's request prior to being transferred to federal immigration authorities). Data come from reports for the Texas Commission on Jail Standards. The dashed and solid green lines mark the announcement and launch of PEP, respectively. The red line marks the month in which PEP was suspended.

Figure 2.2: Number of Detainers Issued in Dallas County

(a) Total Number of Detainers

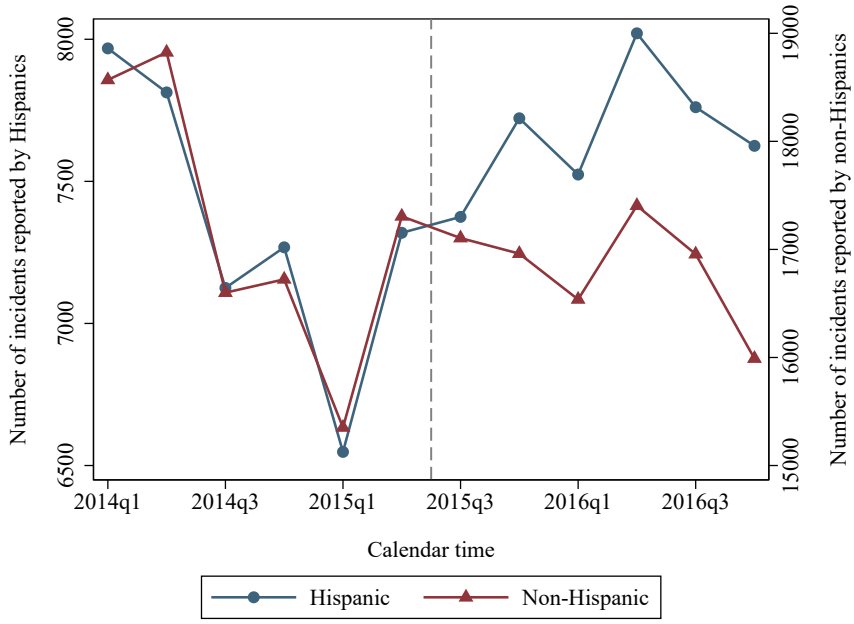


(b) Detainers by Most Serious Criminal Conviction



NOTE: This figure plots the number of detainer requests over time, using data on detainer requests from the Transactional Records Access Clearinghouse (TRAC 2018). "Felony" refers to detainer requests issued for individuals convicted of a felony or aggravated felony. "No conviction" refers to detainer requests issued for individuals who were either charged but not convicted of a crime or neither charged nor convicted of a crime. The vertical dashed and solid lines mark the announcement and launch of PEP, respectively.

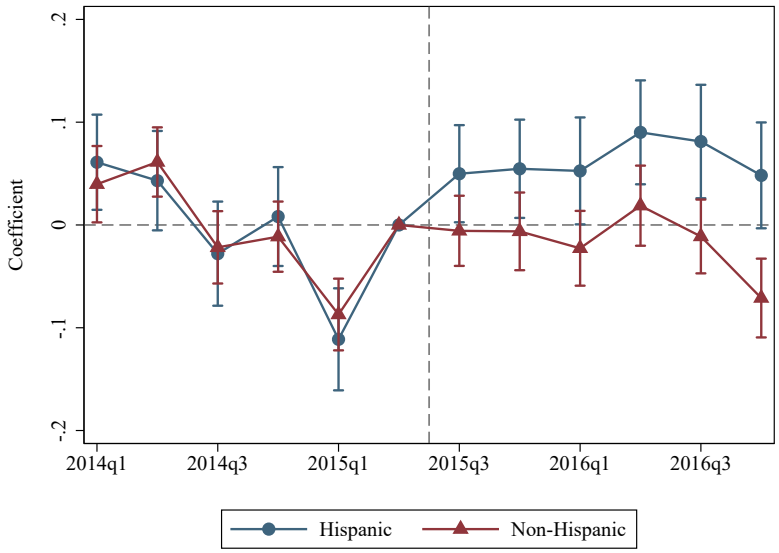
Figure 2.3: Number of Incidents, by Hispanic Ethnicity of the Complainant (Raw Data)



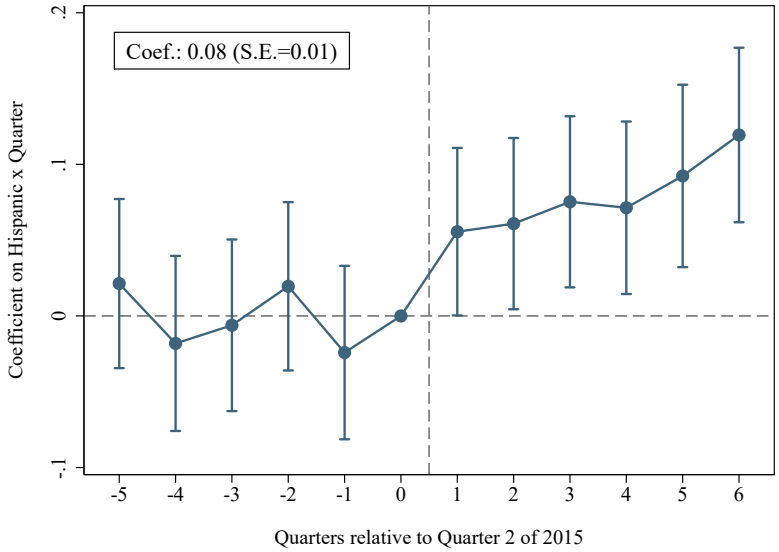
NOTE: This figure plots the number of incidents reported by non-Hispanic and Hispanic complainants (left and right y-axis, respectively). The dashed gray line represents the launch of PEP in the third quarter of 2015.

Figure 2.4: Difference in Crime Reporting for Hispanics and non-Hispanics

(a) Treatment vs. Control



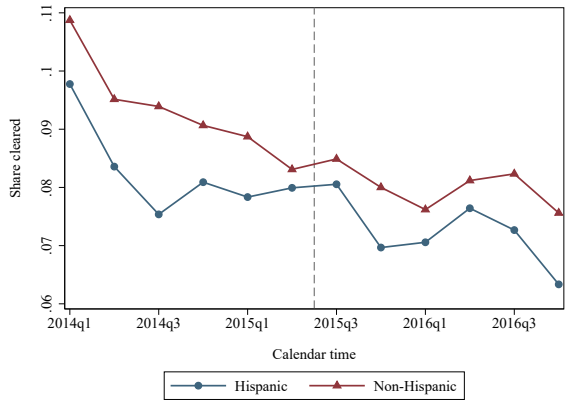
(b) Difference-in-Differences Estimates



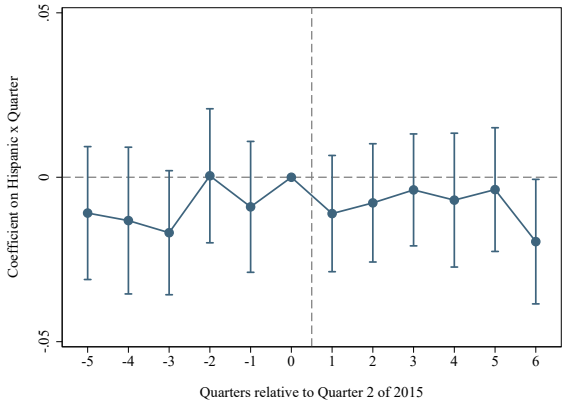
NOTE: This figure uses all police incidents from the Dallas Police Department between 2014 and 2016. Panel (a) plots the time coefficients from equation (2.1) (i.e., reporting by the control group) against the sum of the time coefficients and the corresponding Hispanic × time interactions (i.e., reporting by the treatment group). Panel (b) plots the coefficients on Hispanic × Quarter from equation (2.1). The coefficient and standard error reported are the difference-in-differences estimates from equation (2.2). Standard errors are clustered at the Census tract level.

Figure 2.5: Clearance Rates for Incidents with Hispanic and non-Hispanic Complainants

(a) Raw Data



(b) Difference-in-Differences Estimates



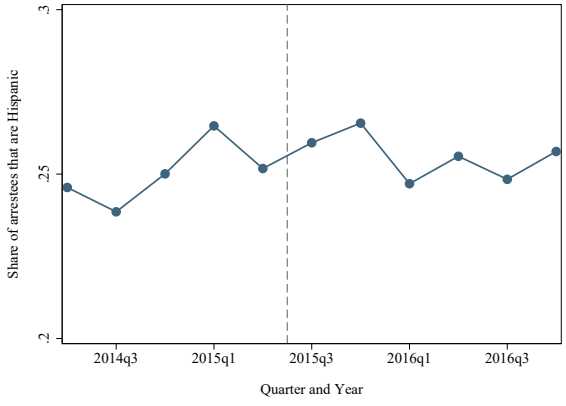
NOTE: This figure uses all police incidents from the Dallas Police Department for 2014–2016. An incident is considered "cleared" if it was cleared by arrest or if it was closed. Panel (a) plots the raw data of clearance rates for incidents reported by Hispanics and non-Hispanics over time. Panel (b) plots the coefficients on Hispanic × Quarter from an analogous version of equation (2.1), which uses the clearance rate of incidents reported by ethnicity h in neighborhood n and in time period t as the dependent variable. Standard errors are clustered at the Census tract level.

Figure 2.6: Share of Arrestees that are Hispanic

(a) For incidents reported by non-Hispanics



(b) All arrests



NOTE: These figures plot the share of arrestees that are Hispanic using arrest data from the Dallas Police Department for 2014–2016. Hispanic arrestees are identified using the race/ethnicity marked down by the DPD. Panel (a) only uses arrests that can be linked to the incident-level data, and whose complainant was non-Hispanic. Panel (b) uses all arrests. The dashed gray line indicates the launch of PEP.

Table 2.1: Summary Statistics for Police Incidents in Dallas, by Ethnicity of Complainant

	All Incidents	Hispanic Complainant	Non-Hispanic Complainant
Hispanic	0.31	.	.
Black	0.38	.	0.55
White	0.31	.	0.45
Violent crime	0.12	0.14	0.12
Property crime	0.30	0.33	0.28
Low-level offense	0.19	0.19	0.19
Non-criminal incident	0.39	0.35	0.41
Observations	294,397	90,069	204,328

NOTE: These data include all incidents for 2014–2016 in which the complainant was classified as white, black, or Hispanic. Non-Hispanic refers to individuals categorized as "white" or "black" in the data. All crimes are classified using the Uniform Crime Report offense code. Violent crime refers to assault, human trafficking, kidnapping, intoxicated manslaughter, murder, robbery, terrorist threats, weapons offenses, and organized crime. Property crime refers to arson, burglary, thefts, and unauthorized use of a motor vehicle (UUMV). Low-level offenses refer to crimes classified as alarm incidents, criminal mischief, criminal trespassing, disorderly conduct, DWIs, embezzlement, escaping/evading, forge and counterfeit, fraud, liquor offenses, lost property, narcotics and drugs, offenses against child, prostitution, resisting arrest, runaway, seized property, and sex offenses. Accidents refers to firearms accidents, motor vehicle accidents, and occupational injuries, among others. Non-criminal incidents refer to all other incidents reported to the police that are not criminal in nature (namely, motor vehicle or firearms accidents, missing persons, injuries, etc.).

Table 2.2: Difference-in-Differences Estimates: Main and by Crime Type

	(1) All Crimes	(2) All Crimes	(3) Violent and Prop. Crimes	(4) Murder	(5) Non-criminal Incidents	(6) Low-level Offenses
Hispanic x Start of PEP	0.080***	0.082***	0.036**	-0.011	0.075***	0.049***
	[0.014]	[0.015]	[0.018]	[0.007]	[0.020]	[0.018]
Mean of Outcome	2.90	2.89	2.16	0.03	2.06	1.56
Average Incidents	35.44	34.84	14.82	0.05	13.20	6.82
Full Sample Period	Yes	No	No	No	No	No
Observations	8,304	6,920	6,920	6,920	6,920	6,920

NOTE: This table uses incident-level data from the Dallas Police Department. The first column shows the main estimate, using all incidents between 2014 and 2016. The rest of the columns restrict the sample period to start in the third quarter of 2014. The second column re-runs the same specification as column 1. The remaining columns split the sample using the Uniform Crime Report offense code for each incident (for more details, see the footnote to Table 2.1). "Average incidents" refers to the average number of incidents per tract per quarter. Standard errors are clustered at the tract level.

Table 2.3: Difference-in-Differences Estimates: Alternative Variable Definitions

	(1) Main Estimate	(2) Police Beats	(3) Reporting Area	(4) Using Names
Hispanic x Start of PEP	0.080***	0.104***	0.062***	0.082***
	[0.014]	[0.015]	[0.011]	[0.015]
Mean of Outcome	2.90	3.69	1.81	2.84
Average Incidents	35.44	52.40	10.64	32.49
Observations	8,304	5,616	27,648	8,280

NOTE: The first column reproduces the main estimate. The second and third column vary the definition of neighborhood. The fourth column uses the complainant's surname to identify whether he or she is Hispanic. "Average incidents" refers to the average number of incidents per tract per quarter. Standard errors are clustered at the beat and reporting areas level in columns 2 and 3, respectively, and at the tract level otherwise.

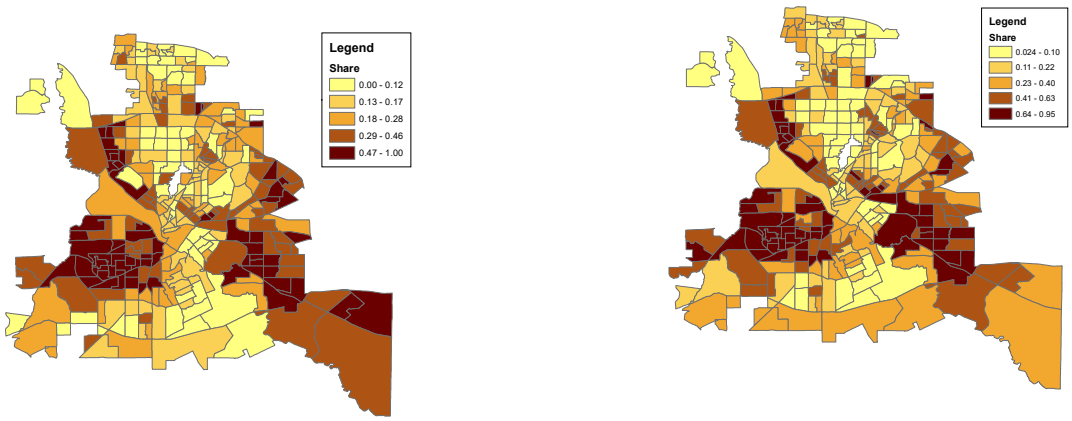
Appendices

A Appendix Figures and Tables

Figure 2A.1: Spatial Distribution of Hispanic Complainants and Population, by Census Tract

(a) Share of incidents with Hispanic complainant

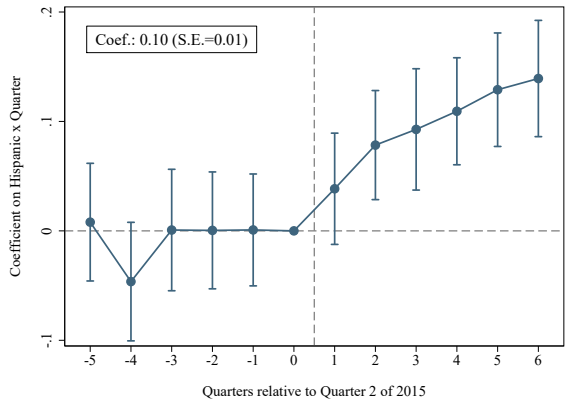
(b) Share of Census Tract that is Hispanic, 2010



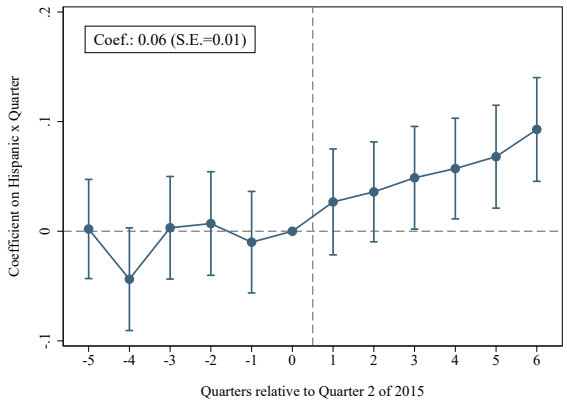
NOTE: The first map uses data on all incidents reported by white, black, and Hispanic individuals between 2014 and 2016. Each color represents a quintile in the corresponding distribution (share of incidents with a Hispanic complainant per tract, respectively). The second panel uses uses tract-level data from the 2010 Census from TIGER/Line Shapefiles (2017).

Figure 2A.2: Difference in Crime Reporting Using Different Definitions of Neighborhoods

(a) Beats

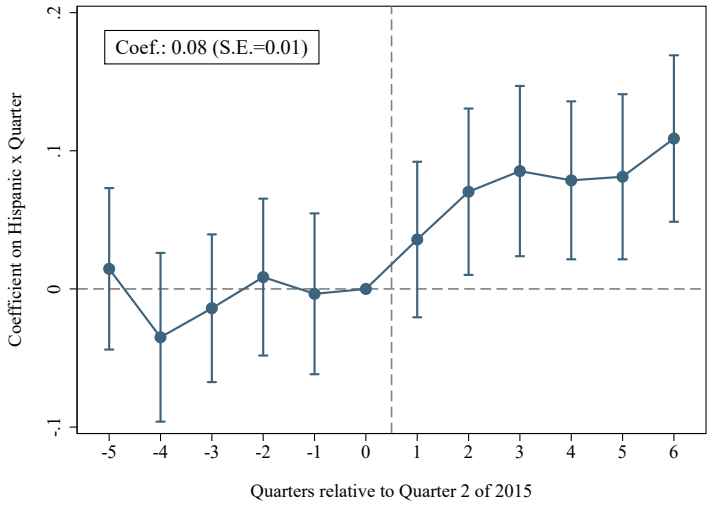


(b) Reporting Areas



NOTE: This figure plots the coefficients on Hispanic × Quarter from equation (2.1) using all police incidents from the Dallas Police Department between 2014 and 2016. The coefficients and standard errors reported in the figures are the difference-in-differences estimates from the corresponding equation (2.2). Standard errors are clustered at the neighborhood level.

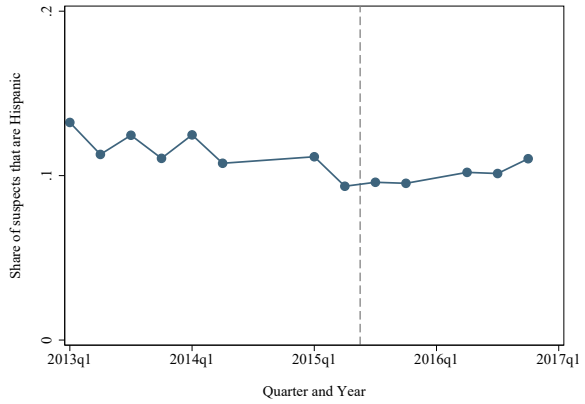
Figure 2A.3: Difference in Crime Reporting Using Complainant Surnames



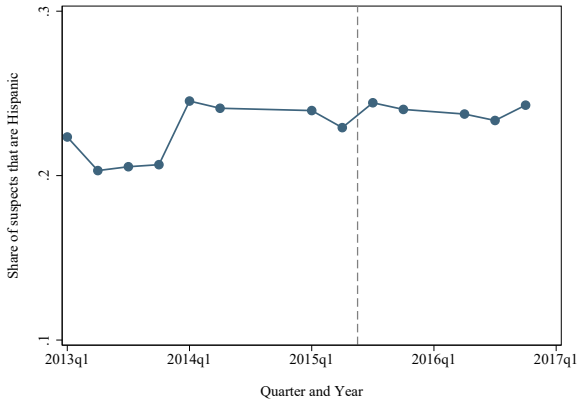
NOTE: This figure plots the coefficients on Hispanic × Quarter from equation (2.1) using all police incidents as well as the surname of the complainant. If the probability that the complainant's last name was Hispanic was greater than 80 percent, the individual is identified as Hispanic. The coefficient and standard error reported in the figure are the estimates from the corresponding equation (2.2). Standard errors are clustered at the Census tract level.

Figure 2A.4: Share of Suspects that are Hispanic

(a) Incidents with non-Hispanic complainant



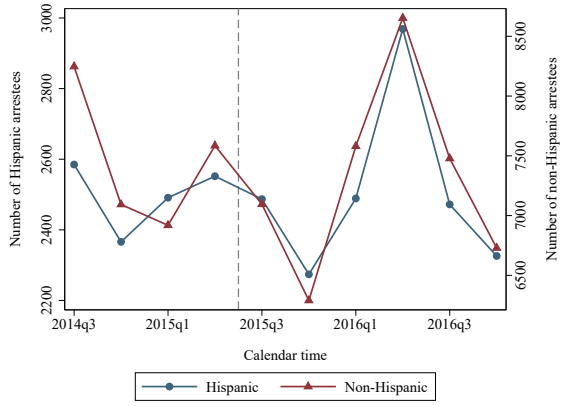
(b) All incidents



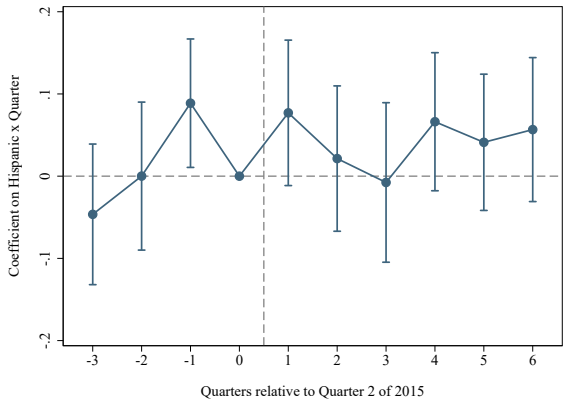
NOTE: These figures plot the share of suspects that are Hispanic using data from the Dallas Police Department for 2013–2016. Hispanic suspects are identified using the race/ethnicity marked down by the DPD. Panel (a) uses suspects for incidents that can be linked to the incident-level data, and whose complainant was non-Hispanic. Panel (b) considers suspects in all incidents. Data are missing for some months in 2014 and 2016 due to changes in the DPD's Records Management System. The dashed gray line indicates the launch of PEP.

Figure 2A.5: Comparison of Hispanic and non-Hispanic Arrestees

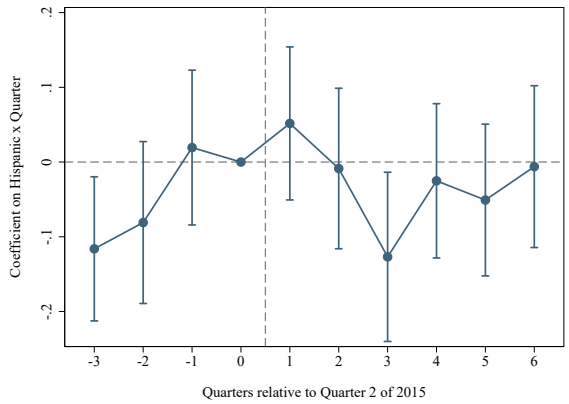
(a) Raw Data, All Arrestees



(b) Difference-in-Differences, All Arrestees



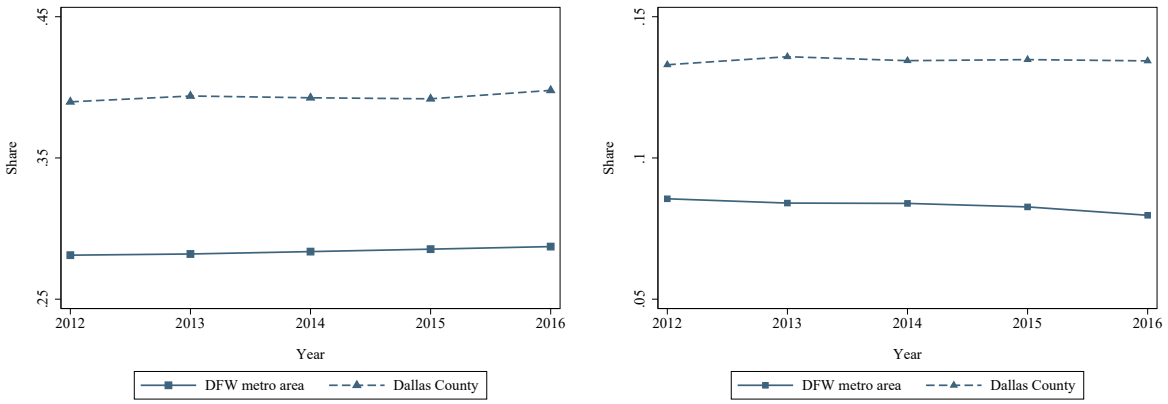
(c) Difference-in-Differences, Misdemeanors



NOTE: This figure uses arrest data from the Dallas Police Department for 2014–2016. An arrestee is identified as Hispanic using the race/ethnicity marked down by the DPD. Panel (a) plots the raw data, showing the number of Hispanic and non-Hispanic arrestees over time. Panel (b) plots the coefficients on Hispanic × Quarter from an analogous version of equation (2.1), which uses the logged number of arrestees of ethnicity h in neighborhood n and in time period t as the dependent variable. Panel (c) restricts the sample to arrestees charged with a misdemeanor crime. Standard errors are clustered at the police beat level. The dashed gray line indicates the launch of PEP.

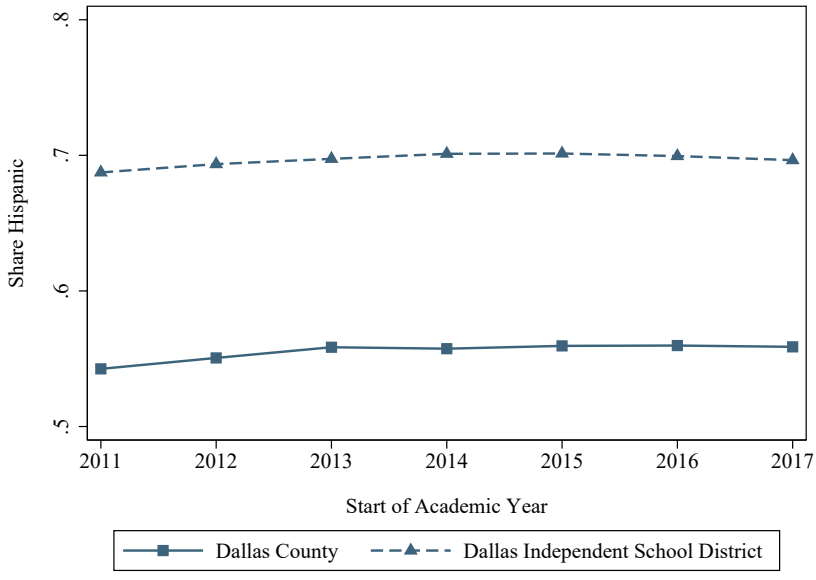
Figure 2A.6: Share of Population that is Hispanic, 2012–2016

(a) Hispanic (b) Hispanic non-citizen



NOTE: This figure uses the 2012–2016 American Community Survey to plot the share of the population that is identified as Hispanic and as Hispanic non-citizen in the metropolitan area of Dallas-Forth Worth-Arlington (DFW) and in the Public Use Microdata Areas (PUMAs) representing Dallas County (2301–2322).

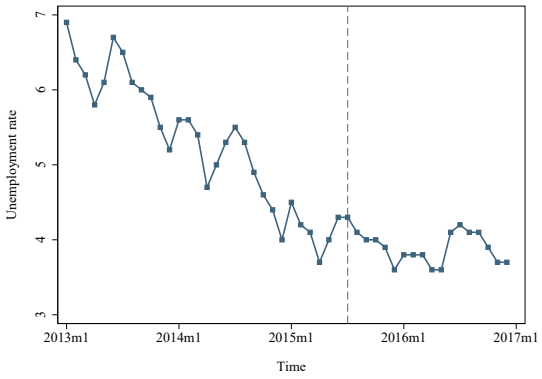
Figure 2A.7: Share of Students that are Hispanic, 2011–2017



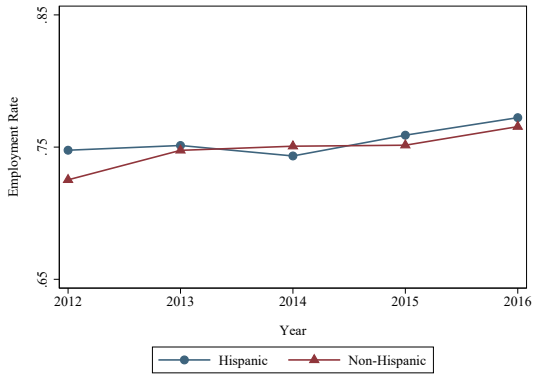
NOTE: This figure uses student enrollment reports from the Texas Education Agency's Public Education Information Management System (PEIMS) to plot the share of students who are Hispanic enrolled in Dallas County and in the Dallas Independent School District.

Figure 2A.8: Employment outcomes in Dallas Area

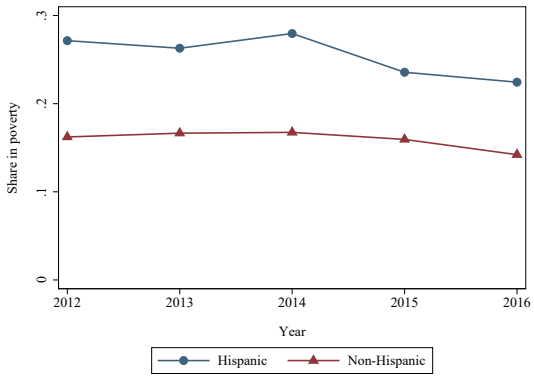
(a) Unemployment rate



(b) Employment rate, by ethnicity



(c) Poverty rate, by ethnicity



NOTE: Panel (a) uses data from U.S. Bureau of Labor Statistics (2018) to plot the monthly unemployment rate in the Dallas-Forth Worth-Arlington (DFW) Metropolitan Area. Panel (b) uses the American Community Survey to plot the share of the Hispanic and non-Hispanic population ages 25–64 that is employed in the Public Use Microdata Areas (PUMAs) representing Dallas County (2301–2322). Panel (c) uses the annual American Community Survey to plot the poverty rate for the Hispanic and non-Hispanic population in the PUMAs representing Dallas County.

Table 2A.1: Difference-in-Differences Estimates by Crime Type: Robustness Checks

(a) Violent and Property Crimes

	(1) Main Estimate	(2) Police Beats	(3) Reporting Area	(4) Using Names
Hispanic x Start of PEP	0.036**	0.044***	0.015	0.040**
	[0.018]	[0.013]	[0.013]	[0.017]
Mean of Outcome	2.16	1.91	1.19	2.10
Average Incidents	14.82	14.61	4.45	13.56
Observations	6,920	14,040	23,040	6,900

(b) Non-criminal Incidents

	(1) Main Estimate	(2) Police Beats	(3) Reporting Area	(4) Using Names
Hispanic x Start of PEP	0.075***	0.069***	0.046***	0.088***
	[0.020]	[0.014]	[0.013]	[0.020]
Mean of Outcome	2.06	1.81	1.12	2.01
Average Incidents	13.20	13.01	3.96	12.13
Observations	6,920	14,040	23,040	6,900

(c) Low-level Offenses

	(1) Main Estimate	(2) Police Beats	(3) Reporting Area	(4) Using Names
Hispanic x Start of PEP	0.049***	0.041***	0.023**	0.052***
	[0.018]	[0.015]	[0.012]	[0.019]
Mean of Outcome	1.56	1.38	0.72	1.52
Average Incidents	6.82	6.72	2.05	6.26
Observations	6,920	14,040	23,040	6,900

NOTE: This table uses incident-level data from the Dallas Police Department. The first column reproduces the main estimate from Table 2.2. The second and third column vary the definition of neighborhood. The fourth column uses the complainant's surname to identify whether he or she is Hispanic. "Average incidents" refers to the average number of incidents per tract per quarter. Standard errors are clustered at the beat and reporting areas level in columns 2 and 3, respectively, and at the Census tract level otherwise.

Table 2A.2: Difference-in-Differences Estimates: Additional Robustness Checks

	(1) Main Estimate	(2) One Complainant	(3) Reporting Rate
Hispanic x Start of PEP	0.080***	0.073***	0.197***
	[0.014]	[0.014]	[0.047]
Mean of Outcome	2.90	2.87	2.32
Average Incidents	35.44	34.23	2.32
Observations	8,304	8,304	7,397

NOTE: This figure uses data from the DPD using all incidents between 2014 and 2016. The first column reproduces the main estimate from Table 2.2. The second column only counts each incident once regardless of the number of complainants (if any of the complainants were non-Hispanic, I assume the incident was reported by a non-Hispanic individual). The third column uses the rate of reporting (i.e., the number of incidents reported by ethnicity h divided by the population of ethnicity h in that tract) as the dependent variable. "Average incidents" refers to the average number of incidents per tract per quarter (except for in the third column, in which it refers to the average reporting rate). Standard errors are clustered at the tract level.

Table 2A.3: Difference-in-Differences Estimates: Clearance Rates and Levels

	Clearance Rate				Logged Number of Clearances			
	(1) All Crimes	(2) Serious Crimes	(3) Non-criminal Incidents	(4) Low-level Offenses	(5) All Crimes	(6) Serious Crimes	(7) Non-criminal Incidents	(8) Low-level Offenses
Hisp. x PEP	-0.003	0.001	0.005	-0.008	0.053**	0.042	0.066***	-0.017
	[0.004]	[0.006]	[0.007]	[0.007]	[0.025]	[0.026]	[0.023]	[0.015]
Mean of Outcome	0.08	0.08	0.09	0.04	1.05	0.64	0.62	0.15
Average Incidents	0.08	0.08	0.09	0.04	3.13	1.47	1.42	0.25
Observations	6,225	5,968	5,952	5,299	6,225	6,225	6,225	6,225

NOTE: This figure uses incident-level data from the Dallas Police Department using all incidents starting in the third quarter of 2014 through 2016. The first four columns use the clearance rate as the dependent variable. Columns 5–8 use the logged number of cleared incidents. "Serious crimes" refers to violent and property crimes. "Average incidents" refers to the average number of cleared incidents per tract per quarter (except for in the first three columns, in which it refers to the average clearance rate). Standard errors are clustered at the tract level.

B Appendix B: Endogenous Behavioral Responses

In this appendix, I more-formally consider the endogenous behavioral response that PEP would likely elicit from criminal offenders. More precisely, the classic Becker model of crime posits that criminal offenders commit crime if the expected utility of committing a crime exceeds the utility from abstention:

$$(1-p)U_{c1} + pU_{c2} > U_{nc}$$

where p is the probability of being punished, U_{c1} is the payoff from committing crime if the individual is not punished, U_{c2} is the payoff from committing crime if the offender is punished, and U_{nc} is the utility associated with the choice to abstain from crime (Becker 1968b).

Response to Increased Reporting

I expand on this framework to allow for victims to have different likelihoods of reporting based on their ethnicity, for offenders to face different punishments based on their immigration status, and for the possibility that offenders know the ethnicity of their victim. In this context, there are two types of offenders α, where $\alpha \in \{A, U\}$ represents authorized and unauthorized offenders. Similarly, there are two types of victims ν, where $\nu \in \{H, N\}$ represents Hispanic and non-Hispanic victims.[55] I consider the two possible relationships between victims and offenders, since they yield slightly different predictions.

[55] As noted above, Hispanic individuals with non-citizen contacts might be less willing to report a crime. For this reason, I divide victims by ethnicity, rather than by immigration status.

Offender Knows Victim Type

In scenarios where offenders know the potential victim's type, the expected utility for offender α of committing a crime against victim ν is:

$$EU_{\nu}^{\alpha} = (1 - p_{\nu})U_{c1}^{\alpha} + p_{\nu}U_{c2}^{\alpha}$$

I assume that $p_N > p_H$ (i.e., Hispanic victims are less likely to report crime as a result of immigration-related costs).[56] This assumption implies that, all else equal, $EU_H^{\alpha} > EU_N^{\alpha}$, so that an offender of type α prefers to commit a crime against a Hispanic individual than against a non-Hispanic individual.

By altering the probability that immigrant victims report crime, PEP increased p_H, and therefore lowered EU_H^{α} for both types of offenders. We therefore expect that the overall number of crimes committed against Hispanic victims likely decreased with the introduction of PEP.[57]

Offender Does Not Know Victim Type

There are also instances in which the offender does not know the potential victim's type. In this case, offender α considers a weighted average of the two possible expected utilities:

$$EU^{\alpha} = S_H \underbrace{[(1 - p_H)U_{c1}^{\alpha} + p_H U_{c2}^{\alpha}]}_{EU_H^{\alpha}} + S_N \underbrace{[(1 - p_N)U_{c1}^{\alpha} + p_N U_{c2}^{\alpha}]}_{EU_N^{\alpha}}$$

[56] I also assume that $U_{c1}^{A} = U_{c1}^{U}$ (i.e., offenders get the same payoff from committing crime if they are not punished, regardless of their type) and $U_{c2}^{A} > U_{c2}^{U}$ (i.e., authorized offenders have a higher payoff from committing crime if they are punished since they do not face the possibility of deportation).

[57] If EU_H^{α} decreases, but $(EU_H^{\alpha})' > EU_N^{\alpha}$, then we expect offenders with relatively high outside options to be deterred from crime altogether, so that the number of crimes against Hispanic victims decreases (and the number of crimes against non-Hispanic offenders remains unchanged). If EU_H^{α} decreases such that $(EU_H^{\alpha})' = EU_N^{\alpha}$, then we might expect some offenders to become indifferent between committing crimes against Hispanic and non-Hispanic individuals. In this scenario, we would expect the number of crimes against Hispanic victims to still decrease overall, but might expect the number of crimes against non-Hispanic victims to rise slightly.

where S_H and S_N are the shares of the population that are Hispanic and non-Hispanic, respectively, in that neighborhood. Similarly to above, we expect PEP to increase p_H, and thus decrease the total expected utility of committing crime for both types of offenders. We therefore expect that the policy will deter some criminal offenders from committing crime against *both* types of victims.

Additional Deterrence Effects

So far, I have considered two scenarios, which suggest that the number of victimizations against Hispanic individuals will decrease and that this decline will be greater than the decrease in the number of victimizations against non-Hispanic victims. I also consider here the possibility that PEP might have had an additional deterrence effect on offenders via an improved allocation of police resources. It is plausible that increased reporting by Hispanic individuals improved law enforcement's effectiveness (e.g., in the distribution of police officers across the city). If so, then the probability of being punished likely increased for all offenders, and we again can expect that the policy will deter both types of offenders from committing crime against both types of victims.

One final possibility is that the introduction of PEP improved immigrants' labor market or safety net participation (Alsan and Yang 2018; East et al. 2018). In such a case, then we might think that PEP increased U_{nc}, and might have therefore deterred some unauthorized immigrants from committing economically motivated crimes. We might therefore expect the policy to have an additional effect, deterring unauthorized offenders from committing crime against both types of victims.

Differing Predictions for Low-Level Offenses

Finally, I note that the predictions up until this point have applied to the likelihood that offenders will be deterred from committing *serious* crimes. However, it is worth acknowledg-

ing that PEP's focus on detaining serious criminals might have also decreased the likelihood of deportation for low-level offenders. In other words, PEP might have increased an unauthorized offender's payoff from committing a low-level offense, U_{c2}^{U}; if the change in U_{c2}^{U} outweighed the change in p_H, such that the expected utility of committing crime, EU^{U}, increased for low-level offenses, then we might expect to see an increase in the overall number of low-level offenses committed. Analogously, for crimes in which the offender knows the victim type, if the change in U_{c2}^{U} outweighs the change in p_H such that EU_{H}^{U} increases for low-level offenses, then an increase in reported crime by Hispanic victims might partially be the result of unauthorized offenders committing more low-level offenses against them. However, an increase in U_{c2}^{U} would also increase EU_{N}^{U}, suggesting that we might also expect to see an increase in reported crime by non-Hispanic victims. Overall, unlike with serious crimes, PEP's priorities might have altered the payoff for committing low-level offenses, and we thus might expect to see an increase in the amount of less-serious offenses reported by Hispanic and non-Hispanic victims.

Chapter 3

Intergenerational Mobility of Immigrants in the US over Two Centuries[1]

3.1 Introduction

Immigrants who move to the United States aspire to offer a better future for their children. Both today and in the past, many immigrants earn less than US-born workers upon first arrival and do not completely catch up within a single generation.[2] However, a defining feature of the "American Dream" is the view that even immigrants who come to the United

[1]This chapter was co-authored with Ran Abramitzky, Leah Boustan, and Santiago Pérez and was published in the *American Economic Review* in February of 2021. We thank the editor, Esther Duflo, for her most valuable guidance, and three anonymous referees for their very useful feedback. We also thank the feedback of Chris Becker, Alvaro Calderón, Bruno Caprettini, Raj Chetty, Jennifer Hunt, Michele Rosenberg, Martin Saavedra, Kjell Salvanes, seminar participants at Copenhagen Business School, Copenhagen University, Munich, Princeton, Norwegian School of Economics in Bergen, Rotterdam, Stanford, Tilburg, and UC Davis, as well as conference participants at the 12th International conference on Migration and Development in Madrid, the "Intergenerational Mobility, Gender, and Family Formation in the Long Run" conference in Oslo, the Social Science History Association annual meeting, and SITE conference on immigration at Stanford. A longer version of the paper including additional results is available as a National Bureau of Economic Research working paper (#26408).

[2]See Abramitzky et al. (2014) on earnings convergence for immigrants in the past and Lubotsky (2007) for more recent immigrants.

States with few resources and little skills have a real chance at improving their children's prospects.

This paper studies the intergenerational mobility of the children of immigrants (the "second-generation"). We ask whether the sons of immigrants achieve earnings parity with the sons of the US-born, and how their relative intergenerational mobility changed between the late 19th century and today.[3] On the one hand, children of immigrants might be in a particularly good position to move up the ladder, given that their parents may earn less than their true talent or ability would suggest (for example if they had little exposure to US education or faced discrimination in the labor market). Immigrant families who recently arrived in the US might also be more footloose, thus allowing them to settle in areas with better prospects for their children. On the other hand, children of immigrants might grow up in segregated neighborhoods, suffer from discrimination themselves, and otherwise enjoy more limited opportunities than the children of the US-born.

We find that, both historically and today, children of immigrants at the bottom of the income distribution have higher rates of upward mobility than children of similarly ranked US-born fathers, and to a strikingly similar degree in each time period. Second-generation immigrants growing up at the 25th percentile of the income distribution end up 5–6 percentile rank points higher than the children of the US-born who were also raised at the same income rank. Second-generation immigrants today exhibit a similar degree of upward mobility, despite notable shifts in countries of origin away from Europe toward Latin America and Asia, as well as major changes in US immigration policy from a regime of nearly open borders (to European immigrants) to one of substantial restrictions.

Our analysis spans 130 years of US history and focuses on three cohorts of immigrants who entered the US during the 19th or 20th centuries. The first two cohorts consist of four million first-generation immigrants observed with their children in the US in either the

[3]In historical data, we are only able to link sons across Census waves, given that daughters often change their names at marriage. Thus, for consistency, we mostly focus on father-son pairs throughout our analysis.

1880 or 1910 Censuses.[4] The 1880 cohort contains mostly immigrants from Northern and Western Europe (e.g., Ireland, Germany, and the UK), whereas the 1910 cohort includes more immigrants from Southern and Eastern Europe thought to have faced greater initial disadvantages in the US labor market.[5] We follow the children of these immigrants to the 1910 and 1940 Censuses, respectively—using information on their name, year of birth, and birthplace—and compare their adult outcomes to those of the children of US-born whites.[6] Because the Censuses did not collect income data before 1940, our analysis of these cohorts relies on computing a number of alternative proxies for individual-level income ("income scores") based on a person's detailed occupation, age, and state of residence.

The third cohort includes children of immigrants born around 1980. Unlike the historical cohorts, immigrants in this modern cohort entered the US during an era of substantial immigration policy restrictions and came mainly from poorer and more ethnically diverse countries in Latin America and Asia. To study this cohort, we use aggregate administrative data made public by the Opportunity Insights project, which is based on links of parents and their nearly six million children (Chetty et al. 2020, Chetty et al. 2018). In these data, we observe children's outcomes in 2014–15 and the outcomes of their parents in 1994–2000. In addition, we use data from the General Social Surveys (GSS), in which we observe the outcomes of children of immigrants and the US-born in 2000–2018. The GSS have smaller sample sizes but include some children of undocumented immigrants as well as information on occupations to facilitate comparisons with the historical data.

[4]Immigrants in these cohorts entered the US before the imposition of strict quotas based on country of origin imposed in 1921, which ended the era of nearly open borders for European migrants and dramatically reduced the inflow of immigrants from Eastern and Southern Europe.

[5]For more background on immigration during the Age of Mass Migration, see Abramitzky and Boustan (2017).

[6]More than 95 percent of immigrants in this period were white. Focusing on US-born whites as the comparison group ensures that the higher mobility of second-generation immigrants that we observe is not due to black-white differences in mobility. Collins and Wanamaker (2017) find that African Americans had much lower rates of upward mobility than whites historically. Thus, including African Americans in the sample would make immigrants' mobility advantage appear even larger.

We start by documenting income score gaps between immigrants and US-born workers. In both the past and present, there is a wide variation in labor market outcomes among first-generation immigrants from different countries. Immigrants from countries like Finland and Norway in the past and Vietnam and the Dominican Republic today earn below the US-born on average, whereas immigrants from countries like England (historical) and India (today) out-earn their US-born counterparts. With a few exceptions, earnings differences for immigrant groups range between +20 and −40 log points. However, for most sending countries in which first-generation immigrants earned less than the US-born, we find that second-generation immigrants catch up or even overtake the earnings of the children of the US-born.

Our main analysis then uses father-son links to compare the average rank that children can expect to reach in the national income distribution, conditional on their parent's income or income-score rank and immigration status (a similar approach to Chetty et al. 2014 and Chetty et al. 2020). In both the past and present, we find that the children of immigrants are more upwardly mobile than the children of the US-born: conditional on the rank of their parents, children of immigrants have a higher expected rank in adulthood. The higher level of upward mobility among children of immigrants is especially meaningful in relatively poor families: The estimated gaps imply that children of immigrants with parents in the 25th percentile have a similar expected rank as children of US-born individuals whose parents were ranked about 15–20 percentiles higher, which is about one half of the contemporaneous gap between blacks and whites in the US (Chetty et al. 2020). We find a smaller mobility gap between children of immigrants and children of the US-born at the top of the income distribution.

Our data enable us to estimate rates of intergenerational mobility separately by an individual's country of origin. When doing so, we find that, both in the past and today, immigrants at the bottom of the income distribution from nearly every sending country

have higher rates of upward mobility than the children of the US-born. Moreover, despite all the changes that have occurred over the last century–more restricted borders, a shift to poorer sending countries relative to the US, the growing importance of services relative to manufacturing—children of immigrants today achieve strikingly similar rates of upward mobility to those of the past. For example, children of poor Mexican and Dominican immigrants today have a similar advantage relative to the children of the US-born as did the children of poor Danish or Swiss immigrants in 1910. This finding suggests that the descendants of immigrants of all backgrounds can eventually integrate into the US economy.

In the last part of the paper, we explore the question of why children of immigrants are more upwardly mobile, focusing primarily on the historical data but comparing with the modern data whenever possible. First, geography matters. Both today and in the past, we find that immigrant parents were more likely than US-born parents to move to areas offering better prospects for their children. In the historical cohorts, the intergenerational gap between immigrants and the US-born is reduced by about 50 percent when comparing children growing up in the same US region (Census division) and disappears entirely when comparing children growing up in the same county. In the modern cohort, the immigrant mobility advantage is 25 percent smaller in the average county than at the national level. Finally, we discuss the role of immigrants' self-selection, and provide suggestive evidence that immigrant fathers were "under-placed" in the income distribution (that is, their earnings did not fully reflect their abilities), thereby giving their children more room to improve.

3.2 Literature

Our paper contributes to the understanding of immigrant assimilation and intergenerational mobility in the US. Closest to our paper is Card et al. (2008), which uses cross-sectional data to compare average outcomes by country of origin for first- and second-generation immigrants in the labor market from 1940 until the mid-1990s. Like this paper, we also

find that the children of immigrants fare better on average than the children of the US-born conditional on the average outcomes of the first generation. Our paper broadens the analysis to study immigrants and their children over more than a century (1880 to 2015). More importantly, our paper adds actual parent-child linkages, enabling us to assess the view that even poor immigrants can substantially improve their children's prospects, as well as explore the channels through which immigrant families achieve their mobility advantage.

Most existing work on economic outcomes during the Age of Mass Migration focuses on assimilation within a generation (see for instance, Ferrie 1997; Hatton 1997; Minns 2000; and Abramitzky et al. 2014). One exception is Ward (2020), who documents persistent outcomes across generations by ancestry (national origin of grandfather) in the early 20th century. We go beyond Ward by contrasting mobility rates for the children of immigrants with those of the US-born, and by comparing mobility rates in both the past and present.[7]

Finally, our paper adds to the broader literature on intergenerational mobility in the United States. A number of papers estimate contemporary levels of intergenerational mobility (e.g., Lee and Solon 2009; Chetty et al. 2014; Chetty et al. 2017; Mazumder 2015; Hilger 2015; and Davis and Mazumder 2017).[8] A series of related studies document historical rates of intergenerational mobility, and compare these historical rates to present-day levels (e.g., Long and Ferrie 2013; Feigenbaum 2015; Feigenbaum 2018; Olivetti and Paserman 2015; Ferrie et al. 2016; Card et al. 2018; Olivetti et al. 2018; Tan 2018; Derenoncourt 2019; and

[7] Borjas (1993) uses cohort-level data from the 1940 and 1970 Censuses to study the intergenerational mobility of immigrants in the US. Perlmann (2005) and Perlmann and Waldinger (1997) compare the outcomes of second-generation immigrants in the past and the present but do not have parent-child linkages. Our work also relates to studies that focus on the intergenerational progress of specific immigrant groups, including the Irish famine immigrants (Collins and Zimran 2019), and Mexican Americans (Duncan et al. 2020; Kosack and Ward 2020).

[8] Bhattacharya and Mazumder (2011), Chetty et al. (2020) and Collins and Wanamaker (2017) focus on black-white differences in mobility, and Hilger (2016) focuses on Asian Americans.

Pérez 2019). Borjas (1992) develops a theory of ethnic capital, which suggests that the social mobility process may differ between immigrants and the US-born.[9]

3.3 Data

3.3.1 Historical Datasets for Studying Economic Mobility

We measure historical mobility rates using two new datasets of linked Census records. The first dataset links sons observed in the 1880 Census to the 1910 Census, and the second links sons observed in the 1910 Census to the 1940 Census.[10] These data allow us to observe an individual's own labor market outcomes during adulthood and his father's labor market outcomes during his childhood. Women cannot be systematically matched across historical Censuses because they typically change their last names after marriage, so the past-present comparison focuses on men. Because the modern data define immigrant status based on father's birthplace, we use the same definition in the historical data for comparability.[11]

Linking Fathers and Sons

To create each linked sample, we matched all males aged 0–16 in a childhood Census (that is, either in 1880 or in 1910) to a later Census (1910 or 1940, respectively) using information on first and last names, age, and state of birth. We use the linking algorithm developed in

[9]Outside of the US, Aydemir et al. (2009) focus on the intergenerational mobility of immigrants to Canada, Bolotnyy and Bratu (2018) on immigrants to Sweden, and Gielen and Webbink (2019) on the Netherlands.

[10]The 1880 Census data are from Sobek et al. (2002). The 1910 and 1940 data are from Ruggles et al. (2018). For reasons of both data availability and historical interest, our analysis does not include immigrants in the 1940–1970 cohorts. First, the 1940 Census is the last Census for which information on names is publicly available, allowing for backward but not forward links from 1940. Second, neither of the sources that we use for the modern period include immigrants in these cohorts: the GSS started collecting data in 1972 and the Opportunity Insights data covers children of immigrants born after 1978. In addition, these cohorts coincide with a period of limited migration into the US: in 1970, only 4.7 percent of the US population was born abroad (compared to nearly 15 percent in the past [1880–1920] and today).

[11]Appendix A and the Online Appendix in the NBER working version of this paper show that the results in both the past and today are similar when defining immigrant status using the mother's place of birth (or using both parents' birthplaces in the historical data when such information is available).

Abramitzky et al. (2012) and Abramitzky et al. (2014), which is explained and evaluated in Abramitzky et al. (2019). We restrict the analysis to father-son pairs in which: (1) both the father and the son were white, (2) the son was living with his father at the time of the earliest Census (so that we can observe a father's labor market outcomes),[12] (3) the father was born in the US or in one of the 17 largest sending countries during this time period,[13] and (4) both the father and the son were aged 30–50 by the time we measure their labor market outcomes.[14] Finally, we exclude father-son pairs with missing information on occupation.[15] Appendix A provides further details on the matching procedure as well as sensitivity checks for possible "false positives" and lack of representativeness.[16]

Appendix Table 3A.1 shows the sample size in each of the historical cohorts as we impose these restrictions. In our baseline samples, we match 23 percent of individuals in the 1880 Census to 1910 and 29 percent of individuals in the 1910 Census to 1940, standard match rates for historical samples given the presence of common names and various causes of non-matches (mortality, return migration, under-enumeration, transcription error; see Abramitzky et al. 2019). After imposing the sample restrictions, our baseline samples

[12]Most children in this age group were living with their father regardless of immigration status: 88 and 90 percent among children with a US-born father in the 1880 and 1910 Census, respectively, and 89 and 90 percent of children with a foreign-born father.

[13]The countries we consider are Austria, Belgium, Canada, Denmark, England, Finland, France, Germany, Ireland, Italy, Norway, Portugal, Russia, Scotland, Sweden, Switzerland, and Wales. These countries are the largest source of immigrants in the 1850–1910 period, accounting for 93 and 88 percent of the foreign-born population in the 1880 and 1910 Censuses, respectively. We restrict the analysis to large sending countries so that we have enough observations to estimate our country-by-country results. Although we refer to the children of immigrants as "the second generation," we do not distinguish between sons born inside and outside of the US, because the Opportunity Insight Data does not include such information (that is, we include both the "1.5 generation" and the "second generation" in our samples of children). The vast majority of sons in our data are born in the US (98 percent in the historical cohorts and 95 percent in the GSS modern cohort).

[14]We restrict our sample to men ages 30–50 in order to measure fathers and sons at the same moment in the earnings lifecycle, and to avoid measuring income when children or parents are too young or old (Mazumder 2005).

[15]Our findings are similar when we assign an income of zero to those with missing occupation (Appendix Figure 3B.2).

[16]In the Appendix, we also show that our results are similar when we instead implement the linking approach developed in Abramitzky et al. (2020). Finally, in the Online Appendix of the NBER version of the paper we show that the results are similar when we construct the historical samples to match the characteristics of the Opportunity Insights sample.

include 1.3 million men in the 1880–1910 (30 percent of whom are second-generation immigrants) and 2.7 million men in 1910–1940 (24 percent of whom are second-generation immigrants). The largest origin countries are Germany, Ireland, and England in the first cohort, and Germany, Russia, and Canada in the second.

Assigning Income Measures in Historical Data

Because the 1940 Census is the first US Census to include information on individual earnings, we need to construct proxies of individual income for fathers and sons in our pre-1940 historical samples (we refer to these proxies as "income scores").[17] Our preferred approach is to use a statistical model to predict income from a rich set of covariates for white men aged 30–50 in the 1940 Census, and then use this model to predict income for men in earlier years (we discuss farmers below, whom we handle separately because there is limited information on farm income in the 1940 Census). In particular, we regress log income in 1940 on a number of fixed effects and complete set of interaction terms using 3-digit occupation, age, and current state of residence as our explanatory variables.[18]

The 1940 income variable excludes income from self-employment. Because the vast majority of farmers are self-employed, we compute income for farmers using a method developed by Collins and Wanamaker (2017). Specifically, we make use of the facts that the 1940 Census records the incomes of farm laborers, and that later Census years record how much farmers earn relative to farm laborers. We thus compute farmer incomes by multiplying the

[17] In our baseline analysis, we also use predicted (rather than actual) income for the sons we observe in the 1940 Census (so as to use a consistent measure across all of our historical samples). However, the results are similar when we instead use individual income data from the 1940 Census (see Appendix Figure 3B.2).

[18] In Appendix B, we show that results are similar when we use a person's country of birth as an additional variable in the income prediction. In all interaction terms, we interact covariates with 1-digit Census occupations and with Census region, instead of state. This method is similar to the machine-learning approach for computing income scores proposed by Saavedra and Twinam (2020). Indeed, the correlation between our predicted earnings and those estimated using their approach is 0.99 for fathers in the 1910–1940 cohort.

income of farm laborers in 1940 with the ratio of earnings for farmers versus farm laborers in the 1960 Census, by region and immigration status.[19]

One limitation of using the 1940 Census to compute income scores for individuals in earlier Censuses is that we assume that the relative rank between income cells is stable from 1880 to 1940. Appendix B shows that results are robust to using more contemporaneous sources to compute income scores, namely the 1901 Cost of Living Survey and the 1900 Census of Agriculture. The appendix also documents that immigrants have a similar mobility advantage when using a sample of father-son pairs for whom we can observe fathers in the 1940 Census and thus can measure actual, rather than only predicted, income for both father and son.[20] Overall, our conclusions are unaffected by the specific income proxies we use.[21] Appendix B provides further details on the construction of the income scores and sensitivity checks.

3.3.2 Modern Datasets for Studying Economic Mobility

Our linked Census data cannot be extended beyond 1940 because confidentiality restrictions dictate that individuals' names are only released to the public 72 years after a Census is

[19]When performing this calculation, we focus on US-born whites and immigrants from the sending countries we include in the paper. We note that Collins and Wanamaker use the 1960 Census (rather than 1950) because the 1950 Census only asked the income question to a small subset of the population and is therefore not useful for this exercise. In addition to adjusting for farmer income, we also follow Collins and Wanamaker (2017) and scale up farm managers' and farm laborers' incomes to account for in-kind transfers. Finally, when using actual rather than predicted income for the sons we observe in the 1940 Census (second row in panel (b) of Appendix Figure 3B.2), we also adjust the income of self-employed non-farmer sons following Collins and Wanamaker (2017). To do so, we calculate the ratio of mean earnings for self-employed non-wage workers in an occupation to the mean earnings of wage workers in the same occupation from the 1960 Census and then use this ratio to scale wage earnings of the self-employed in the 1940 Census.

[20]We do so in Appendix B by considering the *fathers* who are young enough in 1910 that they can be linked to the 1940 Census.

[21]The one exception to this claim is the set of results based on the IPUMS occupational income score, which maps occupations to median income in the 1950 Census. Reliance on the 1950 income distribution is problematic because the incomes of farmers deteriorated between 1880 and 1950 due to secular changes in the agricultural sector, leading farm income derived from 1950 data to be too low for fathers in 1880 or 1910. Appendix Figure 3B.2 shows that once farmers' incomes in the IPUMS occupational score are adjusted so that farmers have the same average rank as in our baseline score, we again see a large immigrant mobility advantage.

taken. Instead, we rely on two other sources of data to follow father-son pairs in the modern labor market.

First, we use publicly available aggregate data from the Opportunity Insights project (Opportunity Insights 2020). These data are based on nearly six million children born between 1978 and 1983, out of whom 310,000 have an immigrant father born in one of the 21 largest sending countries to the US.[22] These data are built by the Opportunity Insights team in two steps: In the first step, the 2000 US Census and the 2005–2015 American Community Survey (ACS) are linked to federal income tax returns (using a person identifier assigned by the Census Bureau). This procedure enables the researchers to observe a person's income from the tax records as well as his or her race and place of birth from the Census or the ACS. In the second step, the Opportunity Insights team obtained information on the family income of an individual's parents during his/her childhood. A parent is defined as the first person who claims an individual as a dependent in the federal income tax records between the years 1994 and 2015. Note that this procedure for linking parents to children will exclude pairs in which either the child or the parents lack a Social Security Number. As a consequence, this sample only includes children who are either US citizens or authorized immigrants, and whose parents are also US citizens or authorized immigrants.

Income for this cohort is measured as the average annual income from 2014 to 2015, when these children were 31 to 37 years old, whereas parental income is measured as the average household income from 1994 to 2000 (father's income is not reported separately in this dataset). We use the publicly available version of the data, which reports information collapsed by ventile bin in the national income distribution for immigrants from 21 countries of origin.[23] Hence, for each parental ranking, we observe the average income rank of children

[22] We use the 21 countries for which the Opportunity Insights project reports children outcomes by ventile of the parental income distribution. The countries are Canada, China, Colombia, Cuba, the Dominican Republic, Ecuador, El Salvador, Germany, Greece, Guatemala, Haiti, India, Israel, Italy, Jamaica, Japan, Mexico, the Philippines, South Korea, the UK, and Vietnam.

[23] These data correspond to the "Non-Parametric Estimates of Income Ranks for Second Generation Immigrant Children by Parent Income, Country of Origin, and Gender" and "Parametric Estimates of Income

whose parents fall into that ventile, by a father's country of origin. When focusing on country-by-country outcomes at the 25th and 75th percentiles (rather than on outcomes throughout the full parental income distribution), we are able to expand our analysis to 51 countries of origin.

The Opportunity Insights dataset has several attractive features for the study of the intergenerational mobility of immigrants: It is very large relative to other panel datasets (no other longitudinal dataset would enable us to look at mobility differences by country of origin in the modern period), and includes administrative income data both for parents and their children.[24] However, the dataset has two limitations for our purposes. First, it does not include information on individual-level occupations—the main labor market outcome we observe in the historical data—thus complicating our past-present comparisons of intergenerational mobility. Second, because unauthorized immigrants do not have a Social Security Number (SSN), these data exclude the children of unauthorized immigrants. This limitation affects Hispanic immigrants more than other groups. Specifically, Chetty et al. (2020) compares the Opportunity Insights data to the American Community Survey and reports that they include about 79 percent of Hispanics, whereas the coverage is close to 100 percent for other ethnic groups. We add data from the General Social Survey (described below) as well as cross-sectional data from the Current Population Survey, both of which include unauthorized immigrants, to partially address this concern. We also note that the Opportunity Insights data do include first-generation migrants (and their children) who were at some point undocumented, but who later on became authorized and hence received a Social Security Number. For example, the data include first-generation immigrants who were

Ranks for Second Generation Immigrant Children by Parent Income, Country of Origin, and Gender" files available on the Opportunity Insights website at `https://opportunityinsights.org/data/`.

[24]For instance, the PSID (which includes information on parental income) started in 1968 and thus does not have much coverage of immigrant families who arrived in the US after the border reopened to immigration in 1965. Other panel datasets (for instance, the NLSY used in Borjas 1993) include retrospective questions (asked of the children) about the occupation and education in the parent's generation, but do not contain information on parental income.

in the country by 1982 and benefited from the 1986 Immigration Reform and Control Act amnesty of 2.7 million previously unauthorized immigrants, a number that represents nearly all of the undocumented immigrants living in the US at the time (Passel 1986).[25]

We supplement our analysis of the Opportunity Insights data with data from the General Social Survey (GSS). Although the GSS (Smith et al. 2019b) has substantially smaller sample sizes, it includes questions on occupation and covers unauthorized immigrants, both of which facilitate comparison with the historical data.[26] The survey includes information on a respondent's occupation, parents' places of birth (in the US or abroad), and retrospective questions on parents' occupations when the respondent was 16 years old. We use this information to assign income proxies for fathers and their sons using an analogous procedure to the one we implement with the historical data. To make this sample as comparable as possible to the historical data, we focus on respondents who: (1) are males, (2) lived in the US by age 16, (3) were 30 to 50 years old at the time of the survey, and (4) have available information on their and their fathers' occupations. Finally, to maximize comparability with the Opportunity Insights sample while retaining enough observations, we restrict the analysis to survey years 2000 to 2018. After imposing these sample restrictions, our GSS sample includes 3,100 respondents, nine percent of whom are second-generation immigrants.

[25]Conceptually, we might expect the children of undocumented immigrants to be even more upwardly mobile than the children of authorized migrants. As we document below, lack of US-specific human capital among first-generation immigrants partly explains the high mobility among their children (i.e., first-generation immigrants are "under-placed" in the US income distribution). Given the restrictions that unauthorized immigrants face in the labor market, we might expect them to be even more "under-placed" in the US labor market relative to their true abilities, making their children (who will often be born in the US and have citizenship) especially likely to be upwardly mobile.

[26]We use the 2006–2015 CPS to create income proxies for fathers in the GSS sample. To do so, we regress logged total earned income on a number of fixed effects and complete set of interaction terms using occupation, age, region, and immigrant status (and their interaction terms) as our explanatory variables. We then use this regression to predict income scores for GSS respondents and their fathers. We note that the GSS reports parental immigrant status but does not record country of origin and that the most precise measure of geography in the GSS is region. In addition, the GSS does not include information on parental age.

3.4 Preliminary Evidence: Earning Gaps between Immigrants and US-born since 1880

We start by estimating earning gaps between US-born men and first- and second-generation immigrants by country of origin. For this exercise, we use cross-sectional samples, rather than father-son pairs.[27] Our interest is in documenting the average earnings disadvantage faced by first-generation immigrants by country of origin, and then to ask whether the average child of these immigrants was able to erase some or all of these gaps by the next generation. Specifically, we estimate the following equation, first for the father's generation and then for the son's:

$$Y_{iac} = \alpha + \sum_{c=1}^{N} \beta_c \text{Country}_c + \delta_a + \epsilon_{iac} \tag{3.1}$$

where Y_{iac} is the logged income score of father or son i of age a from sending country c. The β_c coefficients reveal the average difference in income score between US-born individuals and immigrants from country c. All regressions include a quadratic in father's or son's age δ_a.

Figure 3.1 plots the β_c coefficients for first-generation (in black) and second-generation (in gray) immigrants in each cohort. For most countries of origin and time periods, the gaps in income scores between first-generation immigrants and US-born men range between -40 log points and +20 log points. In the modern cohorts, four countries faced larger gaps of about -60 log points (the Dominican Republic, Haiti, Mexico, and Vietnam). In the past,

[27] For our historical cohorts, we use the 1880, 1910, and 1940 Censuses of Population. For the modern cohort, we use the 1980 Census for the fathers' generation and the CPS's Annual Social and Economic Supplement for 2006–2015 for the sons (Flood et al. 2018 and Ruggles et al. 2018). We restrict the 1980 Census sample to men aged 30–50, who have a son present in the household, and who were born in the US or in one of the top sending countries identified in the Opportunity Insights sample. We restrict the CPS sample to US-born men aged 30–50 and whose fathers were either born in the US or in one of the top sending countries. For both of these datasets, we construct occupation-based income scores using an analogous procedure to the one that we use in both the GSS and the historical data. In the appendix to the NBER version of the paper, we show that the results are similar when using actual income whenever possible (Figure A1) and when excluding the US South from the historical samples (Figure A4).

immigrants from Scandinavian countries faced the largest earnings penalties. In all cohorts, immigrants from a set of sending countries outperform the US-born in the labor market. Today, these high-earning countries include Canada, Germany, India, Japan, and the United Kingdom, whereas in the past the highest-earning immigrants were from countries such as England and Scotland.

In each of the three cohorts, second-generation immigrants from countries in which first-generation immigrants earned less than the US-born close or even reverse the corresponding earnings gap in the second generation, leading children of immigrants to out-earn children of US-born individuals. There are only a few cases in which the gap remained above 5 log points by the second generation (Norwegians in 1910 and 1940, Finns in 1940, and Jamaicans, Haitians, and Mexicans in the modern data), but for most sending countries, the earnings gap is substantially smaller by the second generation.

For countries whose first-generation immigrants already out-earned US-born individuals in the labor market, the pattern is more mixed, with the corresponding earnings advantage becoming smaller for some countries, remaining similar in size for others, or even growing by the second generation in some cases. Although earnings gaps for countries that start below the US-born diminish substantially across generations, it is still the case that the children of low-earning immigrants tend to earn less than the children of high-earning immigrants, consistent with the persistence results in Abramitzky et al. (2014) and Ward (2020).

3.5 Main Results: Intergenerational Mobility of Immigrants since 1880

We have so far considered average earnings gaps, without any regard for the parents' rank in the income distribution. The higher earnings growth for the children of immigrants relative to the children of US-born parents in Figure 3.1 could be driven by higher rates

of upward mobility from the bottom of the income distribution, lower rates of downward mobility from the top of the income distribution, or both. The essence of the American Dream is that even immigrants at the bottom of the income distribution can have children that succeed. Thus, we turn to the linked data, both historical and modern, to study the intergenerational mobility of children of immigrants and US-born individuals whose parents had comparable labor market outcomes, as measured by their rank in the national income distribution (following Chetty et al. 2014 and Chetty et al. 2020).

To do so, we first rank each son based on his income score (or actual income when using the Opportunity Insights data), relative to other sons born in the same year. Similarly, we rank fathers relative to all other fathers with sons born in the same year.[28] Both when ranking sons and when ranking fathers, the ranks are based on a person's position in the national income distribution.[29] We then estimate a model in which we regress a son's rank on his father's rank, allowing both the slope and intercept to differ for sons of immigrants and US-born individuals:

$$\text{Rank Son} = \alpha + \beta_0 \text{2nd Gen.} + \beta_1 \text{Rank Father} + \beta_2 \text{2nd Gen.} \times \text{Rank Father} + \epsilon \qquad (3.2)$$

The constant term α measures absolute rank mobility for the children of the US-born, that is, the expected rank of children of US-born individuals with fathers at the very bottom of the income distribution. The parameter β_0 captures the degree to which this expected rank is different for the children of immigrants. The parameter β_1 measures the rate of relative mobility of children of US-born individuals, or the association between the ranks of

[28] In the Opportunity Insights data, we use family income rank because we do not have separate information on a father's income rank.

[29] If there are individuals in the sample who have the same father, we assign the father the average rank across his sons. Due to the GSS's much more limited sample size compared to the historical linked data and the Opportunity Insights data, we pool all survey years (2000 to 2018) and include survey-year fixed effects in the regressions.

children and those of their fathers. Finally, β_2 measures the degree to which this association is different for immigrants and their children.

We compare the economic mobility of the sons of immigrants to the sons of the US-born in Figure 3.2. To do so, we plot the regression lines corresponding to equation (3.2), separately for each of our cohorts, as well as a binned scatterplot showing the mean income rank of the children of immigrants and of the US-born by parental income ventile rank. Panels (a) and (b) suggest that in the past, the sons of immigrants on average out-earned comparable sons of US-born individuals throughout the income distribution, and particularly for sons of fathers with below-median rank. Second-generation immigrant sons with fathers at the 25th percentile in 1880 and 1910 reached an average income rank that was 6 percentiles above the average rank reached by sons of the US-born with similar fathers' incomes.[30] Panels (c) and (d) show the mobility patterns for the modern cohort when using the GSS and Opportunity Insights data, respectively. Today, the sons of immigrants remain more upwardly mobile than their counterparts with US-born parents, and this pattern also mainly holds in the bottom half of the income distribution. In particular, and similar to the historical results, sons of immigrants in the 25th percentile rank about 5 percentile points higher than sons of US-born individuals, regardless of whether we use the GSS or the Opportunity Insights data.

Table 3.1 reports the estimated values of the coefficients corresponding to equation (3.2) for each of the samples. For all three cohorts, we find a higher intercept (a positive β_0) for the sons of immigrants relative to the children of the US-born, indicating higher levels of absolute mobility. The difference in intercepts ranges from 7 to 8 percentiles in all cohorts. The slopes for immigrant families are in all cases smaller than the slopes for the US-born (a

[30] We also find that the sons of unskilled immigrant fathers moved into white-collar and skilled blue-collar jobs at higher rates than the sons of unskilled US-born fathers, who were more likely to transition into farming (see the Online Appendix of the NBER version of the paper).

negative β_2), suggesting a weaker association between a father's and his son's rank among immigrant families.

In Figure 3.3, we allow the intercept and the slope of the rank-rank relationship to differ by country of origin, both in the historical and the modern data. We then use these estimates to compute the expected income rank for sons born at the 25th percentile for each country of origin.[31] The figure shows that sons from nearly all sending countries in the past and the present (except Norway and Belgium in 1880, Norway in 1910, and Haiti, Trinidad and Tobago, and Jamaica in the modern cohort) had a higher expected rank than the children of US-born individuals with parents at the 25th percentile.[32] Immigrants from Asian countries, notably China and India, appear especially upwardly mobile in the current period.[33]

Figure 3.4 shows that the sons of immigrants and the US-born appear more similar when looking at those who grew up with parents in the 75th percentile of the income distribution, particularly in the modern period. In the past, sons of immigrants had slightly higher expected ranks, and today the two groups achieve similar expected ranks. At the top of the income distribution, sons of immigrants from some sending countries like Portugal and

[31] In this exercise, we focus on the Opportunity Insights for the modern data because the GSS data does not record the father's country of origin (only whether the father is foreign-born).

[32] One noticeable pattern from Figure 3.3 is that the only three countries where sons of immigrants at the 25th percentile have a lower expected rank than the sons of the US-born are countries where a significant share of the population is black. This pattern is consistent with a broader pattern of black-white mobility gaps in the US (see for example Chetty et al. 2020). In Figure 5 of the NBER version of this paper, we also show mobility patterns by immigrant race and ethnicity. In contrast, when focusing on daughters, we no longer observe that the children of immigrants from Caribbean countries with a significant black population have a lower expected rank than the children of the US-born (see Figure 3.5 in this paper).

[33] While this paper cannot explain why immigrants from some countries are more mobile than others, it is natural to ask if factors such as linguistic, physical, and religious distance from the US play a role in immigrants' mobility. For each of the countries in our data, we calculated: (1) linguistic distance to the US using the measure from Chiswick and Miller (2005), (2) physical distance to the US, and (3) an indicator that takes a value of one for predominantly Protestant countries. In the modern data, there is a clear positive correlation between all distance measures and mobility. This correlation is largely driven by Asian countries, which are both very distanced in all measures from the US and whose immigrants are very upwardly mobile. In contrast, in the past there is little correlation between the various distance measures and mobility. This is mainly because in the past all sending countries (besides Canada) were in Europe, so there is very little variation in their physical distance to the US. The main variation in linguistic distance in the past is driven by whether the country is English speaking, but immigrants from English-speaking countries did not seem to be more or less upwardly mobile than the average immigrant.

Belgium in the past and Jamaica, El Salvador, and Mexico today did worse than the sons of the US-born, whereas those from other countries like Russia and Ireland in the past and India and China today did better.

The modern data also enable us to look at the intergenerational mobility of the daughters of immigrants. Panel (a) of Figure 3.5 suggests that daughters of immigrants on average out-earned daughters of US-born individuals throughout the income distribution. Panel (b) shows the expected income rank for daughters born at the 25th percentile for each country of origin. As for sons, we find that daughters of immigrants from nearly every sending country achieve higher levels of upward mobility than the daughters of the US-born.

Overall, we conclude that both in the past and today, second-generation immigrants are more upwardly mobile than the children of the US-born. This advantage is not driven by children of immigrants from certain ethnicities or country-of-origin groups: there is higher upward mobility among children of immigrants from nearly every sending country, and particularly at the bottom of the income distribution.

3.6 Why are children of immigrants more upwardly mobile?

3.6.1 Location Choices in the First Generation

Figure 3.6 documents that both historically and today, immigrants tend to settle in areas that offer higher levels of upward mobility for their children. Specifically, the correlation between the share of migrants in an area and the share of upwardly mobile sons is 0.36 for the 1880 cohort, 0.51 for the 1910 cohort, and 0.23 for the most recent cohort (upward mobility is defined here as the share of sons of US-born fathers in the bottom fifth of the

national income distribution who reach the top fifth of the distribution).[34] Immigrants in the modern cohort continue to settle in places that offer strong prospects for upward mobility, but there are also a number of high-mobility locations (e.g., Wyoming and Utah) that do not attract many immigrants today.

We next illustrate more explicitly that differences in the geographic distribution of immigrants and US-born individuals might explain the mobility gap between the two groups. To do so, we re-run our rank-based specification from equation (3.2), and sequentially add fixed effects for the US South, Census division, state, state-by-urban status, and county, all of which are based on an individual's childhood location (we only have data to run these specifications for the historical cohorts). Regressions without location fixed effects reveal the mobility gap between children of immigrants and children of the US-born at the national level. Regressions with location fixed effects measure the gap between children who grew up in the same location. If immigrant fathers chose to settle in areas that offered high prospects for mobility overall, we expect the gap to be smaller in the regressions with location fixed effects. Figure 3.7 shows that indeed including fixed effects for the US South or for Census division diminishes the intergenerational gaps at the 25th percentile by roughly 50 percent in both historical cohorts.[35] The intergenerational gaps are further reduced when we add childhood state fixed effects (fourth bar of each graph), and they fully close when we further include either childhood state-by-urban status (fifth bar) or childhood county fixed effects (sixth bar). We thus conclude that immigrant parents' location choice was an important driver of the immigrant mobility advantage we see in the past.

In the modern data, we are more limited in our ability to illustrate the role of geography in explaining the mobility advantage for the children of immigrants. Specifically, the GSS survey

[34]Rates of intergenerational mobility both historically and today vary greatly by region (Chetty et al. 2017; Tan 2018; Connor 2020). Previous research has also found that first-generation immigrants in the modern period are more likely than the US-born to settle in regions that offer them higher wages (Borjas 2001; Cadena and Kovak 2016).

[35]The gaps also diminish substantially when we focus on children at the 50th and 75th percentile of the income distribution (see Appendix Figure A12 in the NBER WP version).

only contains information on Census division of residence at age 16 and the Opportunity Insights data allows for some aggregate comparisons by county of residence. Similar to the past, the intergenerational gap is reduced when comparing children growing up in the same region in the GSS, although the magnitude of the reduction is smaller than in the past (Figure 3.7, panel (c)). The Opportunity Insights data report the average ranks of children who grew up at the various percentiles of the income distribution by parental nativity for 1,053 counties. For each of these counties, we calculated the gap in expected rank between children of immigrants and children of the US-born at the 25th percentile who grew up in the same county. We find that the average gap is 4.1 percentiles in these 1,053 counties, which is smaller than the national gap of 5.8 percentiles, suggesting that location choice still matters today but to a somewhat lesser degree than in the past.

Overall, our findings suggest an important role for migrants' location choices in explaining the higher mobility of their children. There are two main reasons why immigrants might have been in a better position than the US born to choose an area with more opportunities for their children. First, immigrants who have just arrived in the US likely have greater flexibility than the US born (who are already settled in an area) to locate in a specific region of the country. Second, by leaving their home country, immigrants have already revealed themselves to be willing to move, which might further help them avoid low-mobility regions once in the US.

3.6.2 Father's income not reflecting earnings potential

Another factor that could explain the higher rates of upward mobility for the children of immigrants is that immigrant fathers did not earn up to their potential in the US labor market, thereby giving their children more room to improve. In other words, if immigrant fathers are ranked lower in the distribution than their skills or ability would suggest (e.g., due to a lack of language ability, discrimination, or more limited labor market networks),

then the children of these immigrants are expected to perform better than their fathers' outcomes would predict.

One way to test this mechanism is to compare the children of immigrant fathers who arrived in the US at different ages. Immigrant fathers who arrived as children likely assimilated into the labor market more easily (for instance, by being exposed to US education from an early age), so that their observed income is likely closer to their potential earnings.[36] Panel (a) of Figure 3.8 shows that the mobility gap at the 25th percentile is indeed four times larger for the children of immigrants who arrived as adults (17 years old or older) than for the children of immigrants who arrived before schooling age (7 years old or younger). Indeed, the latter group exhibits only a small mobility gap relative to the children of the US-born.[37] Panels (b) and (c) of Figure 3.8 show that the mobility gap grows with fathers' age of arrival, but especially for individuals with fathers from non-English-speaking countries. This finding is consistent with the idea that immigrant fathers from non-English speaking countries end up lower in the income distribution partly due to an imperfect command of English, thus leaving their children with more opportunity to move up the income distribution (see also Bleakley and Chin 2010).

The children of US-born fathers who migrated internally within the US are a useful comparison group that can shed light on the relative roles of location choices and father's under-placement in the income distribution. On the one hand, internal migrants were likely to sort into areas with better prospects for their children. On the other hand, these fathers were raised in the US and so would not likely suffer from under-placement in the income distribution in their new location. Indeed, Appendix Figure 3C.2 shows that the upward

[36]The gaps also diminish substantially when we focus on children at the 50th and 75th percentile of the income distribution (see Appendix Figure A12 in the NBER WP version).

[37]We focus this analysis on the 1910–1940 cohort because the 1880 Census did not include information on year of arrival to the US. Parental age of arrival to the US is also not available in the GSS. We also find evidence that fathers are less under-placed as they spend more time in the US labor market. Specifically, Appendix Figure 3C.1 shows that the immigrant-native gap at the 25th percentile is about 25% (40%) smaller for children whose immigrant fathers are aged 40–50 relative to children whose immigrant fathers are aged 30–40 in the 1880–1910 (1910–1940) cohort.

mobility of children of US-born fathers who migrated internally falls between that of children of immigrants and children of US-born fathers who remained in their state of birth, suggesting an important role for both geographic choices and under-placement.

3.6.3 Immigrant self-selection

A study of the long-term implications of immigration for the destination country requires a focus on the individuals who chose to migrate and raise children in the host country. Individuals who choose to move to the US are self-selected, which could be one reason why the children of immigrants are more upwardly mobile (for example, if immigrants are more forward looking or care more about the upward mobility of their children, and are thus more likely to invest in them). Interestingly, we find that the immigrant mobility advantage is present even for children of immigrants from countries where most of the migration flow is comprised of refugees, for whom selection is arguably less important (for instance, the Vietnamese in the modern data).

Yet immigrant selection may have manifested itself in many ways. Our historical data allow us to study one aspect of self-selection as an explanation for immigrants' higher upward mobility: namely, that immigrant families invested more in their children's educational attainment, which in turn served as a vehicle toward achieving better labor market outcomes. Figure 3.9 considers the 1910–1940 cohort—the only cohort for which we have data on completed years of schooling for the sons—and shows that, if anything, sons of US-born fathers were more likely to graduate from high school (panel (a)) and reach higher grades in school (panel (b)). Similarly, panel (c) shows that sons of US-born fathers who were aged 12–16 were more likely to be attending school in 1910 than the sons of immigrant fathers.[38] Hence, we conclude that historically, higher educational investments among immigrant families were

[38]Similarly, using the 10 percent sample of the 1880 Census, we see that sons of immigrant fathers aged 12–16 are 8 percentage points less likely to be attending school than sons of US-born fathers (57 versus 65 percent).

unlikely to be the explanation for their higher upward mobility, although education may play a larger role today.[39]

3.7 Conclusions

We use newly constructed linked datasets of fathers and sons to study whether the children of immigrants achieve earnings parity with the children of the US-born, and how the intergenerational mobility of immigrants has changed over the last two centuries. We find that, both in the past and today, children of immigrants, especially those growing up in poorer families, had greater chances of moving up in the income distribution relative to the children of US-born parents. This finding is not driven by immigrants from any particular country or ethnic origin. Rather, when estimating mobility rates by country of origin, we find that children of immigrants from nearly every sending country have higher rates of upward mobility than the children of the US-born.

That immigrants were similarly upwardly mobile both a hundred years ago and today is remarkable. For example, whereas immigrants in the past predominantly hailed from Europe, immigrants today have more diverse ethnic backgrounds, coming from various Latin American and Asian countries. To the extent that members of ethnic minorities might have different levels of intergenerational mobility (as shown in a number of studies including Chetty et al. 2020), we might have expected this change in the ethnic mix to have an influence on the rates of intergenerational assimilation. The past-present similarity is also remarkable given that immigrants today come from countries with lower income levels relative to the US, and given the dramatic changes in US immigration policy over the last century (the ending

[39]Despite these lower levels of educational attainment, panel (d) confirms that the children of immigrants enjoyed higher income mobility because they earned a higher income at any given level of education. The fact that educational differences do not explain the mobility gap is not entirely surprising because, as Goldin (1998) shows, the returns to schooling in 1940 were lower than in recent years. However, we also note that the children of immigrants enjoyed a faster rate of educational mobility: For a given level of father's education, the children of immigrants in the bottom half of the distribution tended to achieve more years of schooling than the children of the US-born, consistent with the finding by Card et al. 2018 (see panel (e)).

of open borders for European immigrants and the imposition of additional regulations for immigrant entry).[40] Overall, our findings stand in contrast to the nostalgic view that it was easier for immigrants in the past to integrate into the US economy and society.

We find that an important explanation for why the children of immigrants are more upwardly mobile is that immigrant families are more likely than the US-born to move to areas that offer better prospects for their children. This finding is especially relevant in light of the decline in both geographical (Molloy et al. 2011) and economic (Chetty et al. 2017) mobility that has taken place in recent decades in the US. It suggests that to the extent that "pockets of opportunity" remain available in the US, immigrants might be able to enjoy high levels of mobility even if overall mobility declines.

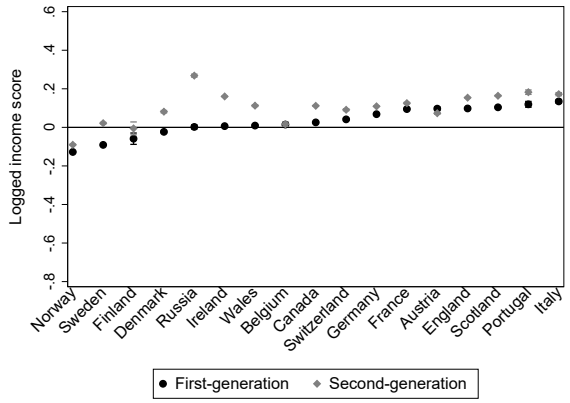
The success of second-generation immigrants gives a more optimistic view of immigrant assimilation than previous studies that have focused only on the first generation. Indeed, we find that second-generation immigrants overtake, rather than just catch up with, the children of the US-born with comparable family incomes. Although some politicians have a short-term perspective on immigrant assimilation, our findings suggest that this view might underestimate the long-run success of immigrants. Our findings are more consistent with the idea of the "American Dream," by which even immigrants who come to the United States with few resources and little skills have a real chance at improving their children's prospects.

[40] US GDP per capita was more than 7 times higher than in Mexico or China when migration flows from these countries took off in 1970, whereas it that was only 2–3 times higher than in European sending countries circa 1900. Lower initial income level of a country-of-origin group might reduce their mobility if a group's average "ethnic capital" affects their upward mobility (Borjas 1992).

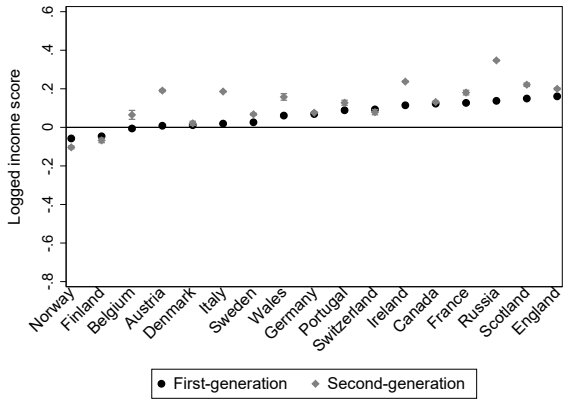
Figures and Tables

Figure 3.1: Cross-Sectional Earning Gaps for First- and Second-Generation Immigrants relative to the US-Born, by Country of Origin

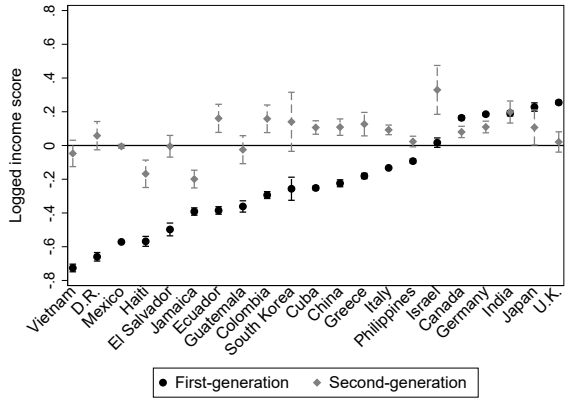
(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



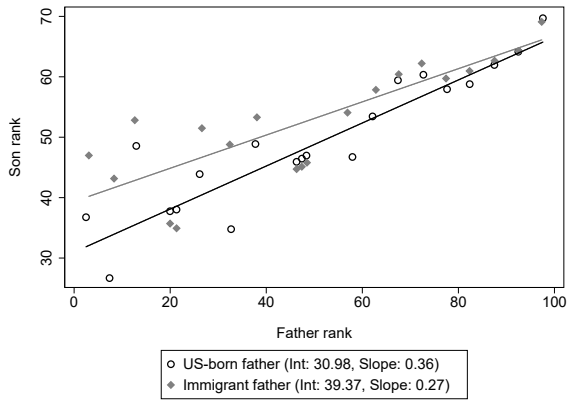
(c) 1980–2010 Cohort



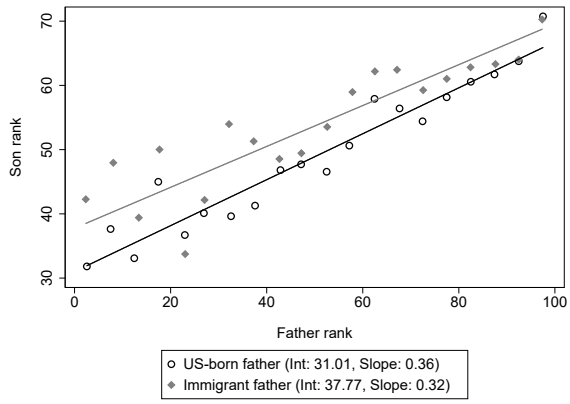
Note: The historical samples use the 1880, 1910, and 1940 Censuses. The historical samples of first-generation immigrants and the US-born are restricted to white men in the earlier Census who are aged 30–50, who have a child in the household, and who were either born in the US or in one of the 17 source countries. The second-generation samples are restricted to white men in the later Census who are aged 30–50 and whose fathers were either born in the US or in one of the 17 source countries. For the most recent cohort, we use the 1980 Census and the 2006–2015 Current Population Surveys (CPS). We restrict the 1980 sample of first-generation immigrants and their counterparts to men aged 30–50, who have a child present in the household, and who were born in the US or in one of the top sending countries identified in the Opportunity Insights sample. We restrict the CPS sample of second-generation immigrants and their counterparts to men aged 30–50 whose fathers were either born in the US or in one of the top sending countries identified in the Opportunity Insights sample. For all cohorts, we use predicted income (described in the main text) as the measure of income.

Figure 3.2: Intergenerational Mobility of Immigrants and the US-born, Rank-rank Correlations

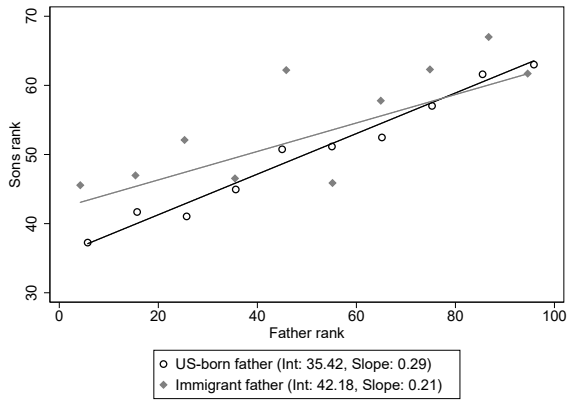
(a) 1880–1910 Cohort



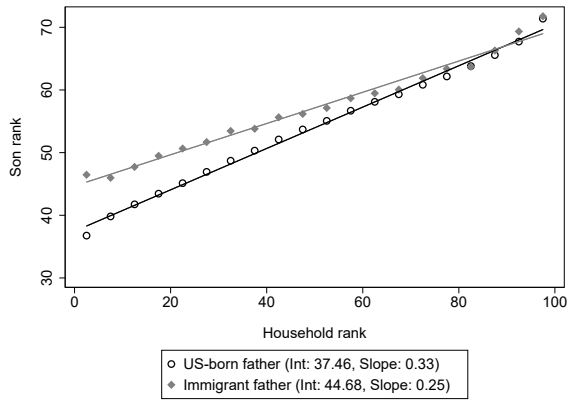
(b) 1910–to 1940 Cohort



(c) GSS: 1984–2006 Cohort



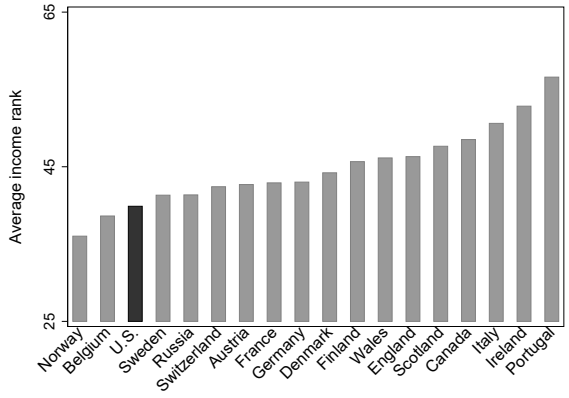
(d) Opportunity Insights: 1997–2015 Cohort



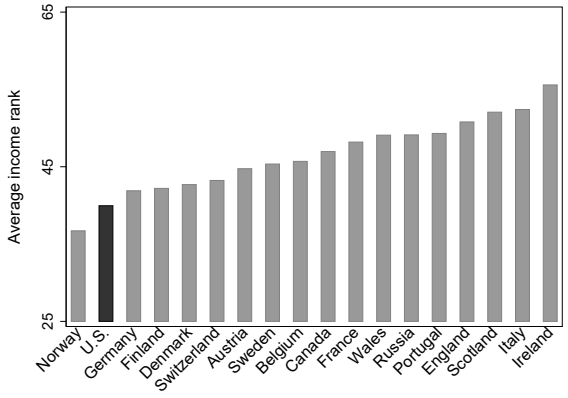
NOTE: Sons are assigned percentile ranks relative to all other sons born in the same birth year. Fathers are ranked relative to all fathers with children born in the same birth year. The figures plot the mean income rank of children by father's income rank, for sons with and without foreign-born fathers, as well as the corresponding regression lines using equation 2. In the GSS sample, the outcomes for sons are measured between 2000 and 2018 (with the median year being 2006). The outcomes for parents are measured via a retrospective question about parents' occupations when the respondent was 16 years old; the data therefore correspond to parental outcomes between 1966 and 2004 (with the median year being 1984). The Opportunity Insights Data come from Opportunity Insights (2020); son's income is measured in 2014–2015 and parental income is measured in 1994–2000.

Figure 3.3: Average Income Rank for Children Born to 25th Percentile, by Father's Birthplace

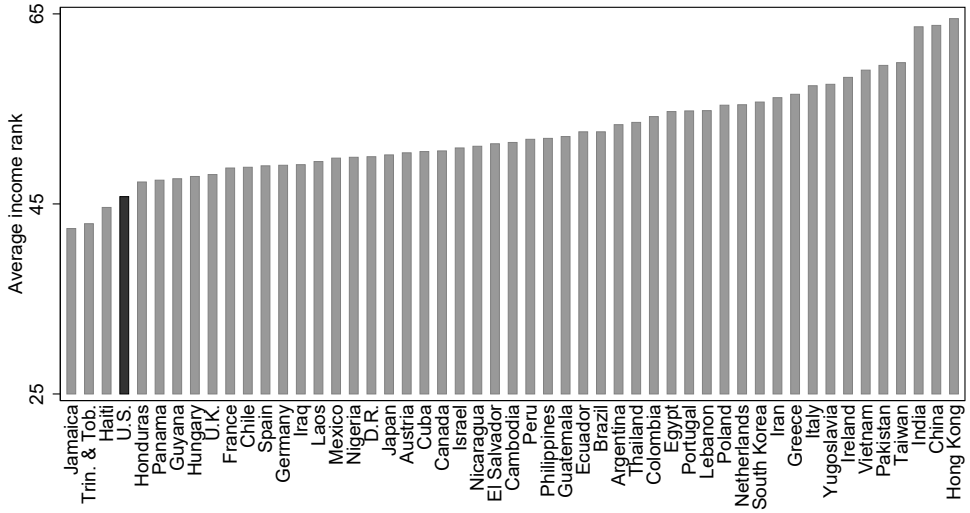
(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



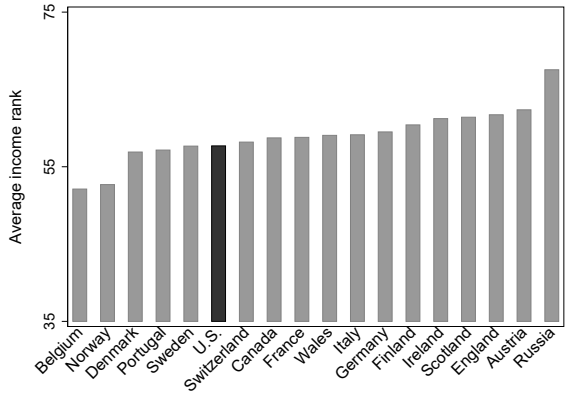
(c) Opportunity Insights: 1997–2015 Cohort



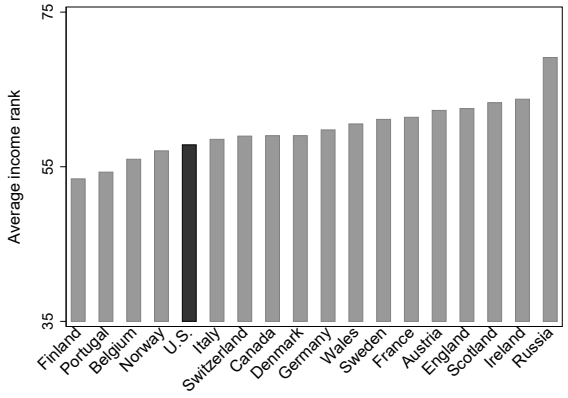
NOTE: This figure plots the average income rank for children born to the 25th percentile of the parental income distribution. Data for the Opportunity Insights cohort come from Opportunity Insights (2020); son's income is measured in 2014–2015 and parental income is measured in 1994–2000.

Figure 3.4: Average Income Rank for Children Born to 75th Percentile, by Father's Birthplace

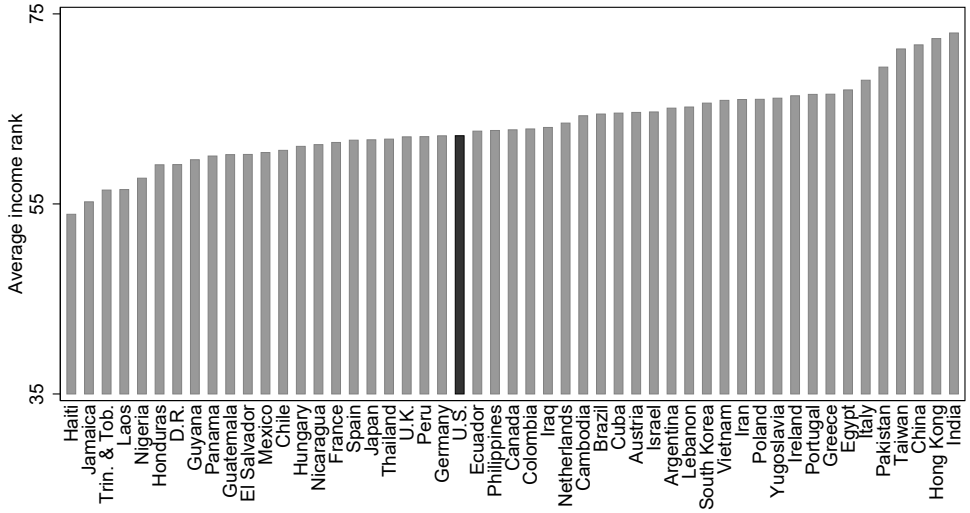
(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



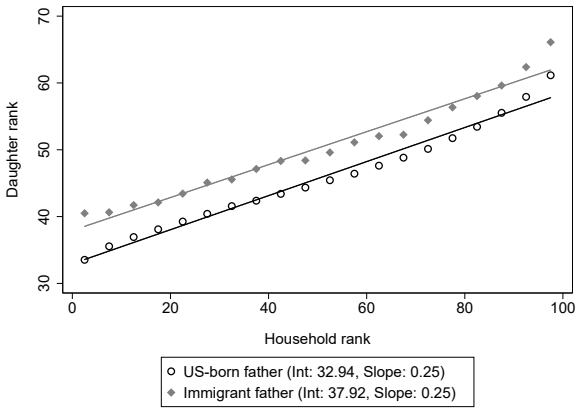
(c) Opportunity Insights: 1997–2015 Cohort



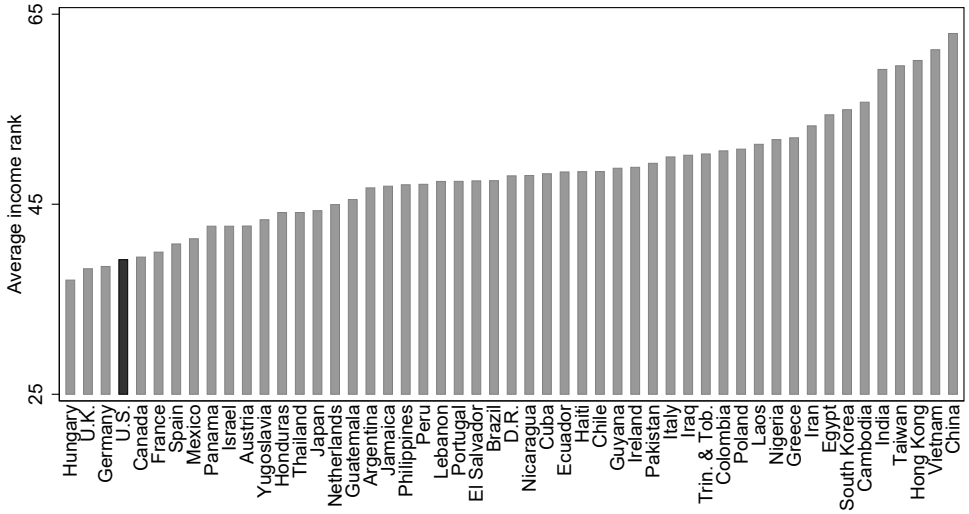
NOTE: This figure plots the average income rank for children born to the 75th percentile of the parental income distribution. Data for the Opportunity Insights cohort come from Opportunity Insights (2020); son's income is measured in 2014–2015 and parental income is measured in 1994–2000.

Figure 3.5: Intergenerational Mobility of Immigrants' Daughters, 1997–2015 Cohort

(a) Rank-rank Correlations



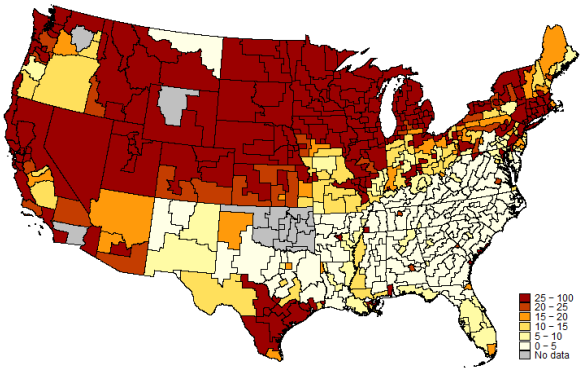
(b) Average Income Rank for Daughters Born to 25th Percentile, by Father's Birthplace



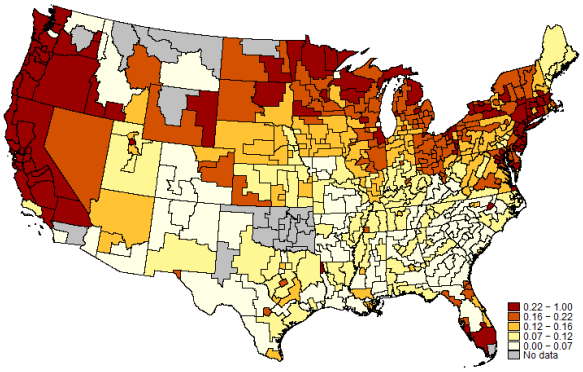
NOTE: Panel (a) plots the mean income rank of daughters by parental income rank, for daughters with and without foreign-born fathers, as well as the corresponding regression lines using equation 2. Panel (b) plots the average income rank for daughters born to the 25th percentile of the parental income distribution, by father's country of origin. Data are from Opportunity Insights (2020); daughter's income is measured in 2014–2015 and parental income is measured in 1994–2000.

Figure 3.6: Share of Immigrants and Upward Mobility, by State Economic Area or Commuting Zone

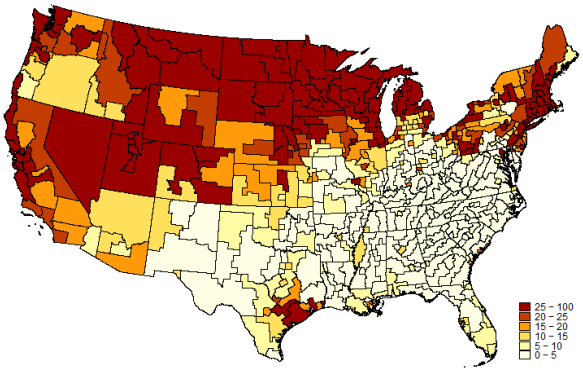
(a) Share of sons that are second-generation (1880–1910)



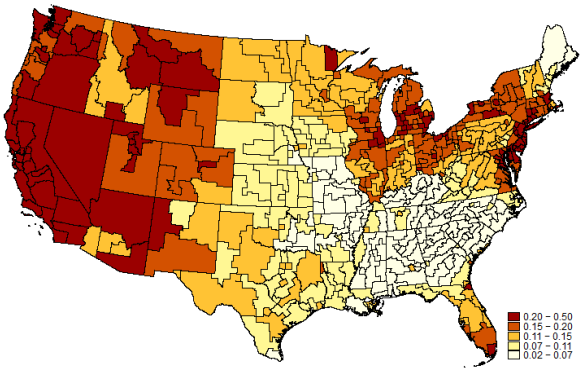
(b) Upward mobility of sons of US-born fathers (1880–1910)



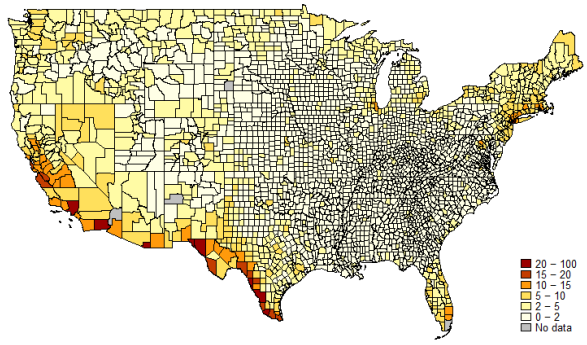
(c) Share of sons that are second-generation (1910–1940)



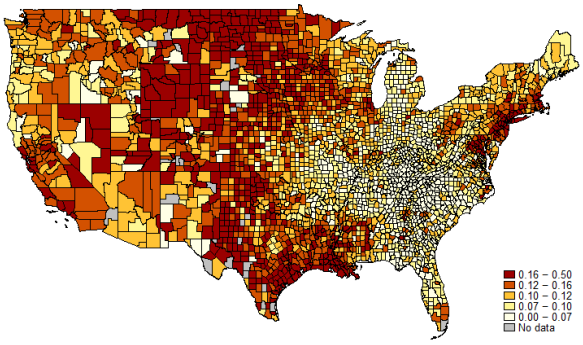
(d) Upward mobility of sons of US-born fathers (1910–1940)



(e) Share of population that is foreign-born, 1980



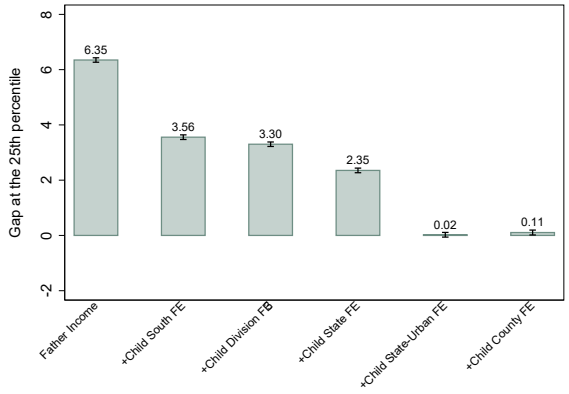
(f) Upward mobility of Opp. Insights cohort



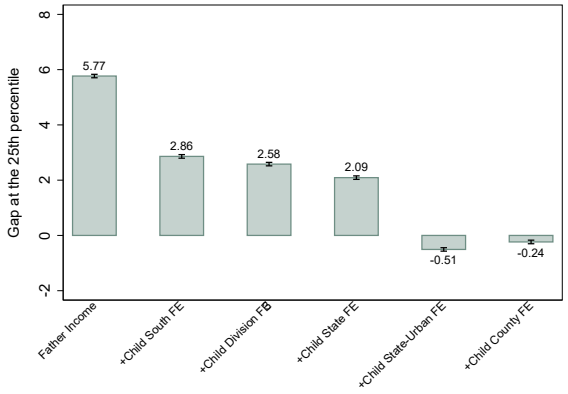
NOTE: Panels (a) and (c) show the share of the individuals in our linked sample that are second-generation immigrants in each 1880 or 1910 state economic area, respectively. Panel (e) shows the share of each county's population that is foreign-born in 1980, using data from Manson et al. (2019). In panels (b) and (d), upward mobility is measured as the share of sons of US-born fathers in each state economic area that reached the top fifth of the national income distribution, conditional on having had a father in the bottom fifth of the national income distribution. Panel (f) shows the share of non-Hispanic white sons who are upwardly mobile using data from Opportunity Insights (2020); upward mobility is measured as the share of sons in the top 20 percent of the income distribution, conditional on having had household income in the bottom fifth of the national income distribution.

Figure 3.7: Intergenerational Gap at the 25th percentile, Comparing Children in Similar Childhood Locations

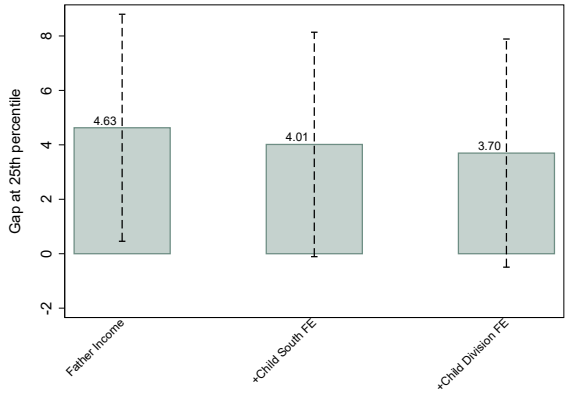
(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



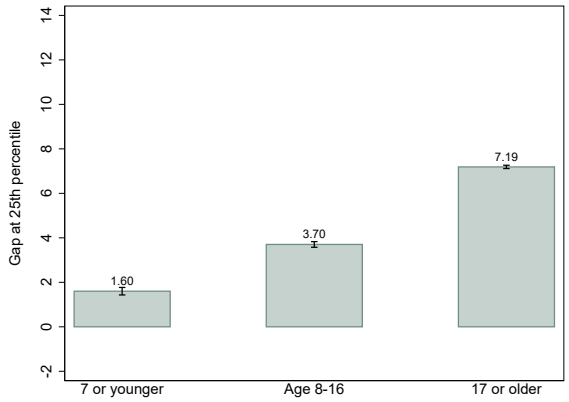
(c) GSS: 1984–2006 Cohort



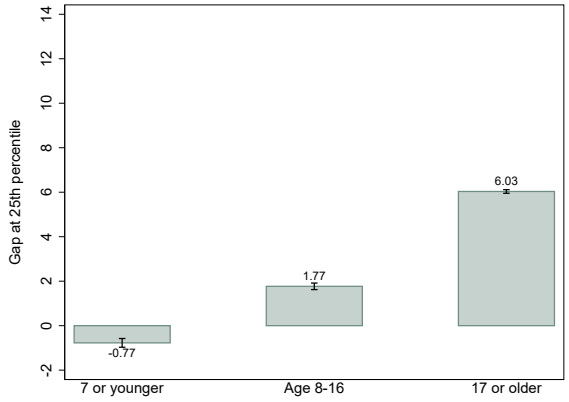
NOTE: This graph plots the intergenerational gap between children of immigrants and children of US-born men, for children born to the 25th percentile of the income distribution. The first bar for each cohort is estimated by regressing son's income ranks on their fathers' ranks, an indicator for having a foreign-born father, and the interaction of these two variables. The second and third bars plot the gap after including an indicator variable for growing up in the US South and childhood Census division fixed effects, respectively. In the historical cohorts, the fourth bar plots the gap after including childhood state fixed effects and the fifth bar plots the gap after including state × urban fixed effects. The sixth bar plots the gap after including childhood county fixed effects.

Figure 3.8: Intergenerational Gap at the 25th percentile for 1910–1940 Cohort, by Father's Age of Arrival and Language in Sending Country

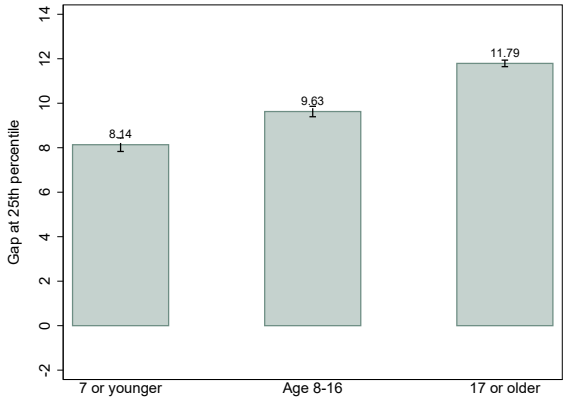
(a) All Countries



(b) Non-English-Speaking Countries



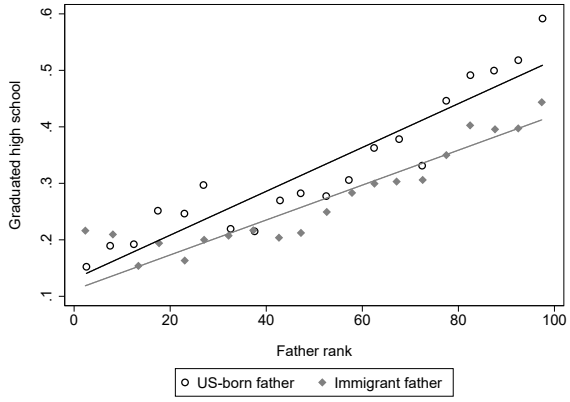
(c) English-Speaking Countries



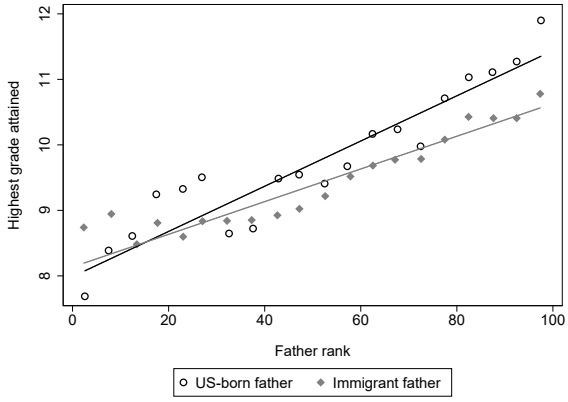
NOTE: This graph plots the intergenerational gap between children of immigrants and children of US-born men, for children born to the 25th percentile of the income distribution. Sons of immigrants are divided into groups based on their father's age of arrival to the United States. The latter two figures also separate sons based on the language spoken in their father's sending country (English-speaking countries are Canada, England, Ireland, Scotland, and Wales).

Figure 3.9: Educational Attainment, 1910–1940 cohort

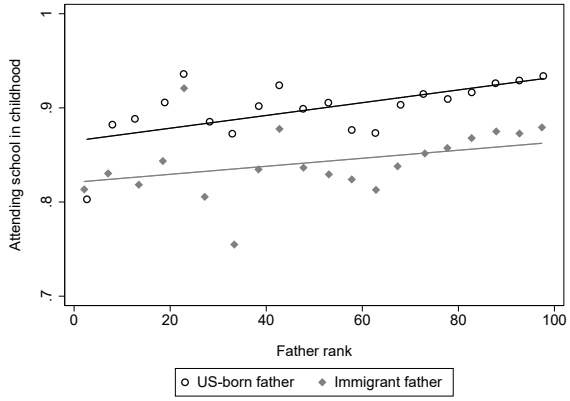
(a) Son graduated high school



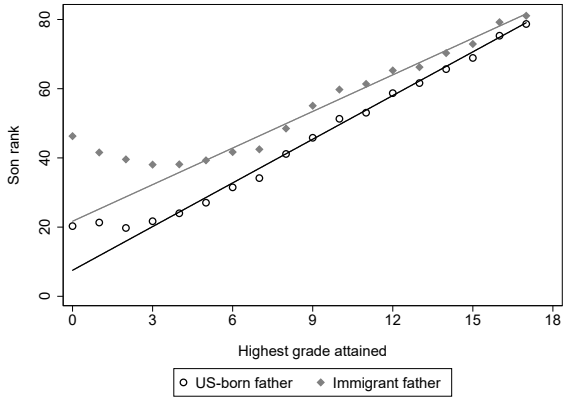
(b) Son's highest grade in school



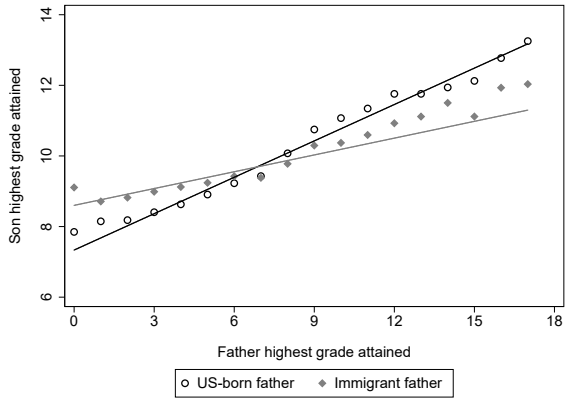
(c) Son's school attendance in 1910



(d) Son's income rank and educational attainment



(e) Son's and father's educational attainment



NOTE: Fathers are ranked relative to all fathers with children in the same birth year. Panels (a) and (b) plot the mean educational attainment of children by their father's income rank, for sons with and without foreign-born fathers, as well as the corresponding regression lines. Panel (c) considers linked sons aged 12–16 in the earlier Census and plots school attendance as a function of his father's income rank (after controlling for age fixed effects). Panel (d) plots a son's mean income rank as a function of completed years of schooling. Panel (e) plots a son's average years of completed schooling as a function of his father's years of schooling (for fathers who can be linked to the 1940 Census).

Table 3.1: Intergenerational Mobility Estimates, by Cohort

	Intercept (α)	Immigrant father (β_0)	Father's rank (β_1)	Immigrant father $\times$ rank (β_2)
1880–1910 Cohort	30.98 [0.06]	8.39 [0.12]	0.36 [0.00]	-0.08 [0.00]
1910–1940 Cohort	31.01 [0.04]	6.75 [0.09]	0.36 [0.00]	-0.04 [0.00]
GSS Cohort	35.42 [1.17]	6.76 [3.06]	0.29 [0.02]	-0.09 [0.06]
Opp. Insights Cohort	36.64 [0.40]	7.42 [0.60]	0.33 [0.01]	-0.08 [0.01]

NOTE: This table reports the estimated coefficients from equation 2 in the paper (standard errors in brackets below each estimate). Data for the Opportunity Insights cohort come from Opportunity Insights (2020).

Appendices

A Appendix A: Sensitivity of Results to Linking Procedure and Sample Construction

Linking Algorithm

A first concern with Census links based on the available information on names, age, and place of birth is the potential for linking children to the wrong adults ("false positives"). Appendix Figure 3A.1 shows that the results are similar when using more conservative versions of the Abramitzky, Boustan, and Eriksson linking algorithm, which reduces sample size but also likely reduces matching errors. First, we restrict the sample to exact matches by full (non-standardized) name and age. Second, we restrict the sample to individuals whose first and last names are unique within a five-year age band. Third, we require matches to be both exact and unique within a five-year age band.[41]

A second concern is the extent to which we are able to generate representative samples. It is well-known that linked samples are not fully representative of the population, as individuals with uncommon names and other attributes are more likely to match between Census waves (Abramitzky et al. 2019). Appendix Table 3A.2 compares the sons in our linked samples to the relevant population of US-born men in the 1910 and 1940 Censuses.[42] Our 1880–1910 sample of linked sons has a similar share of second-generation immigrants as the full population (26 vs. 29 percent), and has an average income score that is roughly 3 percent higher than the population. Our 1910–1940 linked sample has fewer second-generation immigrants than the full population (22 vs. 24 percent), and has an average income score that

[41] Our main results are similar if we instead implement the linking algorithm developed in Abramitzky et al. (2020), although the exact ordering of countries is slightly different. We provide further details on the results using this approach in the Appendix.

[42] That is, we focus on white men aged 30 to 50 whose fathers were either born in the US or in one of the source countries of interest.

is 5 percent higher than the population. We address the non-representativeness of our linked data by running a version of our main specification in which we reweight the matched data so as to mimic the population on characteristics such as age, state of residence, birthplace, and occupation. Appendix Figure 3A.1 shows that the results when reweighting the data are similar to those in the main specification.[43]

Sample Construction

Our baseline approach defines a son's immigrant status using his father's birthplace. In Appendix Figure 3A.1 we show that our main results remain unchanged when defining immigrant status using mother's birthplace, or the immigrant status of both parents. Second, unlike with the historical data, our main specification for the modern period compares immigrants to the entire US-born population regardless of race.[44] Figure A29 in the NBER working version of this paper shows that results are similar when comparing white immigrants to the white population in the modern period or comparing all immigrants to the white population in the modern period.[45]

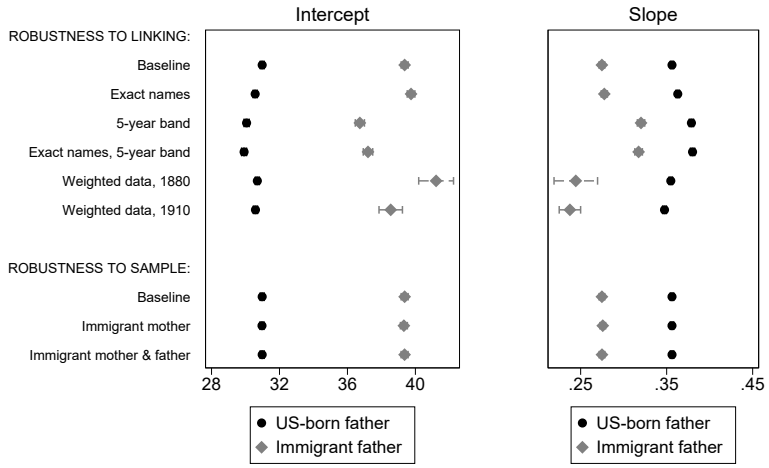
[43]These weights are computed using two alternative procedures. In the first procedure, observations are weighted using earlier Census (i.e., childhood) characteristics: To construct this weight, we use the cross-sectional version of the earlier Census and identify the individuals (i.e., the sons) we are able to link forward. We then regress the indicator for being a "matched" individual on fixed effects for the state of residence, the son's age, the birthplace, as well as the father's birthplace and broad occupational category. In the second procedure, observations are weighted using later Census (i.e., adulthood) characteristics: To construct this weight, we use the cross-sectional version of the later Census and identify the individuals (i.e., the sons) we are able to link backward. We then regress the indicator for being a "matched" individual on fixed effects for the son's state of residence, age, birthplace, and broad occupational category. In both cases, the weights we use to re-weight observations are the inverse probability that an individual can be linked across Censuses.

[44]In the past, immigrants were overwhelmingly white, and so we compare immigrants to the white US-born population in the historical data. Today, in contrast, immigrants are more ethnically diverse.

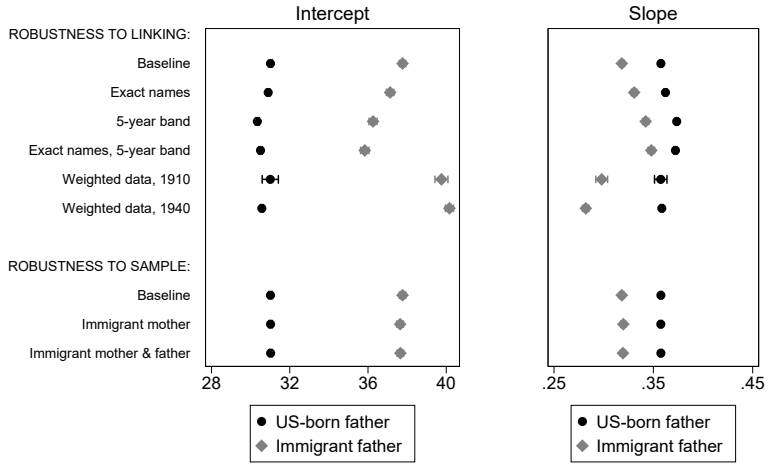
[45]We note that in this case we define second-generation status in the historical cohorts using the mother's birthplace for comparability with the corresponding modern data.

Figure 3A.1: Robustness to Weighting & Linking Algorithm and to Sample Construction

(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



NOTE: These figures report the slope and intercept from regressions of son's rank on father's rank, using alternative weighting schemes and linking algorithms as well as alternative approaches for constructing the sample. After showing the baseline estimates, the next four estimates in each panel show the results from the rank-rank specifications that use alternative strategies for linking fathers and sons across the earlier and later Censuses, as described in Appendix A. "Weighted data" refers to weighing observations by the inverse probability of being a linked individual. Each cohort has two possible weights: one that uses childhood characteristics and the earlier cross-sectional Census, and one that uses adult characteristics and the later cross-sectional Census. "Immigrant mother" refers to a sample that uses the mother's birthplace to classify men as sons of immigrants. "Immigrant mother & father" refers to a sample that classifies men as immigrants only if both their mother and father were born outside of the United States.

Table 3A.1: Sample Size, by Cohort and Father's Country of Origin

	1880–1910 cohort		1910–1940 cohort	
	US-born father	Immigrant father	US-born father	Immigrant father
White men, age 0–16	6,184,219	2,814,910	10,492,593	4,079,624
Linked men	1,432,861	602,726	3,224,578	1,068,064
living with father	1,287,253	544,197	2,931,570	965,839
white father, age 30–50	931,754	414,933	2,175,175	740,814
from largest sending countries	931,754	397,314	2,175,175	696,007
non-missing labor outcomes	893,046	380,590	2,056,119	657,827
Share of white men in sample	14.44	13.52	19.60	16.12

NOTE: The first row refers to the number of white men ages 0–16 in the earlier Census. Rows 2 through 6 show the sample size as we restrict the sample based on the fathers' and sons' characteristics. "Immigrant father" refers to sons whose fathers were born outside of the United States, and "largest sending countries" refers to the 17 countries considered in the historical cohorts. The last row refers to the share of white men ages 0–16 in the final sample.

Table 3A.2: Comparison of Cross-Sectional and Linked Sample of Sons

(a) Sons of US-born Fathers

	1880-1910 cohort		1910-1940 cohort	
	Non-Linked	Linked	Non-Linked	Linked
Age	37.11	37.27	36.28	37.44
Farmer	0.24	0.25	0.30	0.33
White-collar	0.32	0.33	0.14	0.14
Skilled	0.25	0.25	0.32	0.34
Unskilled	0.19	0.17	0.24	0.19
Income	1,046.49	1,078.48	1,064.45	1,140.82
South	0.35	0.29	0.33	0.31

(b) Sons of Immigrant Fathers

	1880-1910 cohort		1910-1940 cohort	
	Non-Linked	Linked	Non-Linked	Linked
Immigrant (full sample)	0.26	0.29	0.22	0.24
Age	37.28	37.70	36.10	38.09
Farmer	0.27	0.27	0.36	0.37
White-collar	0.19	0.22	0.08	0.10
Skilled	0.34	0.33	0.35	0.36
Unskilled	0.20	0.18	0.21	0.17
Income	1,161.67	1,165.21	1,199.60	1,255.94
South	0.08	0.08	0.05	0.06

NOTE: In this table, the linked sample has not yet been restricted based on the fathers' characteristics (i.e., whether the father is white, aged 30–50, living with the child in the early Census, and without missing labor market outcomes).

B Appendix B: Sensitivity of Results to Income Assignment

Appendix Figure 3B.1 shows the main results for father-son pairs for whom we observe actual rather than predicted income in the 1940 Census. We do so by linking fathers who were relatively young in 1910 (20–30 years old) from 1910 to 1940, which enables us to observe these fathers' actual income in 1940 when they were 50–60 years old.[46] Combining this sample with our linked sample of sons, we are able to observe actual income of both fathers and sons for 62,000 pairs, of which 7,800 are immigrants (i.e., those pairs for which we are able to link both the son and his father from 1910 to 1940). For this set of father-son pairs, we show a similar immigrants' mobility advantage when using either actual income or income scores. We also continue to find that children of immigrants from most sending countries are more upwardly mobile than the children of the US-born (but naturally, the exact ranking of countries changes once we divide the 7,800 immigrant fathers into 17 sending countries).

Appendix Figure 3B.2 summarizes the robustness of our estimates for the 1880-1910 and 1910-1940 cohorts to using various proxies of income. Each row in this figure shows the values of the intercept and the slope of the rank-rank relationship for both sons of immigrant and US-born fathers. First, we show that the results are similar when we use actual rather than predicted income for the sons we observe in the 1940 Census (second row of panel (b)). Second, we continue to find an immigrant mobility advantage when we use alternative measures of income for farmers (second and third row in panel (a) and the third and fourth row in panel (b)). Specifically, we use the 1900 Census of Agriculture to calculate average farmers' net earnings (farm income minus expenses) for each US county.[47] Alternatively,

[46] We remind readers that the limitations of this exercise are: (1) that actual income in the 1940 Census is only available for wage earners and is notably missing for farmers, and (2) fathers are observed in a different age range than sons (50–60 years old, instead of 30–50 years old). When performing this exercise, we rank fathers and sons only relative to other fathers and sons who are also wage earners.

[47] The Census of Agriculture data are from Haines et al. (2018). Income is calculated as the sum of the value of farm output not fed to livestock and the value of house rent and food/fuel produced on farm and consumed by the family. Expenses are calculated as the sum of expenditures for farm labor; fertilizer; feed, seed, and threshing; taxes; depreciation of value of buildings; and depreciation of machinery. For more details on this calculation, we refer the reader to Goldenweiser (1916) and Abramitzky et al. (2012). We note

given the prominence of farming and the difficulty in assigning farmers an income measure, we show that the main results do not change when we drop fathers and sons who are farmers from the sample and compute ranks based on the non-farming national income distribution.[48]

Our baseline income score is based on the 1940 Census. We show that results are similar when we use more contemporaneous income sources for 1880 and 1910 fathers and sons (fourth row of panel (a) and fifth row of panel (b)). Specifically, for non-farmers, we use information on average earnings by occupation from the 1901 Cost of Living Survey (Preston and Haines 2014).[49] For farmers, we use the 1900 Census of Agriculture (as described above).

We next use a measure of income that also incorporates a respondent's country of birth in the income prediction (i.e., allows for an immigrant penalty in earnings), and find that sons of immigrant fathers continue to be more upwardly mobile than comparable children of US-born fathers.[50]

Unlike when we use other income scores, children of immigrants and children of the US-born look similarly mobile if we use the IPUMS occupation score, which is based on assigning each occupation its median earnings in 1950 (sixth and seventh rows in panels (a) and (b) of Appendix Figure 3B.2, respectively). However, the IPUMS occupation score is not well suited to our setting mainly because the score for farmers is too low: Farming was a relatively high-paid occupation in the early 1900s, but it became a low-paid occupation

that for farmers living in counties for which we could not calculate farmer net earnings, we assigned them the state estimate for farmer net earnings. For farmers living in counties for which this approach yielded negative net earnings, we assigned them the minimum positive value of county-level farmer earnings in their state. Finally, for farmers living in counties for which this approach yielded very large net earnings (i.e., obvious outliers above the 99th percentile of the farmer earnings distribution), we assigned them the median value of farmer earnings in their state.

[48]Of course, moving into or out of farming was by itself an avenue for mobility, which is why we prefer to include farmers in our baseline sample despite the challenges in measuring farmer income.

[49]The 1901 Cost of Living Survey is closer in time to the earlier Censuses but less detailed than our baseline measure (based on the 1940 Census): it includes roughly 150 non-agricultural occupations, whereas the 1950 occupational classification in our linked sample has around 220 occupations.

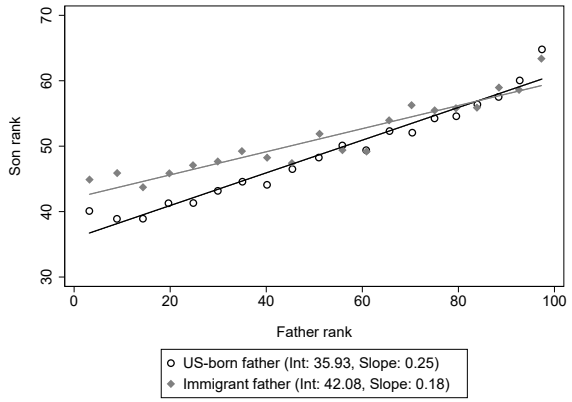
[50]The NBER working paper shows all results are similar using this measure. There, we also present a specification that incorporates the father's country of origin into the son's income prediction. In that specification, the average income rank of children of immigrants is higher than the average income rank of children of the US-born except at the very bottom of the income distribution (and as a result, the estimated intercept of the rank-rank relationship ends up being similar for both groups).

by 1950.[51] That means that the IPUMS occupation score is too low for farmers in 1880 and 1910. This assignment is problematic in our context because US-born individuals were much more likely than immigrants to be farmers (and the children of both US-born and immigrants tended to shift away from farming). Therefore, assigning farmers an income that is too low results in an underestimation of incomes of US-born fathers, and to an overestimation of the rates at which children of the US-born moved up the income ladder relative to children of immigrants. Indeed, if we use the IPUMS occupation score and only adjust farmers' incomes in 1880 and 1910 to correspond to their appropriate rank in the 1900s (based on our baseline income score), we again see that immigrants have a mobility advantage in both historical cohorts (seventh and eighth rows in panels (a) and (b) of Appendix Figure 3B.2, respectively) .

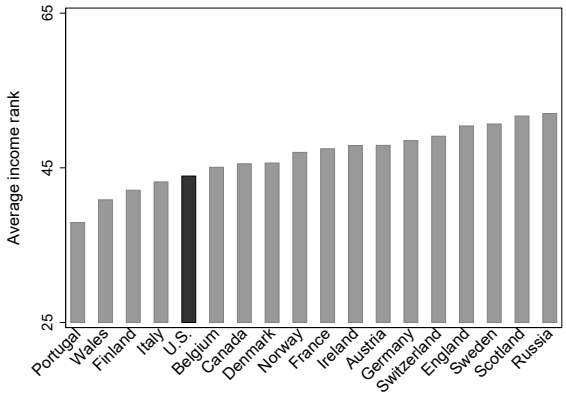
[51]For instance, farmers were at the median of the income distribution in the Iowa 1915 Census (which included information on farmer's income) but were at about the bottom 10th percentile nationally in 1950 (Feigenbaum 2018).

Figure 3B.1: Robustness to Using Actual Income for Fathers and Sons, 1910–1940 Cohort

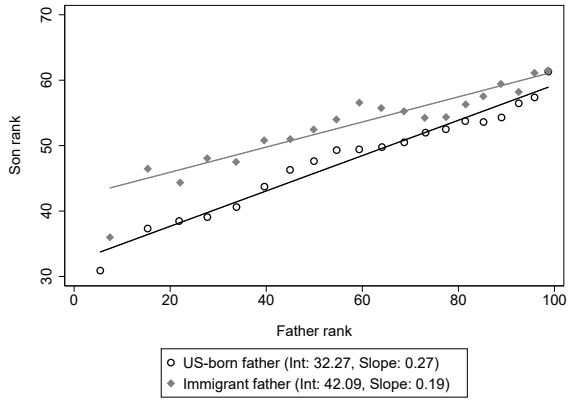
(a) Rank-rank, Actual Income



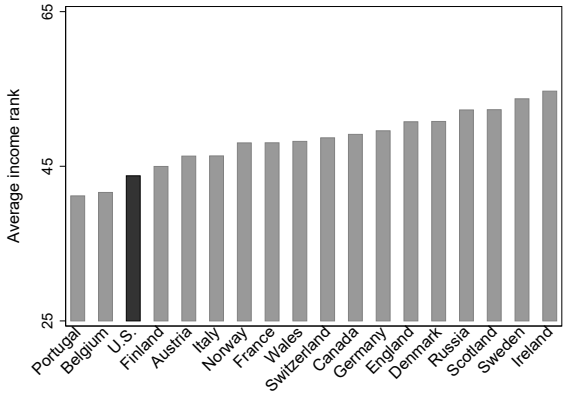
(b) Avg. Rank at 25th Percentile., Actual Income



(c) Rank-rank, Income Score



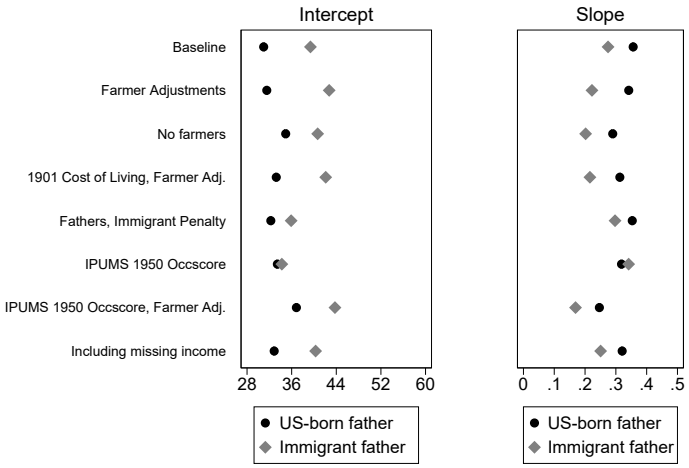
(d) Avg. Rank at 25th Percentile., Income Score



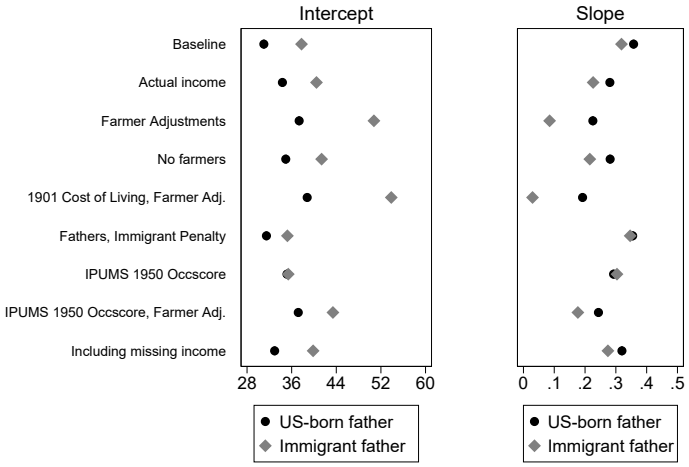
NOTE: These figures use a sample of father-son pairs for which we are able to link both fathers and sons between 1910 and 1940. The sample is restricted to father-son pairs in which both the father and the son received their income from wages. Panels (a) and (b) are based on individual-level income from wages from the 1940 Census, whereas panels (c) and (d) use the predicted income scores used throughout the paper.

Figure 3B.2: Robustness to Using Alternative Income Measures

(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



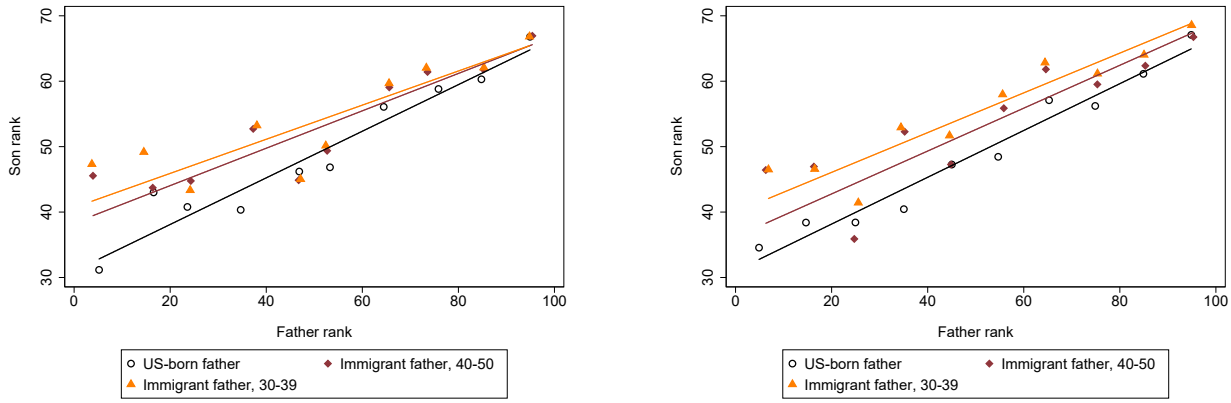
NOTE: These figures report the slope and intercept from regressions of son's rank on father's rank, using alternative approaches for measuring fathers' and sons' incomes. "Actual income" refers to specifications that use actual income for the 1940 sons, adjusted for self-employment and farm income (note we can only run this exercise for the 1910–1940 cohort). "Farmer Adjustments" keeps predicted income for non-farming occupations, but adjusts 1880 and 1910 farmers' incomes using county-level measures derived from the 1900 Census of Agriculture; 1940 sons are adjusted using the 1950 IPUMS "occscore" variable. "No farmers" drops fathers and sons who are farmers from the sample and re-runs the specification using ranks from the non-farming income distribution. "1901 Cost of Living, Farmer Adj." uses the farmer adjustments from above, and also adjusts non-farmer income for all fathers as well as 1910 sons using the average earnings in a person's occupation from the 1901 Cost of Living Survey. "Fathers, Immigrant Penalty" adjusts predicted income so that a father's birthplace is included in the income prediction (or immigrant status for farmers). "IPUMS 1950 Occscore" refers to using the IPUMS 1950 "occscore" variable to measure income. "IPUMS 1950 Occscore, Farmer Adj." refers to using the 1950 "occscore" variable for non-farmers, but scaling up 1880 and 1910 farmers' income so that farmers are ranked roughly 10 percentiles higher on the income distribution (at the same level as in our baseline approach). "Including missing income" refers to specifications in which fathers and sons with missing occupation or income are assigned an income of zero.

C Appendix C: Results by Father's Age and Immigrant Status

Figure 3C.1: Rank-rank Correlations, by Age of Immigrant Fathers

(a) 1880–1910 Cohort: Rank-rank

(b) 1910–1940 Cohort: Rank-rank

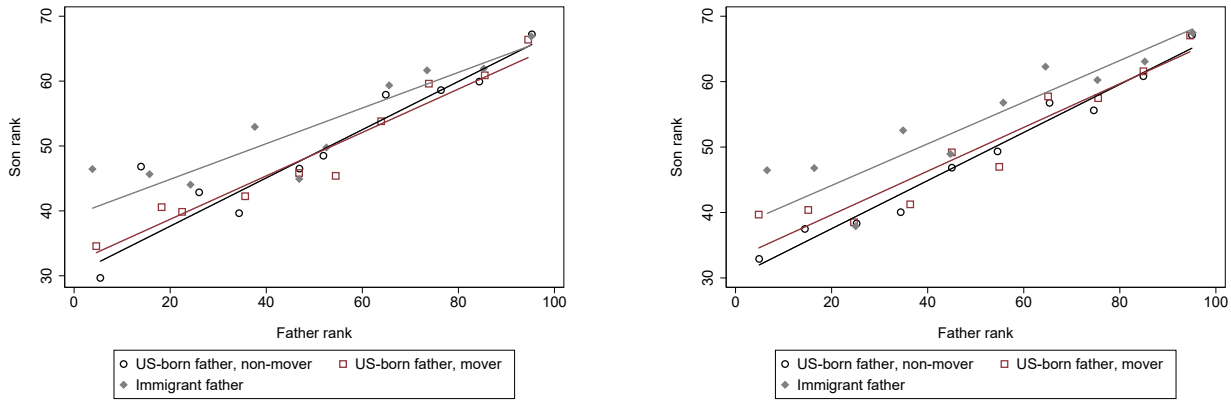


NOTE: The figures plot the mean income rank of children by father's income rank as well as the corresponding regression lines for each group. This figure divides sons with immigrant fathers into two groups based on the father's age: those whose fathers were aged 30–39 and those whose fathers were aged 40–50 in the earlier Census.

Figure 3C.2: Rank-rank Correlations, by US-born Father's Internal Migration Status

(a) 1880–1910 Cohort: Rank-rank

(b) 1910–1940 Cohort: Rank-rank



NOTE: The figures plot the mean income rank of children by father's income rank as well as the corresponding regression lines for each group. This figure divides sons with US-born fathers into two groups based on the father's migrant status: those whose fathers moved within the US (i.e., their state of residence differs from their state of birth) and those whose fathers did not move.

D Appendix D: Sensitivity of Results to using the Abramitzky et al. (2020) Linking Algorithm

Our baseline results use the linking algorithm developed in Abramitzky et al. (2012) and Abramitzky et al. (2014), which is explained and evaluated in Abramitzky et al. (2019). In this section, we replicate our main results using the algorithm introduced in Abramitzky et al. (2020). This method uses the Expectation Maximization (EM) algorithm to combine age and name distances into a single score reflecting the probability that each potential pair of records is a true match. An advantage of this approach is that it enables researchers to create samples with very low false positive rates, although at the expense of generating smaller sample sizes (see Abramitzky et al. (2019) for an evaluation of this method). Indeed, Appendix Table 3D.1 shows that the samples that we generate in this case are between one fourth and one half the size of our baseline samples.

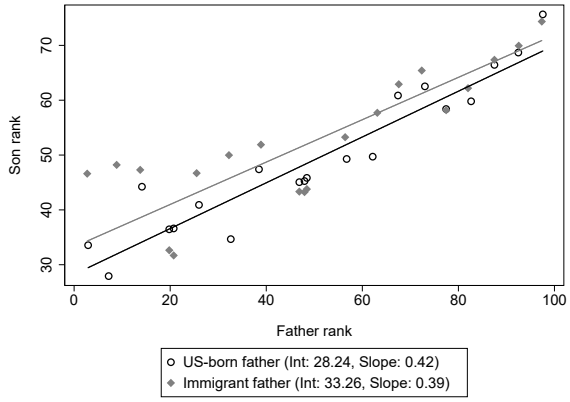
Online Appendix Figures 3D.1 to 3D.4 show that our main results hold when using this alternative linking strategy. Specifically, we continue to find that: (1) children of immigrants are more upwardly mobile (Figure 3D.1), (2) this is true for immigrants from most sending countries (Figures 3D.2 and 3D.3), and (3) differences in rates of upward mobility disappear once we compare children of the immigrants and children of the US born who grew up in similar areas (Figure 3D.4).

The main difference between the results that use this sample and our baseline results is that the rank-rank slopes are higher and the intercepts are lower, both for immigrants and for the US born. This is likely driven by the combination of two facts. First, this method likely generates lower false positive rates, which would tend to increase rank-rank associations. Second, because the method is more conservative with respect to which observations it deems as a match, it is more likely to select individuals with stable characteristics and who report their identifying information with accuracy.

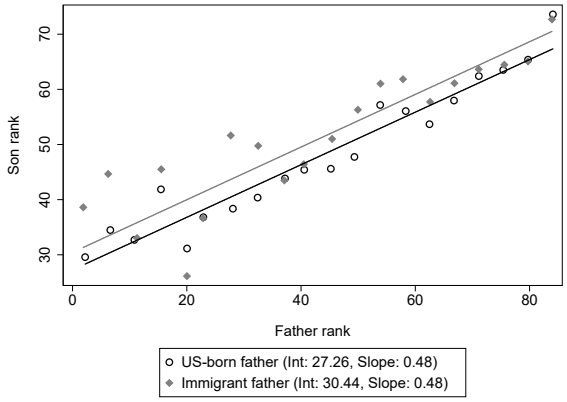
Finally, while the ranking of countries of origin in terms of mobility that we generate using this method is highly correlated with the ranking in our baseline approach, the exact ordering of countries is slightly different. Specifically, the correlation between the country ranks using the baseline samples and these alternative samples is 0.85 for the 1880–1910 cohort and 0.97 for the 1910–1940 cohort.

Figure 3D.1: Intergenerational Mobility of Immigrants and the US-born, Rank-rank Correlations, EM Samples

(a) 1880–1910 Cohort



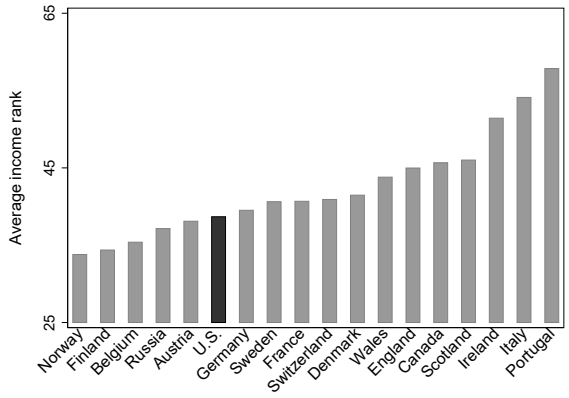
(b) 1910–to 1940 Cohort



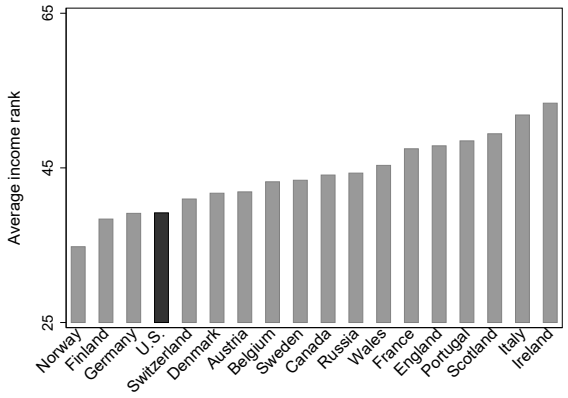
NOTE: This figure replicates Figure 3.2 in the main text using the linking algorithm in Abramitzky et al. (2020).

Figure 3D.2: Average Income Rank for Children Born to 25th Percentile, by Father's Birthplace, EM Samples

(a) 1880–1910 Cohort



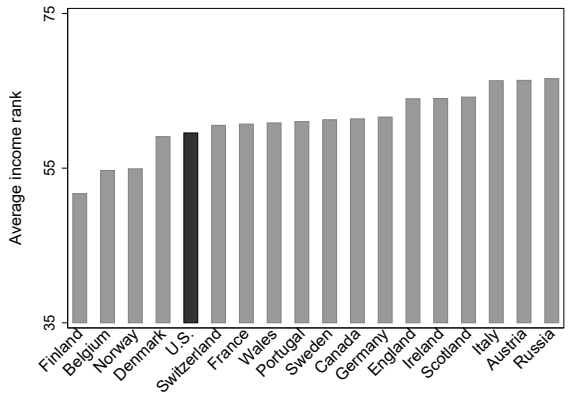
(b) 1910–1940 Cohort



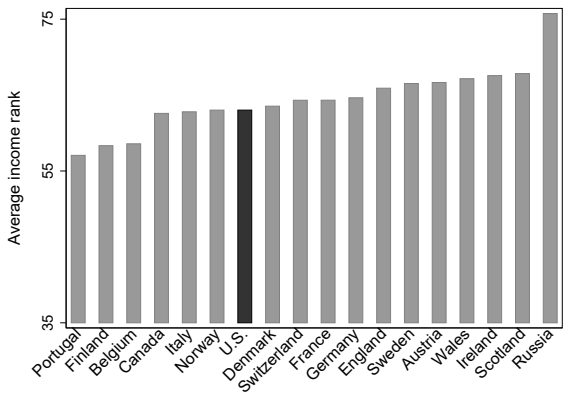
NOTE: This figure replicates Figure 3.3 in the main text using the linking algorithm in Abramitzky et al. (2020).

Figure 3D.3: Average Income Rank for Children Born to 75th Percentile, by Father's Birthplace, EM Samples

(a) 1880–1910 Cohort



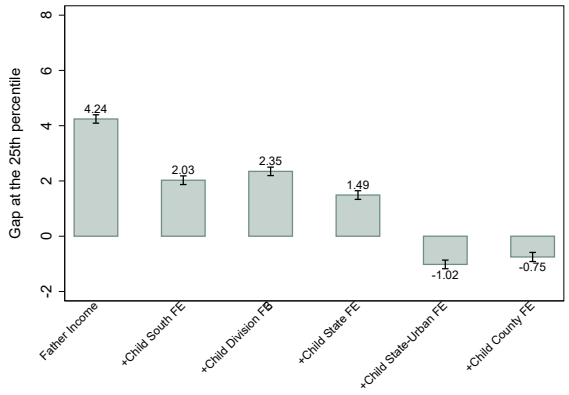
(b) 1910–1940 Cohort



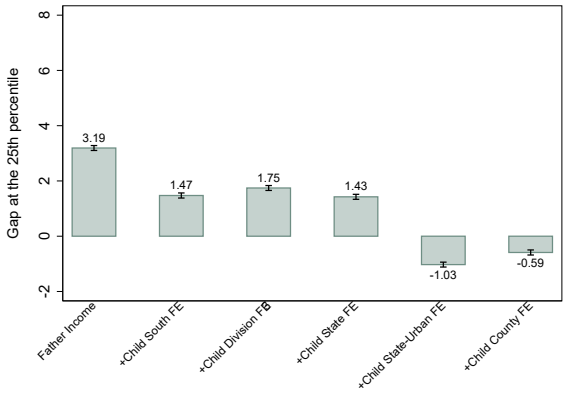
NOTE: This figure replicates Figure 3.4 in the main text using the linking algorithm in Abramitzky et al. (2020).

Figure 3D.4: Intergenerational Gap at the 25th percentile, Comparing Children in Similar Childhood Locations, EM Samples

(a) 1880–1910 Cohort



(b) 1910–1940 Cohort



NOTE: This figure replicates Figure 3.7 in the main text using the linking algorithm in Abramitzky et al. (2020).

Table 3D.1: Sample Size, by Cohort and Father's Country of Origin, EM samples

	1880–1910 cohort		1910–1940 cohort	
	US-born father	Immigrant father	US-born father	Immigrant father
White men, age 0–16	6,184,219	2,814,910	10,492,593	4,079,624
Linked men	504,963	183,660	1,517,949	457,833
living with father	461,021	168,543	1,395,790	419,681
white father, age 30–50	332,508	127,964	1,037,349	319,322
from largest sending countries	332,508	122,675	1,037,349	302,560
non-missing labor outcomes	272,430	97,560	870,967	245,046
Share of white men in final sample	4.41	3.47	8.30	6.01

NOTE: This table shows sample size by cohort and father's country of origin when using the EM samples.

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