THE CRIMINAL JUSTICE SYSTEM AND SOCIAL MOBILITY IN THE UNITED STATES

by

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Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

In

Law and Economics

May 14, 2021

Nashville, Tennessee

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To my parents.

ACKNOWLEDGMENTS

This work could not have been completed without the generous educational, administrative, and financial support of Vanderbilt's Law & Economics program. I feel incredibly grateful to have been a part of such a unique program with outstanding faculty, staff, and peers.

Special thanks to the members of my dissertation committee, Professors Joni Hersch, Kathryn Humphreys, Nancy King, and Edward Rubin. You all provided unique and valuable insights that greatly improved this work, and I look up to each of you. I am particularly indebted to the chair of my dissertation committee, Professor Joni Hersch. Your mentorship has been invaluable to me for the past six years. Not only have you taught me the skills necessary to perform academic research, but you have motivated me to seek out the topics I am most passionate about.

Many individuals at Vanderbilt went out of their way to help me succeed and provided encouragement when I needed it most. For six years' worth of knowledge and mentorship, thank you to Professors Michael Bressman, Andrew Daugherty, Joseph Fishman, Brian Fitzpatrick, Terry Maroney, Timothy Meyer, Mary Miles Prince, Jennifer Reinganum, Lauren Rogal, J.B. Ruhl, Jennifer Shinall, Paige Skiba, Christopher Slobogin, Kevin Stack, Cara Suvall, Jennifer Swezey, and W. Kip Viscusi. For countless pep talks, thank you to Laurel Donahue, Christina Stoddard, Sarah Dalton, and Allison Timmons. For helping me survive my fourth year, thank you to the 2018–19 JETLaw executive board.

To my peers in the Law and Economics program—especially Scott DeAngelis, Danielle Drory, Hannah Frank, Clayton Masterman, Carlie Malone, Rachel Dalafave, Zachary Sturman, and Scott Jeffrey—there is no one I would rather spend far too much time in a basement with. You all inspire me, and I am forever grateful for your friendship. Special thanks to Nick Marquiss for being the best cohort-mate I could have asked for.

I am especially grateful to everyone who supported me through a difficult and isolating year of writing my dissertation during a global pandemic. To the friends I spent every Tuesday night with, you helped me become a better version of myself every week. Thank you to Andy Kahrs, Blair Prescott, and Carlie Malone for keeping me fed and introducing me to camping (and so much more). Thank you to the friends—Nick Baniel, Mitchell Galloway, Sarah Klein, Claudia Manzo, Melissa Reighard, Sam Sergent, and many others—who were always a phone call away. I owe much of my sanity to my roommate Paige Anders.

I can never thank my parents, Jan and John Meyers, enough. Even though I doubt you will ever read this manuscript in its entirety ("Did you really need to provide that much detail?"), the immeasurable investments you have made in my education and growth made this accomplishment possible. More importantly, you have supported and encouraged me to pursue what I want to do and, ultimately, to decide what I value and who I want to be. That is the greatest gift you could have given me.

And, of course, thank you to R.J.

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INTRODUCTION

A staggering number of Americans experience criminal justice contact each year, ranging from arrest to long-term incarceration. One 2014 *Wall Street Journal* report estimated that approximately one in three Americans are represented in the FBI's master criminal database. Many scholars and commentators have called into question the desirability of mass criminalization and the resulting large-scale arrests. This dissertation adds empirical context to the ongoing discussion. Chapters 1 and 2 study the costs, both absolute and distributional, of large-scale arrests. Chapter 3 considers increased access to health insurance as a possible alternative to drug arrests.

Chapter 1 studies the relationship between race and ethnicity and conviction rates among adult, male arrestees using nationally representative data. The results demonstrate that Black men are less likely to be convicted conditional on arrest and that the results are limited to crimes involving high levels of police discretion in the arrest decision. I suggest that this result reflects over-arrest of Black men for either crimes with insufficient evidence or low-level crimes.

Chapter 2 investigates the impact of convictionless arrests on labor market outcomes. I find that adult men who are arrested but never actually convicted of a crime experience a higher risk of job loss in the year of arrest. They further earn an approximately 9% lower hourly wage and are less likely to have jobs that provide medical insurance or retirement benefits up to 8 years after the arrest. This finding is concerning because it means that, with their power to arrest, police officers have large amounts of unchecked discretion to dole out private punishment in the form of restricted labor market opportunities. The combination of results from Chapters 1 and 2 indicate that Black men bear an outsized portion of the costs of large-scale arrests.

Chapter 3 examines whether state-level Medicaid expansion reduced the rate of drug arrests at the state level. I find limited evidence that Medicaid expansion is associated with a reduction in arrests for synthetic opioid possession and heroin/cocaine sales. This decrease appears to be limited to arrests of non-Black individuals. This finding is concerning, given the already existing disparity in drug arrests between Black and White individuals.

CHAPTER 1. RACIAL DISPARITIES IN CRIMINAL JUSTICE PROCESSING

I. Introduction

Factors affecting criminal justice outcomes can be categorized as either warranted (i.e., legally relevant and related to criminal involvement) or unwarranted (i.e., legally irrelevant characteristics of an individual) (Schleiden et al. 2020). In a perfectly fair criminal system, outcomes would be based solely on warranted factors. However, very few—if any—social systems operate in a perfectly fair manner.

Many studies investigate whether race and ethnicity act as unwarranted factors that affect criminal justice outcomes. Arrest statistics consistently show racial differences in arrests that disfavor the Black population. For instance, Black individuals made up approximately 12.7% of the US population in 2018 but 27.4% of the arrests that year. Statistics such as these are sometimes cited to support the proposition that Black individuals are more likely to commit crimes than White individuals. However, this characterization grossly oversimplifies the criminal justice process.[1] Indeed, differing criminal behavior by race is only one of multiple possible explanations that could underlie arrest rate disparities.

Existing empirical research, on the whole, supports the proposition that at least a sizable portion of the disparity in arrest rates is unwarranted. These findings—coupled with many studies showing harsher sentencing decisions for Black individuals—are cause for great concern, given that any level of criminal justice contact can derail an individual's life.

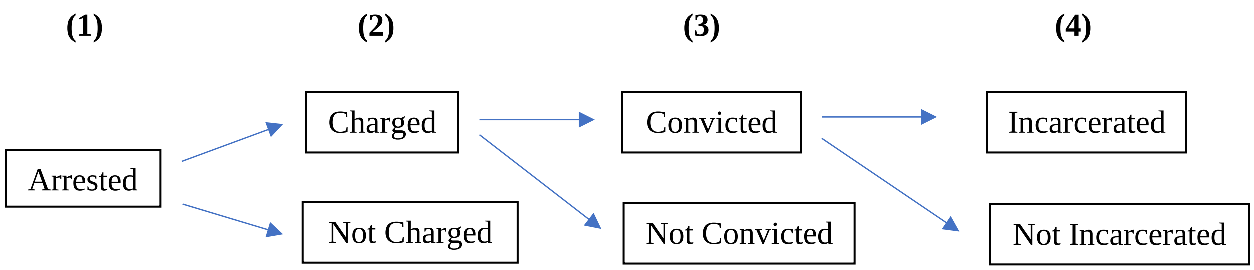
[1] "American culture has so long associated criminality with blackness and continues to do so, even though the racial makeup of the criminalized population is itself a result of law enforcement selection and prosecution policies." (Dolovich & Natapoff 2019).

Much prior research on racial disparities has focused on the endpoints of criminal justice processing: the initial arrest and sentencing. These endpoints are salient, but they make up only part of a complicated system. Police, prosecutors, juries, and judges all play important roles at different points in the process. Studying when and how racial disparities arise in criminal justice processing can inform more targeted policy proposals to reduce unwarranted racial disparities.

For instance, police are directly involved in the arrest stage, with prosecutors and judges occasionally playing a role through coordination and approval of search and arrest warrants. At the charging stage, prosecutors and juries exercise discretion, depending on the process used. Prosecutors generally have wide discretion in determining which, if any, charges to file. Prosecutors, judges, and juries are involved in the conviction step, through either plea bargaining and its subsequent approval, or trial. Since the vast majority of cases plead out, a jury is rarely involved and conviction decisions are most often made by the prosecutor and, through approval of the plea bargain, the judge. Finally, judges have discretion at the sentencing stage in most cases, although prosecutors can also exert substantial indirect influence at this stage.[2]

The following flow chart maps out the four major stages of criminal justice processing.

Figure 1: Stages of Criminal Justice Processing



[2] Sometimes the only discretion exercised by the judge at sentencing is the approval of a sentence agreed to in plea negotiations and, in these cases, the prosecutor acts as the de facto decision-maker of the defendant's sentence. Further, prosecutors decide which charges to bring at the outset, which can greatly influence the ultimate sentence due to mandatory minimums and sentencing guidelines, which vary depending on the details of the charge.

We know from existing data and empirical research that Black individuals experience worse outcomes at stages (1) and (4): they are arrested at higher rates and are subject to harsher sentencing than White individuals, even after controlling for self-reported criminal behavior and other legally relevant factors. By focusing on the endpoints of arrest and sentencing, existing research has left a gap in our understanding of the role that race plays in the middle stages of criminal justice processing.

Some recent research has attempted to fill that gap by studying outcomes by race at stages (2) and (3). One subset of this research focuses on the broad question of whether an arrestee is ultimately convicted of any crime. These studies examine racial differences in prosecutor's decisions to dismiss charges (Berdejó 2018; Kutateladze et al. 2014; Franklin 2010; Tomic & Hakes 2008).[3] However, these studies have yielded conflicting results, and the relationship between race/ethnicity and conviction rates is an open question.

Two Department of Justice (DOJ) investigations highlight the uncertain nature of racial disparities in conviction rates. A DOJ investigation of the Baltimore police department found equal dismissal rates by race for serious offenses, but a higher rate of case dismissals for Black individuals in cases when police officers had wide discretion to make arrests (U.S. Dep't of Justice 2016; D'Souza, Weitzer & Brunson 2019). The DOJ posited that this finding indicates that Baltimore police use their discretion in a discriminatory manner. In contrast, another DOJ investigation of the Ferguson police department found a higher rate of overall case dismissal for White individuals. (U.S. Dep't of Justice 2015; D'Souza, Weitzer & Brunson 2019). Thus,

[3] While not the focus of this paper, another subset of this research is more granular and examines results such as procedural outcomes (e.g., differences in pretrial detention by race) and the types of crimes an individual charged with or convicted of (e.g., differences in charging crimes associated with mandatory minimums by race). Some studies in this line of research find outcomes disfavoring black arrestees in the charging and plea-bargaining stages (e.g., Rehavi & Starr 2014; Kutateladze et al. 2014).

answering the question of what role race plays in conviction appears to vary significantly by jurisdiction and crime type.

This variation across jurisdictions indicates that a nationally representative dataset can be useful in gaining a picture of the average racial disparity in conviction rates across the United States. I use the National Longitudinal Survey of Youth – 1997 (NLSY97) to examine racial disparities in charging and conviction rates of adult, male respondents. For reasons discussed later, I combine the outcomes of charging and conviction for my main analysis. Thus, my outcome of interest is the probability an individual is convicted, conditional on having been arrested. I additionally examine racial disparities in arrest and sentencing rates to confirm that my analysis aligns with previous studies' finding of worse outcomes for Black individuals at those stages.

I find that Black men are convicted at significantly lower rates than White men, even after controlling for other potentially relevant individual characteristics, such as education, work experience, self-reported criminal behavior, drug use, and arrest history. My regression analyses indicate that a Black male arrestee is approximately 16 percentage points less likely to be convicted than a similarly situated White male arrestee. This finding varies by region. The highest disparities occur in the Northeast and North Central regions, where Black male arrestees are 38 and 24 percentage points less likely to be convicted than their White counterparts, respectively. Some results indicate that Hispanic arrestees are less likely to be convicted than their White counterparts, but these results are less robust.

I consider four possible explanations for this finding. First, this could reflect the systemic over-arrest of Black men (i.e., that police are more likely to arrest a Black man for either minor crimes or crimes with insufficient evidence). Second, this could reflect either a general mistrust of the police among the Black community and the related lower willingness of Black victims or witnesses to cooperate in police investigations or prosecutor bias against Black victims' cases. Third, it may be that more Black arrestees are already on probation or parole than White arrestees at the time of arrest and, because of this, prosecutors decide to revoke probation or parole rather than charging or convicting of a new crime. Fourth and finally, it could be that Black individuals are treated less harshly than White individuals by prosecutors at this decision point.

I investigate each of these possibilities and find that the most likely explanation is that of systemic over-arrest. First, I argue that racial bias against non-Black arrestees is highly unlikely. Researchers have found consistent evidence of racial bias at nearly all other points in the criminal justice system. It is unlikely that the decision to not charge an arrestee or later drop the charge differs from all other stages.

To determine whether probation or parole could be driving the results, I perform a supplemental analysis that includes first arrests only. I find that a large disparity persists in these first-time arrests, with Black arrestees being 14 percentage points less likely to be convicted.

To determine which of the two remaining mechanisms—over-arrest or differences in victim/witness cooperation—is most likely, I look to see whether there are differences in conviction rates by type of crime. For those individuals who are charged of a crime, the NLSY97 has data on the type of crime charged. I split crimes into two categories. The first includes drug possession, drug sales, public disorder, and major traffic offenses. This category compromises the crime types for which police are likely to have higher levels of on-the-spot discretion in the arrest decision. The second category includes assault, burglary, robbery, theft, destruction of property, and other property crimes. Arrests for these crimes are less likely to involve on-the-spot decision-making. The first category also generally includes "victimless" crimes, while the second category of crimes generally involve a victim.

To the extent racial bias and over-arrest drives lower conviction rates, one would expect to see the disparities concentrated among crimes for which police have greater discretion to arrest or not arrest someone. To the extent victim and witness cooperation drive lower conviction rates, the disparity would likely arise in the second category of crimes. I find that the disparity is heavily concentrated in the first category of crimes, with no statistically significant difference in conviction rates among those charged with the second category of crimes. Given this pattern, I propose that racial disparities in conviction rates are driven by systemic over-arrest of Black men for crimes in which police have large discretion in the arrest decision.

Previous research finds that Black men are arrested at rates that exceed any difference in self-reported criminality and other relevant factors. By looking to post-arrest outcomes, my analysis provides additional evidence of over-arrest of Black men. To the extent that convictions reflect prosecutors' determinations that an arrest is (1) based on sound evidence and (2) for the type of crime that is worth expending resources to prosecute, the large difference between Black and White men's conviction rates raises serious questions of the validity and desirability of arresting so many Black men each year.

If arrests were costless, my findings would be positive, at best, and unimportant, at worst. It indicates that prosecutors are, to some extent, correcting for the over-arrest of Black men, which some might interpret as indicative of a fair legal system. However, arrests are far from costless. Policing is costly and undertaking many arrests that are ultimately not fully prosecuted would be considered by many to be a large waste of tax dollars. Further, current policing practices result in more than half of Black men being arrested at least once by young adulthood. Arrests can cause immediate harm to the arrested individual in terms of job loss, financial costs, and psychological stress. Arrest records can further close doors to future opportunity in education and the labor market. Large-scale arrests of Black men also cultivate stigma more generally, leading to misperceptions about criminality. Arrests that are seen as illegitimate or racially motivated create distrust of the police among the Black community. Finally, the more stops and arrests that occur, the higher the likelihood of instances of police violence.

II. Background

My analysis focuses on conviction rates of male, adult arrestees. Research on criminal justice outcomes is often split along the lines of age and sex. Because the adult criminal justice system differs significantly from the juvenile justice system in both its goals and processes, researchers generally do not analyze them together. Similarly, men are often studied separately from women, as their offending and arrest patterns vary drastically from one another. Thus, for example, a single study might focus specifically on sentencing decisions for juvenile boys (e.g., Stevens & Morash 2014).

In general, men's criminal justice outcomes are studied more often than women's because they make up most of the criminal-justice-involved population. For instance, men made up 72.8% of arrests in 2018, per the FBI's Uniform Crime Reporting database. The NLSY97 also reflects this, as men make up approximately 68% of those arrested in the sample. Because of their higher rates of criminal justice involvement and larger sample size of arrests within the NLSY97, I focus my study on adult men.

As described above, criminal justice processing consists of four major decision points: arrest, charging, conviction, and sentencing. In this Article, I focus on the middle stages of charging and conviction. My discussion and analysis of sentencing disparities is limited to confirming that the results conform with prior research that finds worse sentencing outcomes for

Black individuals.[4] However, because those charged and convicted are a subset of those arrested, it is necessary to understand the arrest process before turning to the middle stages of criminal justice processing.

A. Arrest and Selection Effects

Arrest represents the entry point into the criminal justice system and is undertaken by police officers or other law enforcement agents. The various explanations for arrest-rate disparities by race can be split into "warranted" and "unwarranted" factors (Schleiden et al. 2020; Gase et al. 2016). Differing underlying levels of criminal activity by race may manifest as disparities in arrest rates by race, which would reflect a warranted factor. In contrast, police racial bias—such as racial profiling in police stops or applying different evidentiary thresholds for arrest depending on an individual's race—would be considered an unwarranted factor driving arrest disparities. Given the striking differences in Black and White arrest rates, much research attempts to determine whether warranted or unwarranted factors drive these differences.

One approach for determining whether unwarranted factors play a role in arrest rate disparities uses self-reported data that includes a measure of underlying criminal activity. This research has generally found that self-reported delinquency and other individual characteristics cannot account for the large arrest disparities, indicating that there are at least some unwarranted factors contributing to the discrepancies (Anersen 2015; Gase et al. 2016; Schleiden et al. 2020). Schleiden et al.'s 2020 analysis is perhaps the most striking of these, which found that Black young

[4] Research abounds finding differential sentencing and incarceration outcomes based on race. Many researchers using administrative datasets have found a sentencing disparity favoring White individuals that remains after controlling for legally relevant variables (Curry & Corral-Camacho 2008; Everett & Wojtkiewicz 2002; Mustard 2001; Doerner & Demuth 2009; Schlesinger 2007).

adults (ages 24–32) had experienced an arrest rate[5] seven times higher than their White counterparts after adjusting for contextual and behavioral factors.

Many studies using the same methodology focus specifically on drug-related arrests and reach similar conclusions (Fite et al. 2009; Koch, Lee & Lee 2016). Some studies even find that Black respondents are less likely to report using drugs but are nonetheless more likely to report having been arrested for drug use (Kakade et al. 2012; Mitchell & Caudy 2015). In sum, this line of literature indicates that unwarranted factors play a role in arrest disparities by race, given researchers' consistent inability to explain away arrest disparities using various warranted factors in regression analyses.

B. Determinants of Charging and Conviction

Once an individual is arrested, prosecutors, judges, and juries are responsible for several decisions that ultimately affect the case's outcome. First is the prosecutor's decision of whether to bring charges against an arrestee. This step often acts as a screening mechanism that accounts for factors such as:

- strength of evidence
- likelihood of victim cooperation
- evidence of police misconduct in the arrest
- seriousness of the offense
- resource constraints
- characteristics of the individual defendant
- participation as a witness in a related case
- revocation of parole or probation instead of charging
- various procedural deficiencies

Once an arrestee has been charged, prosecutors may decide to drop the charge for any of the above reasons, or the judge may decide to dismiss the case. In some jurisdictions, prosecutors

[5] This included both juvenile and adult arrests.

charge arrestees almost universally and screen cases by later dismissing charges. For instance, Kutateladze et al. (2014) report that New York County, the prevailing practice in the prosecutor's office is to charge arrestees nearly universally, but later to drop charges against a significant number of arrestees. In contrast, other jurisdictions have lower initial charging rates, meaning that prosecutors are meaningfully screening at the charging stage rather than afterward. For example, the average percent of cases declined for prosecution in Florida for the years 2009–2013 was 22%, in comparison to Kutateladze et al.'s finding that only 4% of cases were declined for prosecution at the outset in New York County from 2010–2011.

These statistics suggest that across jurisdictions, prosecutors may use the decision not to charge someone (charging conditional on arrest) and the decision to drop a charge (conviction conditional on charging) in nearly identical manners. Indeed, Measures for Justice, an organization that collects data on criminal justice outcomes, recommends combining charges rejected for prosecution with cases dismissed when analyzing data (Measures for Justice). Given the cross-jurisdictional nature of my data, my main analysis combines the outcomes of charging and conviction to examine the likelihood an individual is convicted, conditional on having been arrested.

C. Potential Racial Disparities

Racial differences in charging and conviction rates are plausible in either direction. Given that I ultimately find lower conviction rates for Black arrestees, I focus the discussion in this section on plausible explanations for lower conviction rates. Either racial bias or race-neutral prosecutorial screening mechanisms could explain the lower conviction rates. This section discusses each in turn.

1. Racial Bias

Ample empirical research has established the existence of unexplained worse outcomes for Black individuals at the arrest and sentencing stages. Some researchers have presented evidence that some aspects of prosecutorial decision making—such as pretrial detention, the decision of whether to bring charges with a mandatory minimum, or charge reduction—disfavor Black arrestees. As such, it is highly unlikely that prosecutors exhibit racial bias against non-Black arrestees solely in the decisions to bring an initial charge or later drop that charge.

However, prosecutor racial bias against Black victims could translate into a lower interest in pursuing crimes with Black victims. This type of racial bias could result in lower conviction rates for Black arrestees, given the intraracial nature of crime.

2. Race-Neutral Screening Mechanisms

Various race-neutral screening mechanisms may lead to lower conviction rates for Black arrestees. First, selection effects at the arrest stage could lead to this result. As mentioned above, prosecutors often screen cases based on the strength of evidence, seriousness of the offense, and any evidence of police misconduct in the arrest. To the extent that police over-arrest Black individuals in a discriminatory manner, these race-neutral screening mechanisms could lead to a lower conviction rate for Black arrestees. Namely, if police officers are more willing to arrest Black individuals than White individuals (1) in situations when evidence of a crime is weak, (2) for relatively minor crimes, or (3) via use of any form of police misconduct, one would expect to see a higher conviction rate for White individuals based on prosecutorial (as well as judicial) screening.

Second, race-neutral screening based on victim or witness cooperation could lead to lower rates of conviction for Black arrestees. Crime is generally intraracial in nature. Mistrust of the criminal justice system among the Black population might lead to a lower willingness of victim or witness cooperation, which could ultimately lead to lower conviction rates for Black arrestees.

Third, racial differences in criminal records could plausibly result in lower conviction rates for Black arrestees. If Black individuals are more likely to already be on parole or probation at the time of arrest, prosecutors may be less likely to pursue a conviction against them and instead revoke parole or probation, which would yield a technically lower conviction rate.

III. Literature Review

A. Available Data

Empirical researchers study criminal justice outcomes using two categories of data. Some studies are based on longitudinal datasets that collect self-reported data on criminal justice involvement. The two major, nationally representative datasets that fall into this category are the National Longitudinal Survey of Youth (NLSY79 or NLSY97) and the National Longitudinal Study of Adolescent to Adult Health (ADD Health). While both studies focus on youth, they contain years' worth of information on respondents once they reach adulthood. Smaller longitudinal studies also occur at the local level and have been used in some instances to study criminal justice outcomes.

In the alternative, some studies use administrative datasets. A few databases compile data on criminal justice processing across jurisdictions, but most of these datasets tend to be jurisdiction-specific. These datasets typically contain detailed data on the procedural aspects of criminal justice processing but lack information on arrestees' personal backgrounds and characteristics. For example, the Bureau of Justice Statistics data “State Court Processing Statistics: Felony Defendants in Large Urban Counties”—a dataset used in two of the studies looking at the relationship between race and case dismissals—contains detailed procedural data. This information includes the defendant’s legal representation, the number of charges brought against the defendant, whether the presiding judge was elected or appointed, and whether the case was resolved via guilty plea or at trial. The dataset, however, lacks any information on the defendant’s personal characteristics beyond age, race, sex, and prior felony convictions.

Studies that have previously examined racial disparities in conviction rates have used administrative datasets, whereas I use the NLSY97, a self-reported dataset. Three main benefits come from using the NLSY97 to expand on prior studies. First, the NLSY97 contains a wealth of information on personal background and characteristics, such as respondents’ income, work history, education, household structure, neighborhood characteristics, and self-reported criminal activity and substance use. To the extent that criminal justice outcomes are related to income, neighborhood, and education level (among other variables)—which research has established are correlated with race—examining administrative datasets that do not include these variables may capture an effect, or lack of effect, of race that is attributable to other factors. I can account for this possibility by controlling for these characteristics in regressions. I am also able to control for self-reported delinquent activity.

Second, the NLSY97 questions on criminal justice involvement are not limited to certain types of crimes or specific jurisdictions. Rather, the NLSY97 asks all respondents, which constitute a nationally representative sample, about all arrests they experience. Where many studies based on administrative datasets are limited to specific jurisdictions (e.g., Berdejó 2018;

Kutateladze et al. 2014) or specific crime types, such as felonies (e.g., Franklin 2010; Tomic & Hakes 2008), the NLSY97 accounts for a nationally representative sample of all arrests.

Finally, the NLSY97 contains data on both arrests that do not ultimately result in a criminal charge and charges that do not ultimately result in conviction. As described above, prosecutorial screening can occur at either the charging stage or after charges have already been filed. Many administrative datasets contain only post-charging data and therefore may miss a major aspect of prosecutorial screening, depending on the jurisdiction being studied.

In sum, using the NLSY97 gives a broad, nationally representative picture on the disparities experienced by arrest. While I cannot look closely at the mechanisms through which disparities arise—for instance, through pretrial detention and bail decisions—my paper provides a broad, nationally representative understanding of conviction rates.

B. Prior Mixed Findings

Consistent patterns have emerged in the extensive research on the relationship between race/ethnicity and arrests and sentencing, but the middle stages of charging and conviction have been referred to as a black box as far as empirical research goes. Theory on the direction of racial disparities in conviction rates is ambiguous, and empirical research has yet to provide consistent evidence in either direction. Some studies find that Black individuals experience worse charging and conviction outcomes than White individuals, some find no difference, and some find that Black individuals are treated less harshly at these decision points.

Tomic and Hakes (2008) examine Bureau of Justice Statistics (BJS) data on 1990s felony defendants in highly populated US counties, which account for approximately 1/3 of the US population. Franklin (2010) performs a similar analysis using the same dataset but a more limited timeframe, which focused on felony defendants in 1998. When pooling all felony defendants, both studies find no impact of race on likelihood of case dismissal.

However, both studies find differences in case dismissal rates by race when examining certain subsets of felony defendants. When Franklin splits his analysis by region, he finds that charges against Black defendants in the South are less likely to be dropped than White defendants in the South. Tomic and Hakes split their analysis by crime type and find a higher case-dismissal rate for Black arrestees when the type of crime arrested for is one that involves police making on-scene, snap judgments.

Kutateladze et al. (2014) use a large dataset from the New York County District Attorney's office in 2010-2011 to examine the cumulative effects of race and ethnicity on criminal justice outcomes. They find that, compared with White individuals, Black and Latino[6] individuals experience many harsher outcomes in that they are more likely to be detained pretrial, be offered a plea agreement that includes incarceration (as compared to being offered a plea agreement with no incarceration), and be ultimately sentenced to a period of incarceration. However, they find that of arrestees who are charged by the DA's office, Black and Latino individuals are more likely to have their charges dropped than White individuals. They note two possible explanations for this finding, corresponding to the theories described above. First, police may be more willing to arrest Black and Latino individuals "even when insufficient evidence exists to support prosecution." Second, they suggest the possibility that victims in crimes involving Black and Latino perpetrators—who tend to be Black and Latino themselves due to the often intraracial nature of crime—may be less willing to cooperate with police, which may lead to lower conviction rates.

[6] I switch between using the terms "Hispanic" and "Latino" throughout this Dissertation. I do so to reflect the language used by the data and prior research I use. For instance, the NLSY97 uses the term "Hispanic," while Kutateladze et al. use the term Latino.

Berdejó (2018) uses detailed data from Dane County, Wisconsin to examine conviction outcomes by race. He finds that of individuals charged with a crime, White defendants were twenty-five percent more likely to have their principal charge either dropped or reduced.[7] He finds that these disparities are most pronounced in low-level crimes and in cases when the individual had no prior criminal record.

Notably, all four of these studies contain data only on those arrests in which prosecutors filed initial charges. To the extent that prosecutors' offices in the data use charging as a meaningful screening point, these studies may be capturing only a portion of relevant prosecutorial decision making.

Studies that focus on other outcomes also contain information on the relationship between race/ethnicity and charging and conviction. For instance, Stevens & Morash (2014) use the NLSY79 and NLSY97 to determine whether the trend towards a punitive focus in juvenile justice policy between 1980 and 2000 impacted boys' likelihood of various juvenile justice outcomes. While not their question of interest, one of their findings is that, overall, arrested black juveniles experience lower conviction rates than arrested white juveniles, and note that "[c]ontext-specific research is needed to sort out occasional findings of this apparent break in a pattern of harsher treatment of Black youth." Rehavi and Starr (2014) examine how charging decisions affect sentencing outcomes of federal defendants. Their main result is that prosecutors are more likely to bring a charge that comes with a mandatory minimum sentence against Black arrestees than White arrestees, leading to longer sentences. However, their descriptive statistics of whether an arrestee was charged and convicted present practically identical rates for Black and White defendants.

[7] Berdejó combines the outcomes of dropping charges with reducing charges. It is possible that splitting the outcomes and examining dropped charges only would yield different results.

In sum, existing research on the middle stages of criminal justice processing is mixed in its results and taken as a whole, leaves open questions as to how race impacts charging and conviction decisions. In contrast, studies concerning race's relation to arrests and sentencing decisions consistently reveal worse outcomes for Black individuals. My study is the first to use the extensive data available in the NLSY97 to examine nationally representative charging and conviction outcomes by race and ethnicity, while controlling for other individual-level variables that could impact criminal justice outcomes.

IV. Data and Methodology

A. Data

I use data from the NLSY97 to examine the relationship between race/ethnicity and conviction rates. The NLSY97 is a nationally representative[8] dataset that follows 8,984 American individuals who were born between 1980 and 1984. The initial survey was conducted in 1997, when the respondents were ages 12-17. The study was conducted annually from 1997 to 2011 and biennially after. The most recent available data comes from the 18th wave of the survey in 2017, when the respondents were ages 32-36.

This dataset is particularly well-suited to examine criminal justice processing in depth because of its longitudinal, nationally representative nature, extensive information on respondents'

[8] The NLSY97 sample is representative of the US civilian, noninstitutional population who were ages 12 to 16 as of December 31, 1996. The survey consists of an original cross-sectional sample and an additional, supplemental sample that oversampled Hispanic and non-Hispanic Black youth. The supplemental sample was included to provide statistical power when analyzing outcomes specific to these two groups.
To create the sample, interviewers selected over 90,000 households using probability sampling over non-overlapping geographical sampling areas. These sampling areas covered all fifty states and the District of Columbia and consisted of either metropolitan statistical areas, single counties, or clusters of neighboring counties. Probability sampling was also performed within these areas at the block level. Within these 90,000 households, 9,808 eligible youth screened into the survey.

underlying characteristics, and detailed data on self-reported criminal activity, arrests, charges, convictions, and incarcerations. Further, youth and young adults commit the vast majority of crimes, meaning that I capture data for the years in which individuals are most likely to commit crimes.

At each interview, respondents were asked whether they were arrested since the date of their last interview and, if so, how many times. Respondents can report an unlimited number of arrests for each period, which sometimes yields very high numbers of reported arrests.[9] Through 2002, the NLSY collected arrest-specific charging, conviction, and incarceration data on up to nine arrests. In the 2003 round and later, the NLSY did not collect charging, conviction, or incarceration data at the individual arrest level for survey periods in which a respondent reported more than three arrests. Because of this, I limit my main regression analysis to the first 3 arrests reported each survey period.[10]

For each of the first three arrests, the survey then collects detailed information on each arrest. For each arrest, respondent is asked whether they were charged and, for each charge, is then asked whether they were convicted. Subsequently for each conviction, the individual is asked if they were incarcerated as a result.[11]

For individuals who were charged, the data also contains information on the type of charge. Individuals can report being charged with one crime and convicted of a different crime.

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[9] For instance, in 2004, three respondents reported having experienced more than 10 arrests since the date of their last interview.

[10] For those respondents who reported more than 3 arrests in 2003 and later, the NLSY asked for outcomes about all arrests combined. I treat their responses as a single arrest for that period and include a flag variable to indicate these observations. I additionally run two robustness checks. First, I treat these respondents' data as three separate arrests (i.e., I include the observation for that year in the regression three times) and again include a flag variable for those observations. Second, I exclude the data from these individuals. Neither of these meaningfully changes my results.

[11] Note that sentencing data is not available for the 2003 wave of the survey.

Unfortunately, the NLSY97 does not contain data indicating whether each charge was for a misdemeanor or felony. While some charge categories are almost always felonies, such as robbery, and some are clearly misdemeanors, such as public disorder, most of the charge categories make it difficult to differentiate. Appendix Figure 2A provides a flow chart of the relevant criminal justice questions to illustrate how these questions progress.

B. Empirical Model

I limit my analyses to adult men, so respondents only enter the analysis sample after they turn 18. As of the 2017 survey, 2,126 of 4,599 male respondents had been arrested at least once (approximately 46%), and 1,837 had been arrested at least once as an adult (approximately 40%).

I estimate three main sets of regressions. Table 1 summarizes my sample for each regression equation. Regression (2) is the focus of this Article, but I also include results from regressions (1) and (3) in order to ensure that the patterns for arrest and sentencing in the NLSY97 reflect those of prior research.

Table 1: Description of Regressions

	(1)	(2)	(3)	(4)
	Outcome of Interest	**Observation Level**	**Dependent Variable Description**	**Number of observations** (Adult Men)
(1)	Arrests	Individual-Year (*it*)	0 if respondent *i* is not arrested in year *t* 1 if respondent *i* is arrested in year *t*	53,491 - 562 (missing arrest data) **52,929**
(2)	Convictions	Arrests (*a*)	0 if arrest *a* does not result in conviction 1 if arrest *a* results in conviction	5,450 arrests - 161 (missing charge data) - 370 (missing conviction data) - 495 (see footnote)[12] **4,424**
(3)	Incarcerations	Convictions (*c*)	0 if conviction *c* results in noncustodial sentence 1 if conviction *c* results in custodial sentence	2,161 convictions - 202 (missing sentencing data) **1,959**

My main regression equation is as follows:

$$Y_{j=1,2,3} = \alpha_0 + \beta X_{it} + \delta Z_i + \gamma_t + u_i + \varepsilon_{it} \qquad (1)$$

Y_j is an indicator variable for the three sets of outcomes as described in column (3) of Table 1. α_0 is the intercept term. X_{it} is a vector of individual, time-varying characteristics (e.g., census region where currently residing), while Z_i is a vector of individual, time-invariant characteristics (e.g., race/ethnicity). γ_t is a set of year fixed effects that captures overall time trends.

The individual error term is represented by u_i, and ε_{it} is the random error term. ε_{it} captures random variation over time, while u_i captures all unobserved, time-invariant characteristics of individual i that impact outcome Y. I will return to discussing assumptions about u_i and ε_{it} in the section discussing panel data below.

[12] Men reported 4+ arrests in 165 individual-years in 2003 and later. This means there are 495 arrests that should otherwise be included in the sample for which I do not have individual arrest-level data.

1. Explanatory Variables

My coefficients of interest are those associated with the variables for race/ethnicity (contained in vector Z_i). The NLSY directly asks respondents one question about race and one about ethnicity. The survey then combines these two variables into a single category of mutually exclusive variables for race/ethnicity, consisting of Black, Hispanic, Mixed Race (Non-Hispanic), and Non-Black/Non-Hispanic. For race/ethnicity, I generally follow the NLSY-created variable for race/ethnicity, with one exception. While the NLSY's Non-Black/Non-Hispanic category contains "White," "Asian or Pacific Islander," and "American Indian, Eskimo, or Aleut" individuals, I separate White individuals from the other two categories. Ultimately, I have four mutually exclusive indicator variables for Black, Hispanic, White, and other race.[13] The category for other race includes respondents marked as Mixed Race (Non-Hispanic), "Asian or Pacific Islander," and "American Indian, Eskimo, or Aleut." Because of the small sample size (282 respondents), I exclude anyone in the other race category from my analysis. In my regressions, I include indicator variables for Black and Hispanic, while White is the omitted reference group. Thus, my race/ethnicity regression coefficients indicate how each racial/ethnic group compares to their White counterparts.

Because the NLSY has a vast array of information on each individual respondent, I can observe and control for many characteristics that may be associated with criminal justice outcomes. To isolate the relationship between race/ethnicity and criminal justice outcomes, I control for other observable characteristics that may be correlated with race and impact an individual's criminal justice outcomes.

[13] It is likely that some individuals who fall into the Hispanic or other race category also identify as Black.

X_{it} (Z_i) is a vector of time-variant (time-invariant) characteristics that are likely to predict criminal justice processing outcomes. Geographic characteristics within X_{it} include an indicator variable for whether the respondent lives in a high-risk neighborhood[14] and mutually exclusive indicator variables for whether the respondent lives in the Southern, North Central, or Western regions (with the Northeastern region as the reference group). Age at the beginning of the survey is included in Z_i. I include this as a time-invariant variable rather than age each year because of the close correlation between age and year, which I control for in the time-varying intercept term, α_t.

Household characteristics are captured by indicator variables for whether the individual lives with a spouse or child. Respondents' labor market status is captured within X_{it} by indicator variables for whether the individual was employed or enrolled in school (either high school or college). I also include an indicator variable for whether the respondent was a college graduate. I capture respondents' financial wellbeing by an indicator variable for whether a respondent's family falls below 2x the poverty line. All household, labor market, education, and financial variables mentioned in this paragraph are measured at time t-1, so as to reduce concerns of simultaneity bias.

Within X_{it} and Z_i, I also include a measure of respondents' likely criminality activity by including variables that capture their self-reported criminal/delinquent activity and illegal substance use. Ideally, I would control for each of these as time-variant variables at each survey date. However, the NLSY stopped asking questions regarding criminal activity of the majority of

[14] Following Mitchell and Caudy (2015), I create a variable that combines whether the respondent resides in an inner-city neighborhood with whether the respondent reports that their neighborhood or school has gangs, to create a single dichotomous variable that reflects whether an individual lives in a high-risk neighborhood.

respondents after 2003. Because of the age range of NLSY respondents, the youngest respondents were 18 as of their 2003 interview, while the oldest respondents were 24 at the time of their 2003 interview. I therefore limit my measure of criminality to criminal activity reported prior to age 18, to have a consistent measure of criminality across ages. Because my analysis is limited to adult men, my variable for delinquency is time-invariant and is represented in Z_i.

The NLSY creates its own delinquency scale that takes on values from 0-10. In 1997, the survey asks respondents 10 separate questions about whether they have engaged in various types of delinquent activities. These include:

1. Have you ever run away, that is, left home and stayed away at least overnight without your parent's prior knowledge or permission?
2. Have you ever carried a hand gun? When we say hand gun, we mean any firearm other than a rifle or shotgun.
3. Have you ever belonged to a gang?
4. Have you ever purposely damaged or destroyed property that did not belong to you?
5. Have you ever stolen something from a store or something that did not belong to you worth less than 50 dollars?
6. Have you ever stolen something from a store, person or house, or something that did not belong to you worth 50 dollars or more including stealing a car?
7. Have you ever committed other property crimes such as fencing, receiving, possessing or selling stolen property, or cheated someone by selling them something that was worthless or worth much less than what you said it was?
8. Have you ever attacked someone with the idea of seriously hurting them or have a situation end up in a serious fight or assault of some kind?
9. Have you ever sold or helped sell marijuana (pot, grass), hashish (hash) or other hard drugs such as heroin, cocaine or LSD?
10. Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?[15]

The NLSY then sums the number of questions that respondents answered yes to, creating a discrete variable that can take on values 0-10. In follow up surveys, this same variable is created, except the questions change from "Have you ever" to "Since your last interview, have you". Rather than use one value of this measure in a single survey year, I combine respondents' answers to these

[15] NLSY97 CODEBOOK SUPPLEMENT

questions up through age 18 to create a similar, cross-year delinquency measure. I exclude juvenile arrests from this measure, as this is likely to be a racially biased measure and does not directly reflect delinquent activity. My ultimate delinquency measure reflects the number of the above-listed activities (0-9) that an individual respondent reports having engaged in prior to the survey in which he turned 18.

For drug use, I include indicator variables for whether the individual used marijuana or hard drugs at time $t-1$. Finally, I include a variable for the number of prior arrests the respondent had experienced to capture the effects of a criminal record on conviction.

2. Panel Data Considerations

The data I am using is panel data, meaning that each respondent is interviewed multiple times, and the N observations in my dataset are made up of $t = (1, 2, \ldots, T)$ observations across $i = (1, 2, \ldots I)$ individuals. The panel nature of the NLSY data is useful because it allows for examination of both between-individual and within-individual variations. This means that I am able to study the between-individual relationship between race and criminal justice outcomes—as I would be able to do using cross-sectional data—while also gaining the added benefit of being able to study within-individual impact of characteristics that vary over time.

Another benefit of panel data over cross-sectional data is the ability to account for some omitted variables that might otherwise introduce bias in a standard regression analysis. By using panel data, researchers can net out unobserved, time-invariant factors (represented by u_i in my equations outlined above), reducing concerns of omitted variables bias. An example of a potential omitted variable is parental criminal justice contact—a measure not included in the NLSY—which may influence a respondent's knowledge and perception of the criminal justice system and impact likelihood of conviction.

The most basic means to achieve this goal is the use of a fixed-effects regression model. The fixed-effects model treats u_i as a time-invariant, individual-specific effect, which is netted out in the estimation. An important strength of fixed-effects estimation is that no assumptions about the correlation between u_i and the right-hand-side variables is required for the estimator to be unbiased and consistent.

An alternate regression model used in panel data analysis is that of random effects. The random effects model requires stronger assumptions about the underlying data than the fixed effects model. Namely, the random effects model assumes that the individual error term, u_i, is a set of random variables that follows a specified probability distribution. Most important is that consistent estimation of a random effects model requires that u_i be uncorrelated with the right-hand-side variables (Allison 2009).

While the fixed effects estimator does not require that the individual effect u_i be uncorrelated with the right-hand-side variables, it also comes with one major drawback. Fixed effects regressions eliminate the ability to estimate the coefficient on any time-invariant characteristic. Given that the relationship between race/ethnicity—a time-invariant characteristic—and criminal justice outcomes is my question of interest, I am unable to use a fixed-effects model. The random-effects model also proves potentially problematic because of the assumption I must make that u_i is independent of X_{it} and Z_i.

One can test whether the more stringent assumptions of a random-effects model are appropriate by comparing the results from a fixed-effects model with a random-effects model. This is done using a Hausman test. If the Hausman test reveals that the time-variant coefficients resulting from each model are sufficiently similar, one can assume that the results from the random-effects model are consistent. However, if the Hausman test reveals that the coefficients from each model are sufficiently different, it indicates that the random-effects model is biased and that the fixed-effects results should be preferred.

After running each model and using a Hausman test, I find that the traditional random-effects model is biased. This is problematic due to the fixed-effects model's inability to estimate my coefficient of interest. As such, I use an alternate model: random effects with a Hausman-Taylor correction. This is known as a "hybrid" model, as it seeks to incorporate the benefits of the fixed-effects model (relaxed assumptions) with the benefits of the random-effects model (ability to estimate the coefficient on time-invariant characteristics).

The Hausman-Taylor estimator uses an instrumental variables approach. I outline the steps of the H-T process below:

1. Split the right-hand-side variables into four categories: time-variant exogenous (X_{1it}), time-variant endogenous (X_{2it}), time-invariant exogenous (Z_{1i}), and time-invariant endogenous (Z_{2i}). There groupings are based on theoretical assumptions, where X_{1it} and Z_{1i} are assumed to be uncorrelated with both u_i and ε_{it}, and X_{2it} and Z_{2i} are assumed to be uncorrelated with ε_{it} but may be correlated with u_i. Thus, my regression equation (1) from above becomes:

$$Y_j = \alpha_t + \beta_1 X_{1it} + \beta_2 X_{2it} + \delta_1 Z_{1i} + \delta_2 Z_{2i} + u_i + \varepsilon_{it}$$

2. Use a within-effects estimator to consistently estimate the fixed effects coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$.
3. Use these estimates to obtain within residuals, $\hat{d}_i$.
4. Regress $\hat{d}_i$ on Z_{1i} and Z_{2i}, using X_{1it} and Z_{1i} as instruments. This regression provides intermediate, consistent estimators, $\hat{\delta}_{1\mathrm{IV}}$ and $\hat{\delta}_{2\mathrm{IV}}$.
5. Use $\hat{\beta}_1$, $\hat{\beta}_2$, $\hat{\delta}_{1\mathrm{IV}}$, and $\hat{\delta}_{2\mathrm{IV}}$ to obtain an estimate of σ_u^2, an estimate of the variance of the individual random error effect.
6. Perform a standard random-effects GLS transform on each variable.
7. Fit an instrumental-variables regression of the GLS transformed variables.

I make the following assumptions about endogeneity of the right-hand-side variables:

- **Exogenous, time variant variables (X_{1it}):** Year indicators, region indicators
- **Endogenous, time variant variables (X_{2it}):** Indicator for living below 2x the poverty line, indicator for enrolled in school, indicator for college graduate, indicator for employed, indicators for hard drug and marijuana use, indicator for living in a high-risk neighborhood
- **Exogenous, time invariant variables (Z_{1i}):** Race/ethnicity indicators, age in 1997
- **Endogenous, time invariant variables (Z_{2i}):** Measure of delinquency

V. Results

A. Descriptive Statistics

I begin by checking whether my conviction rates are reasonable using other publicly available data on criminal justice outcomes. Measures for Justice, an organization dedicated to providing access to accurate criminal justice data, has data available for 11 states on the percentage of cases filed in court that result in a conviction, which corresponds to my data on convictions per charge. Unfortunately, data on the rate of charges that were declined for prosecution—which corresponds to my data on charges per arrest—is only available for one state. I do, however, find that my convictions per charge is reasonable as compared to the 11 states with that available data, which average 68.12%. My average conviction rate per charge across the entire sample is 71.18%.[16]

Table 2 shows the raw disparities in rates for arrest, conviction, and incarceration rates. The final column represents whether there are differences between each group at the 95% significance level. There are raw differences in all groups with the exception of the Hispanic-White arrest rate and the Black-Hispanic incarceration rate. Notably, Black respondents have the highest arrest and incarceration rates of any group.

[16] I include both women and juveniles in this estimate, as the Measures for Justice data is missing data on age and sex for many of the 11 states.

Disparities disfavoring the Black population in arrests are also borne out in cumulative statistics. For instance, 50.35% of Black men had been arrested as an adult at least once as of 2015, while 39.34% of Hispanic men and 34.73% of White men had been arrested as adults. This translates to a 28% and 45% higher probability of arrest for Black men than Hispanic and White men, respectively.

Given these outcomes, one might expect similarly poor outcomes for Black individuals at the conviction stage. However, as seen in Table 2, relative to both Hispanic and White men, Black respondents have the lowest conviction rate. These raw differences are in line with the subset of the literature that finds Black individuals receive more favorable treatment in terms of case dismissals (Kutateladze et al. 2014; Tomic & Hakes 2008).

Table 2: Descriptive Statistics

	Black Men	Hispanic Men	White Men	Difference
Arrested (Per Year)	9.32%	6.32%	5.98%	B-H, B-W
Convicted (Per Arrest)	41.69%	48.53%	55.27%	All
Incarcerated (Per Conviction)	64.34%	60.21%	45.96%	B-W, H-W

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. Statistical differences tested using Bonferroni multiple comparison test at the 5% level. Arrest rates (per year) exclude those who were in prison at the time of the prior survey.

B. Main Results

I next turn to regression results, reported in Table 3. After examining the descriptive statistics, regression analysis is important to determine whether the disparities reflect systematic differences between racial and ethnic groups in other characteristics that might be legally relevant for case processing. Columns (1), (2), and (3) report my results from the Hausman-Taylor specification for the outcomes of arrested per survey period, convicted per arrest, and incarcerated per conviction, respectively.

Table 3: Likelihood of Arrest, Conviction, and Incarceration

	(1) Arrest	(1) Conviction	(2) Incarceration
Black	0.067***	-0.157***	0.228***
	(0.009)	(0.028)	(0.048)
Hispanic	0.013	-0.072**	0.229***
	(0.008)	(0.031)	(0.051)
Delinquency Index	0.049***	0.002	0.099***
	(0.015)	(0.019)	(0.038)
Southern Region	0.001	-0.044	0.092*
	(0.008)	(0.033)	(0.055)
North Central Region	0.023**	0.015	0.083
	(0.009)	(0.033)	(0.053)
Western Region	0.011	-0.017	0.032
	(0.009)	(0.034)	(0.058)
High Risk Neighborhood	0.021***	-0.047	-0.036
	(0.007)	(0.046)	(0.070)
Household Below 2x Poverty Line	-0.001	0.022	-0.001
	(0.003)	(0.029)	(0.038)
Spouse in Household	-0.018***	-0.070	0.106
	(0.004)	(0.055)	(0.085)
Child in Household	-0.018***	-0.053	-0.115*
	(0.004)	(0.042)	(0.067)
Enrolled in School	-0.002	-0.002	-0.107*
	(0.004)	(0.040)	(0.065)
Employed	-0.005	-0.048	0.000
	(0.005)	(0.032)	(0.054)
College Graduate	-0.021***	-0.222*	0.193
	(0.005)	(0.126)	(0.179)
Hard Drug User	0.038***	-0.027	0.075
	(0.008)	(0.037)	(0.060)
Marijuana User	0.017***	0.022	-0.039
	(0.005)	(0.030)	(0.048)
Number of Prior Arrests	-0.083***	0.000	0.013
	(0.004)	(0.006)	(0.010)
Observations	50,540	4,388	1,963
Respondents	4,304	1,631	1,036

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.
*** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997) and survey year (at time *t*) are included in all regressions. Flag variable for four plus arrests included.

The patterns from my regression analysis generally follow that of the disparities from descriptive statistics. Compared to White men, Black men are more likely to be arrested (approximately 6.7 percentage points per survey period) and incarcerated (approximately 22.8 percentage points per conviction). Hispanic men were also more likely to be incarcerated than White men (approximately 23 percentage points per conviction). My regression of interest, shown in column (2), indicates that Black arrested men were 15.7 percentage points less likely to be convicted than White arrested men, and Hispanic arrested men were 7.2 percentage points less likely to be convicted than their White counterparts.

Those with a higher delinquency index, based on self-reported delinquent activity as juveniles, are significantly more likely to be arrested and incarcerated. These results lend support for these self-reported measures being valid proxies for an individual's actual criminal behavior as an adult. Other than the indicator for Black and Hispanic, the only other statistically significant predictor of conviction is whether the respondent is a college graduate. The fact that arrestees with college degrees are less likely to be convicted may reflect prosecutors', judges', and juries' perceptions that these individuals are less likely to commit future crimes or are less blameworthy for the crimes they have been arrested for.

It may appear odd that having a more extensive arrest record yields lower arrest and rates. However, this result can be explained by the nature of the regression. The Hausman-Taylor regression coefficients on time-variant variables reflect the within variation for individual respondents, and not the variation between different respondents. When comparing between two respondents—one with a prior arrest and one without—it is true that the individual who has been arrested in the past is more likely to be arrested in a future period. However, when comparing the variation within individuals who are ultimately arrested, the likelihood of future arrest decreases with each arrest, since not all individuals who are arrested once will be rearrested in the future.

C. Controlling for Charge Type

One possible explanation for the lower conviction rate for Black individuals is that they are arrested for different types of crimes than White individuals—namely, crimes that are less likely to result in a conviction. In order to check this, I estimate regressions that also include charge type.

The regressions in Table 4 take the same form as my main regressions, with two changes. First, I add a vector of indicator variables, W_c, on the right-hand side of the equation. W_c is a vector of indicator variables that captures the type of crime the individual was charged with, as this is highly likely to impact conviction outcomes. The charges are not mutually exclusive, in that an individual can report multiple charges for a single arrest. The charges include: assault,[17] robbery,[18]

[17] Answered yes to: "Did the police charge you with assault, that is, an attack with a weapon or your hands, such as battery, rape, aggravated assault, or manslaughter?"
[18] Answered yes to: "Did the police charge you with robbery, that is taking something from someone using a weapon or force?"

burglary,[19] theft,[20] destruction of property,[21] other property crimes,[22] possession of illegal drugs,[23] selling illegal drugs,[24] major traffic offense,[25] public order offense,[26] and other offense.[27]

Second, because I do not have data on the type of charge unless the individual was charged with a crime, I only run two regressions: conviction conditional on charging (as opposed to conviction conditional on arrest in Table 3) and incarceration conditional on conviction.

Table 4: Likelihood of Conviction and Incarceration (Controlling for Charge Type)

	(1) Conviction	(2) Incarceration
Black	-0.114***	0.185***
	(0.031)	(0.049)
Hispanic	-0.023	0.208***
	(0.032)	(0.050)
Delinquency Index	-0.016	0.088***
	(0.018)	(0.028)
Southern Region	-0.004	0.085
	(0.034)	(0.053)
North Central Region	0.070**	0.086*
	(0.033)	(0.052)
Western Region	0.062*	0.039
	(0.036)	(0.058)
High Risk Neighborhood	-0.070	-0.035
	(0.050)	(0.069)
Household Below 2x Poverty Line	0.038	0.012
	(0.028)	(0.037)

[19] Answered yes to: "Did the police charge you with burglary and breaking and entering, that is, breaking into private property without permission in order to steal?"
[20] Answered yes to: "Did the police charge you with theft, that is, stealing something without the use of force, such as auto theft, larceny, or shoplifting?"
[21] Answered yes to: "Did the police charge you with destruction of property, that is, vandalism, arson, malicious destruction, or shoplifting?"
[22] Answered yes to: "Did the police charge you with other property offenses, such as, fencing, receiving, possessing or selling stolen property?"
[23] Answered yes to: "Did the police charge you with possession or use of illicit drugs?"
[24] Answered yes to: "Did the police charge you with the sale or trafficking of illicit drugs?"
[25] Answered yes to: "Did the police charge you with a major traffic offense, such as, driving under the influence of alcohol or other drugs, reckless driving, or driving without a license?"
[26] Answered yes to: "Did the police charge you with a public order offense, such as, drinking or purchasing alcohol while under the legal age, disorderly conduct or a sex offense?"
[27] Answered yes to: "Did the police charge you with any other offense we have not talked about?"

Spouse in Household	-0.077	0.085
	(0.062)	(0.086)
Child in Household	-0.044	-0.103
	(0.046)	(0.063)
Enrolled in School	-0.017	-0.106*
	(0.046)	(0.064)
Employed	-0.056	-0.007
	(0.037)	(0.053)
College Graduate	-0.127	0.113
	(0.147)	(0.161)
Hard Drug User	0.010	0.070
	(0.038)	(0.056)
Marijuana User	-0.034	-0.034
	(0.032)	(0.047)
Number of Prior Arrests	0.009	0.012
	(0.006)	(0.009)
Charged with Assault	-0.064**	0.042
	(0.032)	(0.045)
Charged with Burglary	0.102**	0.081
	(0.040)	(0.057)
Charged with Robbery	0.060	0.049
	(0.044)	(0.051)
Charged with Theft	0.009	-0.005
	(0.041)	(0.056)
Charged with Destruction of Property	0.010	-0.012
	(0.045)	(0.060)
Charged with Other Property Crime	0.004	0.056
	(0.046)	(0.079)
Charged with Drug Possession	0.061**	0.023
	(0.029)	(0.040)
Charged with Drug Sale	0.060	0.013
	(0.040)	(0.057)
Charged with Major Traffic Offense	0.095***	-0.059*
	(0.024)	(0.034)
Charged with Public Disorder	0.043	-0.141***
	(0.032)	(0.048)
Charge with Other Crime	-0.002	-0.003
	(0.026)	(0.037)
Observations	3,092	1,950
Number of Individuals	1,320	1,031

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.
*** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997), survey year (at time *t*), and region (at time *t*) are included in all regressions.

Including charge type does not eliminate the racial effect, indicating that my results on conviction and incarceration are not explained by systematic differences in the types of crimes Black and White individuals are arrested for. The magnitude of my result in this regression is smaller than in my main regression (11 percentage points as compared with 16 percentage points). This result is expected, given that this regression is limited to individuals who are charged with a crime and thus does not capture any racial disparity in the original charging decision. This regression does, however, eliminate the ethnicity effect on conviction. Appendix Table 9A reveals that lower conviction rates for Hispanic men are driven entirely by lower charging rates, conditional on arrest. Thus, the effect of ethnicity is not captured in this regression, which looks only at conviction conditional on charging.

The coefficients on likelihood of conviction by charge type align with a recent report from the Bureau of Justice Statistics (BJS) on conviction rates of felony defendants. The BJS reported that in 2018, felony defendants originally charged with assault were the least likely to ultimately be convicted (45% conviction rate) and that those charged with motor-vehicle theft (74%), driving-related offenses (73%), murder (70%), and burglary (69%) were the most likely to be convicted. While the NLSY97 also includes misdemeanors and does not have separate charging categories for motor-vehicle theft or murder, my coefficients generally align with the report. I find a relatively lower conviction likelihood for those charged with assault, which aligns with it being the lowest conviction rate for felony defendants, and I find relatively higher conviction likelihoods for those charged with burglary and driving-related offenses, which aligns with those crimes being among the four highest conviction rates for felony defendants.

D. Results by Region

It is next worth considering whether these results vary by region. I run my main regression again, where conviction conditional on arrest is the outcome, but split the sample by region. Table 5 shows the NLSY97's definition of its four main geographical regions.

Table 5: Regions of the NLSY97

Northeast	North Central	South	West
Connecticut Maine Massachusetts New Hampshire New Jersey New York Pennsylvania Rhode Island Vermont	Illinois Indiana Iowa Kansas Michigan Minnesota Missouri Nebraska North Dakota Ohio South Dakota Wisconsin	Alabama Arkansas Delaware District of Columbia Florida Georgia Kentucky Louisiana Maryland Mississippi North Carolina Oklahoma South Carolina Tennessee Texas Virginia West Virginia	Alaska Arizona California Colorado Hawaii Idaho Montana Nevada New Mexico Oregon Utah Washington Wyoming

I find that the racial disparity exists everywhere but the Western region of the United States. Conviction rate differences are especially large in the Northeast (36.7 percentage points lower) and North Central (28.4 percentage points lower) regions. Further, the conviction disparity between Hispanic and White individuals is limited to the Northeast. Also of note is that poverty is highly predictive of conviction in the North Central region. This could indicate that cash bail practices may be particularly problematic in that region.

Table 6: Likelihood of Conviction by Region

	(1) Northeast	(2) South	(3) North Central	(4) West
Black	-0.367***	-0.087**	-0.284***	-0.056
	(0.076)	(0.040)	(0.065)	(0.087)
Hispanic	-0.341***	-0.031	-0.004	-0.041
	(0.091)	(0.049)	(0.085)	(0.060)
Delinquency Index	-0.055	-0.001	-0.007	0.006
	(0.045)	(0.034)	(0.029)	(0.045)
High Risk Neighborhood	-0.019	-0.063	-0.063	0.065
	(0.162)	(0.064)	(0.085)	(0.108)
Household Below 2x Poverty Line	0.052	-0.034	0.150***	-0.080
	(0.071)	(0.046)	(0.051)	(0.080)
Spouse in Household	-0.259	-0.132	-0.064	-0.211*
	(0.257)	(0.097)	(0.100)	(0.119)
Child in Household	-0.071	-0.009	-0.075	-0.083
	(0.097)	(0.072)	(0.089)	(0.085)
Enrolled in School	-0.127	-0.042	0.136*	-0.033
	(0.106)	(0.060)	(0.071)	(0.071)
Employed	-0.196**	-0.064	-0.110	0.104
	(0.088)	(0.047)	(0.069)	(0.063)
College Graduate	-0.211	-0.251	0.388	-0.719***
	(0.153)	(0.220)	(0.338)	(0.226)
Hard Drug User	-0.038	0.016	-0.043	0.021
	(0.084)	(0.072)	(0.064)	(0.088)
Marijuana User	0.011	-0.027	0.107**	-0.036
	(0.084)	(0.047)	(0.052)	(0.077)
Number of Prior Arrests	-0.001	-0.006	0.014	0.005
	(0.024)	(0.008)	(0.009)	(0.012)
Observations	628	1,860	1,080	788
Number of Individuals	268	696	390	330

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.
*** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997), survey year (at time *t*), and region (at time *t*) are included in all regressions.

It is not obvious what drives the differences in predictors of conviction by region. One possibility is differences in the use of criminal fines as a mechanism for generating revenue. In jurisdictions where cities are dependent on criminal fines as a large portion of revenue, prosecutors might be more likely to seek quick plea bargains for very minor charges in cases when they otherwise might decline to bring charges or later drop them. Future research on region specific policing and prosecutorial practices is needed to inform the regional differences.

E. Potential Mechanisms

As discussed above, four potential explanations could result in lower conviction rates for Black men. First, I argue that racial bias against non-Black arrestees is highly unlikely. Researchers have found consistent evidence of racial bias at nearly all other points in the criminal justice system. It is unlikely that the decision to not charge an arrestee or later drop the charge differs from all other stages. I can provide some insight into which of the three remaining explanations is true through supplementary analysis.

To determine whether higher rates of active probation or parole among Black men could be driving the results, I perform a supplemental analysis that includes first arrests only. The results of this analysis are presented in Table 7. I find that a large disparity persists in these first-time arrests, with Black arrestees being 14 percentage points less likely to be convicted. These results indicate that differing parole or probation rates by race are not driving the results.

Table 7: Likelihood of Conviction and Incarceration: First Arrest Only

	(1) Conviction	(2) Incarceration
Black	-0.141***	0.198***
	(0.032)	(0.051)
Hispanic	-0.106***	0.179***
	(0.035)	(0.052)
Delinquency Index	-0.005	0.032***
	(0.005)	(0.008)
Southern Region	-0.048	-0.042
	(0.037)	(0.056)
North Central Region	0.015	0.051
	(0.039)	(0.056)

Western Region	-0.016	-0.013
	(0.042)	(0.064)
High Risk Neighborhood	-0.029	0.046
	(0.036)	(0.064)
Household Below 2x Poverty Line	0.006	0.032
	(0.027)	(0.039)
Spouse in Household	-0.006	0.037
	(0.071)	(0.100)
Child in Household	-0.070	0.012
	(0.053)	(0.085)
Enrolled in School	-0.049	-0.141***
	(0.030)	(0.044)
Employed	-0.011	-0.061
	(0.032)	(0.050)
College Graduate	-0.039	-0.262***
	(0.076)	(0.082)
Hard Drug User	0.033	0.066
	(0.041)	(0.057)
Marijuana User	0.015	-0.056
	(0.028)	(0.042)
Observations	1,902	761
R^2	0.057	0.130

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. *** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997), survey year (at time *t*), and region (at time *t*) are included in all regressions.

Next, the conviction disparity could reflect policing practices that result in discriminatory arrest of Black men. Prosecutors use screening processes that should, in theory, eliminate cases with weak evidence, for some minor crimes, or that involve police misconduct. If police are more likely to arrest Black men—as opposed to White men—in these scenarios, prosecutors' screening would result in lower charging and conviction rates for Black men. Second, given the intraracial nature of crime, this disparity could be explained by victim characteristics. It is possible that either prosecutors are less inclined to pursue cases with Black victims due to racial bias or that there is a lower rate of victim cooperation rate among Black victims, given mistrust of the criminal justice system.

To investigate this possibility, I model my analysis off Tomic and Hakes (2008), who find that Black defendants experience higher rates of charge dismissal than White defendants in cases for which police have high discretion and make on the spot decisions. Their category of high-discretion crimes is described as those that "lead to on-scene arrests." In this category, they include driving offenses, weapons offenses, drug trafficking, other drug charges, public offenses, and "other violent" crimes, which tend to involve domestic violence. Their non-high-discretion crimes include murder, rape, robbery, assault, theft, other property, and burglary.[28] I follow their methodology and map my crimes onto their two categories, as follows:

High Discretion Crimes	Low Discretion Crimes
Drug Possession	Assault
Drug Sale	Burglary
Major Traffic Offenses	Robbery
Public Disorder	Theft
	Destruction of Property
	Other Property Crime

Table 8: Likelihood of Conviction: High- and Low-Discretion Crimes

	(1) High-Discretion Crimes	(2) Low-Discretion Crimes
Black	-0.157***	-0.079
	(0.056)	(0.066)
Hispanic	-0.029	-0.030
	(0.058)	(0.076)
Delinquency Index	0.013	0.028
	(0.030)	(0.062)
Southern Region	-0.001	0.115
	(0.056)	(0.075)
North Central Region	0.043	0.218**
	(0.057)	(0.101)
Western Region	0.055	0.208**
	(0.057)	(0.092)

[28] I exclude observations that do not indicate a specific crime type or chose "other crime" for charge type, as these cannot map specifically onto high or low discretion crimes. However, I run robustness checks in which I include these observations in both categories, and their inclusion does not change my results.

Inner City	0.065	-0.170*
	(0.114)	(0.089)
Household Below 2x Poverty Line	0.130***	-0.030
	(0.047)	(0.104)
Spouse in Household	-0.131	-0.482*
	(0.121)	(0.273)
Child in Household	0.097	-0.111
	(0.089)	(0.132)
Enrolled in School	0.007	-0.208**
	(0.069)	(0.093)
Employed	0.014	-0.029
	(0.057)	(0.087)
College Graduate	-0.261	0.239
	(0.332)	(0.299)
Hard Drug User	-0.043	0.079
	(0.062)	(0.096)
Marijuana User	-0.009	-0.045
	(0.053)	(0.069)
Number of Prior Arrests	0.011	-0.003
	(0.012)	(0.017)
Observations	1,324	646
Number of Individuals	791	453

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.
*** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997), survey year (at time *t*), and region (at time *t*), and charge type are included.

I find that disparity in conviction rates is concentrated in high police discretion crimes, supporting their hypothesis that lower conviction rates for Black individuals are related to use of police discretion in a discriminatory manner. Also notable is that in the category of high-discretion crimes, which are also largely made up of misdemeanors, poverty is a large predictor of conviction. This highlights problems with cash bail and the criminalization of poverty more generally.

In addition, the split of high-discretion and other crimes also conveniently maps onto crimes that likely involve victims. The high-discretion crimes, where my results are concentrated, are generally victimless crimes. Thus, it is unlikely that the conviction rate disparity is related to either prosecutors' perceptions of victims or the likelihood of victim or witness cooperation, and I am left with the most likely explanation being over-arrest of Black men for high-discretion crimes.

VI. Discussion

A clear pattern of criminal justice processing emerges in my analysis. First, Black individuals are at a higher risk of arrest each survey period, even after controlling for other relevant factors such as socioeconomic status, living in a high-risk neighborhood, region, education, family, and self-reported delinquency. Second, the likelihood of conviction for Black arrestees is significantly lower than for White arrestees. There is some evidence that Hispanic arrestees experience lower conviction rates than White arrestees, but it is less robust and limited to the charging stage and the Northeastern region of the United States. Finally, of those who are convicted, Black and Hispanic individuals are incarcerated at much higher rates than White individuals.

My findings of disparities that favor White respondents at the arrest and sentencing stage offer nothing more than confirmation of previous researchers' findings. My findings on conviction rates, however, provide new and important information on the experiences of Black male arrestees through the United States. Specifically, I find that they are 29% (approximately 16 percentage points) less likely to be convicted of a crime, conditional on having been arrested. Given that this disparity is limited to crimes for which police make on-the-spot decisions about arrest, I suggest that racial bias in policing likely drives the results.

A. Reconciliation with Prior Research

Given the lack of consensus in the literature, it is worth considering the differences between my underlying sample and those studies that found either no difference or a disparity in favor of White defendants during the charging and conviction stages.

First, the only study to my knowledge that has found an association between Black arrestees and higher case dismissal rates is Berdejó's 2018 study. Berdejó combines the outcomes of case dismissal with charge reduction when analyzing racial disparities, and these two outcomes may be subject to different decision rules. Thus, it is possible Berdejó's results are being driven mostly by the outcome of charge reduction and not that of case dismissal.

Next, studies that use only post-charging data may not capture any meaningful prosecutorial screening if prosecutors in the jurisdiction being studied engage in screening at the initial charging decision. Finally, the crime type appears to matter. To the extent that drug possession, traffic offenses, and public disorder drive my results, it is unsurprising that studies including only felony crimes do not find overall lower conviction rates for Black arrestees, given that the majority of these crimes are not felonies.

In comparing my sample with prior research, I propose that large scale misdemeanor arrests, for which police maintain large amounts of discretion, likely result in lower conviction rates for Black arrestees on a large, national scale. Future research on conviction rate disparities should focus on the type of crimes analyzed, relevant prosecutorial practices of screening at charging versus afterwards, and different mechanisms at play in different prosecutorial decisions (such as case dismissal versus charge reduction).

B. Implications

How did we arrive at a world in which more than half of Black men are arrested by the time they reach young adulthood and many of them are never convicted of a crime? The answer appears to lie in policing practices and, namely, those related to offenses associated with Broken Windows policing and the war on drugs.

Broken Windows policing reflects the practice of arresting individuals for relatively minor public-disorder crimes based on the theory that such arrests will prevent more serious crimes. Issa Kohler-Hausman shows in her book, *Misdemeanorland*, that upon the introduction of Broken Windows policing in New York City in the early 1990s, the number of case dismissals rose dramatically and, in turn, conviction rates dropped. To the extent many arrests are not prosecuted, this style of policing essentially governs through arrest and any resulting consequences are doled out at the sole discretion of the arresting officers.

Research has shown that the Black community and other disadvantaged communities have borne the brunt of costs from Broken Windows policing (Fagan & Davies 2000; Gelman, Fagan & Kiss 2007). Advocates for racial justice have cited these types of arrests as a problem that is "central to the racial contours of American criminal justice." (Natapoff 2018).

Each arrest can be viewed as a decision subject to cost-benefit analysis. Potential societal benefits include reducing crime through deterrence and incapacitation. Potential costs include the money spent on policing and jails, risk of violence in the arrest interaction, psychological and financial costs of arrest, stigma assigned to an arrestee, and future limits placed on education and labor market opportunities of the arrestee.

The cost-benefit analysis in the context of Broken Windows and drug offenses is weaker than for other crimes. Some have argued that arrests from Broken Windows policing are criminogenic themselves, which flips the idea of specific deterrence on its head. (Howell 2009). The theory of deterrence in the context of drug arrests is highly speculative, given the addictive nature of drugs and the rationality assumption of deterrence theory. Despite the reduced benefits of these types of arrests, the costs—for the most part—remain.

Each arrest is psychologically and financially costly to the arrestee, cultivates lasting stigma directed at the arrestee and limits their future labor market opportunities, costs taxpayer money in the form of policing budgets, and increases the likelihood of police violence. At a broader level, racially disparate arrests fuel a perception of higher crime rates among Black individuals as compared with their White counterparts. This phenomenon also reduces trust in the policing system among the Black community. To the extent that Black men are being arrested for crimes that prosecutors do not ultimately pursue, policing practices that disproportionately target Black men waste government resources and create problematic arrest records that limit social mobility.

VII. Conclusion

This paper is the first to look at racial disparities in conviction rates for a nationally representative sample of arrests for all crime types in the United States. I find a large disparity 29% in comparing Black and White male arrestees' conviction rates, with Black men being convicted at lower rates than White men. Further, this disparity is concentrated in crimes for which police typically have high levels of discretion in the arrest decision. Low conviction rates for Black men thus likely reflect over-arrest among the Black population, raising cause for concern.

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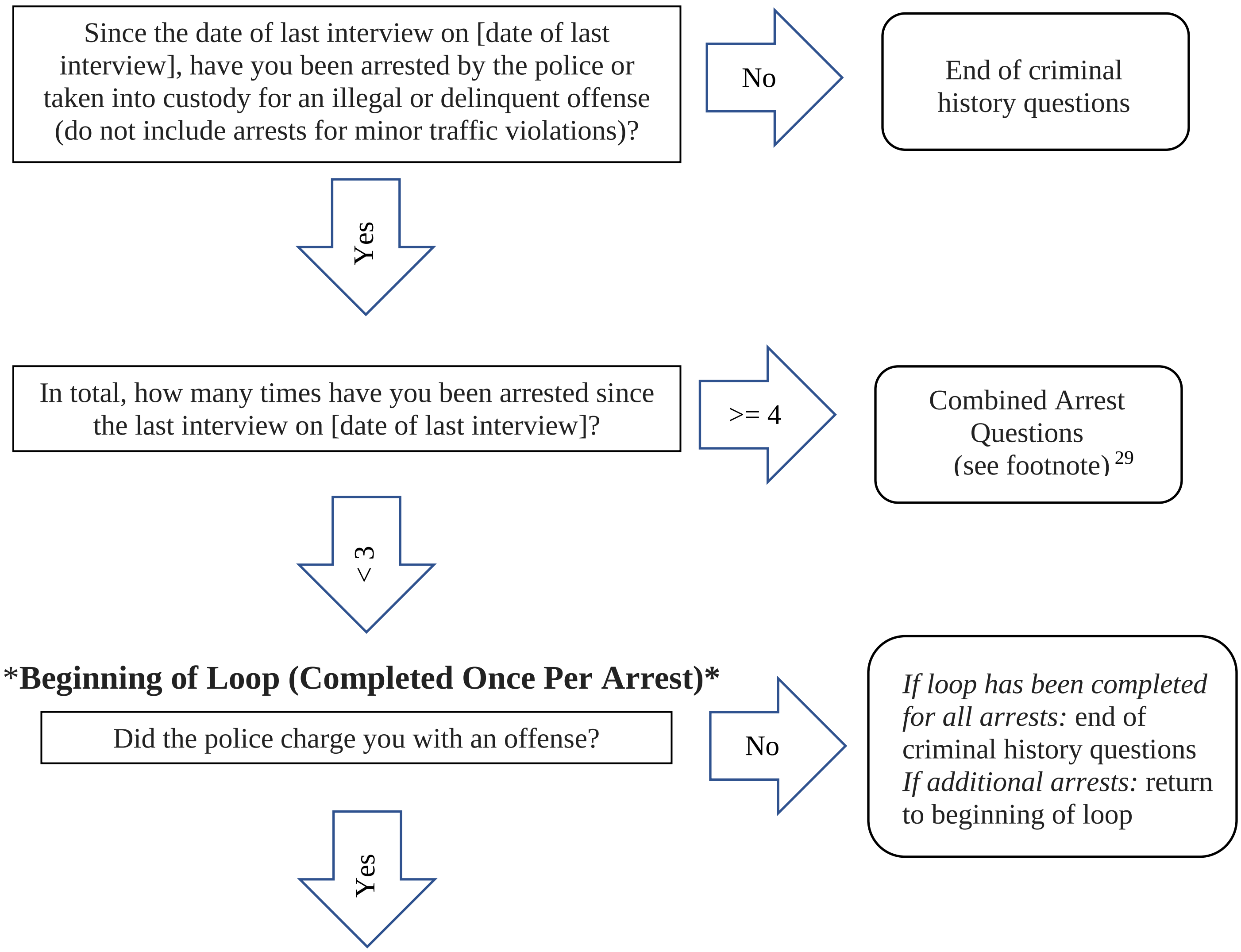
APPENDIX

Figure 2A: NSLY97 Criminal Justice Questions



[29] For those respondents who reported more than 3 arrests in 2003 and later, the NLSY asked for outcomes about all arrests combined.

Which of the following offenses did the police charge you with?
Note: Respondent can choose multiple options or none

- Assault, that is, an attack with a weapon or your hands, such as battery, rape, aggravated assault, or manslaughter
- Robbery, that is taking something from someone using a weapon or force
- Burglary and breaking and entering, that is, breaking into private property without permission in order to steal
- Theft, that is, stealing something without the use of force, such as auto theft, larceny, or shoplifting
- Destruction of property, that is, vandalism, arson, malicious destruction, or shoplifting
- Other property offenses, such as, fencing, receiving, possessing or selling stolen property
- Possession or use of illicit drugs
- Sale or trafficking of illicit drugs
- A major traffic offense, such as, driving under the influence of alcohol or other drugs, reckless driving, or driving without a license
- A order offense, such as, drinking or purchasing alcohol while under the legal age, disorderly conduct or a sex offense
- Any other offense we have not talked about

All

Were you convicted of, or found delinquent (adjudicated delinquent) of any charges, or did you plead guilty to any charges?

No

If loop has been completed for all arrests: end of criminal history questions
If additional arrests: return to beginning of loop

Yes

Which of the following charges were you convicted of or did you plead guilty to?
Note: Respondent can choose multiple options or none

- Assault, that is, an attack with a weapon or your hands, such as battery, rape, aggravated assault, or manslaughter
- Robbery, that is taking something from someone using a weapon or force
- Burglary and breaking and entering, that is, breaking into private property without permission in order to steal
- Theft, that is, stealing something without the use of force, such as auto theft, larceny, or shoplifting
- Destruction of property, that is, vandalism, arson, malicious destruction, or shoplifting
- Other property offenses, such as, fencing, receiving, possessing or selling stolen property
- Possession or use of illicit drugs
- Sale or trafficking of illicit drugs
- A major traffic offense, such as, driving under the influence of alcohol or other drugs, reckless driving, or driving without a license
- A order offense, such as, drinking or purchasing alcohol while under the legal age, disorderly conduct or a sex offense
- Any other offense we have not talked about

All

Were you sentenced to spend time in a corrections institution, like a jail, prison or a youth institution like juvenile hall or reform school or training school or to perform community service?[30]

All

If loop has been completed for all arrests: end of criminal history questions
If additional arrests: return to beginning of loop

[30] Respondents are coded as incarcerated if they mark a sentence of jail or prison, and not incarcerated otherwise.

Table 9A: Likelihood of Conviction Broken Down by Stage

	(1) Charging (Conditional on Arrest)	(2) Conviction (Conditional on Charging)
Black	-0.109***	-0.121***
	(0.023)	(0.030)
Hispanic	-0.092***	-0.010
	(0.026)	(0.033)
Delinquency Index	-0.008	0.005
	(0.014)	(0.025)
Southern Region	-0.051*	-0.002
	(0.028)	(0.035)
North Central Region	-0.055*	0.069**
	(0.028)	(0.034)
Western Region	-0.077***	0.052
	(0.029)	(0.037)
High Risk Neighborhood	-0.044	-0.053
	(0.043)	(0.051)
Household Below 2x Poverty Line	-0.012	0.040
	(0.023)	(0.030)
Spouse in Household	-0.053	-0.089
	(0.053)	(0.064)
Child in Household	-0.027	-0.047
	(0.034)	(0.048)
Enrolled in School	0.033	-0.024
	(0.027)	(0.046)
Employed	-0.017	-0.064*
	(0.025)	(0.037)
College Graduate	-0.117**	-0.121
	(0.052)	(0.150)
Hard Drug User	-0.031	0.006
	(0.028)	(0.038)
Marijuana User	0.025	-0.027
	(0.023)	(0.033)
Number of Prior Arrests	-0.004	0.006
	(0.005)	(0.007)
Observations	4,641	3,092
Number of Indivdiuals	1,687	1,357

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.
*** p<0.01, ** p<0.05, * p<0.1. Controls for age (in 1997), survey year (at time *t*), and region (at time *t*) are included in all regressions.

CHAPTER 2. NONCONVICTION ARRESTS AND LABOR MARKET OUTCOMES

I. Introduction

Those who experience criminal justice contact—ranging from arrest to long-term prison incarceration—face a wide array of post-contact challenges. In the short term, the time spent in jail and attending legal proceedings can disrupt an individual's life, causing potential job loss and personal-life turmoil. In the long term, housing and employment can be hard to find, as landlords and employers who run background checks may be disinclined to rent to or employ those with criminal records.

While these challenges, among others, are major concerns among criminal justice reform advocates, the fact that arrests resulting in no conviction can impact a person's life is especially concerning given the collective notion of "innocent until proven guilty." A substantial percentage of arrests do not lead to criminal charges or conviction, and this Chapter explores how these non-conviction arrests alter an individual's trajectory in the labor market.

In this Chapter, I expand on a line of literature that uses the National Longitudinal Survey of Youth - 1997 (NLSY97) to determine how different levels of criminal justice contact impact respondents' labor market trajectories (Fernandes 2020; Seely 2020; Apel & Powell 2019; Brown 2020; Apel & Sweeten 2012). I examine whether non-conviction arrests impact multiple outcomes, including likelihood of job loss, wages, and whether a respondent's job has benefits such as medical insurance or a retirement savings plan.

I begin by examining whether convictionless arrests cause job loss. To do so, I limit my analysis to those who are employed at the outset of a calendar year and determine whether they are still employed at the end of that calendar year. For those who are employed at the beginning of any given year, I find a significantly elevated likelihood of end-of-year unemployment (5.6 percentage point increase). This represents nearly a 50% increase in the likelihood of job loss, as compared with years of no arrest.

I next look to how an arrest can impact the quality of an individual's job by examining wages and benefits. I find that those with a non-conviction arrest experience wages that are approximately 9.0% lower in the years following the arrest. My analysis of timing indicates that this penalty eventually disappears, but still exists at least 8 years following the non-conviction arrest. I also find that they are approximately 3.7 percentage points less likely to have a job with medical insurance and 8.6 percentage points less likely to have a job that provides retirement benefits after experiencing an arrest.

The fact that nonconviction arrests result in employment penalties is concerning for three reasons. First is the fact that these penalties are largely prescribed by a sole government actor: the police officer(s) who performed the arrest. The criminal justice system consists of multiple actors: police, prosecutors, judges, and juries. In terms of the government-sanctioned punishment associated with a conviction, multiple parties play a role and can act as a check on the other parties. Indeed, prosecutors decline to bring charges against a large percentage of arrestees. Many criminal defendants have the right to a jury trial in order to protect them from unjustified punishment. In contrast, the private punishment that exists in the labor market based on non-conviction arrests is doled out at the sole discretion of police officers. The only exception to this is cases in which a magistrate judge oversees the provision of search or arrest warrants.

The second cause for concern arises from my results in Chapter 1. Chapter 1 provides evidence that, relative to White men, a larger percentage of Black men are arrested for crimes they are not ultimately convicted of. Thus, any limitations that cnovictionless arrests place on labor market opportunities will disproportionately impact Black men. To the extent these non-convictions are indicative of arrests for minor crimes or those lacking evidence, it is especially concerning that this group of arrestees faces a penalty in the labor market.

Finally, economic theory suggests that there exists some tradeoff between income from crime and income from legitimate employment (Becker 1968). To the extent this theory is correct, if criminal justice contact results in worse labor market outcomes, then the criminal justice system itself would act in a semi-criminogenic manner.

II. Background

The ability to obtain a job that provides a living wage allows individuals to become self-sufficient and greatly influences their wellbeing. Indeed, labor market outcomes are studied widely across the social sciences, even beyond the field of economics. In the context of research on the criminal justice system, studying labor market outcomes after contact is of particular importance. Those involved in the criminal justice system are among the most economically vulnerable populations in the United States. To the extent that criminal justice involvement further stymies their ability to earn a living, the system makes a subset of the most vulnerable population even more vulnerable.

The bulk of empirical literature suggests that criminal justice contact is correlated with worse labor market outcomes. However, there remain many unanswered empirical questions about the relationship between criminal justice contact and labor market outcomes.

One of the fundamental questions about the relationship between criminal justice contact and labor market outcomes is that of correlation versus causation. Namely, does criminal justice contact cause poor labor market outcomes, or are poor labor market outcomes simply a reflection of the differing underlying characteristics between people who experience criminal justice contact and those who do not?

For instance, criminal-justice-involved individuals are more likely to experience substance abuse and mental illness and less likely to have a high school degree than the general population, all of which are characteristics that often obstruct success in the labor market. These—along with other characteristics that are correlated with both criminal justice contact and poor labor market performance—could theoretically explain the disparities between those who experience criminal justice contact and those who do not. This concept is known as differential selection and suggests that criminal justice contact itself has no causal effect on labor market outcomes but, rather, reflects solely correlation based on criminal-justice-involved individuals' underlying characteristics (Apel & Powell 2019; Pager 2007).

To the extent that differential selection cannot explain the entirety of labor market disparities, two questions follow. First, what portion of the disparity can be attributed to a criminal-justice-contact penalty, as opposed to differences in underlying characteristics? Next, which levels of criminal justice contact influence labor market outcomes? Is an arrest by itself sufficient to alter an individual's work trajectory? A conviction? Or are labor market consequences limited to only those who experience incarceration?

This section summarizes both theory and past empirical research that seek to answer these questions. Given this paper's focus on the impact of non-conviction arrests, this section summarizes the way in which arrests in and of themselves may impact a person's labor market trajectory, with limited additional discussion on the effects of conviction and incarceration.

A. Theoretical Perspectives

An arrest may impact an individual's work trajectory, both in the context of their current job and future employment prospects. Off the bat, the amount of time involved in an arrest can disrupt a person's career. An individual may be detained upon the initial arrest, which can last up to 48 hours, even if no charges are ultimately brought against the arrestee. Any period of detention could cause an arrestee to miss work, which could lead to dismissal from the arrestee's current job.

If the arrestee is charged, the amount of time away from work can increase exponentially. At the most restrictive end of the spectrum, an arrestee may be subject to pretrial detention, meaning that they must await the resolution of their charge by plea or trial in jail, which can last months or even years if the individual cannot afford or is not granted bail. Even those who are not detained pretrial likely will need to attend numerous court hearings. These procedural experiences surrounding arrest may cause the individual to further miss work, leading to potential discipline or dismissal from their job. Notably, employers of lower-paying jobs often will fire an employee for missing a single shift, exacerbating the vulnerability of those near the bottom of the socioeconomic status (SES) ladder.

It also may be difficult for arrestees to hide an arrest from their employers and, given the stigma surrounding arrest, this may also cause job loss. Based on the Equal Employment Opportunity Commission's (EEOC) guidance and many states' labor laws, an arrest should theoretically impact an individual's work trajectory far less than a conviction. EEOC guidance applies at the federal level and says that an arrest record alone may not be used to take a negative employment action, given that it is not itself proof that an individual engaged in criminal conduct. Instead, an employer may take a negative employment action based on the conduct underlying the arrest, if it makes the person unfit for the position. The EEOC indicates that the employer itself should investigate the underlying conduct before making a decision.[31] Many state laws additionally prohibit employers from considering any non-conviction arrest in hiring or firing decisions.

Despite the EEOC guidance and some states' protective laws, it is likely that arrest-motivated firings still occur in practice due to difficulties in policing hiring decisions. Further, this protection would not extend to firing for missing work due to pretrial detention or court appearances. For example, Virginia has a state law that provides employment protection for individuals who take time off of work because they are required to be in court, either as a jury member or because they are summoned or subpoenaed. However, the law explicitly excludes criminal defendants.[32]

In terms of future employment, an arrest record may place a burden on job applicants via "negative credentialing." The term negative credentialing refers to the stigma associated with a criminal justice record that makes it difficult to obtain employment (Pager 2007). Negative credentialing can function through either legal processes, via state licensing boards, or social processes, via employer hiring preferences.

In the arrest-only context, negative credentialing is most likely to arise in social processes. Most states do not allow non-conviction arrests to be considered for purposes of licensing decisions,[33] and state licensing boards are likely easier to police than private employers. One major reason is that hiring one person often precludes the hiring of another. Thus, it is relatively easy for employers to explain, when choosing to not hire an individual with an arrest record, that they preferred an alternate candidate for various, inarticulable reasons. In contrast, granting a license

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[31] https://www.eeoc.gov/laws/guidance/enforcement-guidance-consideration-arrest-and-conviction-records-employment-decisions

[32] Virginia Statute § 18.2-465.1.

[33] *See, e.g.*, 28 R.I. Gen. Laws Ann. § 28-5.1-14.

does not preclude the licensing of another individual. Therefore, licensing boards must provide a more concrete reason for rejecting a license application than "we decided to go with someone else."

Social processes obstruct access to employment for arrested individuals through employers' aversion to hiring applicants with arrest records. Arrest records can be obtained through third-party background checks and are also readily available for potential employers to find on the internet. Private employers' decisions, while subject to the EEOC guidance and state-specific laws, are difficult to manage, as described above. Ample empirical evidence, discussed below, indicates that employers are highly reluctant to hire individuals with criminal records, even those with non-conviction arrests.

Finally, many indirect pathways can impact an arrestee's labor market trajectory. First, if an arrestee loses his job, he will lose the benefits that tend to come with tenure at an employer. Second, colleges and graduate and professional programs ask about applicants' arrest records. To the extent these institutions make admissions decisions based on arrest records, those with records would have a more difficult time obtaining the type of education necessary for many high-paying jobs.

B. Empirical Research

In her book, *Marked: Race, Crime, and Finding Work in an Era of Mass Incarceration*, Devah Pager provides a helpful summary of the four lines of research that seek to estimate the impact of incarceration on employment opportunities. While her discussion focuses on incarceration specifically, it also generally describes the literature on the impact of any criminal record on labor market outcomes. These four subsets include:

1. Surveys of employer attitudes
2. Interview-based and ethnographic studies
3. Statistical analyses of employment trajectories
4. Experimental studies of hiring behavior[34]

Of these four categories, (1) and (4) examine employment outcomes from the standpoint of the employer, while (2) and (3) study the experiences of the jobseeker/employee. Given that my paper falls into category (3), the bulk of my literature review focuses on prior studies that also fall into this category. However, I also summarize a few studies from (1) and (4) in order to provide background on employer decision-making as a mechanism leading to lower wages and higher unemployment.

1. Statistical Analyses of Employment Trajectories

Statistical analyses in this literature use longitudinal surveys that follow participants for multiple waves, which provide the ability to track their employment trajectories over time. As with any observational data—as compared with experimental data—it can be difficult to differentiate between correlation and causation. To estimate what portion of labor market disparities arise from a penalty of criminal justice contact as opposed to underlying characteristics, these studies use varied statistical techniques. While no study can perfectly discern causality, statistical methods such as individual fixed effects, sibling fixed effects, random effects, difference-in-differences, or instrumental variables techniques allow empiricists to get closer to identifying a causal effect than studies that examine correlation alone.

[34] Another line of research seeks to determine the effectiveness of policy interventions, such as ban-the-box policies and expungement, which I will return to in my discussion section.

The impact of incarceration has been the main focus of this line of research, with a few studies looking at lower-level contact like arrest and conviction. Studies looking at the impact of incarceration generally find that, while some of the disparity in wages and employment experienced by previously incarcerated individuals can be explained by underlying characteristics, at least some portion of the disparity can be attributed to a labor market penalty (Brown 2019). The results from statistical analyses examining arrest penalties, however, are mixed.

Grogger (1995) is one of the most widely cited papers to study the impact of arrests on labor market outcomes. Grogger combines data from the California Department of Justice with the California Employment Development Department. He uses an individual fixed-effects model to examine the impact of pre-1984 arrest, conviction, and incarceration on earnings during the period of 1980-84. Grogger finds that experiencing an arrest between 1980 and 1984 resulted in worse labor market outcomes, but that the impact was moderate and short-lived.

Of vital importance is that the time period Grogger analyzes predates the internet. Beginning in the 1990s, employers gained widespread access to employees' and applicants' criminal records via the internet, and the impact of an arrest on employment today is likely vastly different than in the early 1980s. One would expect that even though Grogger found only a moderate and short-lived impact of arrests on employment and wages, that impact may have grown exponentially when employers gained low-cost access to arrest records. An employee who was fired from a job due to arrest in the 1980s could potentially apply to other jobs without potential employers learning of their arrest, leading to a relatively fast bounce back in the labor market. In contrast, anyone fired from a job due to an arrest now will likely be perpetually followed by their arrest record when applying to jobs.

Finlay (2009) provides empirical evidence for this likely change over time using the NLSY97. Finlay applies a difference-in-differences model to assess the impact of state-level laws that facilitated access to criminal history data on the internet and finds that this access results in those with criminal records being worse off in terms of both income and employment.

A few studies have examined the relationship between arrests and labor market outcomes using data from the internet era. The two most relevant studies for my purposes use NLSY97 data to examine (1) the long-term impacts of arrest on wages and (2) the near-immediate impact on wages and employment. Apel and Powell (2019) examine cross-sectional NLSY97 data on wages that is measured, on average, 13 years after a respondent experiences their first arrest. In direct contrast, Fernandes (2020) focuses on the short-term and looks at the impact of arrest on wages and employment in the year directly following arrest.

Apel & Powell (2019) use a sibling fixed-effects model to estimate the long-term impact of arrests and incarceration spells on wages. This model compares the wages of those who experience arrests or incarceration spells to the wages of their non-arrested/non-incarcerated siblings. This allows them to eliminate concerns of omitted variables bias due to unobservable characteristics at the family level. Apel & Powell's wage data is cross sectional and is taken from each respondent's most recent survey response year, adjusted to 2016 dollars. They analyze the data using quantile regressions, under the assumption that criminal justice contact will affect individuals differently based on their relative position in the wage distribution.

Apel & Powell find a raw disparity in wages for those who are arrested compared to those with no criminal justice contact. However, this disparity disappears in the sibling fixed-effects model. This finding supports the theory of differential selection, at least as far as any wage disparity due to arrest in the long term is concerned, given that Apel & Powell explain any wage differences by observable characteristics and sibling fixed effects. Notably, Apel & Powell appear to include juvenile arrests in their analysis. As compared to adult arrests, juvenile arrests are less likely to impact arrestees in the long-run due to increased privacy surrounding juvenile records. It is thus possible that the inclusion of juvenile arrests in the analysis diluted any true effect of adult arrests.

Fernandes (2020) uses the NLSY97 to estimate an individual fixed effects model on the impact of arrests, charges, conviction, jail sentences, and prison sentences on wages and unemployment in the year following criminal justice contact. In direct contrast to Apel & Powell's study, Fernandes looks only at the immediate impact of arrest on earnings and employment. Fernandes finds that in the survey period following arrest, arrests have no impact on wages but do lead to a reduction in the number of weeks worked.

I expand on these studies in multiple ways. First, I use all years of available NLSY97 data when respondents were adults, rather than focusing on the extremes of immediate or long-term impacts. By doing so, I can capture the relationship between arrest and wages/employment in the short-, medium-, and long-term. To the extent that the impact of arrest on wages and employment is either not immediate and dissipates over time, I am more likely to find that arrests play an important role in labor market outcomes than either of these studies. Second, I perform a more detailed analysis of what happens during the year of arrest.

Further, in contrast to Apel & Powell (2019), I am able to exploit the panel nature of the data to use an individual fixed-effects model, rather than a sibling fixed-effects model, which they are limited to due to the cross-sectional nature of their data. Finally, both of these studies look at the average impact of all arrests on labor market outcomes, regardless of conviction status. I examine non-conviction arrests specifically, which may vary in their impact from conviction and incarceration.

2. Employer Decision-Making

Survey and experimental research further support the theory of criminal credentialing. One generalized finding is a "strong reluctance on the part of employers to hire applicants with criminal histories" (Pager 2007). Two studies are particularly relevant for my purposes.

In a seminal 1962 study, Schwartz and Skolnick applied to various employers on behalf of four types of fictional candidates, who varied only based on their criminal justice records. Three levels of criminal justice contact were represented: convicted of assault, acquitted of assault, and no criminal justice contact. A fourth category consisted of individuals acquitted of assault, but the application included a letter from the judge that confirmed the verdict of "not guilty" and emphasized the legal presumption of innocence.

Employers indicated interest in candidates with no criminal justice contact at a rate of 36% and those who had been convicted of assault at a rate of only 4%. Employer interest in the two groups with assault acquittals was 24% for those with a letter from the judge and 12% for those without a letter. Even with a letter from the judge confirming the finding of not guilty and explaining the legal presumption of innocence, candidates with an acquittal received only 2/3 the level of employer interest as the no-contact group. Without a letter, employer interest in acquitted candidates dropped further, to 1/3 the rate of the no-contact group.

The large disparities in employers' interest between the no-contact group and the acquitted groups is especially striking, given that these candidates were acquitted of assault, meaning they were determined at trial to be not guilty. In the universe of non-conviction arrests, acquittals are a rare outcome, given that most cases do not proceed to trial. Acquittals are also arguably the clearest sign of innocence possible for an arrested individual. Oftentimes, prosecutors decide not to bring charges against an arrestee or later drop charges, which provides a less clear signal of innocence than acquittal.

In more recent research, Uggen et al. (2014) perform another audit study to examine the impact of criminal records. In this study, they examine differences in callback rates between applicants with no criminal justice contact and those with an arrest for disorderly conduct that did not result in conviction. Despite the fact that this crime is a misdemeanor and that the individual was not convicted, they still find a 4% lower callback rate for the applicant with the non-conviction disorderly conduct arrest.

Given the various theoretical reasons to believe that criminal justice contact would result in a labor market penalty—combined with empirical studies showing employers' reluctance to hire those with criminal records—it is somewhat surprising that statistical analyses of employment trajectories have produced mixed results.

III. Data and Methodology

I use data from the NLSY97 to examine the relationship between non-conviction arrests and later labor market outcomes. The NLSY97 is a nationally representative dataset that follows 8,984 American individuals who were born between 1980 and 1984. The initial survey was conducted in 1997, when the respondents were ages 12-17. The study was conducted annually from 1997 to 2011 and biennially after. The most recent available data comes from the 18th wave of the survey in 2017, when the respondents were ages 32-36.

This dataset is particularly well-suited to examine the relationship between criminal justice contact and labor market outcomes because of its longitudinal, nationally representative nature, extensive information on respondents' underlying characteristics, and detailed data on both criminal justice contact and labor market outcomes.

A. Arrest History

At each interview, respondents were asked whether they were arrested since the date of their last interview and, if so, how many times. For each arrest, the respondent is asked the month and year of arrest and whether they were charged. For each charge, the respondent is then asked whether they were convicted.

To limit my analysis to the impact of non-conviction arrests, I exclude all individuals who report experiencing a conviction at any point during the survey. There are 4,453 men in the sample, excluding those who indicated mixed race, "American Indian, Eskimo, or Aleut," and "Asian or Pacific Islander."[35] Of these 4,453 men, 1,137 were convicted and are excluded from my analysis.

For the remaining respondents, I create two separate indicator variables that capture respondents' arrest histories. The first captures whether the respondent experienced an arrest in the current year. The second captures whether the respondent has ever experienced an arrest in the past (excluding juvenile arrests). For example, if a respondent reports a single arrest in 2005, the indicator variable for arrest in the current year will be equal to 1 in 2005 and 0 in all other years. For this respondent, the indicator variable for prior arrest will be equal to 1 in all periods after 2005, and equal to 0 otherwise.

[35] As described in Chapter 1, I omit those who fall into these categories because of insufficient sample sizes.

B. Labor Market Outcomes

I focus my analysis on "employee-type" jobs, which are defined in the NLSY97 as "a situation in which the respondent has an ongoing relationship with a specific employer" (Apel & Powell 2019). I limit my outcomes in this way because these are the types of jobs in which negative credentialing and dismissal due to absence or arrest are most likely to play a role. Indeed, anecdotal evidence suggests that individuals with criminal records who cannot obtain employment in the labor market often turn to self-employed ventures.

I perform two sets of analyses. First, to explore the immediate impact of arrest, I examine whether a non-conviction arrest results in job loss for those who were employed at the time of arrest. To do so, I create indicator variables for whether a respondent was employed during the first week of each calendar year and the last week of each calendar year. Using these two variables, I can determine whether respondents are more likely to experience job loss during an arrest year than during other years.

Second, to examine whether negative credentialing impacts non-conviction arrestees, I create two measures for quality of job. The most direct measure of job quality is wages. For this outcome, I look at the impact of a non-conviction arrest on the log of hourly wages.

Respondents can report multiple jobs over the course of the survey period. Respondents can report their earnings in varying formats (such as yearly, monthly, hourly, etc.), and the NLSY97 calculates an hourly wage variable for each job a respondent held during a survey period. Respondents also report how many hours they typically work per week at that job, either as of the interview date or the jobs stop date, if no longer employed in that job. Using these two variables, I create an average hourly wage that is weighted by the hours worked per week at each job. To account for outliers, I exclude any average hourly wage that is below \$3 per hour or over \$100 per hour.[36]

As an additional measure of job quality, I examine whether a non-conviction arrest impacts the likelihood a respondent is employed in a job that offers employee benefits of medical insurance or a retirement plan. For this, I create two indicator variables that is equal to 1 if the respondent held any job during the calendar year that offered medical insurance or a retirement plan, and 0 otherwise.

C. Empirical Specification

My regressions attempt to isolate the impact of arrests on individuals who were not convicted. In doing so, I follow the strategy of Sheely (2020), which is inspired by Apel and Sweeten (2012). Using this strategy, I limit my regression sample only to the 3,316 men reported no adult criminal convictions (or incarcerations) during the survey period.

My regression to examine job loss is limited to those who are employed at the outset of each calendar year t and is modeled as follows:

$$\mathrm{Y}_{it} = \alpha_0 + \beta_1(arrested\ in\ current\ year_{it}) + \beta_2 X_{it} + \gamma_t + u_i + \varepsilon_{it} \tag{1}$$

Y_{it} represents whether the respondent is still employed at the end of year t. X_{it} is a vector of time-variant individual characteristics that are likely to impact employment outcomes. α_0 is the intercept term, and γ_t represents year fixed-effects. u_i represents an intercept term that varies by individual, and ε_{it} is the random error term.

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[36] These outliers represent approximately 7% of observations. My results do not meaningfully change when including outliers.

Within X_{it}, geographic characteristics include indicator variables for the region the individual resides in (West, North Central, and South, with Northeast as the reference category) and whether individual lives in a rural or inner-city area (with suburban area being the reference category). Household characteristics include indicator variables for whether the respondent lives with a spouse or partner and whether the respondent lives with their child. Educational characteristics include indicator variables for whether the respondent is a high school graduate, is a college graduate, or is currently enrolled in school. Finally, I include indicator variables for marijuana use and hard drug use. All of these questions are asked directly of NLSY respondents. With the exception of the geographic characteristics, I measure them at time $t-1$, so as to minimize the likelihood of reverse causality. I do not include age as a variable because it is closely correlated with γ_t, given the low variability in ages among NLSY97 respondents.

My regressions on ln(hourly wages) and employee benefits are similar to that of my regression on job loss and are modeled as follows:

$$\begin{aligned} \mathrm{Y}_{it} = {} & \alpha_0 + \beta_1(arrested\ in\ current\ year_{it}) + \beta_2(arrested\ in\ prior\ year_{it}) \\ & + \beta_3 X_{it} + \gamma_t + u_i + \varepsilon_{it} \end{aligned}$$

The main differences in this regression model are that I include a measure of prior arrest to determine medium- to long- term outcomes due to negative credentialing, which are captured in β_2. I additionally include a variable for tenure with the current employer and tenure squared in X_{it}, which are not included in my equation for job loss.[37]

[37] I include both tenure and tenure squared because human capital and other economic theories, along with empirical evidence, establish that wages rise with tenure at a decreasing rate.

IV. Results

A. Descriptive Statistics

I begin by presenting descriptive statistics on my sample. Table 1 describes sample characteristics of never-convicted men in 2017, split by whether they ever experienced an adult arrest. These descriptive statistics indicate significantly worse labor market outcomes for those who were ever arrested as adult. On average, those who have been arrested but never convicted earn a $4.58 lower hourly wage, are 15% less likely work at a job that provides medical insurance, and are 21% less likely to work at a job that offers a retirement plan, as compared to their never-arrested counterparts. Again, given the notion of innocent until proven guilty, the EEOC's guidance, and state laws protecting arrested employees, this result should raise a red flag.

Table 1: Characteristics of Never-Convicted Adult Men in 2017

	No Contact	**Arrested** (Never Convicted)
Employment Outcomes		
Hourly Wage	$26.90	$22.32
Job with Medical Insurance	78.50	66.85
Job with Retirement Plan	72.82	57.63
Sociodemographic Characteristics		
Black	13.71	24.46
Hispanic	13.64	15.62
White	72.65	59.93
High School Graduate	95.03	84.46
College Graduate	44.86	25.21
Household Characteristics		
Living with Spouse/Partner	68.95	58.26
Living with Child	56.81	50.13
Criminal Activity and History		
Uses Marijuana	12.25	27.70
Uses Hard Drugs	2.29	3.44

Delinquency Index[38]	1.83	3.01
Arrested as a Juvenile	10.18	25.23
Sample		
Weighted Proportion of Sample	62.59	13.33
Unweighted Sample Size	1,893	450
Nonrespondent in 2017	833	155

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.

Clearly, a number of characteristics beyond labor market outcomes vary based on a respondent's highest level of contact. For instance, those with criminal justice contact are more likely to report drug use, have higher juvenile delinquency scores, and were more likely to be arrested before age 18, as compared with their never-arrested counterparts. They are also less likely to have a high school or college diploma and less likely to live with a spouse/partner or child.

Given that all of the underlying differences are likely to be negatively correlated with labor market outcomes, it is especially important to use regression techniques to further examine the labor market disparity. Doing so can help isolate whether the labor market disparities are reflective of the differences in underlying characteristics or are related to the criminal justice contact itself.

B. Regression Results

I begin by presenting my results on job loss in Table 2. I find that those who experience a non-conviction arrest are 5.6 percentage points more likely to experience job loss during the

[38] This value is the sum of the number of the following activities the respondent reported engaging in prior to age 18: ran away from home, carried a gun, in a gang, stole something worth < $50, stole something worth > $50, attacked another person, sold drugs, destroyed property, and committed another property crime. This delinquency index varies from the NLSY because it does not include arrested as juvenile, which is broken out separately.

calendar year of an arrest. This represents a massive increase, as the overall likelihood of job loss in any given calendar year is approximately 10%.

Table 2: Job Loss During Calendar Year

	(1) Job Loss
Arrested (Current Year)	0.056***
	(0.020)
Spouse in Household	-0.005
	(0.006)
Child in Household	0.013*
	(0.007)
Enrolled in School	0.048***
	(0.008)
High School Graduate	-0.059***
	(0.013)
College Graduate	-0.055***
	(0.011)
Rural	-0.003
	(0.014)
Inner City	-0.008
	(0.006)
Marijuana User	0.011
	(0.009)
Hard Drug User	0.007
	(0.015)
Observations	28,593
Respondents	3,154
R-squared	0.050

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. All regressions include controls for year and region. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3 reports my baseline fixed-effects regression results on ln(hourly wages). The LHS variable of the equation is the log of hourly wage.

Table 3: Ln(Hourly Wage)

Arrested (Current Year)	-0.043**
	(0.017)
Arrested (Prior)	-0.089***
	(0.019)
Spouse in Household	0.050***
	(0.008)
Child in Household	0.016
	(0.010)
Enrolled in School	-0.095***
	(0.008)
High School Graduate	0.018*
	(0.011)
College Graduate	0.208***
	(0.014)
Rural	0.010
	(0.016)
Inner City	0.007
	(0.008)
Marijuana User	-0.018**
	(0.008)
Hard Drug User	0.009
	(0.012)
Tenure	0.096***
	(0.006)
Tenure^2	-0.009***
	(0.001)
Constant	1.748***
	(0.035)
Observations	29,587
Respondents	3,093
R-squared	0.529

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. All regressions include controls for year and region. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

I find that an arrest decreases hourly wages by an average of approximately 9% in the years following arrest, with a an approximately 4.3% decrease the year of arrest. The coefficients on the rest of my control variables reflect expectations. Having a spouse/partner increases wages, which is reflective of the economics literature on men's wages. Having a high school or college diploma both increase wages. The coefficients on tenure—with that of tenure being positive and tenure squared being negative—follow that of the general economics literature. Being enrolled in school is associated with lower wages, which is unsurprising, given that many jobs worked in school are part-time and lower wage. Finally, marijuana users earn a lower hourly wage.

I next turn to examining employee benefits. These results are presented in Table 4. I find that following arrest, individuals are 3.7 percentage points less likely to have a job that offers medical insurance and 8.6 percentage points less likely to have a job that offers retirement benefits. This provides additional evidence of decreases in job quality in the years following a convictionless arrest, above and beyond the decrease in hourly wages.

Table 4: Likelihood of Having a Job with Employee Benefits

	(1) Medical Insurance	(2) Retirement Account
Arrested (Current Year)	-0.004	-0.041**
	(0.024)	(0.019)
Arrested (Prior)	-0.037*	-0.086***
	(0.021)	(0.021)
Spouse in Household	0.045***	0.051***
	(0.010)	(0.010)
Child in Household	-0.008	0.005
	(0.012)	(0.012)
Enrolled in School	-0.124***	-0.091***
	(0.010)	(0.009)
High School Graduate	0.097***	0.052***
	(0.013)	(0.012)
College Graduate	0.205***	0.173***
	(0.015)	(0.015)

Rural	-0.015	-0.041***
	(0.017)	(0.016)
Inner City	-0.001	-0.001
	(0.009)	(0.009)
Marijuana User	-0.032***	-0.047***
	(0.011)	(0.010)
Hard Drug User	0.011	0.000
	(0.016)	(0.015)
Tenure	0.108***	0.119***
	(0.008)	(0.008)
$Tenure^2$	-0.013***	-0.014***
	(0.001)	(0.001)
Observations	31,013	30,998
Respondents	3,179	3,179
R-squared	0.230	0.234

All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. All regressions include controls for year and region. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

V. Discussion

Focusing on men who experience a non-conviction arrest, I find that their likelihood of job loss in the year of arrest nearly doubles. I also find that in the years following their first arrest, they earn approximately 9% less in hourly wages than their never-arrested counterparts. This is significantly lower than the raw wage disparity of 17% between the two groups, indicating that differences in underlying characteristics can account for some of the disparity. However, it may be that some of those underlying differences are due to the arrest itself, such as lower tenure with an employer or lower levels of education. These arrested individuals are also likely to end up in jobs that have worse employee benefits, with lower likelihood of having jobs that provide medical insurance and retirement benefits.

In sum, my analyses indicate that there are both immediate and long-term impacts in the labor market when an arrest occurs, regardless of conviction status. These results imply that criminal justice reform advocates are right to be concerned about the use of arrest records in both firing and hiring. While it appears that it is not uncommon for employers to fire an employee in the event of an arrest, I am unable to sort through whether this is due to the arrest itself, or due to employees missing work due to detention or attending court hearings.

In 1995, Grogger found a moderate and short-lived impact of arrest on labor market outcomes. My analysis on data from 1997 to 2017 indicates that those mild and short-lived penalties have evolved into semi-severe, long-term penalties. This is likely due to increased access to arrest records through the internet.

Various policies could potentially alleviate the problem of non-conviction arrest penalties. For instance, states and the EEOC could invest more resources in investigating instances where it appears an individual was fired or not hired due to an arrest record. Automatic expungement on non-conviction arrests could also alleviate negative credentialing. Ban-the-box policies are another option and have been studied extensively in the economics literature.

Ban-the-box laws, which restrict employers from asking applicants about criminal records during the early stage of the hiring process, may alleviate the issue somewhat. However, even in states with ban-the-box laws, employers can still access criminal records at the end stages of hiring and may choose to not hire an individual based on their record, with a low likelihood of detection via employment laws. Ban-the-Box laws generally prevent employers from asking about criminal records either on initial job applications or during early-stage interviews. They are designed to increase the likelihood an individual with a criminal record advances in the hiring process and is given the chance to be evaluated in an interview. These laws, while passed in 25 states and popular among criminal justice reform advocates, are limited in their effectiveness. First, they only prevent information sharing in the beginning stages of the hiring process. Employers will often still run a background check once they have made the decision to hire someone, at which point they will likely learn about an applicant’s criminal record. While ban-the-box laws do remove bias at the beginning stages, they can only go so far.

One major concern surrounding these laws is potential statistical discrimination. Statistical discrimination occurs when a party assumes the likelihood of a particular trait among certain demographic groups. In cases when an employer can ask about a person’s criminal record in the beginning stages of the hiring process, they can know exactly whether someone has a criminal record. If they are prohibited from asking about that characteristic, they may use other demographic characteristics to impute the likelihood of a criminal record on the individual. As such, Black individuals may be discriminated against because, on average, they experience a higher likelihood of having a criminal record and, as such, are perceived by employers as being more likely to have a criminal record. This leads to the unintended consequence of employers exhibiting a bias in favor of non-Black candidates for employment. This phenomenon has been documented by various economists using field experiments. In general, ban-the-box laws are a good first step towards improving access to employment for those with criminal records. However, given the side effects of statistical discrimination, they may prove more costly than helpful.

All of these options are promising but come with drawbacks. The government cannot perfectly prohibit employers from inquiring into employees’ arrest records. Ban-the-box may tip the scales in more in favor of applicants with criminal records but are unlikely to eliminate bias based on arrest records. They also may increase statistical discrimination against young, Black men. A permanent, internet-based record of arrest cannot be deleted by expungement, even though an individual’s official government record is expunged. Perhaps most concerning, these policies can do nothing to prevent the initial problem of arrestees being fired if those terminations are based on missing work due to time involved in the adjudication process.

The most straightforward solution would be to reduce the number of non-conviction arrests, such that police arrest decisions better reflect the cases that prosecutors prioritize. The benefits of arresting large numbers of people under the theory of broken windows policing are already questioned. The labor market penalties of non-conviction arrests presented in this paper reflect a large, previously unrecognized private cost of large scale, which is likely to disproportionately impact the Black community. From a policy perspective, would be wise for jurisdictions to revisit this theory of policing, especially if statistics indicate that the particular jurisdiction has a large number of non-conviction arrests.

VI. Conclusion

My analysis of NLSY97 data finds a labor market penalty in terms of both wages and employment that arises in the year of non-conviction arrest and extends well into the future. Interestingly, there appears to be no additional wage penalty for arrestees who are convicted of a crime. These results indicate that being arrested for a crime, rather than convicted, is the driving force in labor market penalties for criminal justice contact (outside of incarceration).

This is especially concerning due to ability of police to arrest individuals for very minor crimes, in conjunction with the large amount discretion police are granted in deciding who to arrest. In this sense, police act as police, prosecutor, judge, and jury in doling out the private punishment associated with arrest. Further, these types of arrests—namely, those that do not result in conviction—disproportionately affect Black men. This means that Black men are penalized in the labor market based on arrests for crimes of which they are never convicted. In sum, the labor market penalties associated with non-conviction arrests represent a previously unrecognized private cost of large-scale arrests.

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CHAPTER 3. MEDICAID EXPANSION AND DRUG ARRESTS

I. Introduction

Chapters 1 and 2 investigated potential issues with the US criminal justice system—namely, over-arrest of Black men, as evidenced through lower conviction rates, (Chapter 1) and the cost of convictionless arrests in terms of labor market opportunities (Chapter 2). These Chapters highlighted how the current criminal justice system may suppress social and economic mobility among those who are at high risk for arrest, with a particularly acute effect on Black men.

This Chapter shifts focus to examine a policy intervention that could potentially alleviate some of these problems by reducing the overall reach of the criminal justice system. Theory and empirical evidence suggest that expanded access to public health insurance can reduce crime, an outcome that should, in turn, reduce criminal justice contact. Multiple papers have explored this relationship, finding that expanded access to Medicaid—the source of US public health insurance for low-income individuals—is linked to decreases in reported burglary, larceny theft, motor vehicle theft, assault, robbery, and homicide.

These papers' authors have proffered multiple theories to explain the link between health insurance and crime. One of the most suggested mechanisms is that increased public health insurance allows more individuals with substance use disorders (SUDs) to gain access to treatment. This is because substance abuse is highly linked to crime, with much higher rates of SUDs among the criminal-justice-involved population than among the general population.

Despite this suggested mechanism, no study has examined the relationship between Medicaid and drug crime. In this Chapter, I investigate whether Affordable Care Act (ACA) state-level Medicaid expansion reduced the rate of drug arrests. State-level Medicaid expansion has increased public health insurance dramatically. The legal history of the ACA, which I detail in Part II below, resulted in states making individual determinations of whether to expand Medicaid eligibility under the ACA. This allows me to use a difference-in-differences framework to determine whether drug arrests decreased after Medicaid expansion in states that did expand, while using trends in states that did not expand Medicaid as a control group.

I obtain data on drug arrests from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR) dataset. I find evidence of decreases in arrests for drug sales overall and for possession of synthetic narcotics (e.g., fentanyl, Demerol, methadone) in Medicaid expansion states. The decreases in drug sales arrests appear to be driven largely by sales of heroin/cocaine and their derivatives (e.g., codeine, morphine).

Combined, these two drug categories—synthetic narcotics and heroin/cocaine and their derivatives—comprise all opioids. In theory, reductions in arrests related to opioids could be due to two alternative explanations. First, Medicaid expansion may have led to increased access to legal opioid prescriptions for those who were previously abusing illegally obtained opioids. As a result, individuals who would previously have been arrested for illegal opioid possession would instead be in legal possession. Second, increased access to opioid use disorder (OUD) treatment in Medicaid expansion states may have lowered the rates of OUDs and, in turn, opioid possession and arrests.

Based on supplemental analysis and previous research of prescriptions in Medicaid expansion states, I suggest that these decreases are likely due to reductions OUDs, rather than increases in legally obtained opioid prescriptions. First, I find in my analyses that the reductions in opioid-related arrests are largely concentrated in counties that have the best access to medication assisted treatment (MAT). Further, previous research has found little to no increases in opioid prescription rates in Medicaid expansion states, but large associated increases in prescriptions for drugs that are used to treat opioid use disorders in MAT.

The UCR arrest data also provides information on arrestees' race. Unfortunately, the data do not contain information on arrestees' ethnicity. I run my analyses separately for White, Black, Asian, and Native American arrests to determine whether effects are heterogeneous by race. I find evidence that Medicaid expansion reduced synthetic narcotics possession arrests for the White, Asian, and Native American populations, but not the Black population.

Two possibilities may explain the decrease exclusive to the non-Black population. First, some research has suggested that, in the context of SUD treatment, Black individuals may be less likely to receive treatment due to lack of access to care and racially disparities in physician prescribing behaviors. Medicaid expansion increased the ability to pay for care, but not the actual structures in place for providing care. To the extent that the Black population has lower access levels to effective treatment methods for SUD, they would benefit less from increases in payment for SUD. Second, research has shown that while Black individuals use drugs at similar rates to White individuals, they are arrested for drug use at much higher rates. This indicates that there is more of a direct correlation between White drug use and White arrests than Black drug use and Black arrests. It may be that both Black and White drug users are receiving equal access to SUD treatment, but that any decrease in Black SUDs does not translate as readily to lower drug arrests.

This Chapter further indicates that, while Medicaid expansion appears to be effective at reducing the rate of drug arrests, it may have inadvertently increased racial disparities in the criminal justice system. This is true for two reasons. First, Black individuals are significantly less likely to live in states that expanded Medicaid. This means that relative to their White counterparts, they are less likely to receive the benefits—including SUD treatment—from Medicaid expansion.

Further, even within Medicaid expansion states, it appears that the benefits of reduced drug possession arrests are concentrated among other races. This indicates that even in states that did expand Medicaid, there may be racial disparities in SUD treatment. To the extent Medicaid expansion reduced arrests for drug crimes, it appears to have done so in a way that disproportionately benefited non-Black individuals.

II. Background

A. Medicaid Expansion

The Affordable Care Act (ACA)—also referred to as Obamacare—has prompted passionate debate since before its enactment in 2010. The ACA sought to make various changes to health care in the United States, both in the regulation of private health insurance and the provision of public health insurance. For purposes of this paper, state-level Medicaid expansion (i.e., changes to public health insurance) provides the source of variation for my empirical study and is the focus of this section.

Medicaid was enacted under the Tax and Spend clause and functions as a partnership between the states and the federal government. The federal government does not have the power to directly mandate and regulate healthcare provision among the states. Instead, it must turn to indirect regulation by providing financial incentives to the states via its power to spend. In essence, the federal government can control aspects of the states' provision of Medicaid by offering the states funds that are contingent on compliance with certain federally mandated requirements. While states must comply with certain minimum requirements to receive the attached federal funds, they have considerable leeway to craft their own Medicaid programs.

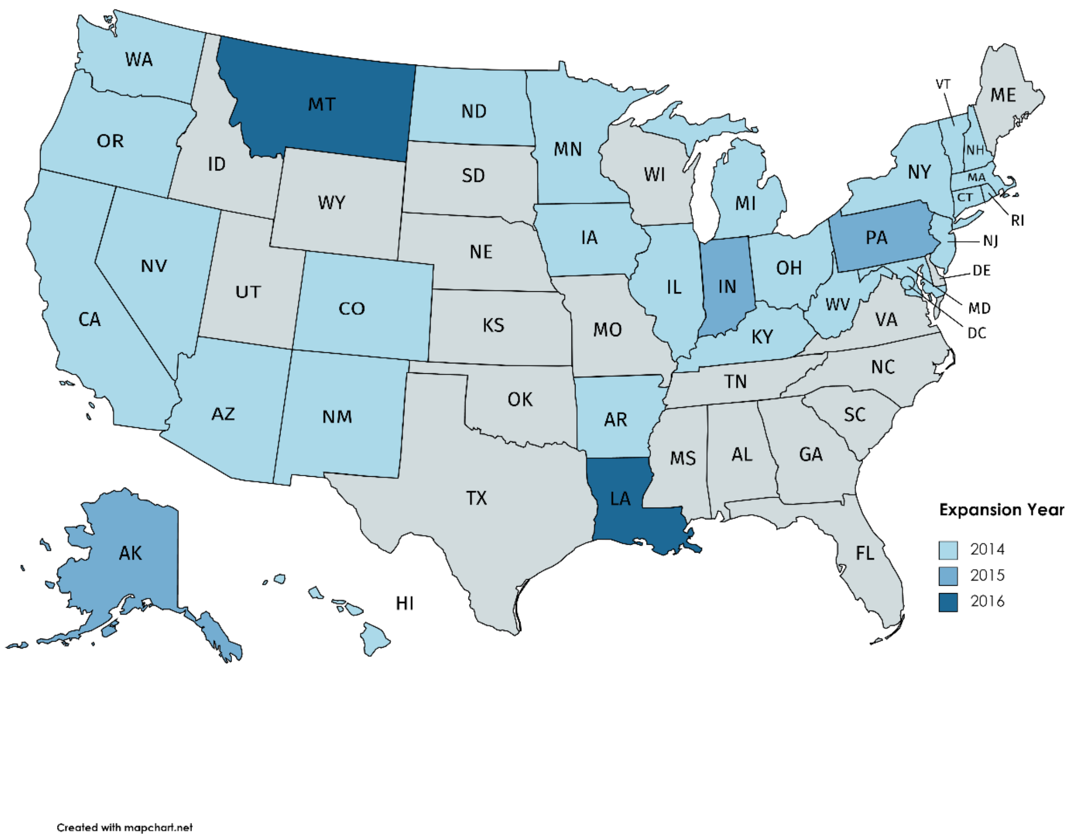
Medicaid plays two major roles in the provision of public health insurance. First, it covers most medical expenditures for specific categories of nonelderly individuals. Prior to Medicaid expansion, this included low-income families and low-income individuals with disabilities. Second, it covers some medical expenses for certain elderly individuals—namely, medical expenditures that are not covered by Medicare for low-income elderly persons and nursing home expenditures for institutionalized elderly persons. Under expansion, there was no change in coverage for individuals with disabilities or elderly individuals.

Pre-ACA, Medicaid was available for families with children living below 100% of the federal poverty line, with some states setting the cutoff even lower. Medicaid expansion under the ACA sought to increase coverage to include childless individuals, as well as expanding the poverty threshold to 138% of the federal poverty line. Thus, Medicaid expansion directly affected two groups: all childless individuals below 138% of the federal poverty line, and families below 138% of the federal poverty line whose incomes exceeded their state's threshold prior to expansion.

When the ACA was first enacted, it required all states that received Medicaid funds (which included all fifty states) to provide coverage for the expanded eligibility groups. If any state refused to comply, the federal government would withdraw all Medicaid funding from that state. In essence, this would have required all states to expand Medicaid, as all states rely heavily on federal Medicaid funds. However, the Supreme Court overturned this provision as coercive, finding that it exceeded Congress's power under the Tax and Spend clause.

After this Supreme Court decision, it was left to the states to choose whether to enact Medicaid expansion. Most states that expanded Medicaid did so in 2014, with some states following in later years. Figure 1 shows states that enacted Medicaid expansion as of 2018. While additional states have since enacted expansion, and the total now includes 36 states plus DC, I only include those expanding through 2018 because that is the date through which I have drug arrest data. States that did not enact Medicaid expansion prior to 2018 act as the control group in my empirical model, while those enacting Medicaid expansion are my treatment group.

Figure 1: States Expanding Medicaid 2018 and Earlier



B. Studies on Medicaid and Crime

Several papers have used quasi-experimental methods to explore the impact of public health insurance availability on crime rates. These difference-in-differences analyses are based on ACA state-level Medicaid expansion, along with three other occurrences of increased eligibility for health insurance: expansion of eligibility for individuals with disabilities, Health Insurance Flexibility and Accountability (HIFA) waivers, and the ACA dependent coverage mandate.

These papers consistently find a decrease in at least one category of crime and no study finds increases in any crime category. While the association between increased health insurance and decreased crime is consistent across studies, there is variation in the types of crimes affected and the mechanisms proposed by the authors. Table 1 summarizes each paper, including which of the four expansions it relies upon in its model, which crimes went down, and what the authors propose as a mechanism for decreased crime.

Table 1: Summary of Prior Studies on Health Insurance and Crime

Authors ***Expansion Relied Upon***	**Crimes Impacted**	**Proposed Mechanisms**
Wen et al. (2018) *HIFA Waivers*	Robbery Larceny Theft Aggravated Assault	SUD treatment
Fone et al. (2020) *ACA Dependent Coverage Mandate*	Burglary Larceny Motor Vehicle Theft Assault	Income effect[39] Increased cohabitation with parents Increased educational attainment SUD treatment
He & Barkowski (2020) *ACA State-Level Medicaid Expansion*	Burglary Motor Vehicle Theft Assault Homicide Robbery	Opportunity cost[40]
Vogler (2020) *ACA State-Level Medicaid Expansion*	Aggravated Assault	Income effect Opportunity cost Changes in state spending

[39] This mechanism proposes that Medicaid expansion leaves people with more disposable income. Because of this, there is a lower need to commit property crimes. There also may be an effect on violent crime, in that the extra disposable income may reduce stress, which could in turn reduce violence. However, the flipside of this theory states that increased disposable income may increase drug-related crime.

[40] This is a utility-maximizing model inspired by the Becker economic model of individual criminal decision making. It assumes that the opportunity cost of committing crime increases when Medicaid expansion occurs, because the value (i.e., opportunity cost) of not being in jail or prison increases by improvements to healthcare access. This theory predicts that Medicaid expansion would have a larger impact on crimes that carry jail or prison sentences, especially those associated with long sentences.

		SUD treatment
Wagner (2020) *Eligibility Expansion for Individuals with Disabilities*	Property Crimes	Opportunity Cost SUD treatment

All but one of these studies hypothesize access to SUD treatment as a plausible explanation for decreases in crime. Despite this, none of these papers focuses on drug crimes. Fone et al. is the sole paper that looks at all at drug-related crimes, and only mentions it in passing in two sentences: "The [dataset used] also contains counts of arrests for crimes that are not considered by the FBI definition to be Part I property or violent crimes: drug crimes, stolen property, weapons violations, and vandalism. We find consistent evidence that the DCM was also associated with declines in arrests for these offenses as well."

Wen et al. (2018) provides an in-depth description of theories on the relationship between SUDs and crime. First, intoxication itself may drive crime. Second, drug addiction is costly and may incentivize property crimes that could generate disposable income. Third, being involved in an illegal drug market via either buying or selling puts an individual in closer proximity to other crimes.

In order for increased SUD treatment to explain the relationship between expansions to Medicaid eligibility and lower crime rates, it must be true that Medicaid expansion did in fact increase access to and use of SUD treatment. The next section discusses the literature on Medicaid expansion's impact on SUD treatment and other proxies for drug addiction.

C. ACA Medicaid Expansion and Substance Use Disorders

Theory underlying Medicaid expansion's impact on SUD rates yields ambiguous predictions. On one hand, Medicaid expansion should increase access to SUD treatment, which should reduce the prevalence of SUDs. On the other hand, Medicaid expansion may increase individuals' access to insurance-covered legal opioid prescriptions, as well as increase the amount of disposable income available to purchase illegal drugs (Averett et al. 2019; Goodman-Bacon & Sandoe 2017). Because of this ambiguity, empirical evidence is necessary to determine the nature of the relationship, and many papers have studied Medicaid expansion's impact on drug-related outcomes.

One large subset of this literature has looked at the relationship between Medicaid expansion and overdose deaths. The empirical results so far are mixed, and the question is a topic of much debate. Notably, Abouk et al. (2020) note that differences in pretrends related to opioid overdoses in expansion vs. non-expansion states make any causal analyses of overdose deaths difficult. Some studies find no evidence that Medicaid expansion impacted opioid overdose deaths in either direction (Averett et al. 2019), some find suggestive evidence that Medicaid expansion increased opioid-related fatalities (Yan et al. 2020), and others find that Medicaid expansion reduced opioid-related fatalities (Snider et al. 2019; Wettstein 2020; Kravitz-Wirtz et al. 2020). In sum, studies examining the relationship between expansions to Medicaid eligibility and overdose deaths have not come to a conclusive answer.

Another line of research seeks to determine whether coverage and treatment utilization for individuals with SUDs increased post-Medicaid expansion. It is clear from the research that health insurance coverage increased for at least a subset of this group, but research on health care utilization is mixed. For example, Olfson et al (2020) finds large gains in health insurance coverage associated with Medicaid expansion among those with SUDs. However, they find that coverage expansion did not translate "to a measurable increase in treatment of common substance use disorders." Notably, however, over three-quarters of their study group had alcohol use disorder, rather than a disorder related to illegal drugs. Olfson et al. point to limited availability of treatment programs that accept Medicaid as a potential explanation for this lack of utilization increase.

In contrast, Meinhofer & Witman (2018) found sizable increases in admissions to specialty treatment facilities in expansion states, at the magnitude of 18%. They report that the majority of this increase was attributable to outpatient medication-assisted treatment (MAT). Medication-assisted treatment involves behavioral therapy combined with the use of one of three FDA approved drugs: buprenorphine, methadone, and naltrexone. According to the Substance Abuse and Mental Health Services Administration, these medications "normalize brain chemistry, block the euphoric effects of alcohol and opioids, relieve physiological cravings, and normalize body functions without the negative and euphoric effects of the substance used."[41]

Saloner and Maclean (2020) similarly report increases in SUD related treatment, finding that 36 percent more people entered specialty SUD treatment by a state's fourth expansion year, as compared to states that did not expand Medicaid. Like Meinhofer & Witman, they find a large increase in MAT and additionally find an increase in admissions to intensive outpatient programs.

Wen et al. (2020) examined data from hospitals and found that Medicaid expansion was associated with reductions in opioid-related inpatient hospitalizations but no change in the rate of opioid-related emergency department visits. They suggest that the decrease in inpatient hospitalizations may be attributable to better outpatient treatment among those who benefitted from Medicaid expansion.

Finally, another line of literature looks at changes in prescribing patterns following Medicaid expansion. Outcomes tested include opioid prescriptions themselves, along with medications used to treat opioid use disorders in MAT. The results within this line of literature are

[41] https://www.samhsa.gov/medication-assisted-treatment.

the most consistent, showing that Medicaid expansion states experience little to no difference in opioid prescribing rates, but very large increases in prescriptions for medication used to treat opioid-use disorders, such as buprenorphine (Cher et al. 2019; Saloner et al. 2018; Sharp et al. 2018; Wen et al. 2017). These findings align with those of Meinhofer & Witman (2018) and Saloner & Maclean (2020) discussed above, which both found increases in utilization of MAT for opioid-use disorders.

D. ACA Medicaid Expansion and Racial Disparities

While the bulk of the evidence appears to show gains in SUD treatment via Medicaid expansion—especially in the context of MATs—some researchers have investigated whether these improvements are concentrated among certain subpopulations. I specifically discuss potential racial disparities, given the concerns already explored in Chapters 1 and 2 about disparities disfavoring the Black population within the criminal justice system.

The clearest potential cause for racial disparities is the fact that states with the largest portion of Black populations were the least likely to expand Medicaid. Thus, Black individuals are less likely to benefit from Medicaid expansion at all. The Kaiser Family Foundation's (KFF) analysis of the American Community Survey shows that of the 12 states plus DC that have Black populations above 15%, only 6 have expanded Medicaid (approximately 46%). In comparison, of the remaining 38 states, 31 states have expanded Medicaid (approximately 82%).[42] KFF highlights that Black individuals make up a larger share of the population in the South—a region that has largely not undertaken Medicaid expansion.

[42] https://www.kff.org/racial-equity-and-health-policy/issue-brief/changes-in-health-coverage-by-race-and-ethnicity-since-the-aca-2010-2018/

Andrews et al. (2015) focused specifically on the population with SUDs and found that Black and Native American populations with SUDs who would have been eligible for Medicaid under expansion were more likely than those of other racial groups to reside in non-expansion states. These findings align with numerous studies that have found an inverse relationship between the generosity of a state's welfare benefits and its proportion of Black residents (Richardson 2014).

Beyond the fact that Black individuals are less likely to benefit from Medicaid expansion at all, research points to potential racial disparities in access to SUD treatment, independent of ability to pay. Prior to Medicaid expansion, a study by Cummings et al. (2014) predicted that local unavailability of SUD treatment facilities might temper the ability of Medicaid expansion to improve SUD treatment rates. They found that, after controlling for other factors, counties with higher percentage of black residents, uninsured residents, and rural counties were less likely to have an outpatient SUD facility that accepted Medicaid. Stein et al. (2018) examined data from 2002 – 2009 and found that counties with higher poverty rates and higher concentrations of black and Hispanic individuals experienced lower rates of increased access to buprenorphine during that time frame.

Other studies have found that Black individuals with SUDs undergoing MAT were more likely to be prescribed methadone, while their White counterparts were more likely to be prescribed buprenorphine (Farahmand et al. 2020; Hansen et al. 2016; Kunins 2020; Lagisetty et al. 2019). Methadone is regulated much more tightly than buprenorphine, sometimes requiring daily visits to an SUD treatment center to receive medication. These requirements make completing MAT treatment much more difficult for those who are prescribed methadone, as compared with buprenorphine. Other factors, such as differences in unemployment and housing instability, have been found to lead to lower SUD treatment completion among Black and Hispanic individuals, as compared with their White counterparts (Saloner & Lê Cook 2013).

III. Data and Empirical Methodology

I use Federal Bureau of Investigation's Uniform Crime Reporting dataset (UCR) on arrests from 2010 to 2018. Agencies represented in the UCR include those at the city, county, state, and federal level, along with university/college and tribal agencies. The UCR collects national arrest data from police agencies on 28 different types of offenses, one of which is "drug abuse violations." Many of the papers that have examined the impact of Medicaid expansion on crime rates have similarly used UCR data. Drug abuse violations accounted for the largest number of UCR arrests in all years from 2010 to 2018 ranging from approximately 1.5 million to 1.6 million arrests each year—with the most common runners up being driving under the influence and larceny-theft.

Arrests for drug abuse violations are further broken down into distinct categories. The two most general categories of drug arrests are total drug sale arrests and total drug possession arrests. The UCR further splits drug arrests by type of drug, with four possible categories: marijuana; cocaine, heroin, and their derivatives; synthetic narcotics that are "truly addicting"; and other dangerous, non-narcotic drugs. In total, the drug-type-specific data create eight additional arrest categories, split into sales and possession. Notably, the UCR employs a hierarchy rule, meaning that police agencies only report one crime for each individual arrest, regardless of whether a person committed multiple crimes leading to the single arrest. Thus, the count of drug abuse violation arrests only include those for which the drug abuse violation was the most serious crime arrested for.[43]

The FBI lists examples of drugs in each category.[44] Table 2 provides these examples, as well as drugs not explicitly listed by the FBI that fall into each category.[45]

Table 2: UCR Drug Categories

Drug Category	**Drugs Included**
Marijuana	Marijuana
Opium/Cocaine and their Derivatives	Cocaine Codeine Heroin Morphine
Other Dangerous, Non-Narcotic Drugs	Benzedrine Barbiturates Methamphetamine
Synthetic Narcotics	Demerol Fentanyl Methadone[46]

I use a version of the UCR arrest data at the agency-year level, which is compiled by Kaplan (2020) and made available through the Inter-University Consortium for Political and Social Research. This dataset provides detailed information on all 10 categories of drug arrests by age, gender, and race.

[43] The 28 crime categories are split into Part I and Part II offenses. Drug abuse violations fall into the category of Part II. If a drug abuse violation arrest also includes any Part I offense, the hierarchy rule calls for the Part I offenses to be reported and the drug abuse violation to be ignored. Part I offenses include criminal homicide, rape, robbery, aggravated assault, burglary, larceny-theft, motor vehicle theft, arson, and human trafficking. If an individual is simultaneously arrested for drug abuse and other Part II offenses, it is left up to the individual agency to determine which crime is the most serious and list the arrest under that crime category. Other Part II offenses include: simple assault, forgery and counterfeiting, fraud, embezzlement, stolen property (buying, receiving, possessing), vandalism, weapons, prostitution, sex offenses (except rape and prostitution), gambling, offenses against family and children, driving under the influence, liquor law violations, drunkenness, disorderly conduct, vagrancy, suspicion, and all other offenses. *See* https://www.fbi.gov/file-repository/ucr/ucr-srs-user-manual-v1.pdf/view.

[44] https://ucr.fbi.gov/crime-in-the-u.s/2017/crime-in-the-u.s.-2017/topic-pages/offense-definitions.

[45] For example, the FBI does not explicitly list Fentanyl as an example of a synthetic narcotic.

[46] While methadone can be prescribed to treat opioid addiction, it can also be abused.

Because reporting is voluntary, many agencies do not report data in every month or year. Nonreporting can take on a variety of forms. For example, some agencies report no arrests for an entire year, while some report arrests only for some months of the year. Further, agencies may report arrests for only some of the crime categories. Following the criminology and economics of crime literature, I include all observations for which a police agency reported at least six months of data for the relevant crime category (Carpenter 2007; Chu 2015; Chu 2014). Some drug crimes are reported more consistently than others, which means that my sample size varies by outcome. Total drug possession arrests have the largest sample and synthetic narcotic sales arrests have the smallest sample.

Maltz & Targonski (2002) show that non-reporting among UCR agencies is not random, with smaller agencies tending to report at lower rates. They highlight that this can be especially problematic for interpreting results in the UCR county-level data. One approach researchers have taken in light of this issue is limiting analysis to agencies that represent large populations (greater than 50,000) in metropolitan statistical areas (MSAs). The FBI regularly communicates with these agencies to ensure higher levels of reporting, and thus non-reporting concerns are diminished (Akiyama and Propheter 2005; Carpenter 2007; Chu 2015).

I include any agency in an MSA that represented a population of 50,000 or more in 2010, as populations generally grow over time, and this guarantees that each agency I include is likely to cover at least 50,000 residents for all years from 2010-2018. As a result, my findings are generalizable only to MSAs and do not represent rural areas. An MSA is defined by the census bureau as an "area containing a substantial population nucleus, together with adjacent communities having a high degree of economic and social integration with that core," which has at least one urbanized area with a population of 50,000.[47]

My difference-in-differences empirical model estimates the following equation:

$$Y_{at} = \beta_0 + \beta_1 Medicaid\ Expansion_{at} + \beta_2 X_{at} + \gamma_t + \delta_a + \varepsilon_{at}$$

Y_{at} represents the various drug arrest rates described above (per 100,000 people). The variable $Medicaid\ Expansion_{at}$ is an indicator variable that represents whether the agency was in a state that had already expanded Medicaid at time t. To illustrate, an agency located in New Mexico would have a value of 0 for this variable any year prior to 2014 and a value of 1 in 2014 and later. An agency located in Georgia would have a value of 0 for this variable in all years. The coefficient associated with this indicator variable, β_1, captures the change in the annual rate of drug arrests (per 100,000 residents) resulting from Medicaid expansion. It is my coefficient of interest.

X_{at} is a vector of county- and state-level characteristics that correspond to the relevant agency. The county-level variables include percent of the population that are young men between the ages of 20 and 29 (as they are statistically responsible for a large portion of crime), percent White, percent Black, percent Hispanic, percent Asian, unemployment rate, median income, and percent of residents below the poverty line. State-level variables include annual spending on hospitals and healthcare, welfare, and education, and whether the state has an active law legalizing medicinal and recreational marijuana. γ_t and δ_a account for year and agency fixed effects, respectively.

[47] https://www.census.gov/programs-surveys/metro-micro/about.html

IV. Main Results

A. Summary Statistics

I begin by presenting summary statistics on my data in Table 3. These statistics are split between expansion and non-expansion states and represent the period before Medicaid expansion (2010-2013).

There are some differences in drug arrest rates, with expansion states reporting more possession arrests for heroin/cocaine and other dangerous non-narcotic drugs, as well as more drug sales arrests overall. In contrast, non-expansion states report more arrests for marijuana possession.

MSAs in expansion states have a higher household median income and lower poverty rates, but higher unemployment rates than MSAs in non-expansion states. One of the more striking statistics in Table 2 is the difference between expansion and non-expansion states by race. Among the MSA police agencies in expansion states, only 8% of the covered population is Black, while that number is doubled (16%) in non-expansion states. White individuals are also slightly less likely to live in MSAs that expanded Medicaid, while Hispanic, Asian, and other/mixed race individuals are more likely to live in MSAs that expanded Medicaid.

Table 3: Summary Statistics (2010 – 2013)

	Expansion States	Non-Expansion States
Arrests (per 100K residents)		
Possession		
All Drugs	354.56	339.56
Marijuana	126.91	223.10*
Heroin/Cocaine	126.67*	77.01
Other Drug	141.22*	77.32
Synthetic Drug	46.55	41.59
Sales		
All Drugs	90.60*	79.30
Marijuana	39.74	39.61
Heroin/Cocaine	53.92	47.36
Other Drug	43.71	41.05
Synthetic Drug	33.50	37.29
Independent Variables		
Demographics		
Population	143,082	158,645
Percent Young Male	10.97	11.04
Percent White	60.62	63.29*
Percent Black	8.35	16.24*
Percent Asian	7.05*	3.04
Percent Hispanic	20.87*	15.02
Percent Other Race	3.12*	2.42
Economics		
Median Household Income	56,914*	52,110
Percent Below Poverty Line	15.04	15.95*
Unemployment	9.20*	7.69
Government Spending (per capita)		
Education	2,916	2,909
Health & Hospitals	756	902*
Welfare	1,640*	1,508

All values are weighted by the population covered by the agency. *Indicates difference at the 5% statistical level.

B. Regression Results

My main regressions are run using the two most general categories: total drug possession arrests and total drug sales arrests. The results from these regressions are presented in Table 4. All regressions are weighted by the population covered by the agency. As mentioned above, the number of observations varies by outcome due to differences in reporting frequency (8,577 agency-years for drug possession and 7,169 agency-years for drug sales). In my main regressions, I find evidence that Medicaid expansion reduced the rate of drug sales arrests, but I find no statistically significant result for drug possession. The coefficient on young men indicates that a 1% increase in the percentage of young men in an agency's covered population is associated with 53 more drug possession arrests per 100,000 residents and 18 more drug sales arrests per 100,000 residents. This finding accords with the knowledge that young men make up the majority of arrests.

Table 4: Rate of Drug Arrests Per 100,000 Population (All Drugs)

	(1) Possession	(2) Sales
Medicaid Expansion	-29.511	-13.183**
	(29.327)	(6.000)
Recreational Marijuana	-18.854	-7.025
	(17.777)	(5.079)
Medical Marijuana	-14.451	-8.612
	(16.241)	(5.819)
Unemployment Rate	8.365	2.838
	(6.192)	(1.794)
Median Household Income ($1,000s)	2.803	0.184
	(1.821)	(0.532)
Percent Below Poverty Line	4.766	1.019
	(3.553)	(0.849)
Percent Young Men (20-29)	53.049*	17.836**
	(28.581)	(6.658)
Percent White	-72.762	-13.594
	(77.208)	(19.437)

Percent Black	-47.575	-2.329
	(70.372)	(19.173)
Percent Latino	-67.626	-16.004
	(70.719)	(18.485)
Percent Asian	-77.012	-12.778
	(75.662)	(18.881)
Welfare	6.189*	0.440
	(3.352)	(0.707)
Health and Hospitals	6.585	-1.252
	(4.843)	(1.296)
Education	2.036	1.420
	(3.695)	(0.929)
Constant	6,085.475	1,087.522
	(6,996.717)	(1,842.663)
Observations	8,577	7,169
R-squared	0.792	0.872

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

To determine if arrests differ by drug type, I run 8 additional regressions where each dependent variable is either sale or possession arrests for one of the four sub-categories of drugs. Table 5 presents my coefficient of interest from each separate regression. Appendix tables A1 – A4 present the full regression results associated with Table 5. In my drug-specific regressions, I find evidence of a reduction in heroin/cocaine sales and synthetic narcotics possession. All opioids are included in one of these two categories.

Table 5: Impact of Medicaid Expansion on Rate of Drug Arrests Per 100,000 Population (By Drug Type)

	(1) Possession	(2) Sales
Marijuana	-16.174	-3.236
	(16.108)	(3.866)
Heroin/Cocaine	-9.067	-9.024**
	(16.463)	(4.332)
Non-Narcotic Drugs	-7.910	-4.444
	(10.942)	(3.004)
Synthetic Narcotics	-12.556**	-8.433
	(5.705)	(5.605)

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each value represents the coefficient on Medicaid expansion from a separate regression. Full regression results presented in Appendix tables A1 – A4.

One could potentially explain these decreases by an increase in the number of legally obtained opioid prescriptions, which would decrease both the number of people illegally possessing opioids and the demand for illegal opioids. However, the research discussed above in Part II indicates this is likely not the case. Of researchers who have looked at Medicaid expansion's impact on prescriptions specifically, the bulk of evidence points to no increase in the number of opioid prescriptions in Medicaid expansion states, with a large increase in the number of prescriptions used to treat opioid use disorders via MAT.

I test the plausibility of this mechanism by using a proxy for MAT treatment availability by county. I use data on the per capita number of buprenorphine-waivered providers in each county in 2018. Numerous studies have shown that the number of buprenorphine-waivered providers in a jurisdiction is related to treatment availability. While the 2018 measure does not capture the availability each year, it provides a rough estimate of the capacity of individual counties to treat residents via MAT. I use this measure to introduce an interaction term into my regression. In this, the coefficient on "Medicaid Expansion in High Treatment Capacity Counties" represents the impact of Medicaid expansion that is concentrated in counties that are among the highest 25% of buprenorphine-waivered providers.

Table 6: Rate of Opioid-Category Drug Arrests Per 100,000 Population

	(1) Possession	(2) Sales
Medicaid Expansion	4.770	-1.889
	(13.312)	(3.846)
Medicaid Expansion in High Treatment Capacity Counties	-23.718*	-11.310***
	(12.302)	(3.870)
Recreational Marijuana	-6.177	-0.773
	(8.542)	(2.343)
Medical Marijuana	3.597	-3.206
	(7.599)	(4.570)
Unemployment Rate	1.165	0.841
	(2.852)	(1.204)
Median Household Income ($1,000s)	-0.006	0.245
	(1.000)	(0.286)
Percent Below Poverty Line	0.336	0.569
	(1.270)	(0.671)
Percent Young Men (20-29)	22.653*	9.132*
	(12.402)	(4.613)
Percent White	-42.397	-18.773
	(39.731)	(12.422)
Percent Black	-30.256	-10.365
	(35.174)	(11.066)
Percent Latino	-36.348	-19.969*
	(35.630)	(11.408)
Percent Asian	-40.331	-18.304
	(38.046)	(11.750)
Welfare	5.041***	0.824*
	(1.572)	(0.477)
Health and Hospitals	5.829**	-0.138
	(2.466)	(0.761)
Education	1.193	0.681
	(1.821)	(0.831)
Observations	6,977	4,316
R-squared	0.812	0.894

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

C. Event Study

I next turn to an event study—which examines the impact of Medicaid expansion on a yearly basis—for two reasons. First, the validity of difference-in-differences estimates depends on the parallel trends assumption, which represents the idea that absent Medicaid expansion, drug arrests in the treatment and control states would have continued trending on a parallel path. The parallel trends assumption can be tested via event study. If the coefficients in an event study corresponding to the periods prior to Medicaid expansion—which represent the differences between those two groups during the pre-periods—are statistically indistinguishable from zero, the parallel trends assumption is met.[48]

Second, recovery from drug addiction can take a long time. To the extent people who previously did not have access to SUD treatment are granted access via Medicaid expansion, it may take multiple years for them to fully recover (if at all). Thus, by examining the impact of Medicaid expansion by year, I can determine whether the impact grows over time.

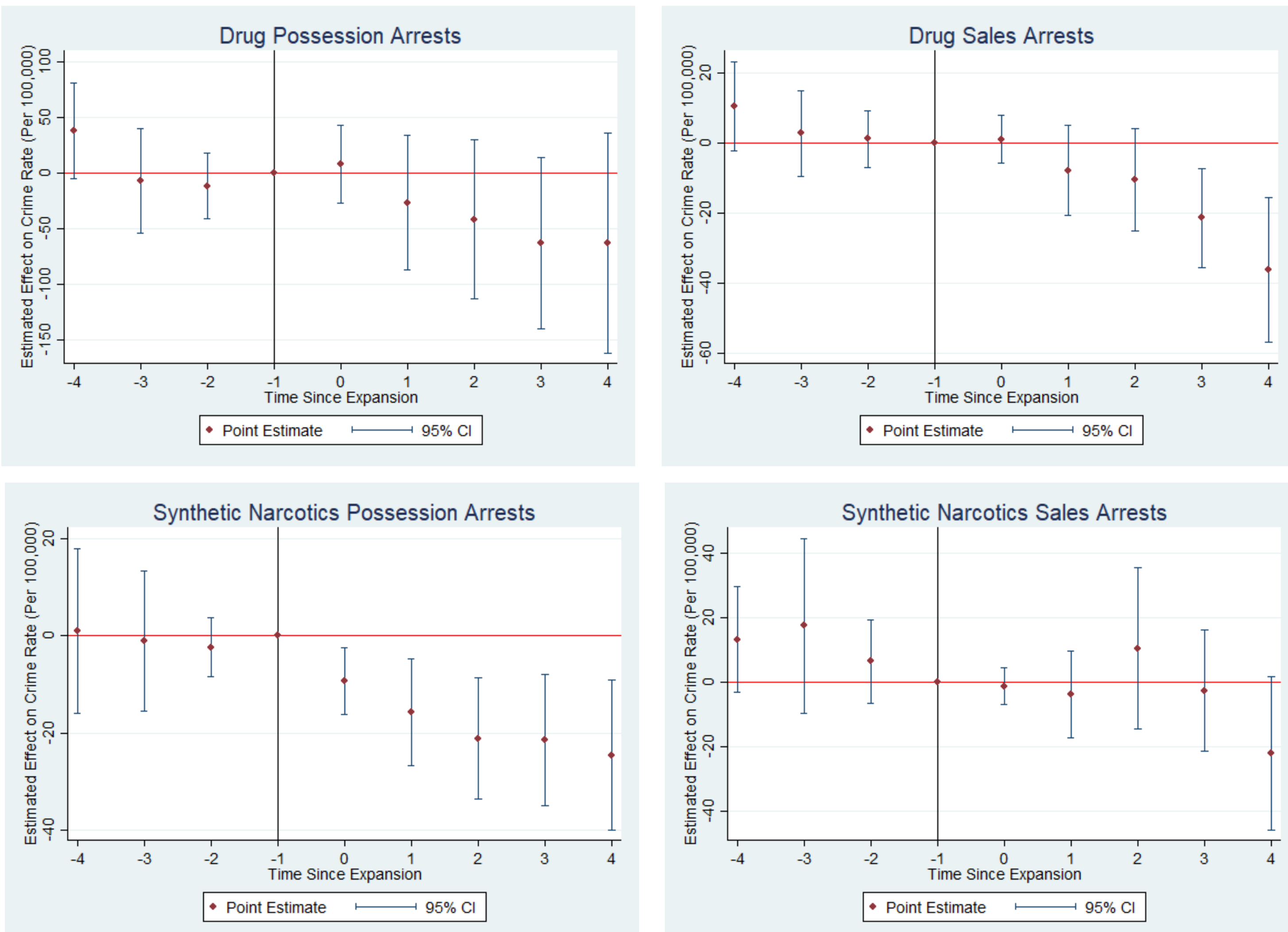
My event study model is similar to my main regression equation. The only difference is that my coefficient of interest is now split into coefficients for each year, ranging from four years prior to Medicaid expansion to four years after Medicaid expansion. My coefficients for each type of arrest are plotted in Figure 1 below.

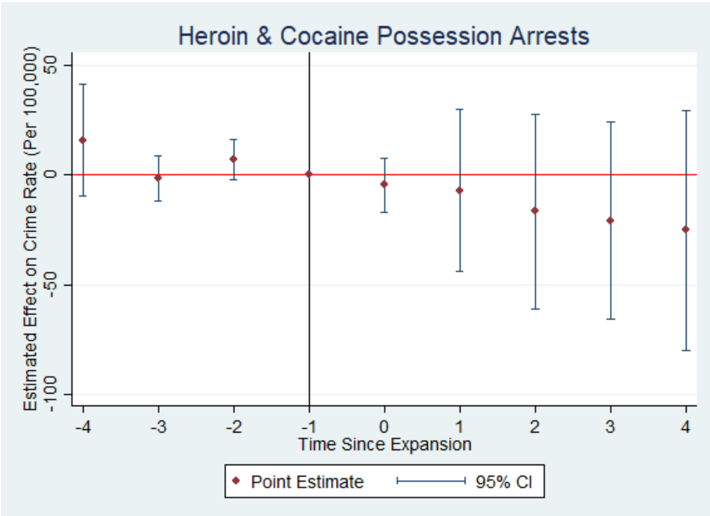
My results from the event study analysis align with those in my main regressions. I find a clear impact on drug sales arrests overall and synthetic narcotic possession arrests. The event study results on heroin and cocaine sales are less clear, but still apparent. It also appears that post-expansion, overall drug possession, heroin and cocaine possession, other non-narcotic drug sales,

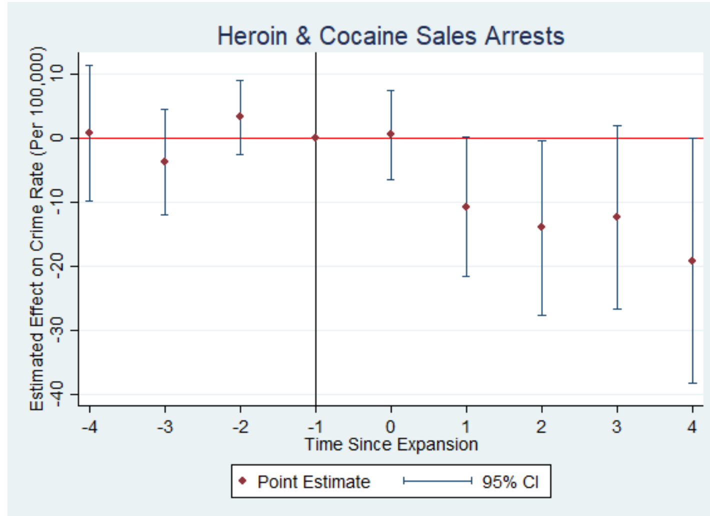
[48] For an example of an event study where the parallel trends assumption is not met, see the event study below for marijuana possession arrests, where the coefficient corresponding to four years prior to treatment is statistically different from zero.

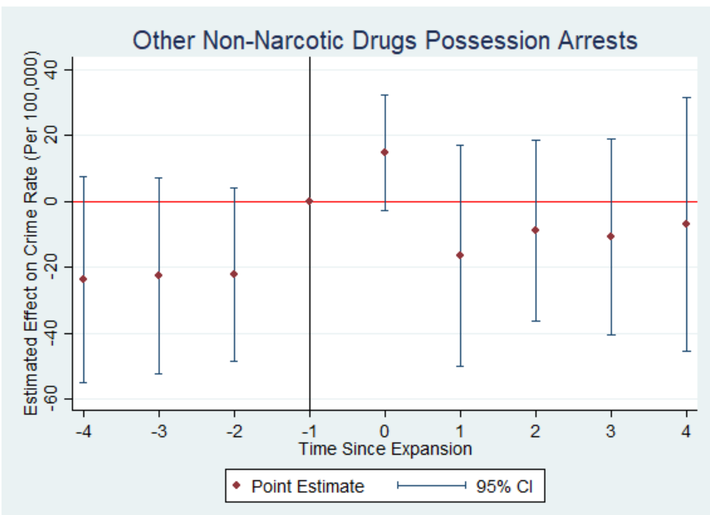
and marijuana sales are all trending downwards over time in expansion states. Marijuana possession arrests also appear to be trending downwards, but this result should be interpreted with caution, as the coefficient in time period -4 for marijuana possession arrests indicates a violation of the parallel trends assumption.

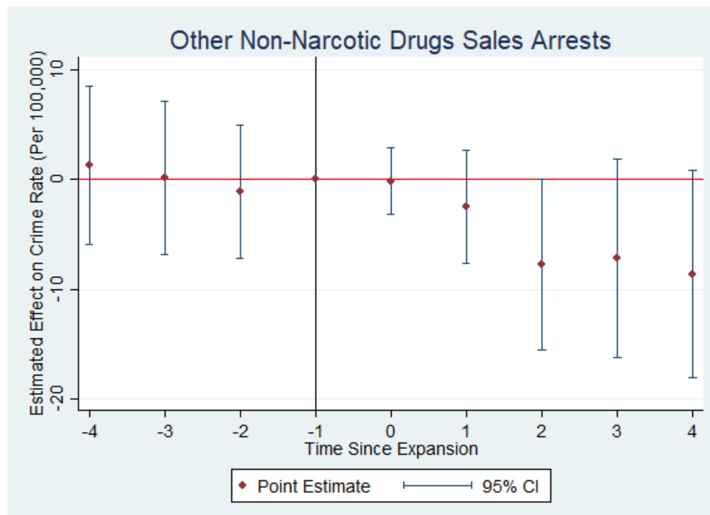
Figure 2: Event Studies (Arrests)

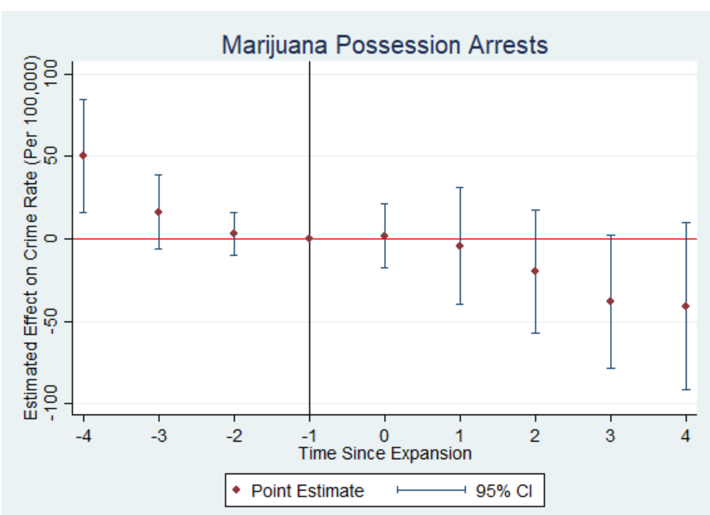


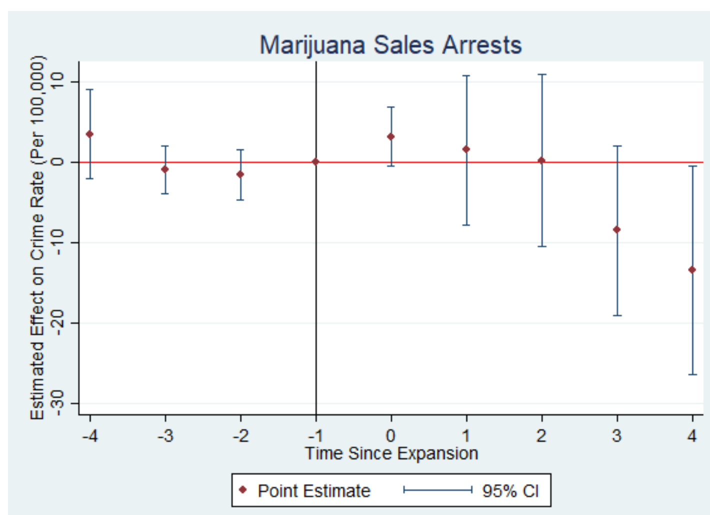

Heroin & Cocaine Possession Arrests
Estimated Effect on Crime Rate (Per 100,000)
50
0
-50
-100
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI


Heroin & Cocaine Sales Arrests
Estimated Effect on Crime Rate (Per 100,000)
10
0
-10
-20
-30
-40
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI


Other Non-Narcotic Drugs Possession Arrests
Estimated Effect on Crime Rate (Per 100,000)
40
20
0
-20
-40
-60
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI


Other Non-Narcotic Drugs Sales Arrests
Estimated Effect on Crime Rate (Per 100,000)
10
0
-10
-20
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI


Marijuana Possession Arrests
Estimated Effect on Crime Rate (Per 100,000)
100
50
0
-50
-100
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI


Marijuana Sales Arrests
Estimated Effect on Crime Rate (Per 100,000)
10
0
-10
-20
-30
-4
-3
-2
-1
0
1
2
3
4
Time Since Expansion
Point Estimate
95% CI

V. Results by Race

Given the potential for racial disparities in the benefits from Medicaid expansion, I next assess the impact of Medicaid expansion by race. I focus specifically on arrests for all drugs as well as for synthetic narcotics, given the statistically significant results on these categories in my main regressions. I also investigated heroin and cocaine sales by race—the other outcome that was impacted by Medicaid expansion in my main results—but found no statistically significant results.

Table 6 presents summary statistics on possession and sales arrests for all drugs and synthetic narcotics only. These statistics are representative of all states, expansion and non-expansion, and cover the period prior to expansion (2010-2013). In the period prior to Medicaid expansion, synthetic opioids appear to be a category of drug that was mainly affecting White individuals, as compared with other drug categories which had much lower rates of White arrests. Notably, some researchers and commentators have discussed that the United States government has treated White drug addiction as a health issue, while simultaneously treating Black drug addiction as a criminal issue (Alexander 2012).

Table 7: Summary Statistics on Drug Arrests by Race

	(1) White	(2) Black	(3) Asian	(4) Native American
2010-2013 (per 100K)				
All Drugs	238.65	98.83	3.45	2.13
	69.6%	28.8%	1.0%	0.6%
Synthetic Narcotics	35.99	6.76	0.29	0.44
	82.8%	15.5%	0.7%	1.0%

All values are weighted by the population covered by the agency. Percentages represent the portion of arrests that are attributable to each racial category for that crime type.

I next run a difference-in-differences regression analysis split by race. Table 7 presents my results, while Appendix figures A1 – A4 present corresponding event study graphs. I find a statistically significant reduction in synthetic narcotics possession arrests for all racial groups except for Black individuals. This finding aligns with concerns expressed by researchers about racial disparities in SUD treatment, especially MAT treatment for opioid addiction.

As shown in Appendix Figure 4A, I do find that overall drug possession arrest sand synthetic narcotic possession arrests for Black individuals are trending downward in the associated event study, without reaching statistical significance. This may be a sign that the Black population is slowly gaining access to SUD treatment in Medicaid expansion states. I also find evidence that Black drug sales arrests are reduced four years post-expansion, which may reflect lower demand for drugs among other races, due to SUD treatment.

While my regression results show a reduction in drug sales overall for the White population, Appendix Figure 3A highlights that this specific category violates the parallel trends assumption, and this result should be interpreted with caution.

Table 8: Impact of Medicaid Expansion on Rate of Drug Arrests Per 100,000 Population (By Drug Type and Race)

	(1) White	(2) Black	(3) Asian	(4) Native American
Possession				
All Drugs	-9.506	-20.522	-0.712**	-0.121
	(9.250)	(23.354)	(0.336)	(0.597)
Synthetic Narcotics	-10.088**	-1.867	-0.154*	-0.423*
	(4.986)	(1.465)	(0.085)	(0.251)
Sales				
All Drugs	-8.162***	-5.142	-0.024	0.009
	(2.844)	(4.486)	(0.140)	(0.089)
Synthetic Narcotics	-7.309	-0.959	-0.005	-0.068
	(4.555)	(1.098)	(0.087)	(0.046)

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each value represents the coefficient on Medicaid expansion from a separate regression. Corresponding event studies presented in Appendix figures A1 – A4.

VI. Implications and Conclusion

My results indicate that expanding access to health care through Medicaid has reduced drug arrests, but that the effect is heterogeneous with respect to drug type and race. I find the strongest evidence for a reduction in arrests due to possession of synthetic narcotics for the White, Asian, and Native American populations, but not the Black population. I also find that, across races, drug sales arrests appear to drop.

At first glance, Medicaid expansion would seem to be a policy that would reduce racial disparities in healthcare, and in some ways, it has. In states that expanded Medicaid coverage, racial disparities in uninsured rates have dropped significantly.[49] However, insurance coverage does not guarantee treatment.

Some research has suggested that, in the context of SUD treatment, Black individuals may be less likely to receive treatment due to lack of access to care and racially disparities in physician prescribing behaviors. My finding that Medicaid expansion results in statistically significant reductions in synthetic narcotic possession arrests in the White, Asian, and Native American populations, but not the Black population, provides additional support that this concern is legitimate.

This Chapter further indicates that, while Medicaid expansion appears to be effective at reducing the rate of drug arrests, it may have inadvertently increased racial disparities in the criminal justice system. This is true for two reasons. First, Black individuals are significantly less likely to live in states that expanded Medicaid. This means that relative to their White counterparts, they are less likely to receive the benefits—including SUD treatment—from Medicaid expansion. Further, even within Medicaid expansion states, it appears that the benefits of reduced drug possession arrests are concentrated among other races. This indicates that even in states that did expand Medicaid, there may be racial disparities in SUD treatment.

To the extent Medicaid expansion reduced arrests for drug crimes, it appears to have done so in a way that disproportionately benefited non-Black individuals. My future work will examine arrests for other crimes by race, to determine whether Medicaid expansion reductions in crime found in previous papers are concentrated among the White population or distributed across races.

[49] https://www.kff.org/racial-equity-and-health-policy/issue-brief/changes-in-health-coverage-by-race-and-ethnicity-since-the-aca-2010-2018/

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Table 9A: Marijuana: Full Regression Results

	(1) Possession	(2) Sales
Medicaid Expansion	-16.174	-3.236
	(16.108)	(3.866)
Recreational Marijuana	-40.653**	-3.554
	(15.845)	(4.107)
Medical Marijuana	5.667	-1.606
	(11.868)	(2.432)
Unemployment Rate	9.195*	1.887**
	(4.609)	(0.933)
Median Household Income ($1,000s)	3.013***	0.264
	(0.988)	(0.300)
Percent Below Poverty Line	1.558	0.518
	(1.979)	(0.557)
Percent Male 20-30	31.082*	5.671
	(18.087)	(3.466)
Percent White	-17.808	-12.500
	(46.087)	(10.995)
Percent Black	-8.467	-8.911
	(42.240)	(12.113)
Percent Latino	-14.435	-11.458
	(42.570)	(10.897)
Percent Asian	-21.634	-12.506
	(44.204)	(10.785)
Welfare	3.149	0.399
	(1.943)	(0.435)
Health and Hospitals	1.530	-0.844
	(2.270)	(0.810)
Education	2.843	0.478
	(2.220)	(0.402)
Constant	1,054.507	1,071.063
	(4,212.706)	(1,082.803)
Observations	7,532	4,505
R-squared	0.814	0.755

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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Table 10A: Heroin/Cocaine: Full Regression Results

	(1) Possession	(2) Sales
Medicaid Expansion	-9.067	-9.024**
	(16.463)	(4.332)
Recreational Marijuana	-7.709	-1.987
	(8.666)	(2.337)
Medical Marijuana	0.267	-4.087
	(7.138)	(4.710)
Unemployment Rate	0.135	0.246
	(2.698)	(1.156)
Median Household Income ($1,000s)	0.001	0.213
	(1.207)	(0.373)
Percent Below Poverty Line	0.083	0.437
	(1.231)	(0.708)
Percent Male 20-30	21.033*	8.172*
	(11.563)	(4.233)
Percent White	-43.377	-19.483
	(39.894)	(12.348)
Percent Black	-30.505	-10.619
	(35.656)	(11.398)
Percent Latino	-36.000	-19.926*
	(35.100)	(11.238)
Percent Asian	-40.386	-18.213
	(38.723)	(11.872)
Welfare	5.734***	1.193**
	(1.827)	(0.550)
Health and Hospitals	5.849**	-0.223
	(2.668)	(0.895)
Education	1.136	0.735
	(1.783)	(0.834)
Constant	3,603.429	1,683.578
	(3,546.306)	(1,109.871)
Observations	6,997	4,316
R-squared	0.811	0.894

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11A: Other Dangerous Non-Narcotic Drugs: Full Regression Results

	(1) Possession	(2) Sales
Medicaid Expansion	-7.910	-4.444
	(10.942)	(3.004)
Recreational Marijuana	16.191**	-2.187*
	(6.070)	(1.289)
Medical Marijuana	-25.436***	2.338
	(8.465)	(3.082)
Unemployment Rate	-1.390	-0.416
	(2.627)	(1.007)
Median Household Income ($1,000s)	0.384	0.064
	(0.831)	(0.170)
Percent Below Poverty Line	5.060	-0.168
	(3.820)	(0.401)
Percent Male 20-30	-2.561	-2.591
	(13.118)	(3.958)
Percent White	-40.560	-2.407
	(27.647)	(5.481)
Percent Black	-32.793	2.295
	(27.731)	(6.666)
Percent Latino	-45.311	-7.342
	(29.126)	(6.250)
Percent Asian	-40.842	-3.733
	(34.421)	(6.351)
Welfare	-2.087**	-0.116
	(1.006)	(0.301)
Health and Hospitals	-2.194	-1.232
	(2.390)	(0.794)
Education	-0.906	0.617
	(1.822)	(0.644)
Constant	4,084.146	351.387
	(2,709.686)	(559.258)
Observations	6,755	3,907
R-squared	0.823	0.813

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 12A: Synthetic Narcotics: Full Regression Results

	(1) Possession	(2) Sales
Medicaid Expansion	-12.556**	-8.433
	(5.705)	(5.605)
Recreational Marijuana	-2.825	-4.597
	(3.365)	(4.305)
Medical Marijuana	5.779	-5.642
	(3.961)	(4.639)
Unemployment Rate	3.739	2.868**
	(2.383)	(1.122)
Median Household Income ($1,000s)	-0.103	-1.521
	(0.518)	(1.057)
Percent Below Poverty Line	-1.541**	1.851
	(0.731)	(2.136)
Percent Male 20-30	6.298	17.250
	(5.440)	(12.470)
Percent White	7.344	15.010
	(11.576)	(18.006)
Percent Black	7.392	9.821
	(12.021)	(14.408)
Percent Latino	4.860	7.617
	(11.537)	(12.671)
Percent Asian	4.417	6.467
	(12.998)	(12.211)
Welfare	0.249	-1.655
	(0.887)	(1.011)
Health and Hospitals	-2.498	-0.276
	(2.066)	(1.865)
Education	0.408	-0.201
	(0.948)	(1.257)
Constant	-652.267	-1,166.809
	(1,146.299)	(1,531.137)
Observations	3,152	852
R-squared	0.773	0.956

All values are weighted by the population covered by the agency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 3A: Event Studies (White Arrests)

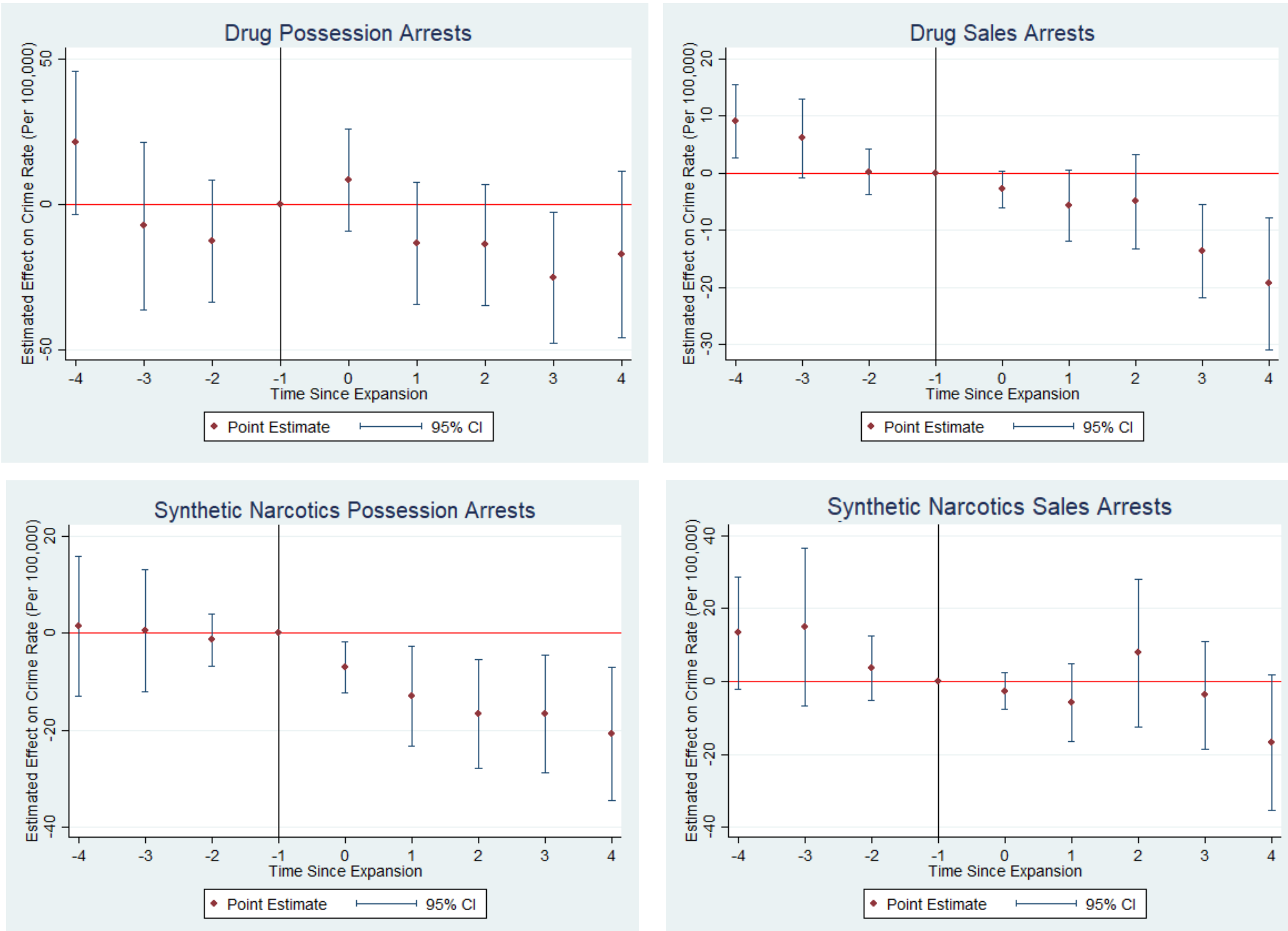

Figure 4A: Event Studies (Black Arrests)

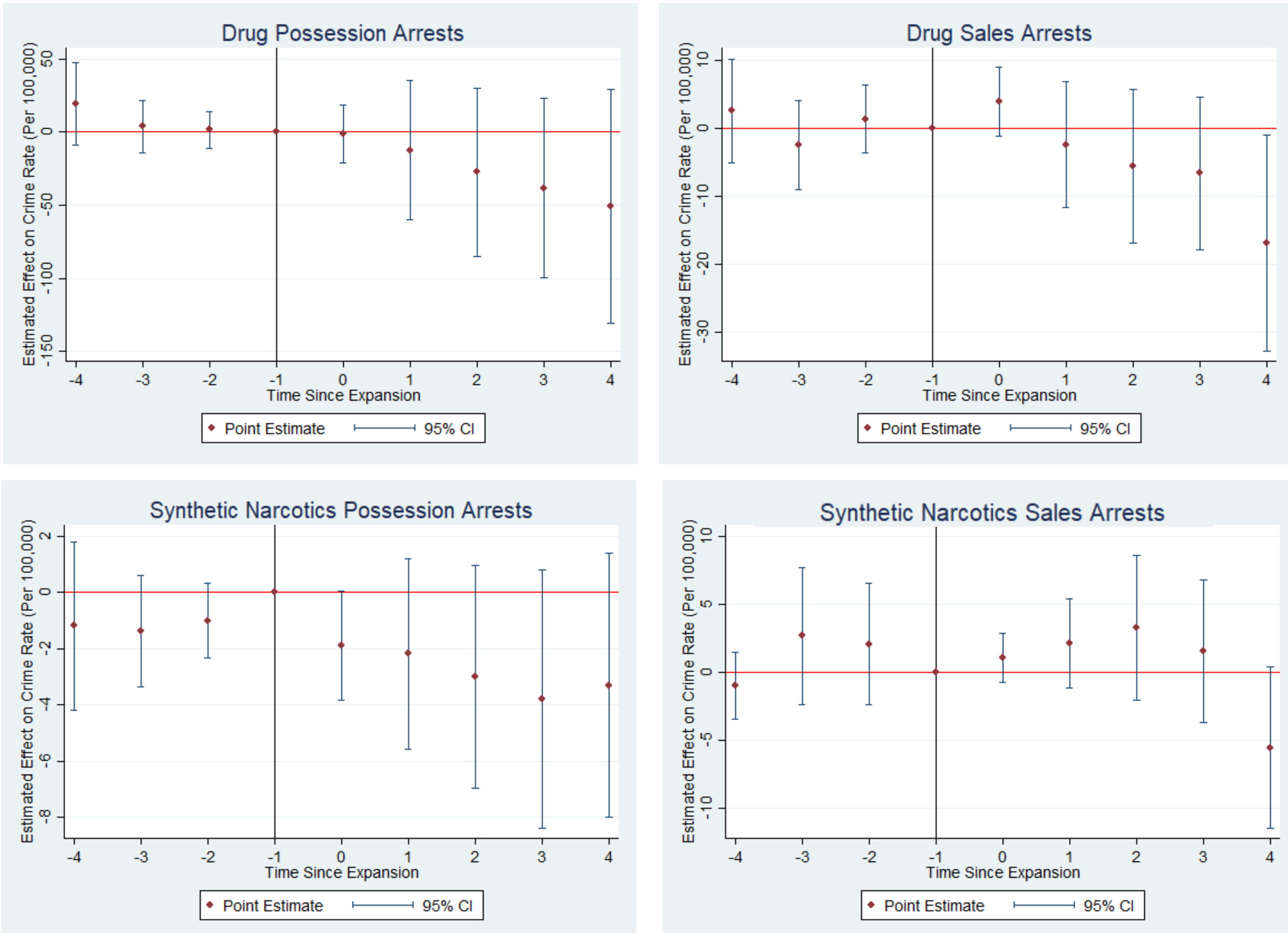

Figure 5A: Event Studies (Asian Arrests)

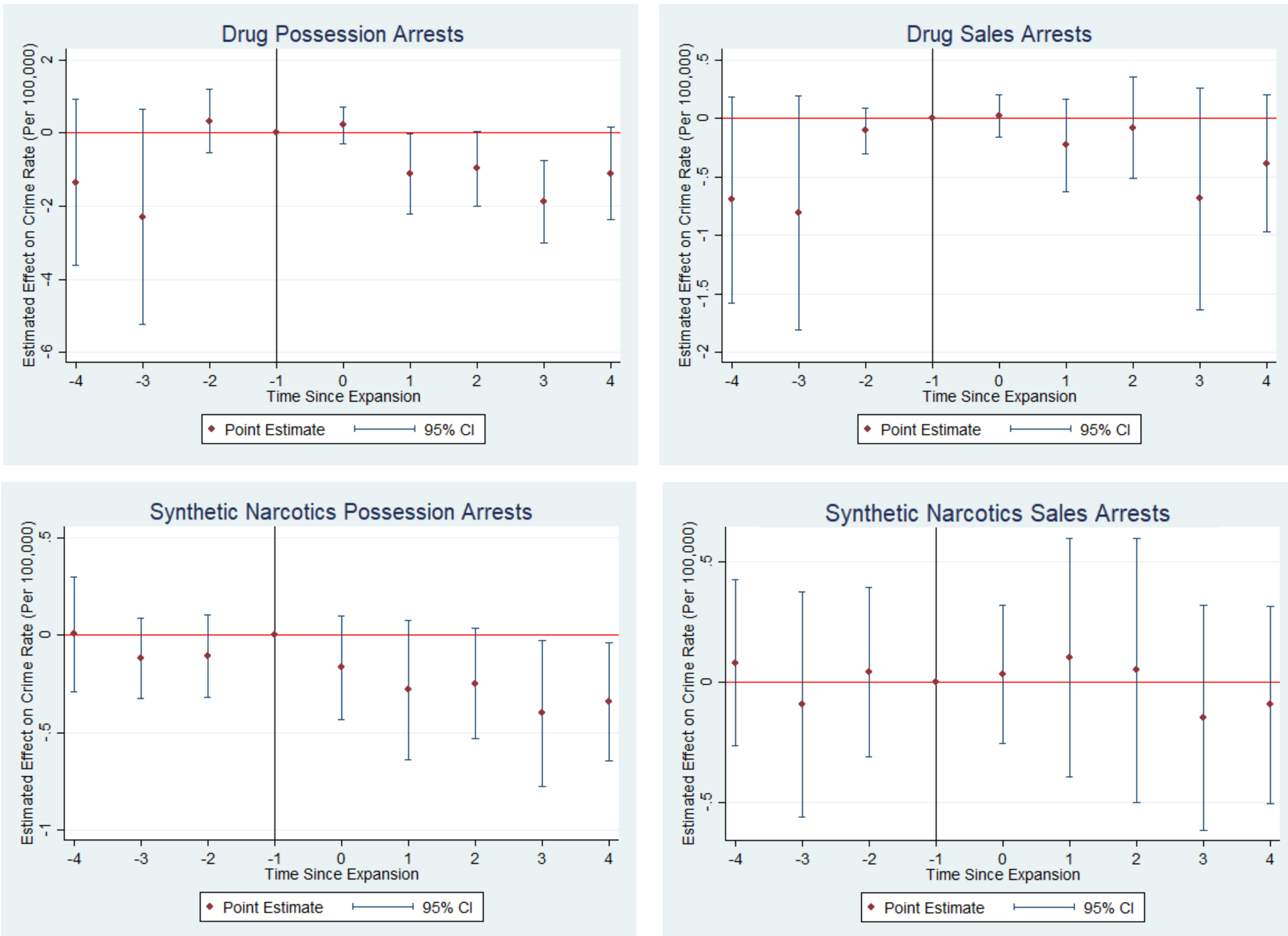

Figure 6A: Event Studies (Native American Arrests)