Criminal Justice Policy, Race, and the Dynamics of Admission to Prison

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Abstract

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Research has long documented racial and economic disparities in imprisonment and the recidivism rate. Despite studies that suggest these disparities cannot be fully explained by differential involvement in crime, scholars have not paid enough attention to how individual characteristics might interact with macro-level factors. This dissertation adds to the literature by examining interactions between individual-level characteristics such as race and history of prior criminal justice contact, and macro-level factors such as criminal justice policies and local racial composition. I structure this dissertation with three chapters that are increasingly focused, in terms of policy and geography.

Chapter One extends criminological research investigating the relationship between racial composition and phenomena in the criminal justice sphere. This chapter investigates how county

racial composition affects racial disparities in recidivism. The analyses test two competing explanations: racial threat, which would predict a positive relationship between the relative size of the nonwhite population and racial disparities in returns to prison, and political representation, which would predict a negative relationship. I use National Corrections Reporting Program (NCRP) data from 18 states and event history methods to test my hypotheses.

The results show that increases in the percent of a county's Hispanic population are associated with a small increase in the hazard of recidivism for Hispanics and Blacks compared to Whites, which is consistent with the racial threat hypothesis. The results also show that an increase in the non-Hispanic Black population is associated with a small overall decrease in the hazard of recidivism as well as a small decrease for non-Hispanic Blacks compared to Whites, which is consistent with the political representation explanation. Future research including other explanatory variables around political representation would further this study. For example, examining the effect of the racial composition of county-level elected officials or the proportion of adults barred from voting due to a jurisdiction's felony disenfranchisement laws would be helpful.

The second chapter focuses on parole, a period of conditional supervised release in the community following a prison term. Returns to prison while on parole are called revocations, and this can occur in two ways. A parole revocation can result from a new crime conviction, or from a technical violation. Technical violations occur from failing to adhere to conditions that typically would not incur a prison term, such as not completing substance abuse treatment or possessing a firearm. Parole revocation policies vary highly across jurisdictions and include differences regarding the importance of past parole terms (episodes).

This chapter advances scholarly understanding of how state differences in parole policy and implementation contribute to differences in the likelihood of experiencing a return to prison. Analyses focus on how the number of parole episodes a person has served interacts with the state they are in to affect the likelihood of experiencing a parole revocation. Comparing jurisdictions in this way can reveal how institutional context and differences in policy shape the outcomes of individuals. It further contributes to the literature by distinguishing between revocations for new crimes and for technical violations, which most studies do not do. Unique parole data for New York and Pennsylvania in the NCRP provide the opportunity to examine revocations in two different institutional contexts. Findings suggest the number of prior parole episodes and the state is significantly associated with differential likelihood and timing of returning to prison and revocation type.

Chapter Three focuses further to understand variation within one state, by exploring drug courts in Georgia. Drug court diversion programs offer people charged with drug possession who have a substance abuse issue an alternative to prison in exchange for participating in a court-supervised intensive treatment program. However, these programs come with high fees that participants must shoulder. If people who do not have the ability to pay the participation fees are excluded from accessing this process of remaining out of prison, drug court implementation may lead to an overall increase in the level of economic disadvantage among people sent to prison for drug possession. Thus, while existing research on drug courts focuses on whether they reduce recidivism for participants, I empirically test whether drug courts increase economic inequity by considering who is excluded from participation. I use a difference-in-differences strategy exploiting time variation in the implementation of drug courts across the state to answer this question. I find that on average, the level of disadvantage as measured by educational attainment does not increase significantly among people admitted to prison for drug possession due to drug court implementation.

Taken together, the findings of this dissertation suggest policy makers and advocates should consider policy levers that address structural factors and not focus solely on programs aimed at altering individual behavior. Scholars should further examine how macro-level factors contribute to recidivism as well as their interactions with individual characteristics to better understand how we can reduce racial and economic disparities in the prison system as well as the cumulative effect of cycling through prison.

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INTRODUCTION

In 2018, 1.29 million people were confined in U.S. state prisons (Carson 2020). Spending time in prison can create and exacerbate individuals' mental and physical health issues and financial difficulties, in addition to having long-term negative effects on the health and wellbeing of their families and communities (Schwartz-Soicher, Geller, and Garfinkel 2011; Turney and Haskins 2014; Wildeman, Lee, and Comfort 2013; Yi, Turney, and Wildeman 2017; Zlodre and Fazel 2012). For example, research suggests that the negative consequences of incarceration on socioeconomic status and family functioning may explain the link between incarceration and major depression for men (Schnittker, Massoglia, and Uggen 2012; Turney, Wildeman, and Schnittker 2012).

The structural racism and economic disparities that exist throughout American institutions are particularly apparent in the criminal justice system. The multitude of harmful consequences are shouldered disproportionately by already disadvantaged groups and particularly by Black people, who had an imprisonment rate 5.2 times that of Whites in 2018 (Carson 2020; Morenoff and Harding 2014; Pettit and Western 2004). Employment and earnings of working age men in the years prior to imprisonment are particularly low and some research shows that almost ten percent of boys born to families in the bottom ten percent of the income distribution were imprisoned at age 30 (Looney and Turner 2018). Although many factors contribute to racial and economic disparities in imprisonment, some of the mechanisms through which structural racism and socioeconomic discrimination operate include the inability to pay for resources such as adequate legal counsel, overpolicing of poorer neighborhoods (Fagan and Davies 2000), and the fact that

Black and Hispanic defendants' cases are more likely to move forward without dismissal at each step of the criminal case process (Kutateladze et al. 2014).

Release from prison is often not the end of time behind bars, as many who leave prison are sentenced to prison again. Recidivism is commonly understood to be defined by two elements: relapse into criminal behavior and receiving new punishment by the criminal justice system for said behavior (Bales et al. 2015). Recidivism, therefore, can be thought of as a return to prison following a new criminal charge. A third to a half of people released from prison will find themselves back in prison at least once more (Durose, Cooper, and Snyder 2014; Rhodes et al. 2016). Given that 567,636 people exited state prison in 2018, this is no small problem (Carson 2020).

Even without recidivating, an exit from prison often does not mark the end of state surveillance. Roughly two-thirds of state prison releases are to parole, which is a period of conditional supervised release in the community following a prison term (Carson 2018; Kaeble 2018a). Similar to racial disparities in imprisonment rates, the non-Hispanic Black parole rate was roughly four times that of non-Hispanic Whites in 2016 (Kaeble 2018a). Parole and returns to prison are tightly linked, as people on parole can return to prison for a new crime conviction, or for a technical violation that leads to a parole revocation. Technical violations can occur from failing to adhere to conditions that typically would not incur a prison term, such as not completing substance abuse treatment or possessing a firearm. Evidence suggests that nationally, over a quarter of people released from prison onto parole end up back in prison as a result of a technical violation (Pew Center on the States 2011).

While many aspects of imprisonment are the same across the country, there is a high level of variation in the levels and characteristics of prison admissions and recidivism across and within states. Laws that determine sentences to state prisons and to some extent, what constitutes a crime, are set by individual state legislatures. For example, the dollar value of what constitutes felony theft, which is punishable by incarceration, varies highly across states; Arizona's felony theft threshold is $1,000 (ARS §13-1802), while Wisconsin has set it at $2,500 (Wis. Stat. §943.20(3)(a)). Some states, like Washington State, have abolished parole and other states have laws that give more or less discretion to the parole board to determine whether a person is released and returned to prison (Klingele 2013). Within states, county prosecutors and judges, who are usually elected, determine the charge and where the statute permits judicial discretion, the sentence. Some within-state jurisdictions may also offer alternative sentences to confinement that are not offered elsewhere in the state (Office of Justice Programs 2020).

Even though recidivism appears to be a straightforward phenomenon, measurement poses many challenges to researchers. Measuring recidivism requires the data to be longitudinal and cover a long period of time, particularly to investigate the phenomenon of cycling through prison. Many studies of recidivism measure the number of people who have returned to prison after some specified period of time since release (e.g., Alper, Durose, and Markman 2018; Durose et al. 2014). This glosses over a number of important questions, such as whether risk differs by a person's prior contact with the criminal justice system. In addition, much of the literature focuses on the individual-level correlates of recidivism and uses data from a single city or state (e.g., Bales and Mears 2008; Spivak and Damphousse 2006; Wehrman 2010). While these studies are important, penal regimes and criminal justice policy environments vary widely across the country. Without accurate large-scale data across many jurisdictions, it is difficult to understand the relationship between imprisonment and factors at the institution level, such as state sentencing laws, and the individual level, such as race and economic status.

It is crucial for scholars to accurately portray who goes to prison, who returns, and under what conditions, because policymakers use this information to pass policies that lead to more or less racial and economic disparities in the prison population. Without considering all aspects of recidivism, researchers may misrepresent who is at highest and lowest risk and what drives these differences. The focus on individual characteristics without a careful treatment of the social, legal, and political context to which people reenter post-release has led to a further criminalization of people of color and beliefs about the impossibility of rehabilitation (Lipton, Martinson, and Wilks 1975; Martinson 1974; Robison and Smith 1971; Von Hirsch 1976).

This dissertation adds to the literature on predictors of prison admission and recidivism. I directly engage with the relationship between individual-level characteristics such as race and socioeconomic disadvantage and macro-level factors such as criminal justice policies and characteristics of the return environment. Investigating this relationship can reveal how racial disparities in imprisonment are tied to institutions and not just due to individual behavior. The field of criminal justice research has become ever more present in public discourse with people all across the country scrutinizing policies that may exacerbate racial disparities. As a result, researchers should consider a wide array of factors associated with prison admissions.

This dissertation aims to highlight the variation in demographic and policy contexts across the country and how they are associated with who is in prison. To that end I examine these conceptual and empirical issues in three studies that investigate prison admissions at different geographic and policy scopes. The dissertation begins with a broad overview of racial disparities in prison recidivism and its relationship to jurisdictional variation with an 18-state study of the interaction between racial composition and individual race on recidivism. From there, it narrows its focus to a comparative case study of two states, Pennsylvania and New York, and the specific pathways of returning to prison through parole revocations. The dissertation ends with a study that dives more deeply through an examination of the impact of one policy in one state, drug courts in Georgia.

The three studies make use of a unique data set that includes all prison admissions and releases to state prison across a large number of states. A key feature of the Bureau of Justice Statistics' National Corrections Reporting Program (NCRP) is that it includes individual-level identifiers that link a person's prison record over time. Many states have data for individuals between 2000 and 2016, with Georgia providing data as far back to 1971. These data have only become available to outside researchers in the last ten years and new data are added every year. I obtained access to the restricted linked data through a data use agreement with the National Archive of Criminal Justice Data at the Interuniversity Consortium for Political and Social Research. The NCRP also includes parole data for some states, which can be stacked with the prison data to observe an individual's flow from prison to parole and back to prison if they recidivate. Few researchers have made use of the parole data.

Chapter 1. The Interaction of Race and County-Level Characteristics on Racial Disparities in Recidivism: A Test of Minority Threat and Political Representation

The first chapter examines how county-level racial composition affects racial disparities in returns to prison. Racial threat theory posits that as the relative size of the nonwhite population increases, perceived threat to White political and economic dominance results in a backlash that manifests in increased control of the nonwhite population and individuals. On the other hand, a larger nonwhite population results in increased political representation that pushes elected officials to reduce punitiveness and racial disparities. Existing criminological research has found significant relationships in both directions between the relative size of nonwhite populations and punitiveness and racial disparities. While many scholars have investigated the relationship between racial composition and phenomena in the criminal justice sphere, no studies to date have looked at the outcome of returns to prison (Bridges and Crutchfield 1987; Hawkins and Hardy 1989; Jacobs and Carmichael 2001; Yates and Fording 2005).

To fill this gap in the literature, I utilize event history analyses to estimate the hazard of recidivism. Unlike other studies, I correct for the correlation within individuals who appear in the data multiple times. In addition, I examine both the relative size of the non-Hispanic Black population as well as the Hispanic population, which only recent studies have begun to do (Chiricos, McEntire, and Gertz 2001; Eitle and Taylor 2008; Jacobs and Carmichael 2001).

Findings suggest the relationship between racial composition and the risk of returning to prison is small in magnitude. I find that a ten percent increase in the non-Hispanic Black population is associated with an overall one percent decrease in the hazard of recidivism. This ten percent increase is also associated with a small decrease in the hazard of recidivism for non-Hispanic Blacks compared to non-Hispanic Whites, two percent for every ten percent increase. A ten percent increase in the Hispanic population corresponds to a 0.41 percent decrease in the risk of returning to prison. These findings are consistent with the explanation that a larger nonwhite political representation reduces overall recidivism and racial disparities. It appears that the classical racial threat theory does not have as much power predicting outcomes for recidivism disparities among Whites and Blacks. However, I also find a positive association for larger Hispanic populations and for being Hispanic relative to being White. The magnitude is again small – a ten percent increase in the Hispanic population is associated with a 0.42 percent increase in the hazard of recidivism for Hispanics compared to non-Hispanic Whites. This is consistent with the hypothesis that a larger nonwhite population poses a perception of racial threat, resulting in racial disparities that negatively impact nonwhites.

CHAPTER 2. REPEATED RETURNS TO PRISON FOR PAROLE VIOLATIONS

The second chapter focuses on parole revocations. Roughly 875,000 people were on parole at year-end 2016 and roughly 105,000 people ended their state parole terms with a revocation in 2016 (Kaeble 2018a). This chapter asks three inter-related questions: Do the recidivism outcomes of people released to parole in New York differ from the outcomes of people on parole in Pennsylvania? Is there a relationship between experiences of prior parole terms and future recidivism outcomes? Does this relationship between prior experiences and recidivism look different for people on parole in New York versus Pennsylvania? Analyses of NCRP data also distinguish whether a new crime or technical violation led to the return to prison. I describe the similarities and differences across these geographically adjacent states with respect to the implementation and policies regarding parole. Directly comparing jurisdictions can reveal how institutional context and policy can shape the outcomes of individuals.

As expected, I find that the number of prior parole episodes and the state of jurisdiction are significantly associated with the likelihood and timing of returning to prison and revocation type. The results suggest that there is a relationship between prior experiences of parole and future recidivism and that this relationship differs across the two states. When considering the likelihood of experiencing a revocation versus completing parole, in both states, having been on parole previously increases the risk of revocation. I further find that people in Pennsylvania have a higher risk of revocation than people in New York, but this difference is only discernable among people on their first parole term. In both states, people return to prison for a technical violation revocation more quickly than for revocations with a new conviction. Further, having been on parole previously increases the risk of experiencing a parole revocation for a technical violation, but only in New York.

Chapter 3. Who is Excluded? Effect of Drug Courts on Composition of Georgia's Drug Possession Prison Admissions

In the last chapter, I estimate the effect of drug courts in Georgia to drill down on prison admissions within one state. Drug courts provide an alternative to prison, but may come with financial barriers to participation. If people who cannot afford to participate are excluded from access to this process of staying out of prison, it may lead to an overall increase in the level of economic disadvantage among people sent to prison for drug possession. Thus, whether individuals charged with drug possession can remain out of prison in exchange for going through a court-supervised treatment program may be shaped by ability to pay fees. Empirical work examines whether the high costs of drug court participation result in a population of people admitted to prison for drug possession that is more economically disadvantaged than would have been the case in the absence of a drug court. I use educational attainment as a proxy for economic disadvantage and employ a difference-in-differences strategy to exploit variation in the timing of drug court implementation between 1991 and 2016 across the state of Georgia. Georgia imposed prison sentences for drug possession during this period, including for marijuana.

On average, the percentage of people admitted to prison for drug possession who have less than a high school degree or GED did not increase significantly after drug court implementation. Supplementary analyses implement Callaway and Sant'Anna's (2019) method of calculating "group-time average treatment effects" to address the criticisms of the traditional two-way fixed effects approach as well as to address potential issues with meeting the parallel trends assumption. Results using this method generally find a null effect. Just one analysis finds the expected positive association, that drug court implementation led to a 6.23 unit increase in the percentage of people admitted to prison for drug possession who have less than a high school degree. While suggestive, this analysis does not address whether the parallel trends assumption is reasonable to infer.

Implications for Research and Policy

This dissertation contributes to the literature on the individual-level and macro-level predictors of prison admissions and recidivism. It lays the groundwork to push the field to consider how these factors interact to affect racial and economic disparities among people in prison. Analyses focus on the interaction between the macro-level factors of county racial composition and sentencing and parole systems, and individual-level factors of race and prior criminal justice contact. It also uses the case of drug courts to examine whether even well-intentioned programs may contribute to economic disparities among people in prison by excluding more disadvantaged people. The studies highlight in three different contexts how important it is to scrutinize the policies and institutions that result in the inequities we see in the criminal justice system today. This can help inform policy reforms that seek to address structural racism in the criminal justice system.

Findings underscore the variation in demographic and policy contexts across the country that are associated with differences in risks of recidivism. Future scholars should be mindful of such variation and be cautious in generalizing their results to speak about how the criminal justice system operates in the country at large. More qualitative and quantitative work examining the causal mechanisms behind the trends this dissertation describes will be key. Furthermore, these studies show three ways of utilizing a large longitudinal administrative data set to answer very different research questions. Despite its limitations, the NCRP is a rich source of large-scale, longitudinal data that is able to answer many more important questions. As criminal justice data availability grows and becomes more comprehensive, it will open the door for researchers to investigate many policies that result in admissions to prison as well as cycling through the system.

In identifying future areas of inquiry, it will be crucial to focus attention on policy levers that may mitigate or exacerbate the racial and economic disparities we see today. One area that is ripe for investigation is the role of parole and probation. The parole population has been steadily increasing for decades even as the prison population has been decreasing (Carson 2020; Kaeble 2018a). An estimated 3.5 million people are on probation at any one time, meaning that roughly one in 55 adults in the U.S. is under community supervision (Kaeble 2018a). Given what Chapter Two shows about the risk of revocation while on community supervision, it will be important to examine how states across the country are implementing these other forms of surveillance and what consequences this may pose for reducing the prison population.

Programs like drug court that purport to be a way to reduce the number of people in prison while offering a therapeutic form of rehabilitation may in the end just magnify existing discrimination in the criminal justice system. Such programs entail higher levels of supervision and court-ordered sanctions including incarceration (Belenko 1998; Gottfredson et al. 2006), and often impose large financial costs on individuals. Program evaluations of what works in criminal justice reform to reduce the prison population need to be mindful of outcomes beyond the obvious, such as commonly used measures of cost-effectiveness or the three-year recidivism rate. Instead, they should consider who programs work for and if they may be producing more racial and economic disparities.

In addition, this dissertation shows the important role of macro-level context, whether at the state or county level. This underscores that it is imperative to consider how the adoption of policies and practices that have one set of results in one jurisdiction may result in unintended consequences in another jurisdiction. Including a process evaluation as part of replicating policies and programs will help decisionmakers better understand how and why policies have certain results and course-correct, especially if outcomes differ from effects in other locales.

Policymakers designing criminal justice reforms need to consider the potential for policies to exacerbate existing racial and economic disparities. Policies that appear to affect people equally may result in more negative outcomes for people who already experience the highest levels of discrimination in the criminal justice system. Chapter Two shows that the extent of a person's prior criminal justice contact interacts with what state they are in with regards to the risk of parole revocations. Thus, proposing policies that focus on prior criminal history are not race-neutral in practice, because structural racism in our criminal justice system has led to people of color, especially Black people, having more extensive records. The ability to reevaluate and adjust policies as necessary will be key to effective long-term change toward healthy communities.

Chapter 1. THE INTERACTION OF RACE AND COUNTY-LEVEL CHARACTERISTICS ON RACIAL DISPARITIES IN RECIDIVISM: A TEST OF RACIAL THREAT AND POLITICAL REPRESENTATION

INTRODUCTION

In 2018, 567,636 people exited state prison (Carson 2020). Yet research suggests that at least a third of these individuals will return to prison (Durose et al. 2014; Rhodes et al. 2016). Recidivism is a phenomenon composed of two parts: relapse into criminal behavior, and receiving new punishment by the criminal justice system for said behavior (Bales et al. 2015). One common operationalization, a return to prison, captures the second aspect of recidivism. While any measure of recidivism, such as rearrest or reconviction, can have detrimental effects, removal from families and social networks for extended periods of time through returns to prison has the potential to be the most disruptive. Spending time in prison can create and exacerbate mental and physical health issues as well as financial difficulties (Binswanger et al. 2007; Lee and Wildeman 2013; Patterson and Wildeman 2015; Turney and Wildeman 2015; Turney et al. 2012; Yi et al. 2017; Zlodre and Fazel 2012). In addition to the effects on families and already disadvantaged communities, policymakers across the aisle have begun searching for ways to address the huge fiscal burden of imprisoning large numbers of people (Takei 2016). Ensuring that people do not return to prison is a goal many stakeholders share, whatever their beliefs on the goals of incarceration may be. Studying who returns to prison and the social contexts to which they return may provide insight into the pathways that lead people to be further entangled in the criminal justice system.

While recidivism has long been the subject of many studies, scholars typically focus on how individual-level characteristics, such as sex, offense type, age, and race, affect recidivism; far fewer studies focus on how individual recidivism outcomes may differ across demographic contexts. Many studies find large racial disparities, with Blacks, Hispanics, and Native Americans consistently experiencing higher hazards of reimprisonment than Whites (Hepburn and Albonetti 1994; Land, Mccall, and Parker 1994; Spivak and Damphousse 2006; Veeh et al. 2018). Yet studies have also long shown that differential involvement in crime cannot fully explain racial disparities in imprisonment, suggesting that larger structural factors may be at play (Bridges and Crutchfield 1987; Bowker 1981; W Nagel 1976; J Nagel 1982; Yates and Fording 2005). While understanding how individual-level factors affect recidivism is an important avenue of research, prisons are not releasing individuals into a vacuum. People encounter a variety of demographic conditions that interact with their individual histories and characteristics in ways that may affect their future contact with the criminal justice system. This study specifically examines the interaction between county racial composition and individual race.

Two competing explanations predominate in the literature on the relationship between racial composition and racial disparities in the criminal justice system: racial threat and political representation. Racial threat describes how the presence of a nonwhite racial minority group and economic deprivation activate White fear of competition and the perceived threat to White political and economic dominance in ways that mobilize social control (Blalock 1957). Social control refers to the formal (e.g. laws and legal penalties) and informal (e.g., social pressure) mechanisms that establish and enforce behavior through individuals and institutions in service of maintaining social order and cohesion (Carmichael 2014). Scholars have tested racial threat theory using racial composition to explain criminological phenomena such as racial disparities in imprisonment and sentencing severity. The empirical tests of racial threat in this literature have produced statistically significant results in both directions (Baumer, Messner, and Rosenfeld 2003; Bellair and Kowalski

2011; Bridges and Crutchfield 1987; Christianson 1982; Hawkins and Hardy 1989; Jacobs and Carmichael 2002; King and Wheelock 2007; Liska, Chamlin, and Reed 1985; Liska, Lawrence, and Benson 1981; Liska, Lawrence, and Sanchirico 1982; Quillian and Pager 2001; Western 2006; Yates and Fording 2005). This suggests that this theory may not be as useful in explaining phenomena in the criminal justice system as once thought.

Another explanation of the relationship between racial composition and imprisonment disparities considers the potential role of political representation. Here, it is hypothesized that communities with larger nonwhite populations also have larger numbers of local political actors who are nonwhite. Some research suggests that Black elected officials are more likely to be liberal and respond to their Black constituents (Fording 2003; Saltzstein 1989; Welch, Spohn, and Gruhl 1985), and that as the Black population increases, overall imprisonment and sentencing severity as well as racial disparities are reduced (Keen and Jacobs 2009; Yates and Fording 2005).

The literature suggests that hypotheses from either a racial threat or political representation perspective may explain phenomena such as differential imprisonment rates. However, studies have not examined these two explanations in relation to returns to prison, even though such mechanisms could plausibly affect recidivism. Scholars should look beyond the existing focus of individual characteristics and consider how these predictors of prison recidivism may vary across social contexts. Expanding the focus of recidivism research may contribute to better understanding how the criminal justice system exacerbates existing racial disparities.

This paper examines the role of the interaction between individual race and county racial composition to explain recidivism trends over time using prison data from 18 states from the National Corrections Reporting Program. Of critical importance, unlike existing studies, these data identify when individuals appear in the data more than once and thus permit more accurate portrayal of recidivism than other studies. Using these data, I examine the hazard of returning to prison for individuals who first left prison in 2004, focusing on interactions between individual race and county-level measures of non-Hispanic Black and Hispanic population percentages. I find that as the percent of a county's population that is Hispanic increases, there is an increase in the hazard of recidivism only for Hispanics, which is consistent with racial threat theory, but the magnitude of the association is very small. The study also finds results consistent with a political representation explanation, but the size of the estimate is also very small – a one-unit increase in the percent of the population that is non-Hispanic Black is associated with a 0.2 percent decrease in the hazard of recidivism for non-Hispanic Blacks.

This study expands the recent trend of interacting individual and ecological variables. It seeks to investigate whether the relationship between county-level racial composition interacts with an individual's race to affect an individual's hazard of returning to prison. To my knowledge, no one has examined the relationship between racial threat and returns to prison, although it is reasonable to expect that nonwhite people who have already had experience in the prison system would be considered particularly threatening and a target for increased social control.

The paper proceeds as follows. The second section presents the literature review on recidivism and the two hypotheses tested in this paper. Section three describes the data and measures and the subsequent section covers the results of the analysis. The last section concludes by discussing policy implications and avenues for future research.

LITERATURE REVIEW

Reentry from prison back into society is challenging, especially so for those grappling with the barriers created by racial and economic discrimination, low educational attainment, lack of stable formal work history, housing instability, and substance abuse issues (Visher, La Vigne, and Travis

2004). The difficulties stemming from these challenges can lead to cycles of rearrest, reconviction, and reimprisonment (Alper et al. 2018). Returning to prison is unfortunately a common experience, with research suggesting that at least a third of people leaving prison will return within five years, and over a third of those individuals will wind up back in prison at least one more time (Durose et al. 2014; Rhodes et al. 2016). While rates of prison returns typically flatten after about five years, one study found that 24 percent of people released from prison in 2005 experienced an arrest even nine years post-release (Alper et al. 2018). Escaping contact with the criminal justice system is difficult for many people who leave prison.

Many factors shape recidivism, and like the many factors that predict prison admissions in general, a combination of micro- and macro-level factors are at play. Scholars have long noted that individual-level factors such as race and sex are associated with imprisonment and recidivism. While the imprisonment rate for Black people has been decreasing since 2009, Black males continue to be overrepresented in prison, as has been the case for over a century (Cahalan and Parsons 1986; Carson 2020). Hispanics also have a higher imprisonment and post-prison rearrest rate than do non-Hispanic Whites, although both rates are closer to those of non-Hispanic Whites than to those of non-Hispanic Blacks (Alper et al. 2018; Carson 2020; Durose et al. 2014). The few studies that distinguish Native Americans from the "Other" race category have found that their rearrest rate and level of contact with the criminal justice system look similar to that of Black individuals (Alper et al. 2018; Janisch 2014).

Research suggests that differential involvement in crime does not fully explain racial disparities in imprisonment (Bridges and Crutchfield 1987; Bowker 1981; W Nagel 1976; J Nagel 1982; Yates and Fording 2005). In spite of this finding, the post-release rearrest rate for Black individuals is the highest, while that of White individuals is the lowest, suggesting that something else is at play (Alper et al. 2018). Studies suggest that while racial discrimination may play a smaller role in arrests for serious violent offenses, it plays a significant role in differential policing and arrest rates for more minor crimes, such as drug crimes (Beckett, Nyrop, and Pfingst 2006; Brown and Frank 2005; Tonry 1996). Racial disparities in arrest rates are exacerbated by the fact that Black and Hispanic defendants' cases are more likely to move forward without dismissal at each step of the criminal case process, meaning the difference in rearrest rates translates to further disparate prison readmission rates (Kutateladze et al. 2014; Swigert and Farrell 1977). Differential arrest rates notwithstanding, the difference in the racial distribution of people arrested and imprisoned suggests that structural racism continues to operate throughout the criminal justice system. Courtrooms may be an example of how organizations reproduce and are created by racial processes that shape institutional racism and individual prejudice (Ray 2019).

In addition to race, offense type and sentence length are important factors to consider when studying recidivism on the micro and macro levels. Approximately 56 percent of people in state prison are serving time for violent crimes, which come with long sentences (Carson 2020). This means they are generally underrepresented in recidivism data compared to people who are in prison for short periods and therefore may be observed returning to prison and released again even without a very long window of observation. People who served their prison sentence for a property offense have the highest rearrest rate, followed by drug offenses, public order offenses, and violent offenses (Alper et al. 2018; Durose et al. 2014).

Although sentence lengths for violent offenses have increased in essentially every state in the last forty years, some states impose longer sentences than others (Gilpin 2012). Concerns over the recidivism rate of violent offenders led to the development of truth-in-sentencing (TIS) laws, which require an individual to serve a substantial portion of their sentence in prison, restricting the possibility of early release (Ditton and James 1999; Gilpin 2012). In 1994, the federal government adopted TIS and Congress authorized funding of the Violent Offender Incarceration and Truth-in-Sentencing incentive grants to provide additional correctional funding to states that require that people imprisoned for violent offenses serve at least 85 percent of their sentences before becoming eligible for release (Ditton and James 1999).

Past violent offenses can also lead to longer sentences on a future offense in states with habitual offender laws, also known as three-strikes laws. States that adopted habitual offender laws earlier, in the 1990s, tended to be more politically conservative, although liberal states were the first to implement it (Washington State and California) (Karch and Cravens 2014). Although there is a wide level of variation in how these laws are implemented across states, most states have some form of a habitual offender law that imposes long sentences, in many cases a mandatory life sentence with no possibility of parole, for a third, and in some states second, "strikeable" felony (Austin et al. 2000). Felonies that qualify are usually violent offenses, but that is not the case in every state. Given the fact that racial discrimination has led to Black and Hispanic people having higher levels of contact with the criminal justice system before the advent of this policy, this law disproportionately applies high probabilities of long sentences to these groups.

The focus on individual characteristics has left a gap in the literature as to how these types of macro-level factors affect racial disparities in recidivism. Recent studies have begun to fill this gap (Bellair and Kowalski 2011; Kubrin and Stewart 2006; Mears et al. 2008; Reisig et al. 2007), but it remains an open question as to how social context might interact with and moderate the effect of individual characteristics on recidivism. Few studies have directly looked at the relationship between racial composition and individual-level race and recidivism. A notable exception is St.

John's (2019) study, which found an unexpected negative relationship between nonwhite population size and odds of felonious rearrest among Black probationers.

There are two primary theoretical explanations for the association between the size of a county's nonwhite population and racial disparities in the criminal justice system. The first comes from racial threat theory, derived from social conflict theory. Social conflict theory is a Marxist-based theory that posits that society is in a constant state of conflict due to competition for limited resources and that those in power maintain social order by suppressing less powerful groups. Racial threat describes how the presence of a racial minority group and relative economic deprivation have positive associations with fear of competition among Whites and the perceived threat to White political and economic dominance (Blalock 1957, 1967; Blumer 1958; Bobo and Hutchings 1996; Brown and Fuguitt 1972; Frisbie and Neidert 1977). These theories suggest that this perceived threat can mobilize or enhance social control of racial groups with less power and lead to forms of discrimination such as restricted political rights for nonwhites, and symbolic forms of segregation. While perceived threat to political and economic domination can take many forms, one operationalization is the percentage of the population that is nonwhite (e.g., Bridges and Crutchfield 1987; Keen and Jacobs 2009).

Although early theorists such as Blalock (1967) did not specify the application of the racial threat theory to the criminal justice system, I follow existing criminological research suggesting that racial disparities in the criminal justice system reflect the dynamic of perceived threat to white dominance mobilizing increased social control of nonwhites (Bellair and Kowalski 2011; Bridges and Crutchfield 1987; Golden 2012; King and Wheelock 2007; Liska et al. 1982; Mears, Wang, and Bales 2014; Quillian and Pager 2001, 2010; Whittle and Parker 2014). Although this work has mostly focused on Black populations, scholars have recently begun to examine Hispanic populations. Some research has found that larger Hispanic populations result in increased fear of victimization among Whites (Chiricos, McEntire, and Gertz 2001; Eitle and Taylor 2008), and to some extent, a higher imprisonment rate (Jacobs and Carmichael 2001). However, some research has found different effect sizes of percent Black compared to percent Hispanic (smaller and not statistically significant) on outcomes such as length of prison sentences (Feldmeyer et al. 2015), restrictiveness of state collateral sanctions (Whittle and Parker 2014), and White attitudes toward racial prejudice (Taylor 1998).

Tests of racial threat theory in the criminal justice system have shown conflicting results. Most studies where the dependent variable is fear of crime or outcome severity (e.g., death penalty legalization) have found support for the theory (Baumer et al. 2003; Bellair and Kowalski 2011; Jacobs and Carmichael 2002; King and Wheelock 2007; Liska et al. 1985, 1981, 1982; Quillian and Pager 2001; Western 2006). Yet, some studies examining the relationship between racial disparities in prison admissions and the size of the Black population have found an unexpected significant negative association (Bridges and Crutchfield 1987; Christianson 1982; Hawkins and Hardy 1989; Yates and Fording 2005).

One aspect of racial threat is that individuals, as well as communities as a whole, may be perceived as threatening (Albonetti 1991; Caravelis, Chiricos, and Bales 2011; Hawkins 1981). Thus, in addition to increased levels of social control generally, the theory predicts that the negative consequences of a larger nonwhite population will be concentrated on defendants who are nonwhite. This leads to the first hypothesis:

Hypothesis 1: Black and Hispanic individuals released to counties with higher percentages of Black or Hispanic populations will have a higher hazard of reimprisonment compared to non-Hispanic White individuals.

The conflicting findings in the literature present the opportunity to explore other hypotheses to understand the relationship between county-level demographic characteristics and racial disparities in the criminal justice system. An important competing explanation suggests that a larger nonwhite population results in more social and political power for the nonwhite group. When elected officials need to be more responsive to their nonwhite constituency, they should be more likely to adopt and enforce processes and policies targeting racial disparities in the criminal justice system. Research suggests that jurisdictions with more nonwhite representation in government may have more favorable outcomes for issues that disproportionately affect nonwhites, including in areas of criminal justice, as these officials are more likely to be dependent on nonwhite votes (Herring 1990; Jacobs and O'Brien 1998; Myers 1987; Saltzstein 1989; Welch et al. 1985; Yates and Fording 2005). Thus, areas with larger nonwhite populations should have policies that reduce the level of control and racial disparities exerted by the criminal justice system. The competing hypothesis is:

Hypothesis 2: Released individuals in counties with higher percentages of Black or Hispanic populations will have a lower hazard of reimprisonment, and these decreases will be larger for Black and Hispanic individuals.

Scholars have used racial threat theory to explain social phenomena for decades. However, research testing hypotheses derived from this theory suggest that it is not as useful to explaining criminal justice outcomes. Examining other explanations may provide the foundation for identifying and testing other causal mechanisms in future research.

In order to examine the relationship between macro-level racial composition and racial disparities in recidivism, the data must include counties with variation in racial composition and a long enough window of observation to measure returns to prison. Interacting individual-level race and county-level racial composition data to predict increases or decreases in the risk of individual-level recidivism will test these two competing hypotheses. I take advantage of a unique large-scale data set covering prison admissions and releases in 18 states.

DATA AND MEASURES

The prison term-level data for these analyses come from the Bureau of Justice Statistics' National Corrections Reporting Program, 2000-2016 (NCRP) (Bureau of Justice Statistics 2019). State prisons annually report data on every admission and release, with individual unique identifiers so researchers can link the data across time. The linked data permit longitudinal examination of recidivism over a long period (see Luallen et al. (2014) for details of the linking algorithm method). Survival analyses of recidivism typically observe individuals until they recidivate or remain out of prison throughout the window of observation. However, in the NCRP, individuals appear in the data every time they return to prison, which may occur more than once. As we might expect that reentry will look different after the first release from prison and subsequent times, it is necessary to take such correlations into account.

Eighteen states provided data to the NCRP between at least 1995 through 2016, with most states providing data prior to that year. Figure 1.1 presents the location of the 18 states. Although the data do not cover every state, the 18 states represent a variety of geographic regions, penal regimes, and political leanings.

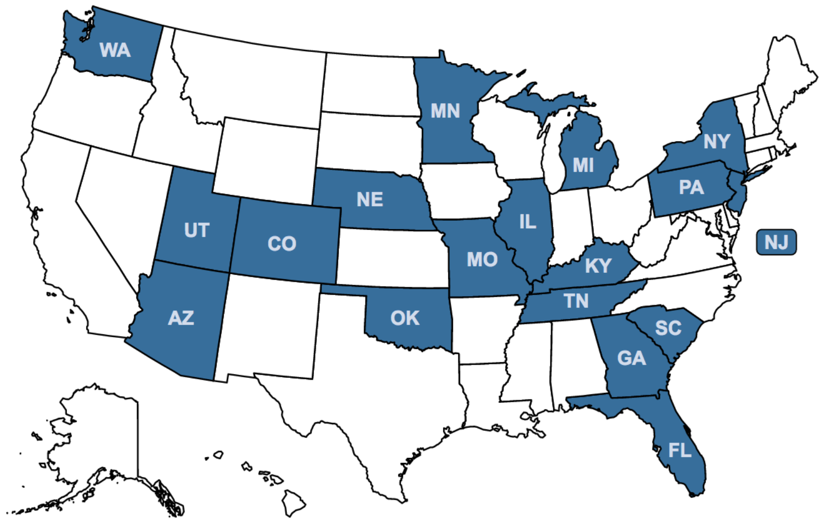

Figure 1.1. States included in analyses.

Error! Reference source not found. shows the years of data that each of the 18 states contributed. These states also provided an indicator of the county of the court that imposed the prison sentence, i.e., the county of jurisdiction. Although this indicator is not necessarily the county of the individual's residence prior to imprisonment, I use this as a proxy for the county in which an individual lives post-release. Given that community supervision programs typically geographically constrain where individuals can live, this assumption may be appropriate. I discuss this issue further in the conclusion.

Table 1.1. States with Data between 1995-2016

State	Release Years in Data
Arizona	1994-2016
Colorado	1989-2016
Florida	1995-2016
Georgia	1971-2016
Illinois	1989-2016
Kentucky	1989-2016
Michigan	1989-2016
Minnesota	1991-2016
Missouri	1990-2016
Nebraska	1994-2016
New Jersey	1995-2016
New York	1994-2016
Oklahoma	1990-2016
Pennsylvania	1990-2016
South Carolina	1994-2016
Tennessee	1994-2016
Utah	1993-2016
Washington	1989-2016

Source: National Corrections Reporting Program, 2000-2016

The sample includes only individuals who exited prison for the first time in 2004 because an individual's prison readmission history is a focus of this study. It is not possible to determine whether it is an individual's first term in prison with absolute certainty. As a result, I take several steps to identify first-time prison releases to the extent the data allow. I use an eight-year cut off rule to address this issue. If the data do not show an individual experiencing a prison release before January 1, 2004, then I assume that the 2004 observation is the individual's first time in prison in that state. Although the NCRP's data managers suggest that a four-year cut off is sufficient, one study looking at repeated returns shows that the difference in prison return rate between four and eight years is about five to six percent (Rhodes et al. 2016) and other research using the NCRP suggests that the median length of time served before initial release is between 1.1 and 1.3 years (Durose et al. 2014; Kaeble 2018b; Langan and Levin 2002). Thus, an eight-year rule should capture the vast majority of first prison terms. For the 16 states that have data before 1995, I am able to be even more certain that I am capturing a first term in prison. Thus, the follow-up period is the thirteen years from 2004 through 2016.

All initial prison releases occurred in 2004, but during the thirteen-year period of observation some individuals returned to prison, then exited prison and recidivated again. As noted above, recidivism in this study is defined as a return to prison. For some individuals, this cycle occurred multiple times. Thus, the outcome measure is the duration between each prison release and return measured in days. Given the phenomenon of cycling, although the data include 258,512 prison terms, it comprises only 142,121 individuals. A key independent variable of interest in these data is an individual-level four-category race measure: White, Black, Native American, and Hispanic.[1] Other individual-level covariates are sex, conviction offense category of the last prison sentence (violent, property, drug, public order, or unspecified), age at prison admission (under 18, 18 to 24, 25-39, 40-64, and 65-100), and a continuous variable measuring number of days served in custody of the last prison sentence. Previous research suggests the likelihood of recidivism is highest for young males convicted of property offenses, but research on the effect of custody length is mixed (Alper et al. 2018; Loughran et al. 2009; Meade et al. 2013; Rydberg and Clark 2016).

Studies of racial threat commonly operationalize macro-level indicators at the county or state level. Given that this study is examining readmissions to state prison and states establish

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[1] The method of collecting the race and ethnicity variables can differ by prison: self-report, prison officials' visual determination, or reliance on court records. In the four-category race measure, "Hispanic" refers to individuals identified as Hispanic, regardless of response in race item. "White", "Black", and "Native American" refer to individuals identified as non-Hispanic or Hispanic ethnicity unknown. This definition of Hispanic as a race follows Bureau of Justice Statistic convention. "Native American" refers to the Census Bureau's American Indian/Alaskan Native category. All other groups summed to less than 0.6 percent of the sample and are not included. This includes Asians, Native Hawaiians/Pacific Islanders, Multiracial, and Other/Unknown.

sentencing policies, one could make the argument that a state-level analysis would be most appropriate (Keen and Jacobs 2009). For example, studying the effect of racial threat on the legality of the death penalty clearly calls for a state-level analysis (Jacobs and Carmichael 2002). However, decision-makers at lower-levels of government have a high level of discretion and influence the process by which an individual might return to prison. Courtroom actors (e.g., county prosecutor, judge) are likely to be sensitive to the context in which they work, and racial composition varies highly within a state. While recently released individuals may search for employment or other resources outside their neighborhoods, it is less likely that they would travel extremely far to access these resources (Hipp, Petersilia, and Turner 2010). Furthermore, in testing Hypothesis 2 from the political representation explanation, county-level measures are more appropriate because prosecutors and judges are elected at the county level. Such individuals have enormous power and are elected in most states, suggesting that their decisions are partly a reflection of local voters' views on race, crime, criminality, and the purpose of the criminal justice system.

The county-level data characterize contextual factors at the year that an individual reenters into the community. The annual non-Hispanic Black and Hispanic-only population percentages come from the U.S. Census Bureau. The Hispanic population percentage is logged to correct for the right skewed distribution of the measure.

To examine how racial composition interacts with an individual's race to affect an individual's hazard of returning to prison, I follow St. John's (2019) study and include interaction terms between individual race and the two county-level racial composition measures.

1. Percent of county population non-Hispanic Black from the U.S. Census Bureau
2. Percent of county population Hispanic logged from the U.S. Census Bureau

A positive coefficient on the interaction between the county-level percent Black and individual-level Black variable would suggest that as the relative size of the Black population increases, the risk of recidivism among Blacks relative to Whites increases, which would be consistent with Hypothesis 1, the racial threat explanation. A negative coefficient would be consistent with Hypothesis 2, the political representation explanation.

Many studies include an indicator for Deep South or Southern states in their models (Carroll and Doubet 1983; Keen and Jacobs 2009; Kleck 1981; Liska et al. 1981; Taylor 1998; Welch et al. 1985). This avoids issues of overestimating the direct effect of percent Black on the hazard of recidivism and Southern states tend to have higher imprisonment rates overall (Bronson and Carson 2019; Carson 2015; Harrison and Beck 2005; West, Sabol, and Greenman 2010). I also included an indicator for Deep South states in my sample, which include Florida, Georgia, South Carolina, and Tennessee.

Some studies have suggested that the relationship between relative nonwhite population size and racial disparities in criminal justice outcomes is curvilinear, combining the two hypotheses in this study: racial threat applies until the nonwhite population reaches a threshold size, after which the nonwhite group possesses enough electoral power (Keen and Jacobs 2009; Whittle and Parker 2014). However, collinearity between the linear and squared measures of percent non-Hispanic Black population (0.94) prevented estimation of this model. The same occurred with the linear and squared measures of percent Hispanic population (0.96).

METHODS

I utilize event history analysis (also known as survival analysis) to estimate the hazard of returning to prison.[2] The use of event history methods in modeling recidivism dates back to the late 1960s and the methods needed to capture the complex nature of recidivism have vastly improved since that time (see Chung, Schmidt, and Witte 1991 for a review of event history methods in recidivism research). Researchers have commonly modeled recidivism using Cox Proportional Hazards models (Cox 1972). However, the Cox model's assumptions do not hold with data where an individual may experience the event of interest multiple times. Given that over 20 percent of the individuals in the sample recidivated more than once (see summary statistics in Table 1.2), the assumption of independence of events does not hold in this case. To account for this, I use a conditional gap time model, which is an extension of the Cox Proportional Hazard model (Cox 1972). Conditional models use a variance-correction approach to account for the fact that the observations are not independent (i.e., correlated at the individual level). Prentice, Williams, and Peterson (1981) proposed conditional models to analyze ordered multiple events and gap time models to "reset the clock" back to zero after each event, which accounts for the sequential development of risk (Lipschutz and Snapinn 1997). I collapse the higher strata, five prison terms and above, into one because the small number of observations in these strata produce unstable estimates and collapsing allows for the preservation of all observations (Box-Steffensmeier and Jones 2011).

The Cox model takes the form of Equation 1.1:

$$h(X_i) = h_0(t)\, exp\, (\beta_x X_i) \tag{1.1}$$

[2] While logit or probit models are more appropriate for estimating the likelihood of recidivism with small samples of data, it is more efficient to use event history methods to estimate the time to recidivating event with large samples.

where $h(X_i)$ is the hazard rate of returning to prison for the ith subject, $h_0(t)$ is the baseline hazard rate (which Cox models do not estimate), and β_x reflects a vector of regression coefficients that when exponentiated $exp\ (\beta_x X_i)$ convert to hazard ratios. X_i is a vector of covariates. The analysis includes two models. One baseline model with no interaction terms and a second model that adds the interactions between individual race and Black and Hispanic population percentages. If the log hazard rate coefficients of the interaction terms are statistically significant and positive, it would be consistent with Hypothesis 1, the racial threat perspective, while negative coefficients would be consistent with Hypothesis 2, the political representation explanation. If the main effect of the Black and Hispanic population percentages in the baseline model is negative while the interaction terms in Model 2 are not statistically significant, this may suggest some support for the hypothesis that nonwhite elected officials may promote policies that reduce the negative impacts of the criminal justice system, but do not reduce existing racial disparities.

RESULTS

Error! Reference source not found. presents descriptive statistics for the NCRP data I use to examine the relationship between individual race, county-level racial composition, and time to prison recidivism. It shows statistics for the total observations and the individuals in the sample. Whites and Hispanics made up a larger percentage of individuals than observations in the data, meaning Whites and Hispanics as a whole recidivated fewer times than Blacks and Native Americans. As **Error! Reference source not found.** shows, although over 50 percent of people incarcerated in state prison were convicted of violent offenses, less than a quarter of the observations in this sample carried a violent offense conviction. As noted in the literature review, this is because this study uses a sample of people who were released from prison and thus the study overrepresents individuals convicted of all other types of offenses, which typically carry shorter sentences. The mean of the outcome variable, days to prison return, is much lower for observations that ended in a return to prison (832.20 days) compared to censored observations (3596.00 days). This is unsurprising, as 55.06 percent of individuals did not recidivate at all during the thirteen-year window of observation.

Appendix Table A.1 shows the breakdown of individual maximum prison terms and the other individual-level covariates by race. It shows that Blacks experienced the highest number of prison terms during the thirteen-year window, followed by Native Americans, Whites, and Hispanics. In addition, Black people were overrepresented in younger age categories, followed by Hispanics, Native Americans, and Whites. For the conviction that led to their first prison sentence, Whites were most commonly in prison for a property offense, Blacks and Hispanics for a drug offense, and Native Americans for a public order offense.

Table 1.2. Descriptive Statistics for Number of Prison Terms, Outcome Variable, and Covariates at the Individual-Level and Observation-Level

	Percent Observations	Percent Individuals
Individual max prison terms		
1		55.06
2		23.85
3		11.58
4		5.54
5+		3.97
Race		
White	44.09	45.28
Black	43.17	41.21
Native American	1.54	1.44
Hispanic	11.20	12.08
Male	89.14	87.86
Conviction offense of prison sentence*		
Violent	23.41	26.52
Property	28.95	27.24
Drug	32.30	32.10
Public Order**	15.34	14.15
Age at prison admission*		
<18	0.49	0.88
18-24	24.00	32.28
25-39	50.49	44.83
40-64	24.73	21.68
65-100	0.29	0.33
Total	258512	142500

	Median	Mean	SD
Days in custody of last prison term	366.00	684.30	921.88
Days to prison return	518.00	832.20	855.37
Days to censor	4429.00	3596.00	1410.49

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes:
Race categories refer to individuals identified as non-Hispanic or Hispanic ethnicity unknown unless otherwise noted. "Hispanic" refers to individuals identified as Hispanic, regardless of response in race item. "Native American" refers to the Census Bureau's American Indian/Alaskan Native designation.
* At first prison term admission.
** Examples of public order offenses include weapons, drunk driving, court offenses, commercialized vice, morals and decency charges, and liquor law violations.

Error! Reference source not found. shows the descriptive statistics for the county- and state-level covariates as they appear across observations in the sample. Only four of the 20 states in the sample are Deep South states, but they comprise a little under a third of the observations (see Appendix Table A.2 for state breakdown). However, the Deep South made up a larger percentage of individuals than observations in the data, meaning individuals in Deep South states as a whole recidivated fewer times than individuals in other states. A state qualified as a determinate sentencing state for this study if it primarily used determinate sentencing in 2004 (Lawrence 2015).The vast majority of the states in the sample had truth-in-sentencing laws in 2004 that met the federal 85 percent funding criterion (Ditton and Wilson 1999; O'Hear and Wheelock 2016; Sabol et al. 2002). Ten of the states in this study had a habitual offender law in 2004 (Greenwood et al. 2002; Schiraldi, Colburn, and Lotke 2004).

Table 1.3. Descriptive Statistics for County- and State-Level Covariates

	Median	Mean	SD
% county Non-Hispanic Black population*	10.99	15.10	13.95
% county Hispanic population*	7.39	12.09	11.82
		Percent Observations	Percent Individuals
Deep South state		28.90	31.95
Determinate sentencing state		41.56	42.36
Truth-in-sentencing state		95.82	95.83
Habitual offender law state		57.96	59.16

Source: *U.S. Census Bureau
Notes:
"Hispanic" refers to population identified as Hispanic or Latino by the U.S. Census Bureau, regardless of response in race item. Population percentages refer to the estimate of the year the individual was released from prison in their county of jurisdiction.
Sentencing variables refer to whether a state had the sentencing system in place in 2004.

Error! Reference source not found. presents results from the two Cox proportional hazards models, which examine differences in the number of days until returning to prison across individual-level race and county-level racial composition measures.[3] Model 1, the baseline model,

[3] Appendix Table A.3 presents the same results as log hazard rates, as the exponentiated hazard ratios of interaction terms shown in **Error! Reference source not found.** cannot be added together and the size of hazard ratios of logged covariates are not intuitively interpretable.

includes the individual-level and county- and state-level covariates without the interactions. The directions of the individual-level covariates are generally as expected. Younger age groups and people convicted of property offenses have higher risks of returning to prison compared to older age groups and people convicted of other offenses. Holding all else constant, males have a 34.5 percent higher risk of returning to prison compared to females. Of the sentencing policy variables, the habitual offender covariate has the largest association with recidivism. People in states with habitual offender laws have a 20.4 percent higher risk of returning to prison compared to states without these laws.

The individual-level race covariates show that Blacks and Native Americans have a 17.3 percent and 10.5 percent higher risk of returning to prison compared to Whites, respectively. Unexpectedly, Hispanics have a 10.2 percent lower risk of returning to prison compared to Whites, and all of these results are statistically significant at the 99.9 percent confidence level. Existing research would predict that Hispanics would have a higher recidivism rate than Whites, although they do have a post-prison rearrest rate that is closer to Whites than to the higher rates that Blacks or Native Americans experience (Alper et al. 2018; Durose et al. 2014). The summary statistics in Appendix Table A.1 preview this finding as it shows that Hispanics experienced the fewest prison terms and 63 percent did not recidivate at all during the thirteen-year window. It is unclear why this might be, as their characteristics across the covariates look closest to that of Blacks.

In Model 1, the county-level covariates are negative and statistically significant at the 99.9 percent confidence level. The Model 1 results on racial composition are consistent with the political representation explanation although the magnitude is quite small. It shows that a ten unit increase in the percent of the population that is Black, and the percent of the population that is Hispanic, is associated with a 1 percent and 0.41 percent decrease in the hazard of recidivism, respectively.[4] However, without the interactions I add in Model 2, it does not test whether increases in county racial composition differentially affect groups by race.

[4] The covariate measuring the size of the Hispanic population is logged. Thus, the hazard rate for a ten percent increase is obtained by exponentiating the product of the coefficient (shown in Appendix Table A.3) and the natural log of 1.10. Increasing the percentage of the Hispanic population does not result in a linear decrease in the hazard of recidivism.

Table 1.4. Results of Prentice-Williams-Peterson Conditional Gap-Time Models Estimating Hazard Ratios of Time to Prison Readmission

	Model 1		Model 2	
	HR	95% CI	HR	95% CI
Prison admit age				
<18	1.240	1.157, 1.330	1.237	1.153, 1.326
25-39	0.733	0.723, 0.743	0.733	0.724, 0.743
40-64	0.521	0.512, 0.530	0.521	0.512, 0.530
65-100	0.201	0.165, 0.245	0.201	0.165, 0.245
Male	1.345	1.318, 1.372	1.345	1.318, 1.372
Race†				
Black	1.176	1.160, 1.192	1.116	1.084, 1.149
Native American	1.093	1.044, 1.144	1.061	0.942, 1.195
Hispanic	0.887	0.867, 0.907	0.643	0.592, 0.698
Conviction offense				
Violent	0.804	0.792, 0.817	0.804	0.791, 0.817
Drug	0.804	0.792, 0.815	0.803	0.791, 0.814
Public Order	0.818	0.804, 0.832	0.818	0.804, 0.832
Days in custody	1.000	1.000, 1.000	1.000	1.000, 1.000
% Black pop††	0.999	0.998, 0.999	1.000	0.999, 1.001
Log (% Hispanic pop)	0.958	0.951, 0.965	0.929	0.920, 0.938
Unemployment rate	0.987	0.984, 0.990	0.986	0.983, 0.989
Deep South state	0.687	0.676, 0.699	0.682	0.671, 0.694
Determinate sentencing state	0.990	0.977, 1.004	0.995	0.981, 1.009
Truth-in-sentencing state	1.061	1.023, 1.100	1.046	1.009, 1.084
Habitual offender law state	1.204	1.186, 1.222	1.222	1.204, 1.241
Race† X % Black pop††				
Black			0.998	0.997, 0.999
Native American			1.008	1.001, 1.015
Hispanic			1.001	0.999, 1.003
Race† X log (% Hispanic pop)				
Black			1.044	1.032, 1.057
Native American			1.004	0.959, 1.051
Hispanic			1.124	1.094, 1.155
BIC		2802909		2802844

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: N = 258,512, N events (returns to prison) = 129,709; Pop = population; HR = hazard ratio; CI = confidence interval; BIC = Bayesian Information Criteria.
Reference categories: prison admit age 18-24, female, White, property crime
† Race categories refer to individuals identified as non-Hispanic or Hispanic ethnicity unknown unless otherwise noted. "Hispanic" refers to individuals identified as Hispanic, regardless of response in race item. "Native American" refers to the Census Bureau's American Indian/Alaskan Native category.
†† % Black pop refers to the percent of the county population that is non-Hispanic Black.

Model 2 adds the race and county-level racial composition interactions. The estimates for the variables not included in the interactions do not shift appreciably. Figure 1.2 shows the predicted survival probability of individuals in each race category using estimates from Model 2. The graph illustrates how Black individuals (the bottommost curve) have the lowest probability of staying out of prison and return the fastest (steepest curve), followed by Native Americans (curves are nearly identical with Blacks), Whites, and Hispanics (the topmost curve). These differences remain across time.

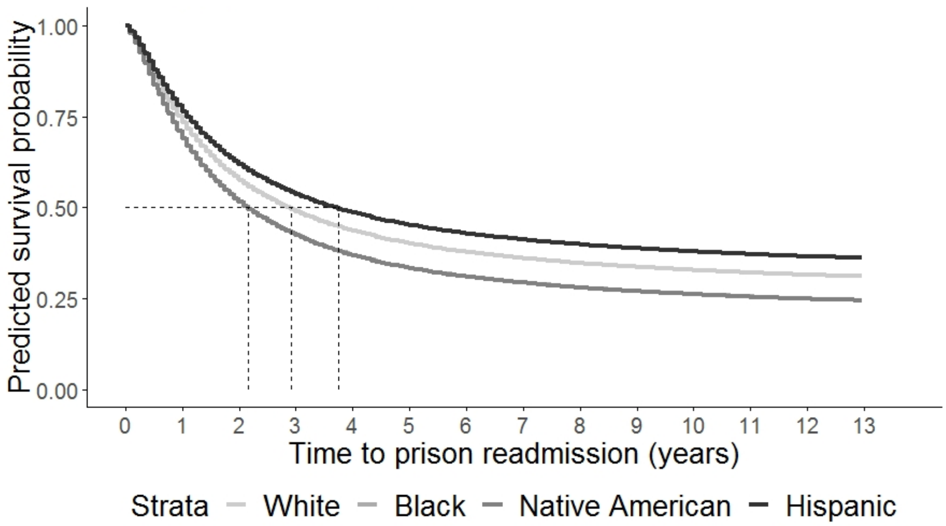

Figure 1.2. Predicted survival curves for time to readmission by race from Model 2 estimates.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: Plot of predicted survival probabilities of returning to prison after first release by race, for a male admitted to prison at 18 to 24 years old for a property offense conviction, having served mean custody length, in a non-Deep South state in a county without determinate sentencing, with truth-in-sentencing, and with habitual offender laws in 2004, with mean unemployment rate, mean percent non-Hispanic Black population, and mean percent Hispanic population. Dotted lines represent median survival.

Model 2 addresses the hypotheses regarding the relationship between race and returning to prison at varying levels of non-Hispanic Black and Hispanic population percentages. There is a very small statistically significant positive interaction between the size of the non-Hispanic Black population and being Black (Figure 1.3) and Native American (Appendix Figure A.1). A one unit increase in the percent of the population that is non-Hispanic Black, is associated with a 0.2 percent decrease in the risk of recidivism among non-Hispanic Blacks. This is consistent with the political representation explanation, although the magnitude of the interaction was very small.

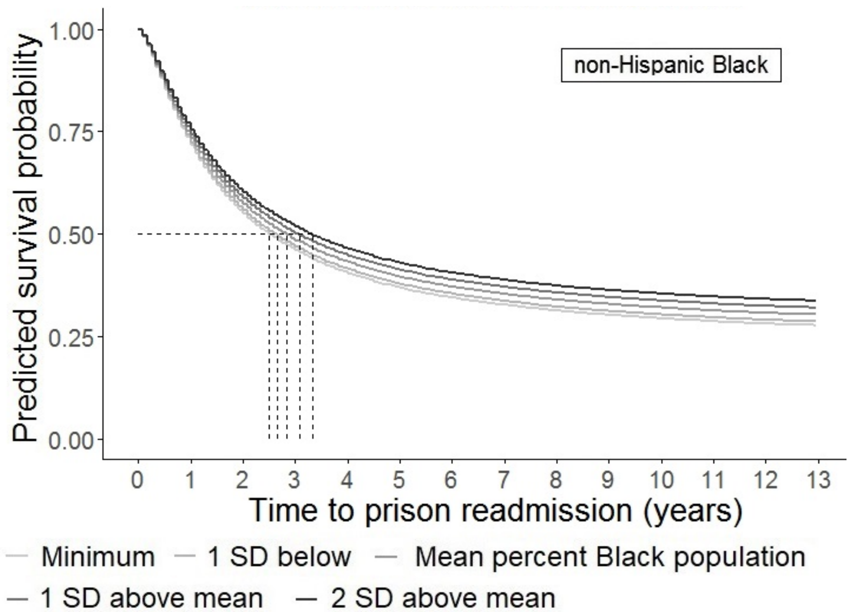

Figure 1.3. Predicted survival curves for time to readmission for non-Hispanic Blacks by percent non-Hispanic Black population from Model 2 estimates.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: Plot of predicted survival probabilities of returning to prison after first release by county percent non-Hispanic Black population, for a Black male, admitted to prison at 18 to 24 years old for a property offense conviction, having served mean custody length, in a non-Deep South state in a county without determinate sentencing, with truth-in-sentencing, and with habitual offender laws in 2004, with mean unemployment rate, and mean percent Hispanic population. Dotted lines represent median survival.

The results of Model 2 also show a small but positive interaction between being Hispanic and the size of the Hispanic population on the hazard of recidivism and the interaction term is statistically significant at the 99.9 percent confidence level (Figure 1.4). A 10 percent increase in the percent of the Hispanic population is associated with a 0.42 percent increase in the hazard of recidivism for Hispanics. This would suggest support for the racial threat perspective, that as the Hispanic population increases, perceived threat to White political dominance results in a backlash that may manifest in greater social control of Hispanics. There is a very small and negative interaction between being non-Hispanic Black and Hispanic population size; a 10 percent increase in the percent of the Hispanic population results in an 0.25 percent decrease in the hazard of recidivism (see Appendix Figure A.2).

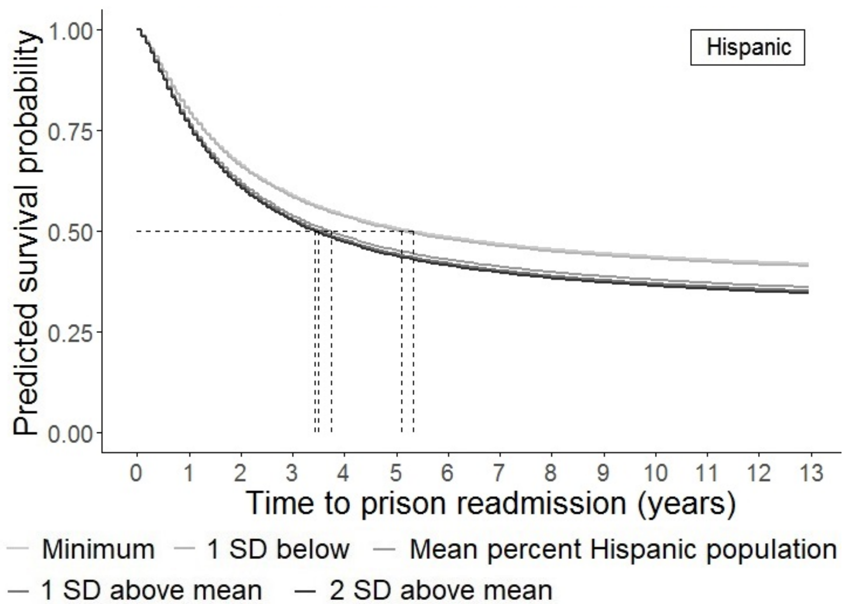

Figure 1.4. Predicted survival curves for time to readmission for Hispanics by percent Hispanic population from Model 2 estimates.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: Plot of predicted survival probabilities of returning to prison after first release by county percent Hispanic population, for a Hispanic male, admitted to prison at 18 to 24 years old for a property offense conviction, having served mean custody length, in a non-Deep South state in a county without determinate sentencing, with truth-in-sentencing, and with habitual offender laws in 2004, with mean unemployment rate, and mean percent non-Hispanic Black population. Dotted lines represent median survival.

DISCUSSION

Until recent years, most recidivism research has focused on individual characteristics to explain who returns to prison and when. The goal of this study is to examine how ecological factors such as racial composition may interact with individual characteristics to produce racial disparities in the hazard of recidivism. Understanding these dynamics may provide insight into the contexts that produce the existing policies and practices in the criminal justice domain and suggest ways to pass better policies. In addition, this study emphasizes the necessity of considering environmental factors when analyzing recidivism. Expanding the focus from just individual characteristics to other macro-level predictors of recidivism is important, especially if they contribute to the compounding cycles of racial discrimination and disparity in the criminal justice system.

This study empirically tests two hypotheses: 1. Whether higher levels of Black and Hispanic population percentages activate racial threat to result in nonwhites experiencing higher hazards of returning to prison; and 2. Whether higher levels of Black and Hispanic population percentages provide enough political power to nonwhites to result in overall lower hazards of returning to prison and less racial disparity. The results of the analyses are consistent with the first hypothesis with regards to percent Hispanic population size, but they are consistent with the second hypothesis with respect to non-Hispanic Black population size. The findings also support the political representation explanation that an increase in the Black population size results in a decrease in the overall hazard of recidivism. While studies have used racial threat theory for decades to explain White-Black disparities, this study suggests that this theory is not as powerful as previously conceived.

Although this study does find evidence consistent with the political representation explanation, the magnitude is very small and does not find evidence that larger Hispanic populations are associated with lower racial disparities. Linking data with other variables measuring political representation may help to understand this phenomenon. Such variables may include the proportion of nonwhite elected officials or levels of disenfranchisement through laws that bar people with felony convictions from voting. The small magnitude of the result consistent with the political representation hypothesis could also be because barriers to voting and political influence prevent even large numbers of people from being fully represented. It is well known that nonwhites vote less than Whites (Fraga 2018). Policies that promote the enfranchisement of nonwhites could lead to increased political power, particularly in communities with large nonwhite populations. Furthermore, the population of Latinx American citizens is younger than other groups (U.S. Census Bureau 2019) – it may be a matter of time until a large Latinx population is able to translate to a large voting age population. Policy change and further demographic changes may present pathways to promote political representation, prevent the mobilization of racial threat, and to make changes to the criminal justice system that reduce the disproportionately high level of surveillance and state control of nonwhites. However, research also suggests that intentional engagement by citizens, politicians, and political parties will also be necessary to mobilize nonwhite voters (Fraga 2018).

However, the findings of this study leave open questions, such as why there is an effect of Black racial composition on recidivism for Native Americans. Further, the effect sizes in all of the results are quite small. It is possible that county-level covariates may be the wrong unit of analysis to test these hypotheses. If racial threat or political representation are more salient in closer quarters, a finer unit, such as city of re-arrest or re-conviction, could provide better tests. However, this would require finer-grain data on where individuals live upon release. Such data are typically not available for large samples or are limited to people enrolled in parole or other community supervision programs, which would produce a biased sample (Hipp et al. 2010; Morenoff and Harding 2014). The NCRP only provides the county of jurisdiction of the sentence, which may not be a good proxy for county of residence especially in areas that rely less on parole.

In addition, it may be that returns to prison are too distal of an outcome to detect a strong effect of racial threat or political representation on recidivism. The effects may be bigger when looking at rearrest rates, as city and county characteristics may more directly influence the scope and policies of local law enforcement than the downstream domain of prison. Merging prison data with arrest data would enable such analyses. A different data set may also allow me to test the hypothesis that the relationship between racial composition and racial disparities in criminal justice outcomes is curvilinear. There may be a threshold that a nonwhite population needs to reach before a group has enough political power to elect and pressure officials for policy change.

One future avenue of research would be to look at changes in racial composition as opposed to absolute measures. It could be the case that a dynamic measure as opposed to a static measure would better test the hypotheses (Caravelis et al. 2011; Wang and Mears 2010). A community may, for example, feel a stronger sense of threat if the percentage of the nonwhite population doubles from 15 percent to 30 percent over the course of ten years, than a community that has a stable 40 percent nonwhite population. This may be a fruitful area to pursue, as there have been large increases in the nonwhite population in the United States in recent decades that continue in the present, but these shifts are not evenly distributed across the country. It will become all the more important to understand the decision-making processes and systems that lead to policies that disproportionately impact groups that are already overrepresented in the criminal justice system.

As scholars continue to look at the effect of racial composition on racial disparities in criminal justice outcomes, they should consider how political representation might translate to reduced disparities. What are the mechanisms through which voting power and larger numbers of nonwhite elected officials can mitigate or remedy the harms that the criminal justice system has wrought on communities of color? In addition, researchers should investigate whether policies impact the ability of communities to have their concerns about the criminal justice system heard. The confluence of concerns about intentional nonwhite political disenfranchisement and response to calls for change to address racism in law enforcement and court institutions make these questions all the more pressing.

Chapter 2. REPEATED RETURNS TO PRISON FOR PAROLE VIOLATIONS

INTRODUCTION

In this paper, I focus specifically on recidivism defined as "returns to prison" among people on parole, also known as a revocation. For released individuals and their families and communities, recidivism defined as returns to prison, as opposed to other operationalizations such as rearrests or reconvictions, may be the most consequential as it not only physically separates families, but removes individuals from their communities and imposes long-lasting consequences in many areas of life, including physically, emotionally, and financially. Furthermore, placing and keeping people in prison is often the costliest form of punishment and policymakers need to carefully scrutinize how taxpayer money is used.

The courts often present parole, the period of conditional supervised release in the community following a prison term, as an alternative to serving an entire prison sentence, or a mandatory period imposed as part of a sentence. An estimated 874,800 people were on parole at year-end 2016 in the United States, and this number has been increasing for decades (Kaeble 2018a). Many people do not complete their parole terms with an end to supervision; 28 percent of people, about 105,000 people, ended their state parole terms with a revocation in 2016 (Kaeble 2018a).

For people on parole, returning to prison generally occurs in one of two ways: a technical violation of the conditions of parole or a new conviction. The first is a failure to adhere to conditions ostensibly meant to encourage reintegration into prosocial society and the second is a failure to adhere to the law. Technical violations can include such behaviors as possessing a firearm or breaking curfew, among many others. Both should be of interest to policymakers, although they represent different pathways to prison. Research shows that nationally, 20 percent of people released from prison return because of a new conviction, while over 25 percent of people released from prison onto parole end up back in prison as a result of a technical violation (Pew Center on the States 2011). The policy of returning a person to prison without a new crime conviction poses the question of the whether the criminal justice system is serving the goal of reducing crime and increasing public safety and puts a strain on an already overcrowded penal system.

Despite these distinctions and downstream consequences, much research still fails to distinguish between returns to prison for a new crime versus returns for a technical violation of the terms of parole (e.g., Durose, Cooper, and Snyder 2014). Research on reentry also shows that desistance, cessation from criminal behavior, is better understood as a long-term process rather than an event (Bushway et al. 2001; Glaser 1964; Laub and Sampson 2001; Maruna 2001; Rydberg and Grommon 2016), but most scholars continue to operationalize an initial recidivating event as the termination of the reentry process as opposed to an event with effects on the continuing reentry process (e.g., Ostermann 2015; Spivak and Damphousse 2006; Yang 2017). Furthermore, sentencing policies, parole guidelines, and penal regimes vary significantly at the state level (Pew Center on the States 2011; Robina Institute of Criminal Law and Criminal Justice 2018), yet existing research often examines recidivism within a single state or even city to make general statements of what recidivism looks like in America (e.g., Bales and Mears 2008; Grattet, Lin, and Petersilia 2011; Kurlychek, Brame, and Bushway 2006; Wehrman 2010). For example, Wehrman (2010) used Wayne County data on 1,515 people to suggest that concentrated disadvantage does not have an effect on recidivism, although the data included only people who recidivated in Michigan. Bales and Mears (2008) suggested that prison visitation reduces and delays recidivism utilizing Florida data on 7,000 people released from prison over the course of five months. However, there are likely jurisdictional differences in how prior contact with prison and parole affect future decisions about how to punish individuals. Directly comparing jurisdictions may reveal how context and policy can shape the outcomes of individuals who often come from our most disadvantaged communities. This may lay the groundwork for future research focusing on the mechanisms by which structural racism is reproduced and creates the policies and biases in the criminal justice system (Ray 2019). This study aims to describe jurisdictional differences to establish the empirical basis for future research that can directly investigate how policy differences affect revocation and contribute to a more nuanced discussion of the goals and effectiveness of parole.

In this study, I seek to explore three inter-related research questions:

- Given the differences in policies and parole systems, do the recidivism outcomes of people released to parole in one state differ from outcomes of people in another state?
- Is there a relationship between experiences of prior parole episodes and future recidivism outcomes?
- Bringing the first two questions together, does this relationship between prior experiences and recidivism look different for people on parole in one state versus the other?

Together, these three questions address how individual characteristics and institutional factors interact and affect the outcomes of parole. Unlike most studies of parole that only use data from a single jurisdiction and treat all prison releases equally, I compare two states and look beyond an individual's single experience with parole to examine the cumulative effects of serving multiple parole episodes. Ideally, I would compare these outcomes across multiple states, but data limitations prevent this. As a starting point, this chapter compares the experiences of individuals in two states, which still allows me to delve into differences in parole policies and to take a first step toward understanding how generalizable findings from smaller sample studies may be.

I use data from the Bureau of Justice Statistics' National Corrections Reporting Program (NCRP) to examine the recidivism outcomes of people first released from prison onto parole in

2006 in two Northeastern states: Pennsylvania and New York. While the three-year recidivism rate for individuals released from prison in both of these states in 2004 was 40 percent, their systems of parole look very different (Pew Center on the States 2011). Pennsylvania has the highest rate of parolees per state residents in the country and unlike New York, almost exclusively uses an indeterminate sentencing framework, which gives much of the decision-making power of release to a parole board. While parole boards in both states consider prior parole experience when making release decisions, the New York system weights this factor more heavily. Thus, comparing recidivism outcomes of individuals in these two states that have many similarities and key differences may reflect how different and complex the cycles of prison, parole, and recidivism can look even in geographically adjacent states with the same overall recidivism rate.

The NCRP provides longitudinal prison and parole data on each individual from 2006 through 2015 and I use this 10-year period to see whether parole episodes end with a completion or in a revocation (return to prison), as well as whether revocations involve a new sentence (new crime conviction versus technical violation). Using the parole data set, I examine whether the likelihood and timing of a parole revocation and the type of revocation differ by state, predicting that the more times an individual has gone onto parole, the more likely the individual is to experience a revocation and a revocation without a new sentence, but these relationships will not look the same across the two states due to differences in their parole policies.

As expected, I find that the number of prior parole episodes and the state systems are significantly associated with the likelihood and timing of returning to prison and revocation type. Overall, my analyses suggest that there is a relationship between prior experiences of parole and future recidivism and that this relationship looks different for people on parole in New York versus Pennsylvania. When considering the likelihood of experiencing a revocation versus completing parole, in both states, having been on parole previously increases the chance of revocation. In addition, I find that people in Pennsylvania had a higher risk of revocation than people in New York, but this difference is only discernable among people on their first parole episode. For the analyses examining type of revocation, I find that in both states, people returned to prison for a technical violation revocation more quickly than for revocations with a new conviction. Further, having been on parole previously increased the risk of experiencing a parole revocation with no new sentence, but only in New York.

The first section of the paper reviews the literature on parole revocations. The second provides details of parole in Pennsylvania and New York. The subsequent sections present the data, methods, and results. I discuss the findings, policy implications, and next steps in the final section.

LITERATURE REVIEW

Parole is designed to serve two related purposes: reintegration and surveillance. While on parole, a parole officer provides resources meant to help reintegrate the parolee into society outside of prison. At the same time, parole is meant to serve a surveillance purpose to reduce criminal behavior by the person on parole and promote public safety (Klingele 2013). However, people on parole can be at particular risk for returning to prison compared to people who are released unconditionally. Violation of parole, either for committing a crime or for technical violations (failing to meet the conditions of parole) can be punished with a return to prison, known as a revocation. In many states including New York and Pennsylvania, general conditions of parole include abstaining from unlawful possession, use, or sale of controlled substances; continuing payments on court fines, costs, and restitution; attending appointments with the parole officer; and notifying the officer of changes in status of employment, on-the-job training, and education (New York State Department of Corrections and Community Supervision 2010; Pennsylvania Board of

Probation and Parole 2018). Distinguishing revocations for a new crime versus for a technical violation may have policy implications when considering where and how we might best support the process of desistance. On one hand, the conditions of supervision create many opportunities for committing technical violations (e.g., by seeing friends or family who have criminal records or missing an appointment with a parole officer), but on the other hand, a parole board may be willing to impose a non-custodial sanction for the first few infractions while a new crime conviction would result in an immediate revocation.

Research shows that the probability of parole failure, and recidivism more generally, is highest immediately following release from incarceration and declines over the course of several years (Grattet and Lin 2016; Petersilia 2003; Solomon, Kachnowski, and Bhati 2005) and supervision tends to be the most intense just after release from prison. It is hard to say whether the high probability of parole failure early on is due to the increase in supervision intensity (Grattet and Lin 2016; Grattet et al. 2011) or higher rates of violation commission (MacKenzie 1991). However, research on the effect of parole on reimprisonment in general suggests heightened supervision drives higher rates of return. Harding et al.'s (2017) Michigan study exploits the random assignment of judges with differing propensities for handing down prison versus probation sentences and finds that imprisonment for technical violations is a key mechanism driving the majority of the “revolving door” effect of prison and that it is the high intensity of supervision on parole (relative to probation) that is likely the cause. In addition, the Urban Institute’s four-city Returning Home Study from 2001 to 2006 finds that the men in their parole sample had similar rates of self-reported new crime commission as the men in their non-parole sample but they were more likely to be rearrested, possibly due to the higher intensity of surveillance (Yahner, Visher, and Solomon 2008).

Research also suggests that how jurisdictions implement parole affects revocation rates. Numerous studies of community supervision find that despite good intentions, individuals on parole who receive rehabilitative services, such as drug treatment or counseling, have a shorter time to revocation. This is also likely due to the increased level of surveillance, which may detect a violation (Albonetti and Hepburn 1997; Clear and Hardyman 1990; Gottfredson, Mitchell-Herzfeld, and Flanagan 1982; Grattet et al. 2011; Land et al. 1994). In addition, states like North Carolina that have policies instituting shorter periods of parole see very low rates of revocations, and particularly revocations for technical violations, but higher rates of returns for new court commitments (Pew Center on the States 2011). Similarly, Oregon's practice is to use revocation only as a last resort and thus their numbers for revocations for technical violations are among the lowest in the country, just 3 percent for the 2004 release cohort, even though many people may have received other non-custodial sanctions (Pew Center on the States 2011). The Pew Center on the States (2011) study shows how the three-year recidivism rate can vary highly by state, although they do not distinguish how these differences may interact with an individual's prior history with parole as the present study does.

Given that different jurisdictions implement parole differently, it is important to assess whether recidivism outcomes of people released to parole differ between states. Previous experiences of parole may affect the likelihood of a future parole revocation but the research on this topic is sparse. Supervision officers may more closely watch people who have had more contact with the criminal justice system (Grattet et al. 2011), and in risk assessment tools previous offenses translate to higher risk (Luallen, Radakrishnan, and Rhodes 2016).

Variation in supervision intensity based on the extent of prior contact with parole and the criminal justice system is particularly concerning in light of racial differences in the implementation of parole policy. Research in California finds that parole officers were more likely to refer Black people on parole than Whites to the parole board for criminal violations than prosecuted in court where the likelihood of revocation is lower. In addition, they were more likely to be reincarcerated than Whites, although this finding did not hold for technical violations (Grattet, Petersilia, and Lin 2008). If past churning through prison and parole affects subsequent parole board decisions, this would further exacerbate racial disparities.

Furthermore, people on parole may behave differently if they have previously experienced parole or a revocation. The Returning Home Study finds that 78 percent of people in their sample who had not experienced parole before had pre-release expectations that it would be easy to avoid a violation, while only 69 percent of those who had previously been on parole felt this way, a statistically significant difference. They also find that parole reduced the likelihood of self-reported crime commission among survey respondents with no prior parole or probation revocations (from 21 to 14 percent). However, those with one or more prior revocations had a higher likelihood of self-reported crime commission (Yahner et al. 2008). Thus, investigating the question of how past experiences of parole are associated with recidivism is important and may have implications for how people can move through the desistance process. This motivates my second research question: is there a relationship between experiences of prior parole episodes and future recidivism outcomes?

This study brings together this research on parole revocations, jurisdictional differences, and the effect of prior parole experiences on future recidivism to examine how individual characteristics and contextual factors interact and affect the outcomes of parole. I tie together my first two research questions with a third: given state differences in implementing parole, does this relationship between prior experiences and recidivism look different for people on parole in one state versus the other? I investigate these relationships using ten years of Pennsylvania and New York data from people released from prison for the first time onto parole in 2006.

PAROLE IN PENNSYLVANIA AND NEW YORK

Pennsylvania and New York offer interesting cases of differing parole systems in terms of their parole population sizes, sentencing frameworks, level of parole board discretion and power, and revocation rates. The two states were very similar in their imprisonment rates in 2006, when my data begin. Although New York's population was much larger, in 2006 both states had a similar rate of sentenced prisoners per 100,000 residents and their three-year prison return rate among people released in 2004 was the same, 40 percent (Pew Center on the States 2011; Sabol, Couture, and Harrison 2007). Pennsylvania has used parole much more widely for many years. In 2006, Pennsylvania had 791 people on parole per 100,000 adult residents, the second highest of all states, while New York had 358, roughly the average of all the states (Glaze and Bonczar 2008). Since that time, Pennsylvania's imprisonment and parole rates have increased while New York's rates have decreased. In 2016, Pennsylvania and New York had 383 and 256 sentenced prisoners per 100,000 residents, respectively, and 1,109 and 285 people on parole per 100,000 adult residents, respectively (Carson 2018; Kaeble 2018a). Pennsylvania's parole rate is now the highest of all states, likely due to long parole sentences, while New York remains at about the average of all states. **Error! Reference source not found.** summarizes these comparisons (Schiraldi 2018).

Pennsylvania and New York have different sentencing frameworks. New York utilizes both determinate and indeterminate sentencing although indeterminate sentences are more common, while Pennsylvania only has indeterminate sentencing with a few exceptions (N.Y. Penal Law § 70.00; 42 Pa. Cons. Stat. §§ 9711-9720; Watts et al. 2016; Watts, Reitz, and McBride 2017). In New York, this means that those who serve a determinate sentence must serve at least 85 percent of their sentence and have a fixed period of parole once they are released. By 2007, this applied to people who have a violent felony conviction, drug offenses, and most sex offenses (N.Y. Penal Law § 70.00). In indeterminate sentences, courts can impose a minimum and maximum prison sentence of any length within a statutory range. After serving the minimum sentence, a person becomes eligible for the parole board to evaluate them for release, or earlier through earned time credits (N.Y. Penal Law § 70.00).

The Pennsylvania parole board determines whether to grant parole to eligible prisoners using a 19-item Decisional Instrument, of which supervision history is one component (Watts et al. 2016). However, the results of the instrument are only advisory and by statute, this instrument is not to "remove the discretionary parole authority of the board" (42 Pa. Cons. Stat. § 2154.5). In New York, statute requires the parole board to consider eight factors to determine release, including an individual's prior criminal record, which includes behavior during any previous parole supervision (N.Y. Exec. Law § 259-i). Given that New York places more weight on prior parole experiences in deciding whether to grant parole, I expect that this state may also more heavily consider this factor in revocations, particularly for revocations without a new sentence, where the decision to revoke is more in the hands of the parole board than where a new crime has occurred.

In Pennsylvania, although people who violated parole are entitled to a parole violation hearing, the standard for deciding whether the violation occurred is lower than for a criminal trial – it requires a preponderance of evidence, meaning it must be more likely than not that the parolee violated the condition (37 Pa. Code § 71.2(1)). One study found that 24 percent of people released from prison in 2004 in Pennsylvania returned to prison for a technical violation within three years, and 16 percent returned for a new crime, although it did not specify whether the individual was on parole for the latter (Pew Center on the States 2011). Due to legal reforms in 2012, parole boards have less discretion in technical violation cases and their use of prison for technical violations is constrained for some types of technical violations. In addition, 34 percent of Pennsylvania prison admissions in 2006 were for a violation of conditional release, and this percentage increased between 2010 and 2014 to 45 percent, while the mean of all states dropped to 28 percent (Watts et al. 2016). Individuals who returned to prison for a technical violation are eligible for automatic re-parole after six months, nine months, or one year while a parole board determines whether to re-parole someone whose parole was revoked for a new crime (Watts et al. 2016).

In New York, the standard of proof for a parole violation is even lower: there must be a finding of probable cause that the violation occurred, followed by a review and a final hearing (New York State Department of Corrections and Community Supervision 2010). In New York returning to prison has been the primary outcome for people on parole who have violations and returns for technical violations are more common than in most states (Hager 2017; New York State Department of Corrections and Community Supervision 2017). One study found that half of the individuals released from New York prisons onto parole in 2012 were reincarcerated within three years, of which 84 percent were reincarcerated for technical violations and not new crimes (New York State Department of Corrections and Community Supervision 2018). In addition, in New York, unlike in other states, there is no cap on the length of the prison term an individual may serve for a technical violation. This can further exacerbate the issue of getting caught up in the prison system due to minor infractions (Schiraldi and Arzu 2018).

Table 2.1. Comparison of Pennsylvania and New York Parole Systems and Prison Rates

	Pennsylvania	New York
Sentenced prisoners per 100,000 residents (2006 / 2016) [a]	353 / 383	326 / 256
3-year prison return rate (released 2004) [b]	39.6 percent	39.9 percent
Parole population per 100,000 adult residents (2006 / 2016) [c]	791 / 1,109	358 / 285
Sentencing framework [d]	Vast majority indeterminate	Indeterminate/determinate
Parole board discretion [e]	Higher	Lower
Technical violation revocation rate [f]	Lower	Higher

Source:
a. Carson (2018); Sabol, Couture, and Harrison (2007).
b. Pew Center on the States (2011).
c. Glaze and Bonczar (2008); Kaeble (2018).
d. N.Y. Penal Law § 70.00; 42 Pa. Cons. Stat. §§ 9711-9720.
e. 42 Pa. Cons. Stat. § 2154.5; N.Y. Exec. Law § 259-i.
f. New York State Department of Corrections and Community Supervision (2017, 2018); Pew Center on the States (2011).

Although this study does not seek to estimate the impact of a particular one of these institutional features on recidivism, comparing outcomes of people subject to these two state systems will provide an opportunity to better understand how parole revocations may play a role as a recidivism pathway that affects long-term contact with the prison system. It also will provide insight into the level of variation that may exist in how this pathway manifests in different criminal justice systems. Findings underscore the need for research to be more nuanced in evaluating programs and policies, and assessing individuals based on past recidivism outcomes. This study will push future research to carefully consider policy differences across jurisdictions.

DATA

Data for the analyses come from the National Corrections Reporting Program (NCRP) (Bureau of Justice Statistics 2017). These data include individual-level longitudinal data on all state prison admissions and releases in most states. Individuals appear in the data multiple times if they entered prison multiple times. In contrast to Chapter One, this study takes advantage of the underutilized state parole admission and release data set. Pennsylvania has provided both parole and prison data from 2001 through 2015, one of the longest continuous periods of any state. New York has provided ten years of continuous data to the parole file (2006-2015). I use these data to examine two outcomes of interest. The first is whether an individual on parole returned to prison and the second is whether an individual who experienced a revocation did so for a new crime or a technical violation. The New York and Pennsylvania data also include indicators for the type of revocation that are more accurate that in other states (Gaes et al. 2016). Given how infrequently researchers have used the parole data, descriptive studies such as this one provide an important first step to studying differences in revocation patterns across states and individual parole histories.

As an individual's prison and parole history is of interest, the sample includes only individuals who served a parole episode from their first release from prison beginning in 2006. Although the dataset does not indicate whether it is an individual's first term in prison, I take several steps to identify first-time prison releases. While the New York parole data begin in 2006, the New York prison admissions and releases begin in 1994. Although the first time an individual appears in the prison data may not be their first time in prison, I employ a rule that if an individual was not previously released from prison in the 12 years between 1994 and 2006, then their release to parole from 2006 onwards was their first release from prison. Pennsylvania's parole and prison data begin in 2001, so I employ a similar rule, removing individuals who have prison releases or parole admissions in the data prior to 2006.[5] Research shows that recidivism is highest immediately

[5] Of the remaining sample, I examined whether the first prison admission type was for a new court commitment, as opposed to a parole revocation, return from prison escape, return from

following release and declines over time and the difference between the prison return rate between a five- and seven-year follow-up is extremely small, less than one percent (Rhodes et al. 2016). Thus, my rule is a valid procedure to capture first time releases in the absence of definitive records. After I employ the cutoff rule, the remaining data provide ten years of follow up from 2006 through 2015.

To address the missing data on two covariates, conviction offense and race, I use multiple imputation using the variables in the analytic model with 20 imputations (Graham, Olchowski, and Gilreath 2007) and Rubin's (1987) rules for pooling the results of analyses. Conviction offense is missing only in Pennsylvania in 2.43 percent of the state's sample, and race is missing in 4.6 percent of the total sample, with the majority of the missing race data in Pennsylvania (see **Error! Reference source not found.** in the Results section). With regards to producing valid statistical inferences for data such as these that are not missing at random, multiple imputation performs better than listwise deletion and other methods, such as mean imputation (Allison 2002; Buhi, Goodson, and Neilands 2008; Cox et al. 2014). Analyses conducted on data without imputation show very similar results.

Measures

Prison recidivism research often does not distinguish between returning to prison for a technical violation or a new crime. Parole research brings attention to this difference, but large-scale data typically do not include reliable indicators of whether a parole revocation was due to a new crime or a technical violation. This study uses unique data to address this gap and add to our understanding of the recidivism pathways parole presents.

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appeal or bond, transfer, etc. Few individuals had any other prison admission type and I removed them from the sample after examining their NCRP records.

Parole Revocation with a New Sentence or No New Sentence

A key outcome measure is whether an individual returned to prison for a parole revocation with a new crime or a technical violation. In the NCRP, the return type is not explicitly coded for "technical violation", but I assume that if an individual was released from parole due to a revocation with no new sentence (NNS), that this was a technical violation. Similarly, a revocation with a new sentence (NS) implies a new crime. Only combining national, state, and local prison, court, police, and parole officer records can completely remove the uncertainty regarding this assumption. However, the NCRP's data managers note that researchers could reasonably make this assumption if they are using data from states with reliable prison and parole term data such as Pennsylvania and New York (Gaes et al. 2016). Figure 2.1 shows a diagram of the key outcome measures coded in this way.

Remaining misclassifications could lead to bias in either direction. For both states, it may be more likely that revocations with a new sentence that have a short prison term are actually revocations with no new sentence, but high variation in time served is expected as offenses vary in severity. In Pennsylvania, revocations with no new sentence that have long prison terms may be more likely to actually be a revocation with a new sentence given their automatic re-parole policies but this is very uncommon in the data so it is unlikely to be a source of much bias. In New York it is less clear because there is no cap on the length of a prison term after a revocation with no new sentence, although the data also uniformly showed relatively short prison terms after this type of revocation, suggesting that this type of misclassification is uncommon.

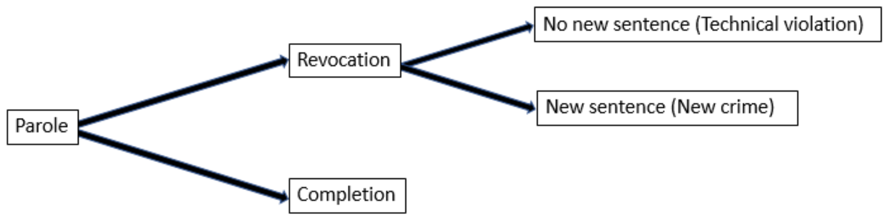

Figure 2.1. Outcomes of parole.

Covariates

The analyses includes seven covariates in the logistic regressions and six covariates in the event history analysis. Of most interest is the New York or Pennsylvania state indicator. The state indicator reflects the state of jurisdiction, where the conviction that led to a prison term occurred and where the individual is serving parole. I include an indicator for the conviction offense type that led to the prison sentence for which the individual was subsequently released to parole. The reference category is property crime convictions, which are likely to have the highest likelihood of revocation and highest hazard, followed by drug-related offenses, public order offenses, and violent offenses (Beck and Shipley 1989; Durose et al. 2014; Langan and Levin 2002). I also include three covariates to capture individual-level demographic characteristics: sex, race divided into three categories (White, Black, Other),[6] and age at the time of release to parole divided into five categories (under 18, 18 to 24, 25 to 39, 40 to 64, and 65 to 100). Very few minors are incarcerated in state prisons and in this sample, they only appear in New York. The reference categories are female, White, and the 18 to 24 age category. I also include a measure of the length of the last prison term served in years, logged to account for the right skew in the variable. Lastly,

[6] Like many administrative data sets, the data on race are highly limited. The vast majority of the observations fall in the White or Black categories and Other is an aggregate of people identified as Native American, Asian, Pacific Islander, Multiracial, or Other. Race information in the NCRP comes from a mix of sources: court records, prison officials' visual determination, and in some cases, self-report.

in the logistic regressions I include an indicator variable for the parole episode number. I collapse episodes four through six as few people experienced that many parole episodes over the ten years. Although these data are limited in individual-level characteristics, these covariates represent some of the key correlates of recidivism used in recidivism research.

METHODS

This study takes different empirical approaches to test hypotheses related to parole revocations, logistic regression and event history analysis. For the first two analyses I employ logistic regressions to examine whether the likelihood of a parole revocation (Model 1) or an NNS revocation (Model 2) that individuals experienced differed in Pennsylvania versus New York and by the parole episode. The key outcome variable of interest is an interaction term between the New York state indicator and the parole episode indicator variable. This interaction term allows me to see if the association between parole episode and the two outcomes differed across the two states. These two models represent the two divergences in Figure 1. I use two separate analyses instead of one multinomial logistic regression using a dependent variable with three outcomes (parole completion, NNS revocation, NS revocation) because it would not meet the assumption of independence of irrelevant alternatives.

Thus, I estimate logistic regressions for both models. In Model 1 the outcome is the probability that a parole episode ended with a revocation (1) or completion (0). The outcome in Model 2 is the probability that a parole revocation was without a new sentence (1) or with a new sentence (0). The Model 2 sample includes only parole episodes that ended in a revocation. In both models, I include the primary covariate of interest, an interaction term between state and parole term.

For the third analysis, I conduct a multi-state event history analysis to examine whether being on parole in New York hastened a return to prison with an NNS revocation versus an NS revocation compared to being on parole in Pennsylvania. While the logistic analyses models the likelihood of events occurring, the event history analysis examines the duration to the event. Figure 2.2 shows a reconfiguration of Figure 2.1, showing the three outcomes in one model.

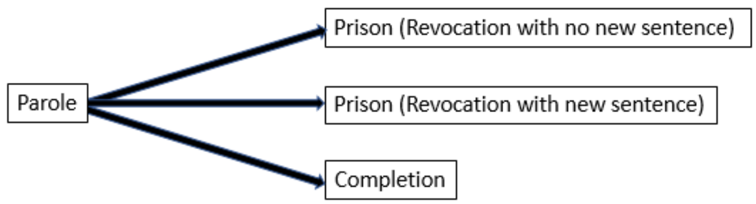

Figure 2.2. Outcomes of parole, unnested for event history analysis.

As Figure 2.2 shows, there are multiple outcomes and transition pathways from one starting point. Thus, I need to compute the probabilities of all the transitions of interest simultaneously, because I want to know about the cumulative incidence of all three outcomes and not just model one outcome at a time. I set this up as a competing risks problem in an event history analysis, where I compute the rate of transition to the possible outcomes with an Aalen-Johansen estimator, which is a non-parametric estimation of a matrix of transition probabilities, meaning individuals can transition to multiple states during the window of observation (Aalen and Johansen 1978). The Aalen-Johanson estimator is essentially a matrix version of the commonly-used Kaplan-Meier estimator, which simply measures the fraction of the starting sample that has not experienced the event of interest over time (Kaplan and Meier 1958).

RESULTS

Error! Reference source not found. shows the descriptive statistics for the data I use to examine the relationship between US state (Pennsylvania or New York) and the likelihood of experiencing a parole revocation or revocation with no new sentence. Almost two-thirds of all individuals were from New York in both samples. The percentage of observations in Pennsylvania that ended in a revocation was almost 5 percent higher than the percentage in New York. However, the percent of revocations that were with no new sentence was over 25 percent higher in New York.

In the revocation versus completion sample, the number of parole episodes that people experienced differs little across the two states. As the number of parole episodes possible over a ten-year period should depend partly on the length of prison terms, it is not surprising that the mean length of prison terms also did not vary highly across the two states: 2.8 years in Pennsylvania and 2.4 years in New York.[7] The state difference in number of parole episodes is starker when looking at revocation type; there are more observations at every higher order parole episode ending in revocation in New York.

Race differs highly across the two states. In both samples, compared to people on parole in New York a larger percentage of Pennsylvanians are White and fewer are Black as was also the case in overall state demographics (Census 2010). There is a smaller percentage in the Pennsylvania Other category, although as noted earlier, a much larger percentage of the sample in Pennsylvania had missing race data. In both states, Blacks are overrepresented relative to overall state demographics (Census 2010). Appendix Table B.1 shows descriptive statistics for the event history analysis.

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[7] The length of last prison term is logged in Table 2.2 and in analyses due to the higher prevalence of shorter prison terms and right skew. Thus, the exponentiated mean length in years reported in this table is useful in comparing across groups but not necessarily as a value in and of itself.

Table 2.2. Descriptive Statistics for Logistic Models 1 and 2 – Data Prior to Multiple Imputation

	Model 1 Revocation v. Completion			Model 2 NNS [a] v. NS [b]		
State	PA	NY	Total	PA	NY	Total
New York (%)			66.35			63.97
Parole revoked / NNS [a] (%)	45.30	40.79	42.31	59.60	84.95	75.82
Parole Episode (%)						
1	72.10	67.68	69.17	69.18	55.61	60.50
2	19.71	20.66	20.34	21.69	26.97	25.07
3	6.06	8.06	7.39	7.01	11.29	9.75
4-6	2.12	3.59	3.10	2.12	6.13	4.68
Conviction offense [c] (%)						
Property offense	18.20	20.21	19.53	20.83	22.97	22.20
Drug	34.97	36.36	30.63	32.64	31.12	33.55
Violent	26.89	32.52	35.89	29.70	35.72	31.67
Public Order	17.02	10.37	12.61	13.34	9.88	11.13
Unspecified	0.50	0.53	0.52	0.58	0.31	0.41
Missing	2.43	0.00	0.82	2.91	0.00	1.05
Log last prison time served	1.03	0.89	0.94	1.06	0.85	0.92
Mean (SD)	(0.49)	(0.55)	(0.53)	(0.51)	(0.54)	(0.54)
Male (%)	90.67	91.78	91.41	93.91	94.32	94.17
Race [d] (%)						
White	42.72	34.09	36.99	39.86	31.85	34.73
Black	44.58	51.59	49.23	48.72	56.74	53.85
Other	0.70	13.51	9.20	0.68	11.00	7.28
Missing	12.01	0.81	4.58	10.74	0.40	4.13
Parole admit age (%)						
Under 18	0.00	0.08	0.05	0.00	0.12	0.07
18-24	13.59	22.34	19.40	16.32	26.78	23.01
25-39	55.19	52.80	53.61	58.50	54.43	55.90
40-64	30.70	24.04	26.28	25.08	18.48	20.86
65-100	0.51	0.75	0.67	0.10	0.19	0.16
N observations	6452	12723	19175	2923	5190	8113
N individuals	4722	8611	13333	2113	3022	5135

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes:
a. NNS: a parole revocation without a new sentence, implying a technical violation
b. NS: a parole revocation with a new sentence, implying a new crime
c. Conviction for which individual went to prison and was subsequently released to parole
d. Other race refers to individuals identified as Native American, Asian, Pacific Islander, Multiracial, or Other

The logistic regression in Model 1 addresses whether the relationship between having prior parole experiences and a subsequent revocation looked different for people in New York versus Pennsylvania. Figure 2.3 presents the results of Model 1 as relative risks. The bars show the risk of experiencing a parole revocation for a person on parole in New York relative to the risk for a person in Pennsylvania with 95 percent confidence intervals. The labels on the left indicate this comparison for people on their first, second, or third time on parole. The top figure shows the relative risks among Black people on parole, while the lower figure is for White people on parole. As the figure shows, the patterns within Whites and Blacks look very similar. This is a result I found across essentially all of my analyses.

Unexpectedly, the results suggest that people in New York had a lower risk of revocation compared to people in Pennsylvania on their first time on parole, but this was not the case in subsequent times. Thus, there appears to be an interaction between state and the number of times a person is on parole for the risk of returning to prison. Only the state difference for the first parole term is statistically significant at the 95 percent confidence level. For example, the baseline risk of recidivism for a White person on parole in Pennsylvania for the second time is 0.685, and 0.699 in New York. The figure shows this as a relative risk of 1.021 (0.699/0.685). Appendix Figure B.1 shows that, as expected, for both states, people on parole a second or third time had a higher risk of revocation than people on parole for the first time, which may possibly be reflecting the higher level of surveillance that people who have not succeeded on parole previously experience.

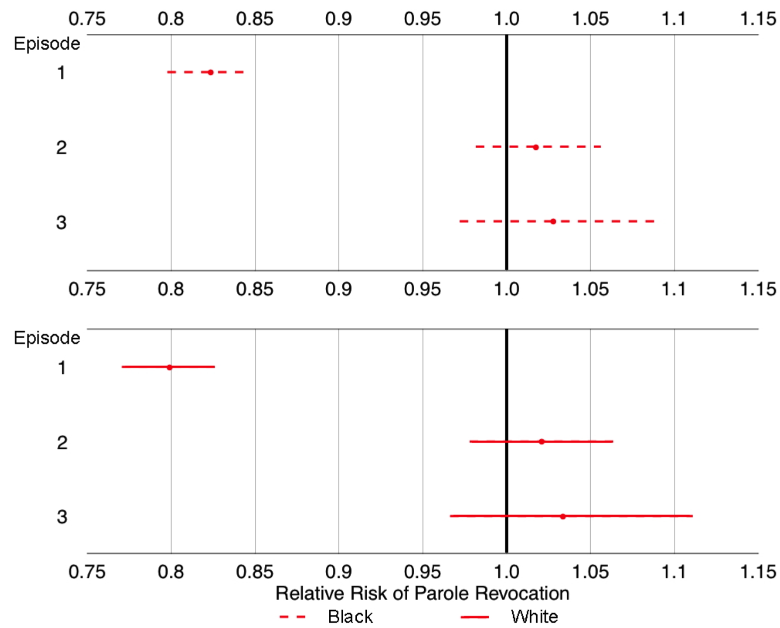

Figure 2.3. Model 1 results: Relative risk of parole revocation by state, race, and parole episode (compared to Pennsylvania) among males, convicted of property offense, aged 18-24, having served mean prison term.

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes: Bars show 95% confidence intervals using the variance-covariance matrix from one imputed data set; results are the same across all data sets.

Error! Reference source not found. presents the full results of Models 1 and 2 as odds ratios. The direction of the other covariates in Model 1 are as expected based on the literature, with the odds of a revocation being highest for people convicted of property crime, males, Blacks, and people who are younger at parole entry. For example, holding all else constant, compared to a person on parole at age 18 to 24, the odds of revocation for a person on parole at age 25 to 39 are 36 percent lower, at age 40 to 64, 57.3 percent lower, and at age 65 to 100, 89.8 percent lower.

Table 2.3. Odds Ratios from Logit Model 1: Parole Revocation (versus Completion) and Logit Model 2: NNS Revocation (versus NS Revocation) with Robust Standard Errors Using Multiply Imputed Data

Outcome	Model 1 Revocation		Model 2 NNS Revocation	
	OR	95% CI	OR	95% CI
Intercept	0.838	0.714-0.984	1.648	1.191-2.282
New York	0.606	0.561-0.654	3.321	2.890-3.815
Parole Episode				
2nd Parole Episode	1.369	1.202-1.559	0.940	0.778-1.136
3rd Parole Episode	1.574	1.269-1.953	0.908	0.672-1.228
4th-6th Parole Episodes	1.186	0.824-1.705	1.117	0.657-1.899
Conviction Offense				
Drug	0.874	0.798-0.958	1.301	1.103-1.535
Public Order	0.580	0.53-0.635	0.961	0.815-1.134
Violent	0.621	0.557-0.692	1.150	0.940-1.405
Unspecified	0.588	0.355-0.973	1.038	0.450-2.367
Log Last Prison Time Served	1.121	1.049-1.199	0.926	0.816-1.052
Male	1.700	1.499-1.928	0.749	0.576-0.974
Race				
Black	1.283	1.194-1.378	0.803	0.706-0.913
Other Race	0.811	0.715-0.92	1.070	0.810-1.410
Age at Parole Entry				
Age Under 18	1.953	0.542-7.045	1.558	0.166-14.582
Age 25-39	0.640	0.591-0.694	1.267	1.103-1.456
Age 40-64	0.426	0.387-0.468	1.899	1.588-2.27
Age 65-100	0.102	0.058-0.179	1.764	0.359-8.659
Parole Episode x New York				
2nd Parole Episode x NY	1.765	1.511-2.06	1.608	1.244-2.079
3rd Parole Episode x NY	1.854	1.44-2.387	1.649	1.108-2.456
4th-6th Parole Episode x NY	4.586	3.001-7.006	1.539	0.802-2.951
N observations (N individuals)		19175 (13333)		8113 (5135)

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes: OR: Odds ratio, CI: Confidence interval, NNS: No New Sentence, NS: New Sentence
Base categories: Pennsylvania, Parole episode 1, Property crime conviction, Female, White, Age 18-24.

The logistic regression in Model 2 examines the likelihood of experiencing a revocation without a new sentence versus with a new sentence. It addresses whether the relationship between prior experiences of parole and subsequent revocation type look different in New York versus

Pennsylvania. Figure 2.4 presents the main results of Model 2 as relative risks. It shows that people in New York who experienced a revocation had a higher risk of a revocation for no new sentence relative to the risk for people who experience a revocation in Pennsylvania. This result is statistically significant at the 95 percent confidence level. For example, the baseline risk of revocation without a new sentence for Black people on parole for the first time in Pennsylvania is 0.480, and 0.754 in New York. The figure shows this as a relative risk of 1.571 (0.754/0.480). This may be reflective of policies in New York that allow more liberal use of imprisonment without a new crime compared to many other states. As Figure 2.4 shows, the results within race look very similar.

Appendix Figure B.2 shows the results as comparisons of the number of times a person has been on parole. In New York, being on parole more than once increased the risk of a revocation with no new sentence than a revocation with a new sentence, but this was not the case in Pennsylvania. This may reflect the statutory requirement in New York to consider past parole behavior in making the decision to revoke parole without a new sentence.

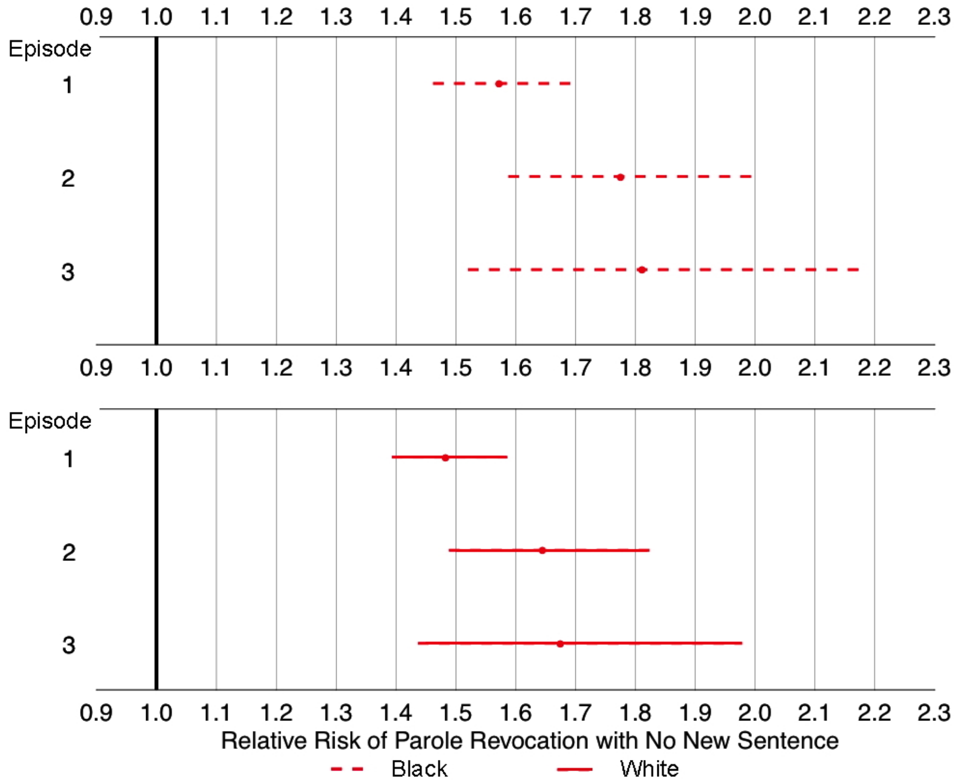

Figure 2.4. Model 2 results: Relative risk of NNS parole revocation by state, race, and parole episode (compared to Pennsylvania) among males, convicted of property offense, aged 18-24, having served mean prison term.

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes: Bars show 95% confidence intervals using the variance-covariance matrix from one imputed data set; results are the same across all data sets.

Figure 2.5 and Table 2.4 show the results of Model 3, the event history analysis estimating the transition probabilities of three potential outcomes from any parole episode: a revocation with no new sentence, a revocation with a new sentence, or a completion of parole. Although survival curves generally show the fraction remaining without experiencing an event, in a multi-state model, they show the fraction who have experienced the event of interest, as these curves are not depicting the people in the starting state of release to parole. Thus, Figure 2.5 presents the transition probabilities to the three outcomes, by race and US state, with the other covariates held constant (male, aged 18-24 at first parole entry, first convicted of a property offense).

The results of Model 3 are consistent with the results of the logistic regression, but this analysis highlights some of the nuances of the relationship between state and revocation type, although it does not distinguish between the number of times a person has been on parole as the logistic regressions did. With these plots it becomes clear that the probability of returning to prison for an NNS revocation was much higher in New York for any given parole episode. As Figure 2.5 shows, the probability of an NS parole revocation was much higher in Pennsylvania and this divergence began almost immediately after release. In line with Grattet, Petersilia, and Lin's (2008) California study, when distinguishing between revocation types, I find that while Black people on parole were far more likely than White people to return to prison with a new sentence, the difference was smaller for NNS revocations. The expected hazard of an NS revocation is 1.454 times higher for Black people as compared to White people, and the expected hazard of an NNS revocation is 1.160 times higher for Blacks compared to Whites, holding all else constant. The difference in race for both types of revocation was also smaller in Pennsylvania. Comparing all of these curves, the probability of an NNS revocation in New York is the highest. Similar to the previous analysis, parole completion (ending supervision) is higher in New York and there are increases in the probability of a completion at each year after release onto parole.

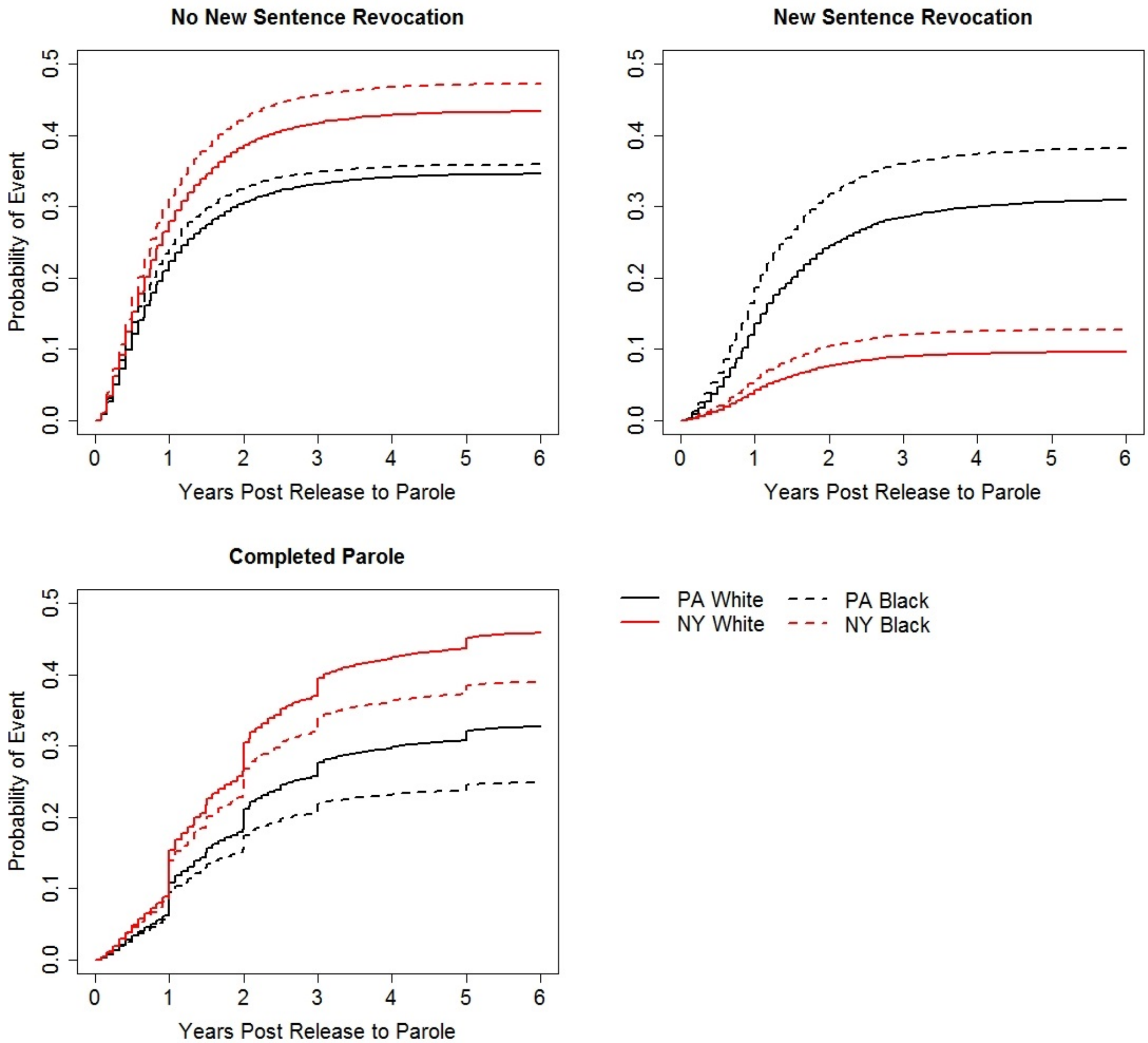

Figure 2.5. Probability of a revocation without a new sentence, with a new sentence, or parole completion by US state and race.

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes: The plot shows the probability of a person on parole experiencing an NNS revocation, NS revocation, or completing parole, by US state and race, for a male aged 18-24 at first parole entry, first convicted of a property offense. Estimates are from a competing risks model (Table 2.4).

Table 2.4. Hazard Rates from Competing Risks Model

	NNS		NS		Completion	
	HR	95% CI	HR	95% CI	HR	95% CI
New York	1.271	1.201, 1.345	0.315	0.287, 0.346	1.448	1.388, 1.512
First Conviction Offense						
Violent	0.763	0.712, 0.817	0.542	0.477, 0.616	0.722	0.680, 0.766
Drug	0.609	0.567, 0.654	0.630	0.557, 0.714	1.055	0.997, 1.116
Public Order	0.717	0.654, 0.788	0.623	0.529, 0.734	1.212	1.130, 1.301
Unspecified	0.447	0.335, 0.597	0.386	0.254, 0.588	0.710	0.588, 0.857
Male	1.434	1.292, 1.592	1.960	1.587, 2.420	0.811	0.762, 0.863
Race						
Black	1.160	1.096, 1.228	1.454	1.312, 1.612	0.948	0.907, 0.990
Other Race	0.747	0.685, 0.815	0.826	0.705, 0.969	0.792	0.746, 0.840
Age at Parole Entry						
Age Under 18	1.566	0.650, 3.769	1.435	0.202, 10.216	1.267	0.475, 3.379
Age 25-39	0.876	0.822, 0.933	0.626	0.563, 0.696	0.940	0.891, 0.992
Age 40-64	0.698	0.646, 0.753	0.340	0.296, 0.392	0.997	0.939, 1.057
Age 65-100	0.203	0.112, 0.367	0.080	0.020, 0.319	1.257	1.030, 1.534
N events	6165		1963		10579	

Source: Author's analysis of the National Corrections Reporting Program 2000-2015 data.
Notes: HR: Hazard rate, CI: Confidence interval, NNS: No New Sentence, NS: New Sentence
Base categories: Pennsylvania, Parole episode 1, Property crime conviction, Female, White, Age 18-24.

DISCUSSION

Due to the high rate of imprisonment in the United States, the number of individuals reentering society every year has ballooned in recent decades. As many people are released from prison to parole, it is imperative to understand what recidivism outcomes look like in the long term so we can consider how parole can support desistance. While the national prison population has slowly declined since 2009 after a 30 year-long explosion, the reincarceration rate for parole violations has continued to increase in some states such as New York and Pennsylvania (Alper 2016; Schiraldi and Arzu 2018). This study analyzes the experiences of individuals released onto parole in 2006 in New York and Pennsylvania, tracking all parole episodes that these individuals served through 2015.

This study seeks to address a complex research question. For people on parole, does the probability of returning to prison vary by the state they are in, whether they have previously been on parole, and an interaction of the two factors? My findings suggest that prior contact with the criminal justice system, and parole specifically, can increase the risk of parole revocation but this relationship interacts with the larger institutional context to produce different outcomes based on the state of jurisdiction. While this was not a causal study but a descriptive study of two states, there are several important findings: in the analyses examining the outcome of parole revocation versus completing parole, the results show that having been on parole previously increases the risk of a revocation in both states. However, the relationship with state of jurisdiction is more complicated. I find that people in Pennsylvania had a higher risk of revocation than people on parole in New York, but this heightened risk is only discernible among people on their first parole episode. In exploring the type of parole revocation, the analyses suggest that in both states people returned to prison for a revocation with no new sentence more quickly than for revocations with a new sentence, but being on parole in New York increased the probability of a revocation with no new sentence. This may reflect the stronger policy constraints in Pennsylvania that limit the use of prison as a sanction for technical violations. It may also reflect the Pennsylvania parole board's culture; comparative qualitative work on this topic would be highly useful. These analyses also show that the association between revocation type and prior parole experiences is contingent on whether a person is on parole in Pennsylvania or New York.

Taken together, the results show that states may be treating an individual's past interactions with the criminal justice system differently. The New York system may more closely watch individuals with more frequent criminal justice contact, with the result that people who find themselves on parole a subsequent time are being sent back to prison even without new sentences. What society might conclude about the state of recidivism cannot be measured with a one-time statistic about what percentage of prison releases end up with a return to prison.

These analyses illustrate how the complex flow between parole and prison can differ significantly across jurisdictions, even two geographically adjacent states with similarities in imprisonment and overall recidivism rates. The differences across the two states are substantial and surprisingly, even larger than Black-White differences in some cases. This may be an indicator that although racial differences remain, adjusting parole policy levers can lead to substantial changes for the whole. Although some of these state differences may also reflect variation in the culture and practices of parole officers as opposed to policy, these informal features are also part of the institutional context that can create variation in individual outcomes. Further qualitative research and policies regarding transparency and oversight may be warranted if implementation does not comport with changes to parole policy. Depending on how conscious jurisdictions are of racialized organizations as they implement parole, it may help mitigate or exacerbate racial disparities (Ray 2019).

Given these findings, it is clear that we should not generalize from research using non-representative samples to make blanket statements about how recidivism operates in the United States as a whole as some studies claim. What works in one context to reduce recidivism may not necessarily work in another. The differences are significant even when we focus on specific populations such as people released onto parole. Furthermore, this study suggests that the outcomes of a given parole episode are significantly different by the extent of one's previous experiences with the criminal justice system. The heterogeneity of experiences may point researchers, advocates, and policymakers to search for practices that are more beneficial for society keeping in mind the importance of institutional context and how this interacts with individuals' histories. This will be particularly important as discrimination throughout the justice system has contributed to racial disparities in the level of contact individuals have had with the criminal justice system.

Conversations about people in prison for parole revocations have become more widespread in the context of prison overcrowding and the transmission of the 2019 novel coronavirus. As health officials began confirming positive cases in New York State prisons, advocates called for the immediate release of people based on a variety of criteria, including people in prison for technical violations. In response, the state established new procedures to release incarcerated low-level technical parole violators as well as people detained in jail for alleged parole violations who pose little risk to public safety, but only so long as they had housing, which introduces yet another question of equity. Pennsylvania state prisons had a similar response to maximize parole releases. However, the problem of overcrowding has existed for many years prior to the global pandemic.

Releasing people who are in prison due to technical violations with only non-violent non-serious offenses is perhaps a politically palatable solution but will only go so far. Furthermore, given what my analyses show, if states are not willing to make investments that meaningfully support desistance and healthy communities instead of relying on high levels of surveillance and sanctions, the end result may be a continuation of the pernicious cycle of recidivism.

This study focuses on how parole interacts with prison, one of the harshest punishments of the criminal justice system. However, understanding the role of parole in incarceration will require more study. In particular, parole boards are more likely to use jail rather than prison as a sanction for more minor technical violations and the data used in this research do not include the jail population. It will be important to examine jail data to obtain a full picture of how individuals on parole may still be removed from their families and communities without appearing in prison data. In addition, research on parole tends to focus on single-state or local samples. This study shows that a comparison of just two states reveals large differences. Once reliable data exist in other states, a more comprehensive study may yield further insights. Lastly, the majority of people under community supervision in the United States are on probation rather than parole. Thus, once large longitudinal data of people on probation become available, a study of the flow of the roughly 3.5 million people on probation will be another way to understand how community supervision interacts with incarceration (Kaeble and Alper 2020).

Chapter 3. WHO IS EXCLUDED? EFFECT OF DRUG COURTS ON COMPOSITION OF GEORGIA'S DRUG POSSESSION PRISON ADMISSIONS

INTRODUCTION

Drug courts combine functions of the criminal justice system and drug abuse treatment systems. The aim is to reduce recidivism among individuals arrested for low-level drug-related offenses who suffer from substance abuse issues. Unlike traditional drug abuse treatment, drug courts have access to court-ordered rewards and sanctions to motivate adherence to abstinence and prosocial behaviors. In exchange for entering a long-term treatment program and community supervision, a person can avoid a jail or prison sentence (Belenko 1998; National Drug Court Resource Center 2020). Drug courts typically last between 12 to 24 months or longer if a person experiences sanctions, and fees can run to thousands of dollars during this period. Prior to entering many drug courts, participants must attest to their ability to afford the fees and they cannot move through the required phases of treatment or graduate without being current on these debts. Such high financial costs may create inequity by differentiating the type of people who can avoid incarceration based on their ability to pay.

The first drug court in the United States began in Miami in 1989 and since that time, drug courts have proliferated all across the country with over 3,000 drug court programs operating today in the U.S. and many more across other nations (Office of Justice Programs 2020). At year-end 2008, there were an estimated 116,300 drug court participants in the U.S. (Huddleston and Marlowe 2011). Research on drug courts is extensive. Huddleston and Marlowe (2011) suggest that the body of research on the effects of drug courts is larger than the literature on almost all other correctional programs combined. Researchers have conducted many studies that find that drug courts reduce recidivism (Aos, Miller, and Drake 2006; Downey and Roman 2011; Gottfredson et al. 2006; Latimer, Morton-Bourgon, and Chretien 2006; Lowenkamp, Holsinger, and Latessa 2005; MacKenzie 2006; Turner et al. 1999; Wilson, Mitchell, and MacKenzie 2006) and are cost-effective (Barnoski and Aos 2003; Belenko, Patapis, and French 2005; Bhati, Roman, and Chalfin 2008; Carey et al. 2006; Finigan, Carey, and Cox 2007; Loman 2004; U.S. Government Accountability Office 2005).

Most study designs compare drug court-eligible individuals who opt to participate or not, or match participants and non-participants on characteristics such as race, criminal history, and socioeconomic status. Yet scholars have not considered how the financial eligibility requirements for these programs lead to disparities in who can access participation in the first place. This study seeks to answer the question: do the high financial barriers associated with participating in drug courts lead to a population of people admitted to prison for drug possession that is more economically disadvantaged? In other words, I expect that once a jurisdiction implements a drug court, people who can afford to participate in drug court will be able to utilize this process to stay out of prison, leading to an increase in the overall level of economic disadvantage among people sent to prison for drug possession offenses. If so, this poses consequences regarding equity, as such disparities could further entrench racial and economic disparities that already exist in the criminal justice system by blocking access to an option of remaining out of prison from people who are already the most disadvantaged in our society.

To test my hypothesis, this paper uses Georgia prison admission data between 1991 through 2016 from the National Corrections Reporting Program in a difference-in-differences strategy exploiting time variation in implementation of drug courts across the state. Using this strategy, I compare the level of socioeconomic disadvantage as measured by educational attainment among people admitted to prison for drug possession before and after implementation. This paper finds that on average, the percentage of people admitted to prison for drug possession who have less than a high school degree did not increase significantly after drug court implementation. The paper proceeds as follows. The second section presents background on drug courts and how they operate in Georgia, the costs of participation, and the variation in implementation timing of these courts across jurisdictions. After describing the data and my estimation strategies in the third section, I estimate the effect of drug court implementation on the composition of education level among people admitted for drug possession in the fourth section. The fifth section presents robustness checks. The final section concludes by discussing findings, limitations, implications for equity and questions for future research.

Background

What Are Drug Courts?

As the War on Drugs swelled the US prison population and overloaded court dockets, governments began looking for alternative ways to approach low-level drug crimes. Drug courts emerged as an option designed to relieve some of this pressure (Sevigny, Fuleihan, and Ferdik 2013). Although the term “drug court” refers to a heterogeneous mix of specialized courts, in this paper, I focus on adult felony courts that target individuals charged with drug possession or nonviolent drug-related offenses who have substance abuse issues. In this way, I limit the focus to courts where non-participation is more likely to result in a prison sentence. In 2009, 54 percent of drug courts in the US were adult felony drug courts (Franco 2010).

Drug courts are highly popular in the US. The federal government budgeted $100 million in grants to support new and continuing drug courts in Fiscal Year 2018 (Office of National Drug Control Policy 2017; Substance Abuse and Mental Health Services Administration 2017), and promotes them abroad, particularly in Latin America (Schliefer et al. 2018). State and local governments provide the majority of funding for drug courts, and areas with more resources tend to implement these courts earlier. A survey of Statewide Drug Court Coordinators in 2008 noted that insufficient state and federal funding limited the ability to expand the availability of drug courts despite local interest in expansion (Huddleston and Marlowe 2011).

The argument for drug courts has centered around three aims: reducing relapse both in criminal behavior and drug use, saving taxpayer money, and promoting successful reintegration. In the standard adult felony drug court model, a team composed of the drug court judge, program coordinator, prosecuting attorney, defense attorney, community supervision officer, treatment providers, and law enforcement all work together to supervise the defendant going through the program (Marlowe, Hardin, and Fox 2019). Treatment includes not only the substance use disorder treatment itself, but also mandatory participation in other services such as those addressing mental health. Other components include abstinence from drugs and alcohol, frequent drug testing, adhering to supervision conditions such as curfew, obtaining employment or education, paying fines and fees, and completing other court sanctions such as community service (Marlowe et al 2019).

Although there are many differences even within adult felony drug court models, there are main components that are the same across courts. Arresting officers, prosecutors, and defense attorneys flag individuals as potential candidates for drug court and make referrals to a screening team (Franco 2010). Eligibility criteria can vary somewhat from court to court, but the core items are that the defendant needs to be charged and reside within the jurisdiction of the drug court, be 18 years of age or older, and cannot have had a previous violent or sexual offense conviction in the past. The current charge cannot involve a violent or sexual offense and must be a drug possession offense or other non-violent offense related to the individual's identifiable substance abuse issue (Franco 2010).

Adult felony drug courts typically include a treatment orientation period and five phases: stabilization, restructuring, transition, life skills, and aftercare, although some courts consolidate these into four phases. Each phase includes increasing levels of expectations (e.g., progressing from attending meetings on time to obtaining employment or education) and increasing magnitude of sanctions, although supervision requirements may lessen over time as well as a reward (Marlowe 2012). The drug court team discusses each participant's behavior to make recommendations but the judge makes the ultimate determination for sanctions and rewards (Marlowe et al. 2019). While the discretionary and tailored feature of the sanction may be an advantage over a mandatory punishment, the drug court team meeting is not on the official court record, which removes the possibility for the defendant to appeal the decision based on statements made during the negotiation (Paik 2011).

Within adult felony drug courts, there are a variety of dispositional models. In pre-plea models (also called deferred prosecution), participants enter the program without entering a guilty plea as part of a pre-trial diversion agreement where the charges are dismissed once participants graduate from the program (Huddleston and Marlowe 2011). This means a guilty plea does not appear on the participant's criminal record. Most programs have taken a post-plea approach, where the defendant must first plead guilty to enter the program (Marlowe et al. 2019). Within post-plea models, there are pre-adjudication programs (also called deferred sentencing), where the court suspends the guilty plea and then vacates or withdraws it when the defendant completes the program. In some jurisdictions, there may be provisions where the court will expunge the record if the defendant remains free of arrests for a period of time after the program ends (Huddleston and Marlowe 2011). The second post-plea model is a post-sentencing approach, where the guilty plea remains on the record, but the participant avoids incarceration or reduces their probation obligations (Huddleston and Marlowe 2011). In 2014, 6 percent of adult drug courts in the country used a pre-plea diversion model, 26 percent used a deferred sentencing model, 27 percent used a post-sentencing model, and 41 percent used a combination of the two post-plea models (Marlowe et al. 2019).

Do Drug Courts Work?

Many studies of drug courts have sought to estimate the program's effect on recidivism and measure cost effectiveness and the overall assessment has been positive (Aos et al. 2006; Barnoski and Aos 2003; Belenko et al. 2005; Bhati et al. 2008; Carey et al. 2006; Downey and Roman 2011; Finigan et al. 2007; Gottfredson et al. 2006; Latimer et al. 2006; Loman 2004; MacKenzie 2006; Turner et al. 1999; U.S. Government Accountability Office 2005). Two meta-analyses found that adult drug courts reduce the recidivism rate of participants compared to that of non-participants (Aos et al. 2006; Latimer et al. 2006) and other studies have found that drug courts are more cost-effective than the business-as-usual model, although in some cases only after considering the benefits of reduced recidivism (Barnoski and Aos 2003; Carey and Finigan 2004; Loman 2004; U.S. Government Accountability Office 2005). However, some studies have cautioned that without fidelity to the program model drug courts may have no effect or may increase recidivism (Lowenkamp et al. 2005; Wilson et al. 2006).

In spite of the large body of research finding positive results of the drug court model, there have been criticisms that the existing studies are not asking the right questions to determine whether drug courts should be expanded. One concern is that drug courts lead to net-widening and increase arrests for drug offenses (Drug Policy Alliance 2014; O'Hear 2009; Schliefer et al. 2018;

Walsh 2011). Drug courts tend to entail higher levels of supervision and much more frequent drug testing and monitoring than other forms of community supervision, which may lead to a higher probability of detecting violations or crime (Belenko 1998; Gottfredson et al. 2006). Several studies have suggested that while drug courts reduce the incidence of incarceration, the long sentences associated with program failure offset these reductions and for some, actually increase the length of time a person would have spent if they had gone through the traditional court process (Rempel et al. 2003; Shelli B Rossman, Roman, Zweig, et al. 2011:70; Sevigny, Fuleihan, et al. 2013). In this way, drug courts may not actually reduce the aggregate experience of incarceration or correctional costs, although according to one meta-analysis, this result varied based on program characteristics such as supervision intensity (Sevigny, Fuleihan, et al. 2013). While best practices in substance use treatment suggest that service providers should operate under the assumption that relapse is an expected phenomenon on the long path to recovery, court models operate under a philosophy that relapse should entail an increase in supervision intensity or other sanction (Marlowe 2012; World Health Organization 2008).

Some scholars have made recommendations for changes to existing drug court implementation to make drug court more useful and not recreate existing disparities. Instead of focusing solely on the defendant's accountability to the court, Paik (2011) recommends that courts consider their own accountability to the defendant to provide them with assistance in completing the program. This would include such considerations as tailoring the program to motivate prosocial behavior based on the individual's interests and goals, conducting upfront mental health evaluations to identify issues that need to be addressed first or that would make drug court ineffective, and offering non-punitive alternatives to defendants who are unlikely to succeed in drug court. On a more holistic level, drug courts may have the best outcomes if they are restructured to have a positive impact on communities through the inclusion of restorative justice principles instead of focusing on maximizing the number graduates (O'Hear 2009). However, this may be more suitable for people charged with drug trafficking that possession.

Notwithstanding the many evaluations of the drug court model, a key piece of evaluating the effect of drug courts is missing. Existing studies have primarily focused on the effects of drug courts on people who are eligible to participate, without considering who is excluded. The few studies that have noted that restrictive eligibility requirements exclude people based on a variety of criteria do not include high program fees in their critique. For example, Sevigny, Pollack, and Reuter (2013) point to laws that preclude drug courts from receiving federal drug court funds if a drug court allows people with a current or prior violent offense to participate; the vast majority of active drug courts receive federal funding. National surveys have noted that drug courts commonly restrict access based on additional factors such as criminal history, substance abuse treatment history, severe mental health disorders, and personal motivation or readiness for change (General Accounting Office 1997; Shelli B. Rossman et al. 2011). Such restrictions may introduce racial disparities to access, as people of color have more extensive records due to institutional racism in policing and the criminal legal process (Beckett et al. 2006; Kutateladze et al. 2014; O'Hear 2009).

Financial Barriers to Access

Drug courts and contracted treatment providers rely on participant fees to cover operating costs. One of the touted benefits of drug courts over traditional court processing and confinement of people convicted of felony drug possession offenses is that it saves taxpayer money partly by making participants responsible for their own mandated rehabilitation. Program fees vary from jurisdiction to jurisdiction, but given reports from the Survey of Household Economics and Decision-making that showed that 39 percent of adults in the US in 2018 could not weather a 400 dollar unexpected expense (50 percent in 2013), most drug court fees are more than enough to disqualify potential participants (Board of Governors of the Federal Reserve System 2019). Drug courts may also introduce other financial barriers to access. One multi-site drug court study noted that one of their drug courts did not allow people with restitution over $1,500 to participate because full payment is a graduation requirement (Shelli B Rossman, Roman, and Rempel 2011b).

While judges may waive some discretionary court fees for indigent defendants, courts and the National Drug Court Institute cite State v. Paige (880 N.E.2d 675, 675 (Ill. App. 2007)) as the basis for denying the right to a financial waiver specifically for drug court program fees (Marlowe and Meyer 2017). The court characterized the assessment of drug court fees as a fine that was rationally related to the crime of drug possession and not a fee, drawing the distinction between fees, which are intended to cover the costs incurred in prosecuting a defendant, and drug court fees, which are intended "for the operation and administration of the drug court" (55 Illinois Compiled Statutes 5/5-1101(f)). Thus, as it was a fine and not a fee, the court denied the financial relief that the plaintiff requested. Not all courts across the country deny financial waivers and some offer community service in exchange for some of the fees (Shelli B Rossman, Roman, and Rempel 2011b). While some state and county courts have ruled that drug court termination solely for inability to pay is unconstitutional,[8] the question of entry has had mixed rulings.[9] However, although some courts deemed that termination from drug court altogether solely for indigence was unconstitutional, a sanction that involves incarceration (jail) is allowable. In addition, court have determined that attending drug court is not a right in any jurisdiction, counties are not required to

[8] People v. Trask, Cal. App. 4th (2010); People v. William Andrew Henry, Cal. App. (2012 - UNPUBLISHED)
[9] Mueller v. State, NE 2d (2005); State v. Anderson, P. 2d (1984)

implement a drug court, and lack of a drug court in a county does not give the right to access a drug court in a neighboring county.[10]

Studies that compare drug court participants and non-participants note that they address issues of selection (e.g., differential motivation) by looking only at individuals eligible for drug court (Carey and Finigan 2004), without considering the fact that the eligible population is not representative of a larger target population: people convicted of drug possession who have substance abuse issues. Drug court professional associations have at times raised the issue of racial and ethnic minorities being underrepresented in drug courts and suggested this may be because the programming is not marketed well towards these populations (Huddleston and Marlowe 2011; Marlowe et al. 2019; National Association of Drug Court Professionals 2019). To the best of my knowledge, there are no studies pointing to the high financial costs of drug courts to the individual as a potential barrier to access. Consequently, there are no studies that examine how drug courts may actually be exacerbating existing economic inequity in prison admissions for low-level drug crimes by offering a pathway out of prison only to people who can afford the fees.

Georgia Context

Georgia provides a suitable context to test the effect of drug courts on the level of socioeconomic disadvantage of people admitted to prison for drug possession. Between 1991 and 2016, 45 court jurisdictions in Georgia implemented drug courts covering 111 counties. Figure 3.1 shows the timing of implementation across the state during my period of observation, 1991 through 2016.

1			1	2			3	5	2	3	2		2	3	2	2	4		7		2	4
1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016

Figure 3.1. Number of drug courts implemented in Georgia by year between 1991 and 2016.

[10] Abdul-Akbar v. Department of Corrections, F. Supp. (1995); Lomont v. State, NE 2d (2006)

The roll out of drug courts across Georgia was not random, but likely related to a jurisdiction's level of funding devoted to criminal justice. While federal funding may assist in starting a drug court, it still requires commitment on the part of local jurisdictions to apply for federal grants and to meet all the contingencies of funding. Drug courts also necessitate fairly strong support across various institutions and establishing a drug court involves cooperation between the court, treatment providers, and law enforcement. Although many forces are involved in pushing a jurisdiction to adopt a drug court, I do not expect the timing of drug court implementation to have been a response to changes in the socioeconomic composition of people sent to prison for drug possession. There are no features of Georgia drug courts that are aimed at reducing economic inequality of people sent to prison. In other words, I do not expect reverse causality to be at play.

In some states, drug possession without intent to distribute would rarely result in prison time and prison admission-level data would be inappropriate. However, Georgia has particularly strict and punitive laws that result in prison time for drug possession. The list below presents some of the state's possession penalties during my period of observation, as defined in Georgia Code §16-13 and summarized by FindLaw (2018).

Controlled substance classification (O.C.G.A.§16-13-24)

- Schedule I: Drugs with a high potential for abuse and no accepted medical use.
- Schedule II: Drugs with a high potential for abuse and the potential for psychological or physical dependence that have accepted medical uses under severe restriction.
- Schedule III: Drugs with a lower chance of abuse, low or moderate potential for psychological or physical dependence and an accepted medical use.
- Schedule IV: Drugs with a lower chance of abuse, limited potential for psychological or physical dependence, and an acceptable medical use.
- Schedule V: Drugs with the lowest potential for abuse, limited potential for dependence, and accepted medical use.

Penalties (O.C.G.A.§16-13-30) Possession of any amount (except marijuana) is a felony.

- Possession of any Schedule I or narcotic Schedule II drugs: punishable with 2-15 years in prison. Subsequent convictions are punishable with up to 30 years in prison.

- Possession of non-narcotic Schedule II drugs: punishable with 2-15 years in prison. Subsequent convictions are punishable with 5-30 years in prison.
- Possession of Schedule III, IV, or V drugs: punishable by 1-5 years in prison. Subsequent convictions are punishable with 1-10 years in prison.

Marijuana penalties (O.C.G.A.§16-13-2)

- Possession of 1 oz or less is a misdemeanor punishable with 1 year in prison and a fine of up to $1,000.
- Possession of more than 1 oz is a felony punishable by up to 10 years in prison with 1 year mandatory and fines of up to $5,000.

Although Georgia drug possession laws have changed in several ways since the early 1990s, changes are statewide and they do not influence how drug court implementation might change the level of socioeconomic disadvantage of people admitted to prison for drug possession. For example, HB 1176 in 2012 reduced prison sentence lengths for Schedule I and II drug possession, but did not reduce any of the penalties to non-prison punishments. In April 2015, Haleigh's Hope Act made low-THC cannabis oil legal to use for a limited list of medicinal purposes. However, as it was still against the law to buy or grow marijuana in the state, patients could only obtain the oil in states where it was legal, then violate federal law if they transported it back to Georgia. In 2019, HB 324 changed this law to allow the purchase of medical marijuana in Georgia, but this change falls outside my period of observation, which is between 1991 through 2016.

Local possession law changes would be problematic to identifying the effect of drug courts on the types of people sent to prison for possession in Georgia. Of the nine cities or counties that reduced penalties for possessing one ounce or less of marijuana to non-prison punishments, only one occurred during the study period. The city of Clarkston reduced the penalties of possessing one ounce or less of marijuana from one year in prison to a fine in July 2016 (Code of Ordinances 12-26). However, municipal code is beneath state law, meaning law enforcement still has the option to charge a person under the state law. In addition, as Clarkston had a population of 12,900 in 2016 in a county of 747,000, it is unlikely that this local law would divert enough people away from prison in the year of policy implementation to appreciably affect my analyses (US Census Bureau Subcounty Resident Population Estimates 2020; US Census Bureau Annual Estimates of the Resident Population for Counties 2020). Atlanta reduced penalties for possession of one ounce or less of cannabis in 2017 (Code of Ordinances 106-182), followed by the cities of Savannah (Code of Ordinances 9-1026), South Fulton (Code of Ordinances 15-1006), Forest Park (Code of Ordinances 11-1-25), Kingsland (Code of Ordinances 15-54), and Statesboro (Code of Ordinances 58-12) in 2018, and the city of Chamblee (Code of Ordinances 58-146) and Macon-Bibb County (Code of Ordinances 16-9) in 2019.

Figure 3.2 shows Speir et al.'s (2013) analysis of Georgia's Computerized Criminal History data. It shows that the percent of all arrests that include at least one drug charge (misdemeanor or felony) between 1990 and 2011 stayed relatively stable, between 11 percent and 15 percent of all arrests. However, after removing misdemeanor marijuana possession charges, the number drops from 10 percent to six percent between 2003 and 2011 (Speir et al. 2013). This makes two points. One, Georgia does charge people for possession of small amounts of marijuana, and two, if it had not done so during that period, there would have been a decline in arrests that included a drug charge. As the penalty for even misdemeanor marijuana possession charges can be a year in prison, even as more serious drug crime drops, many may still be referred to drug court. Analysis of national data shows that even including DUI courts (excluded from this study), marijuana was the primary substance of abuse among 22 percent of adult participants in urban adult drug courts, with lower rates in suburban (8 percent) and rural areas (10 percent) (Marlowe et al. 2019).

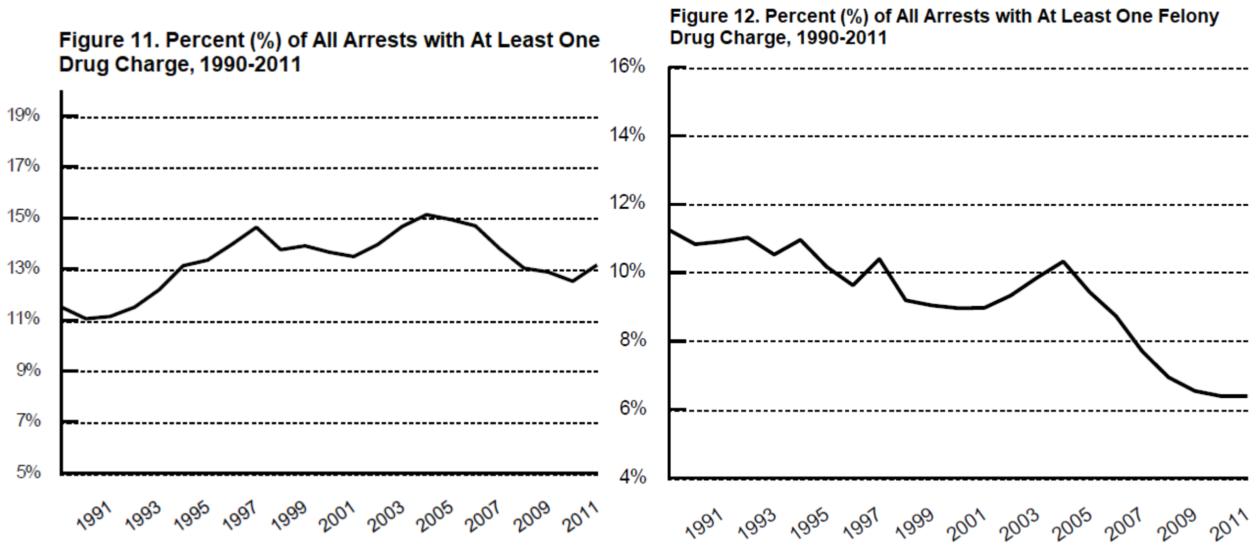

Figure 3.2. Percent of Georgia arrests with at least one drug or felony drug charge, 1990-2011. Source. Figures from Speir et al. (2013:11–12)

Appendix Figure C.1 shows example drug court fees in Georgia, which range from $1,000 to $3,650 over the two-year program period if the defendant completes all requirements on time, and can be much higher if a defendant experiences sanctions and delays. Several Georgia drug court participant handbooks detail that late participant fees result in an inability to transition to the next phase of the program, and one court handbook states that the sanctions for fees that are over $150 and over $300 past due are eight hours of community service and incarceration until fees are paid in full, respectively (Appendix Figure C.2). Inability to progress on time can result in higher fees overall, because as Appendix Figure C.1 showed, fees are attached to the number of months a person is in the program, not a fixed price for the program itself. In many drug courts across the country, participants must sign a contract before enrollment that includes an attestation that they will pay program fees on time (Franco 2010; Shelli B Rossman, Roman, and Rempel 2011b). Appendix Figure C.3 shows this is the case in Georgia with examples of this form from Georgia drug court participant handbooks. In addition, one participant handbook from a Georgia drug court explicitly states that failure to satisfy financial obligations is a violation that can result in termination.

If some jurisdictions do not impose high enough participation fees to exclude anyone from participating while others do, this would cause a downward bias in my results. However, even if a person were able to pay the initial program and drug testing fees, Georgia drug courts require participants to be current on other financial obligations, such as traditional court monetary sanctions, child support, and victim restitution. Such debts can exacerbate existing financial insecurity and thus introduces an additional, possibly even larger financial barrier to access (Cancian, Heinrich, and Chung 2009; Harris 2016). Although the exact level of child support involvement among people with felony convictions is unknown, of the more than five million men under correctional supervision in the United States, over half have open child support cases (Haney 2018; Sorensen, Sousa, and Schaner 2007).

DATA AND IDENTIFICATION STRATEGY

Data

This chapter uses prison admission data from the Bureau of Justice Statistics' National Corrections Reporting Program (NCRP) for Georgia in the years between 1991 and 2016. I include only prison admissions for drug possession/use and other non-trafficking drug offenses such as forging prescriptions and possessing drug paraphernalia. Due to drug court eligibility restrictions on having prior violent felonies, I remove all individuals who were ever previously admitted into Georgia prisons for violent offenses since 1971, when the data begin. Georgia also provided data to the NCRP on the state of the prison admission’s last known address. Thus, I remove anyone who has a last known address not in Georgia, due to the residency and mobility restrictions in drug courts. Figure 3.3 presents the total number of prison admissions for drug possession across the state between 1991 and 2016. It shows that these prison admissions fluctuated over time, with a drop and flattening after 2006. As

Figure 3.2 showed, arrests including a felony drug arrest charge dropped at about the same time.

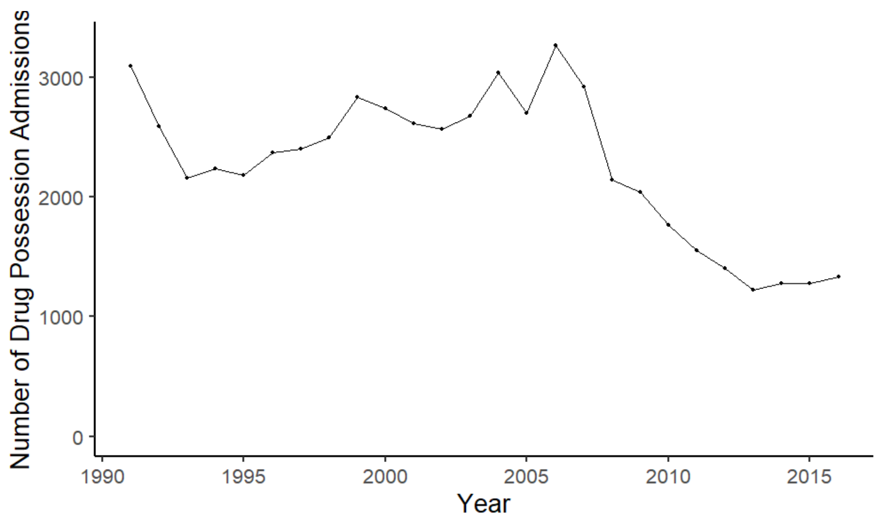

Figure 3.3. Felony drug possession prison admissions in Georgia, 1991-2016.
Source: Author's analysis of National Corrections Reporting Program, 2000-2016 data.

If high program costs lead to economically disadvantaged people being unable to avoid prison, then the proportion of people in prison for drug possession who are economically disadvantaged should increase after the implementation of a drug court. Thus, the outcome variable in my analyses is the percent of socioeconomically disadvantaged people who are admitted to prison for a drug possession offense in a particular year. While the prison data do not include economic status indicators such as a person's income or occupational status prior to imprisonment, they include the highest grade and educational degree completed, which is a commonly-used proxy for socioeconomic status. This measure is more stable across time than income, although it is a step removed from ability to pay. Georgia consistently provides the education information of people admitted to prison, with less than 0.5 percent of observations missing this information in the full admission-level data. I define socioeconomic disadvantage as having less than a high school degree or GED and drop all observations of people under 18 years of age.

The data include the county of jurisdiction associated with a prison admission, which is where an individual could access a drug court. Some drug courts have jurisdictions that cover multiple counties, but county boundaries and drug court jurisdiction boundaries are consistent, making aggregated county jurisdiction data appropriate for estimating the effect of drug courts in various court jurisdictions. Although prisons report data to the NCRP data monthly, I aggregate the data to the year level because some courts have no admissions for drug possession in some months, making it impossible to calculate a change in the composition of people admitted to prison. Some jurisdictions are very small; Georgia is composed of 159 counties, of which 33 had fewer than 10,000 residents in 2016 (US Census Bureau Annual Estimates of the Resident Population for Counties 2020).

Drug court jurisdiction-level information on economic and demographic controls come from aggregated county-level data. Annual data on total population and households, number of households with children that are female-headed with no husband present, and number of people over age 25 with less than a high school degree/GED come from the decennial Census and American Community Survey. The annual size of the population identified as Black come from the National Center for Health Statistics bridged Census race data, monthly unemployment and labor force numbers come from the Bureau of Labor Statistics, and the annually reported number of sworn police officers come from the Law Enforcement Employees Report from the Uniform Crime Report's Law Enforcement Officers Killed and Assaulted Program.

Information on the timing of drug court implementation come from the 2019 court directory of the Council of Accountability Court Judges of Georgia (Cawood 2019) and the Council's Adult

Felony Drug Court directory (Cawood 2016). The state legislature created the Council to establish standards and practices across the state's drug court divisions (Council of Accountability Court Judges of Georgia). I cross-reference the implementation date where possible through online searches and find no inconsistent dates. In order to match the prison data, I transform the drug court implementation month to implementation year. In any court, it takes time between the time a person is arrested and processed through the court. Thus, I lag the implementation of the drug court by one year for the two-way fixed effects models.

Specifications

To estimate the effect of drug court implementation on the education levels of people admitted to prison for drug possession, I employ a difference-in-differences strategy exploiting the variation in implementation timing. I compare the percent of people admitted to prison for drug possession who have less than a high school diploma or GED in jurisdictions and years where a drug court was in effect to jurisdictions and years where a drug court was not in effect. I also conduct a secondary analysis with jurisdiction-specific linear time trends. Equation 3.1 gives the two-way fixed effects estimator:

$$Y_{jt} = B_0 + B_1 haveDC_{jt} + X_{jt}\Upsilon + \sigma_j + \tau_t + \psi_{jt} + \varepsilon_{jt} \tag{3.1}$$

where Y_{jt} is the percent of people admitted to prison for a drug possession who had less than a high school degree or GED in jurisdiction (j) in year (t);

$haveDC_{jt}$ is an indicator of 1 when a jurisdiction had a drug court in place in a particular year and 0 otherwise;

X_{jt} is a vector of observable characteristics that vary at the county/year level that may affect Y_{jt} or could be correlated with whether a county had a drug court: the percent of residents 25 years and over in the jurisdiction who had less than a high school degree/GED, percent of households with children in the jurisdiction who were headed by a woman with no husband present, percent of the population in the jurisdiction who were Black, percent of the population in the jurisdiction who were below the poverty line, total number of sworn police officers in the jurisdiction, and unemployment rate. I also included a full set of drug court fixed effects, σ_j, and year fixed effects, τ_t, to control for observable heterogeneity between treatment and control groups. ε_{jt} is an error term. In the second step of my modeling, I also add jurisdiction-specific linear time trends, ψ_{jt}, to account for change within individual jurisdictions.

I use clustered standard errors, clustered on drug court jurisdiction, with bias-reduced linearization (Bell and McCaffrey 2002; Pustejovsky and Tipton 2016). In addition, I use weighted least squares to more heavily weight the courts with a higher total number of admissions for drug possession. There is a high level of variation in the number of prison admissions across courts (see Table 3.1 in the Results section for summary statistics).

One of the primary difficulties with the traditional difference-in-differences approach to causal identification is meeting the parallel trends assumption (PTA). In order for the estimates to be valid, the average change in the court-years with no drug court (control group) needs to represent what would have happened in the court-years with drug courts (treatment group) in the absence of implementation. The PTA is an untestable assumption, but looking at the outcome variable graphed over time and comparing the trends prior to drug court implementation across courts can be an indicator of whether it is likely that the data met this assumption. Figure 3.4 and Figure 3.5 show these graphs, ordered by implementation timing. The figures also show the median annual number of drug possession prison admissions per court. There were 45 courts that implemented a drug court between 1994 and 2016. I show here only the earliest and latest adopters.

Appendix Figure C.4 presents the remaining middle adopter courts and Appendix Figure C.5 presents the jurisdictions that did not implement a court during this period. The first set of graphs in Figure 3.4 that show the twelve earliest adopters also include the largest of courts, while the second set that show the twelve latest adopters include some of the smaller courts. This is consistent with the assertion that larger and better resourced jurisdictions tend to implement earlier. Although there is some variation in the pre-period trend in the large courts, most look fairly similar. However, the smaller courts show much higher variation, supporting the need to include the weights and suggesting that the PTA may be an issue in the smaller drug courts.

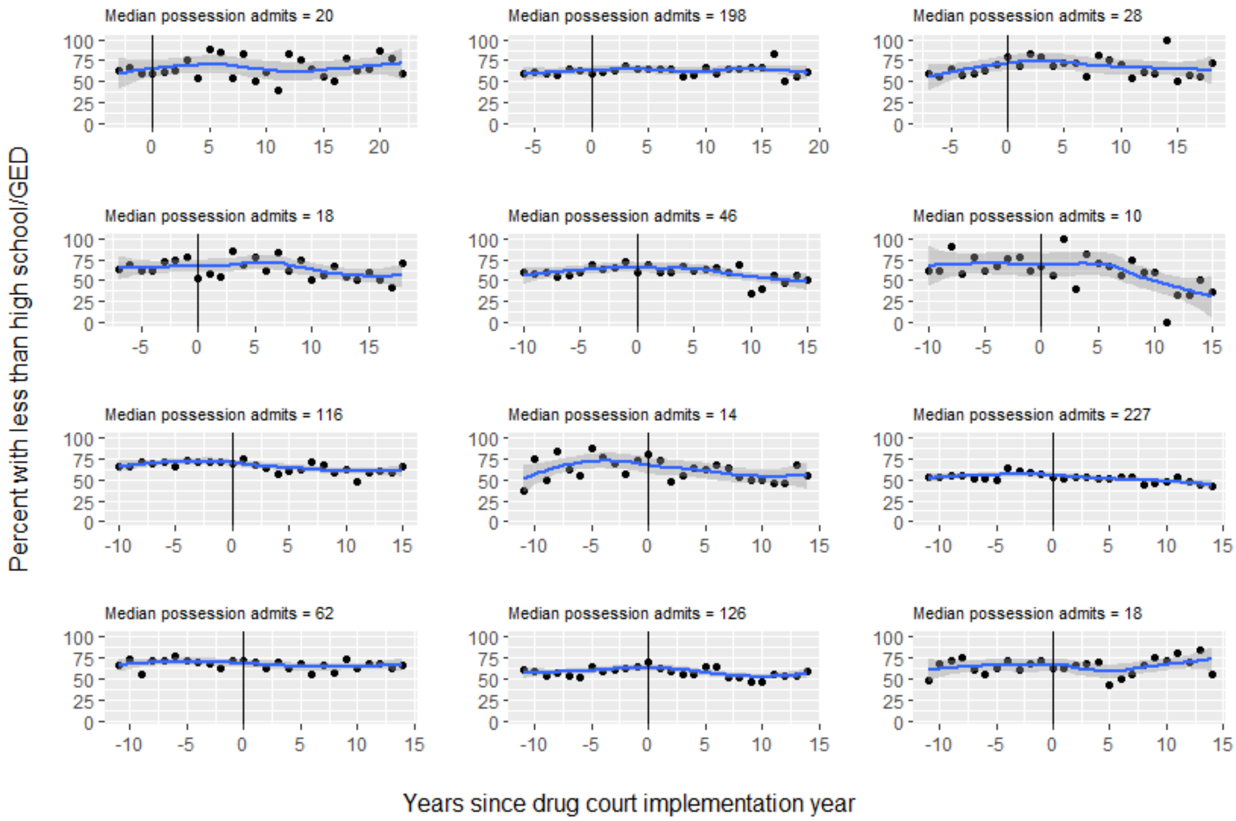

Figure 3.4. Georgia drug courts 1991-2016: Educational attainment of people admitted to prison for drug possession by drug court implementation year – Early adopter courts.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Vertical line represents timing of drug court implementation. Smoother is a loess curve.

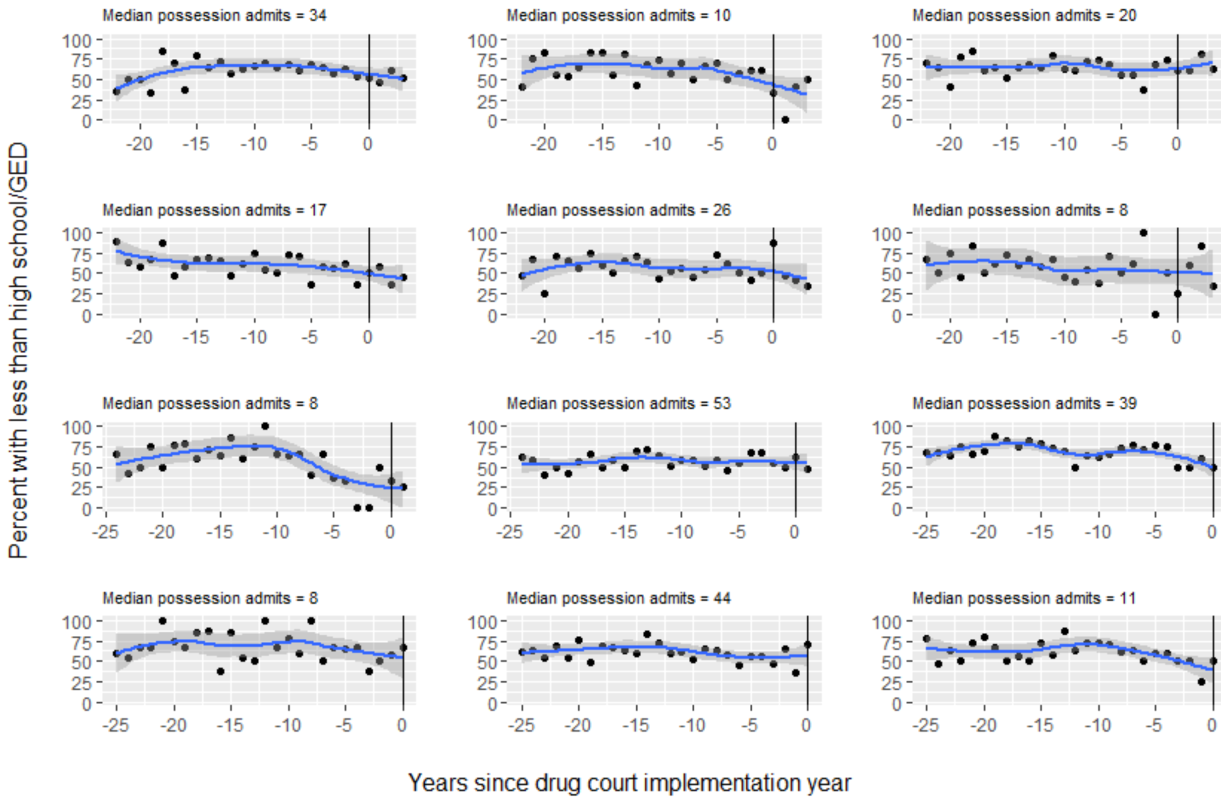

Figure 3.5. Georgia drug courts 1991-2016: Educational attainment of people admitted to prison for drug possession by drug court implementation year – Late adopter courts.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Vertical line represents timing of drug court implementation. Smoother is a loess curve.

Economists have long used the two-way fixed effects specification to exploit variation across groups that receive treatment at different times as a method of causal identification. However, in recent years this method has received some criticism due to its interpretability and application. Unlike the traditional two-period-two-unit condition, one cannot simply interpret the primary coefficient of interest, B_1 in Equation 3.1, as an average treatment effect unless the treatment effects are homogeneous, which is unlikely in many social science applications. Instead, this coefficient is a "weighted average of all possible two-group/two-period [difference-in-differences] estimators in the data", which is not intuitively interpretable and does not appropriately summarize time-varying effects (Goodman-Bacon 2019). The weights are proportional to the group sizes and the variance of the treatment (how close to the beginning or end of the panel the treatment goes into effect, or in this case, that the jurisdiction implements a drug court). This can generate bias in B_1 in different directions based on whether the treatment trends are constant (variance-weighted treatment effect with middle-panel groups having more weight), change monotonically (biased away from true treatment effect), or are differential across groups (biased within each unit in proportion to how close it is to the middle of the panel) (Baker 2019; Goodman-Bacon 2019).

To address this issue as well as that of the PTA, I supplement the traditional two-way fixed effects analyses with a method proposed by Callaway and Sant'Anna (2019). Several other scholars have proposed remedies to apply to particular contexts (Abraham and Sun 2020 (event study context); Cengiz et al. 2019; Goodman-Bacon 2019 (diagnostic test); Imai and Kim 2020 (assumes treatment can turn on and off)). Callaway and Sant'Anna's (2019) method is most appropriate for these data because it allows estimation in cases where the parallel trends assumption is met only after conditioning on the covariates, is flexible in that it applies beyond the event study context, and assumes irreversibility of treatment.

Instead of calculating one single difference-in-differences parameter, Callaway and Sant'Anna's (2019) method calculates "group-time average treatment effects", where groups are defined by when the unit was first treated. The control group for each year-group is defined as the drug courts that have not yet been treated and those that are never treated during the window of observation. Their method involves a two-step strategy. The first step takes a propensity score approach and involves calculating the probability that a unit is treated conditional on the covariates and conditional on being in the control group (not yet or never treated) or a particular group-time (units first treated at the same time). The second step is to calculate the group-time average treatment effect using a non-parametric identification strategy to allow for treatment effect heterogeneity. For each group-time (year-court jurisdiction), I calculate a simple weighted average of the difference in the outcome variable where the observations in the control group that are similar on the covariates are up-weighted. These group-time average treatment effects can then be aggregated either by timing-group, length of exposure, or calendar time.

One problem this method poses is that the first step is difficult to estimate when there are small sample sizes. Although there are some years where few drug courts went into effect (small sample is defined as having any timing-groups with fewer than five observations), there are only two years with only one court implementation. Callaway and Sant'Anna (2020) suggest that while having fewer than five observations in each group is not ideal, by focusing only on the aggregated treatment effect parameters, statistical inference results should still be reliable. However, the presence of years with few observations also causes estimation issues and reduces the number of covariates I can use to compose my conditional parallel trends. I use bootstrapped standard errors clustered on drug court jurisdiction.

The three jurisdiction-level covariates I select to compose the conditional parallel trends include the following: the percent of people over age 25 with less than a high school degree/GED, the number of sworn officers per capita in the jurisdiction, and the percent of households with children that are female-headed with no husband present. I selected the first two based on the direct relationship with the outcome variable and as a measure of law enforcement resources in the area, which is likely related to the number of arrests for drug possession and whether a jurisdiction would have a drug court. The percent of female-headed households with no husband present consistently had a high level of statistical significance across two-way fixed effects models.

I also conducted an integrated moments test to check for parallel trends in all the pre-treatment time periods for all groups using the data with no covariates, and with the three covariates. The test rejected the null hypothesis of parallel trends in the unconditional case (p=0.001) and did not reject the null hypothesis when including the three covariates (p=0.082).[11] This suggests that the PTA may only hold conditional on covariates, although this is only a test of the pre-treatment time periods. This is a better test of pre-trends compared to the traditional event study, as the event study can be unreliable in cases with treatment effect heterogeneity (Abraham and Sun 2020).

RESULTS

Table 3.1 presents the summary statistics with separate columns grouping courts by their drug court implementation year. The pre-drug court statistics show means across all court-years prior to implementation; thus, the number of years of data a jurisdiction contributes to these numbers depends on the timing of implementation. Jurisdictions that implemented a drug court earlier contribute fewer years of data to the pre-drug court statistics and more years to the post-drug court statistics. The jurisdictions that did not implement a drug court during the period of observation are presented in a separate column.

The outcome variable, percent of drug possession admissions with less than a high school degree or GED, decreased between pre- and post-drug court implementation, but the population as a whole also saw decreases in the percent of people with less than a high school degree or GED during this time. The percent of all prison admissions that are for drug possession was higher in the pre-drug court years. Among variables presented as percentages, all t-tests of the differences between pre- and post-drug court years were statistically significantly different at the $p < 0.001$ level with the exception of the percent of people admitted to prison for violent offenses who have

[11] The null hypothesis of this test is that the conditional expectation of the error term is zero with probability 1, given the independent and all lagged dependent and independent variables (Bierens and Wang 2012).

less than a high school degree ($p = 0.81$). One drug court jurisdiction is particularly populous and was also the second to implement a court. As expected, removing this drug court substantially decreased the standard deviation of the absolute number of drug possession admissions and prison admissions (Appendix Table C.1). However, removing the court did not change the other summary statistics or any of the main effects of the regressions.

Table 3.1. Summary Statistics: Prison Admission and Drug Court Jurisdiction Data

	Pre-DC	Post-DC	No DC
Prison admission characteristics			
% drug possession admits with less than HS	63.50	59.35	63.08
(SD)	(12.90)	(13.64)	(17.88)
% violent offense admits with less than HS	68.62	68.46	68.80
(SD)	(9.71)	(10.86)	(11.18)
% of all admits for drug possession	15.04	11.48	11.00
(SD)	(6.40)	(6.25)	(4.62)
Number of drug possession admissions	44.23	45.76	19.33
(SD)	(70.22)	(66.12)	(16.06)
Number of prison admissions	269.19	377.59	165.91
(SD)	(297.85)	(359.47)	(96.93)
Drug court jurisdiction characteristics			
% over age 25 with less than HS	22.49	18.76	25.14
(SD)	(7.15)	(6.24)	(6.04)
% female-headed households	23.34	27.50	26.04
(SD)	(9.28)	(9.78)	(5.74)
Sworn officers per capita	0.27	0.35	0.24
(SD)	(0.12)	(0.26)	(0.08)
% Black population	25.40	28.71	25.72
(SD)	(15.57)	(18.90)	(9.28)
% below poverty line	15.52	17.86	20.12
(SD)	(6.06)	(5.55)	(5.05)
Unemployment rate	5.73	7.12	6.80
(SD)	(2.32)	(2.44)	(2.51)
Number of court-years	738	432	338
Number of courts	45	45	13

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: DC = drug court. No DC = jurisdictions that did not have a drug court during the period of observation 1991-2016. Less than HS degree = Less than high school degree or GED. % female-headed households = percent of households with children that are female headed with no husband present.

Table 3.2 presents the main results of the two-way fixed effects models. Model 1 includes only jurisdiction and year fixed effects, while Model 2 adds the jurisdiction-specific linear time trends. In neither model is the primary variable of interest, whether a jurisdiction had a drug court, statistically significant at $p < 0.05$, although the direction is consistent with expectations.

Table 3.2. Results of Two-Way Fixed Effects Analyses of Drug Court Implementation on Percent of Drug Possession Prison Admissions with Less than a High School Degree/GED

	Model 1		Model 2	
	Coef.	95% CI	Coef.	95% CI
DC implemented	1.371	-0.221, 2.963	0.261	-1.219, 1.741
% pop over age 25 with less than HS	0.253	-0.109, 0.615	0.02	-0.419, 0.459
% female-headed households	0.203	-0.231, 0.637	0.575	0.205, 0.946
Sworn officers per capita	2.533	-3.136, 8.202	0.034	-6.391, 6.458
% Black population	0.11	-0.077, 0.297	-0.164	-0.497, 0.169
Unemployment rate	0.471	-0.435, 1.377	0.346	-0.489, 1.181
% below poverty line	-0.121	-0.572, 0.33	-0.076	-0.407, 0.256

Source: Author's analysis of National Corrections Reporting Program, 2000-2016 data.
Notes: Number of courts = 58. Number of court-year periods = 1508. DC = drug court. Coef = coefficient. CI = confidence interval. Estimation uses weighted least squares weighted by total number of admissions for drug possession. Standard errors are clustered on drug court jurisdiction and use bias-reduced linearization. Model 1 includes year and drug court jurisdiction fixed effects and Model 2 adds jurisdiction-specific linear time trends.

Table 3.3 presents the main results of the group-time average treatment effect models. As noted in the Specifications section, statistical inference from group-time average treatment estimates is tenuous with small group sizes, but aggregated treatment effect estimates are still reliable. Thus, I present only aggregated estimates. Under the unconditional parallel trends assumption, the aggregated treatment effects are all positive, which comports with the two-way fixed effects results, but only the dynamic treatment effect estimate was statistically significant at $p < 0.05$. Although these results were not statistically significant at $p <0.1$, the simple weighted average (average of all available group-time average treatment effects weighted by group size) under unconditional parallel trends shows a 0.41 percent increase in people admitted to prison for drug possession who have less than a high school degree, compared to the 1.37 percent increase in the two-way fixed effects model with jurisdiction and year effects, and 0.26 percent increase in the model with jurisdiction-specific linear time trends. Given the limited number of people admitted to prison for drug possession in many of the jurisdictions, these increases result in minor changes from a substantive perspective.

The “selective treatment timing” estimator allows for the period that a jurisdiction first implements a drug court to affect its group-time average treatment effect; thus, the estimate of 0.18 percent is an aggregate of average treatment effects estimated for each year that a drug court was implemented, weighted by group size. The term “selective” refers to the concept that a jurisdiction could be selecting into treatment at a particular time based on the expectation of future outcomes. What may be more useful in this case however, is examining the “dynamic treatment timing” effect, which accounts for the length of treatment exposure (length of time a jurisdiction has a drug court) and again, is the only statistically significant result in this model. The aggregated dynamic effect is a combination of average effects based on exposure length, averaged over different lengths of exposure. I estimate that drug court implementation led to a 6.23 percent increase in people admitted to prison for drug possession who have less than a high school degree, accounting for dynamic treatment effects.

Lastly, the rows showing the effects accounting for both selective and dynamic treatment timing show the average effects aggregated only for jurisdictions that were exposed to the treatment for a certain number of years. For example, the row $e = 3$ shows the aggregated average treatment effect in the third year of exposure using only groups that had at least three years of exposure (i.e., jurisdictions that implemented a drug court in 2013 or before. These estimates show whether the treatment effect varies the longer a drug court is exposed. None of these estimates were statistically significant at $p < 0.1$). It is worth noting that only 32 of the 45 jurisdictions experienced 5 years of exposure by the end of 2016, so readers should view estimates with longer exposure times with caution. None of the estimates under the conditional PTA are statistically significant at the $p < 0.1$ level.

Table 3.3. Aggregated Group-Time Average Treatment Effect Estimates of Drug Court Implementation on Drug Possession Prison Admissions with Less than a High School Degree/GED

	Unconditional		Conditional	
	Coef.	95% CI	Coef.	95% CI
Simple weighted average	0.407	-3.551, 4.365	1.359	-6.451, 9.169
Selective treatment timing	0.180	-4.106, 4.466	-1.134	-6.822, 4.554
Dynamic treatment timing	6.232	1.969, 10.495	11.70	-8.134, 31.534
Selectivity and dynamics (e = 1)	-3.024	-7.801, 1.753	-3.904	-9.585, 1.777
Selectivity and dynamics (e = 3)	-1.312	-5.572, 2.947	-1.601	-6.97, 3.768
Selectivity and dynamics (e = 5)	-0.568	-4.891, 3.755	-0.907	-6.015, 4.201

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Coef = coefficient. CI = confidence interval. e = exposure years. Number of court-years = 1508. Number of courts = 58. Standard errors are bootstrapped and clustered on drug court jurisdiction. Covariates used for conditional parallel trends = percent of population over age 25 with less than high school degree or GED, percent of households with children that are female headed with no husband present, and sworn officers per capita.

Robustness Checks

The data also include a measure of the percent of people admitted to prison for a violent crime who have less than a high school degree. As all drug courts in Georgia exclude people who are charged with violent offenses, the implementation of a drug court should not have affected this measure. As expected, in both the two-way fixed effects models and group-time aggregated treatment effect models, the results do not suggest that the implementation of a drug court affected the educational composition of people admitted to prison for violent offenses at $p < 0.05$. These results are shown in Appendix Table C.2 and Appendix Table C.3.

With the two-way fixed effects models, I also conduct falsification tests with fake drug court implementation timing. I run the model with the court implemented one and two years in advance of the real timing. In all four cases, the coefficient of interest is smaller than in the main models and none are statistically significant at $p < 0.05$. Appendix Table C.4 and Appendix Table C.5 present these results. This supports the identification assumption that the drug court was not implemented in response to the outcome variable.

DISCUSSION

This study asks the question: do the high financial barriers associated with participating in drug courts lead to a change in the level of economic disadvantage among people who go to prison for drug possession? It hypothesizes that once a jurisdiction implements a drug court, people who can afford to participate in drug court will be able to utilize this process to stay out of prison, leading to an increase in the overall level of socioeconomic disadvantage among people sent to prison for drug possession offenses. Using data from Georgia and educational attainment as a proxy for socioeconomic status, I test this hypothesis with a difference-in-differences approach exploiting the variation in implementation timing of drug courts across the state. With the exception of one model, the results do not support the hypothesis, and I find no statistical significance for the main effect of drug court implementation on the percent of people admitted to prison for possession who had less than a high school education. The one model that finds a statistically significant main effect at the $p < 0.05$ level finds that drug court implementation led to a 6.23 percent increase. This is an aggregated dynamic treatment timing effect, which is a combination of average effects based on exposure length (length of time a jurisdiction has a drug court), averaged over different lengths of exposure. However, this was a model where the PTA may not hold.

Many well-intentioned initiatives have imposed unintended negative consequences on the very people they are meant to help. Thus, it is important to ask how policies and practices affect communities not just based on the primary expected outcomes, such as reduced recidivism, but about potential downstream implications. Unlike the majority of drug court research, the question I seek to answer through this study is not whether drug courts are effective at reducing recidivism. Rather, my study seeks to address an equity issue - if there is any truth to the studies that support the notion that drug courts are beneficial to the wellbeing of participants and public safety, then an inability to pay should not be the difference between who can and cannot access this resource. If instead, economic advantage is necessary for an individual to succeed in the program, then perhaps the drug court model is not as effective as its proponents suggest and its wide application should be viewed with caution. The findings from this study suggest that the implementation of drug courts does not lead to the further siphoning off of more advantaged people out of prison. However, the limitations of this study do warrant future research.

This study does not seek to definitively answer the question of whether drug courts should be expanded. However, I suggest that jurisdictions actively using or considering the implementation of a drug court reflect on whether their model may be exacerbating the problems of racial and socioeconomic discrimination in the criminal justice system. For one, courts should examine each of their eligibility restrictions to determine whether they are necessary for public safety. In making a recommendation, courts should consider whether a less intensive alternative to confinement is more appropriate and the reasoning behind these determinations should be officially recorded to reduce the potential for racial or socioeconomic bias to affect decision-making (Paik 2011). The equity implications of drug courts must be closely scrutinized, especially as the proliferation of the drug court model in the last thirty years has led to a network of drug court professionals who have a vested interest in its continued expansion.

LIMITATIONS

Most of the results of my analyses lacked statistical significance at $p < 0.05$ and often at $p < 0.1$ levels. This may partly be due to my outcome measure. One multi-site study of drug court effectiveness using propensity score matching found that at baseline, their drug court participant group had a significantly higher average annual income and more frequently reported jobs and government programs as sources of financial support and less frequently reported family member financial support, compared to their non-participant group. However, their baseline levels of education were not significantly different across groups (Shelli B Rossman, Roman, and Rempel 2011a:131). Comparing pre-incarceration income levels would be a better test of my hypothesis, although this variable was not available in the data I used. With respect to independent variables, a measure of average participation fees in each court jurisdiction-year, and a different measure of court resources, which tends to be predictive of drug court implementation timing, may be useful.

As discussed in the Specifications section, it is possible that the PTA is not met, invalidating the inferences from the two-way fixed effects model. The large number of counties and court jurisdictions in Georgia mean that there are many jurisdictions that see few people eligible for drug court, resulting in a high degree of volatility in the outcome over time. The group-time average treatment effects analyses under the conditional parallel trends assumption may partly address the PTA issue, as well as the overall problems with the two-way fixed effects model that economists have recently identified.

Lastly, this study only used data from Georgia, a state that has a particularly harsh penal regime. Although drug courts exist in every state, the results of this study should not be used to generalize across all states, particularly states that perceive drug possession to not be such an egregious criminal offense. Furthermore, despite attempts by drug court professional associations to make the program model more consistent, there is heterogeneity in how jurisdictions implement drug courts and this is likely related to regional views across the country on factors such as the criminality of substance use disorders and who is deserving of treatment. Furthermore, there is heterogeneity across and within states to the degree to which individuals must bear the costs of using the "services" of the court, although fines and fees pervade the system (Harris 2016; Harris, Smith, and Obara 2019).

NEXT STEPS

This study focused on how drug courts may lead to inequitable outcomes by changing the composition of people sent to prison for drug possession. In jurisdictions around the country where a prison sentence is less likely for possession, there are other outcomes worth exploring. For example, 27 percent of drug courts use the deferred sentencing model, where a guilty plea can be withdrawn or vacated after successful completion (Marlowe et al. 2019). If financial barriers constrain access to this pathway, drug courts could lead to inequitable outcomes where only wealthier defendants are able to remove a conviction from their records, even if they are not risking prison time (Harris et al. 2019).

However, this study and the vast majority of drug court research do not address some larger questions. Do the sanctions associated with the drug court model pose too high a risk of getting further mired in the criminal justice system? Should the criminal justice system play a role in mandating treatment for substance abuse issues? Is this just another example of Wacquant's (2010) classic thesis of controlling the poor through the penal state? As policy makers and scholars search for ways to reduce incarceration and spend less money on corrections, I hope such questions and issues of equity drive future examinations of alternatives to incarceration.

CONCLUSION

This dissertation presents three studies that show the importance of bearing in mind both individual- and macro-level characteristics when considering the predictors of recidivism and prison admissions. Structural racism in the criminal justice system has become more widely discussed in recent days since the death of George Floyd at the hands of police officers in Minneapolis. This is a time when research on racial and economic disparities in who is admitted and readmitted to prison has enormous power to influence policy. Decades of research on the individual-level determinants of recidivism have shown the large differences by race in the risks of returning to prison. Yet without engaging this literature with the more recent work on macro-level factors, scholars have missed an opportunity to better understand variables that may moderate the association between individual-level characteristics and contact with prisons. Addressing this gap in the literature provides new opportunities to examine how structural racism operates in our criminal justice system.

The chapters of this dissertation present three increasingly focused geographic contexts and research questions. The first chapter is an 18-state study of the association between racial composition and individual-level race on recidivism. The next chapter examines parole in New York and Pennsylvania, and the last chapter studies the effect of drug courts in Georgia on the level of socioeconomic disadvantage among people admitted to prison for drug possession. All three chapters make use of the rich prison admission and release data from the National Corrections Reporting Program. As data quality improves and coverage extends to more states and years, this data set will become an increasingly valuable resource to answer pressing questions in criminal justice.

Chapter One examines the relationship between variation in county-level racial composition and racial disparities in returns to prison. I find that the larger the relative size of a county's non-Hispanic Black population, the lower the hazard of recidivism was for all individuals released to that county, but the magnitude was very small. A ten percent increase in the Black population was associated with a one percent decrease in risk. The relative size of the Black population also had a negative association with the hazard of recidivism for non-Hispanic Blacks compared to non-Hispanic Whites, a two percent decrease in risk for every ten percent increase. The association between being released to a county with a ten percent larger Hispanic population was even smaller, a 0.41 percent decrease in the risk of returning to prison. These negative associations may reflect the political power of a larger nonwhite population to push elected officials to reduce punitiveness and racial disparities. It also suggests that racial threat theory may not be as powerful as once thought in explaining racial disparities in recidivism rates. However, I also find a positive association between larger Hispanic populations and being Hispanic relative to being White. A ten percent increase in the population that is Hispanic was associated with a 0.42 percent increase in the hazard of recidivism for Hispanics. This could reflect the role of racial threat, which describes a dynamic where an increase in the relative size of the nonwhite population activates perceived threat to White political and economic dominance. This perceived threat results in a backlash that manifests in increased control of the nonwhite population and individuals.

Chapter Two focuses on parole revocations, which are returns to prison among people on parole. I find a significant association between the number of prior parole episodes and the state of jurisdiction, and the likelihood and timing of returning to prison and revocation type. The results suggest that in both states, prior experiences of parole increased the likelihood of future recidivism and people returned to prison more quickly for a revocation for a technical violation than for a new crime revocation. I also find that people in Pennsylvania had a higher risk of revocation than people in New York, but this difference was only distinguishable among people on parole for the first time. Lastly, having been on parole previously increased the risk of experiencing a parole revocation for a technical violation, but only in New York.

In Chapter Three, I focus specifically on Georgia to better understand how the implementation of drug courts may unintentionally result in economic disparities in prison admissions. The results show that on average, the percentage of people admitted to prison for drug possession who have less than a high school degree or GED did not increase significantly after drug court implementation. One supplementary analysis suggests that drug court implementation led to a 6.23 unit increase in the percentage of people admitted to prison for drug possession who have less than a high school degree. However, this analysis may not have met the parallel trends assumption, a key requirement for causal inference with this method.

The three studies extend research that examines individual-level and macro-level predictors of prison admissions and recidivism. With this dissertation, I seek to push future scholars to consider how these factors may interact to affect racial and economic disparities among people in prison. This dissertation highlights the variation in demographic and policy contexts across the country and how they are associated with who is in prison. The three chapters underscore the importance for future scholars to closely examine the role that policies and larger institutional contexts play in producing and exacerbating inequities in the criminal justice system. Furthermore, the three studies demonstrate how researchers should be wary of generalizing the results of their study if it only examines data from one legal, social, and political context. One fruitful area of future research will be in qualitative work that examines the causal mechanisms behind the trends this dissertation describes.

POLICY IMPLICATIONS

There are several takeaways for policy from this dissertation. In designing criminal justice reforms, policymakers need to consider the potential for policies to exacerbate existing racial and economic disparities. A policy which may on its face appear to affect people equally may upon closer examination result in more negative outcomes for people who already experience the highest levels of discrimination in the criminal justice system. For example, this dissertation shows that the extent of a person's prior criminal justice contact interacts with what state they are in with regards to the risk of parole revocations. This is important to keep in mind as policymakers suggest reforms; proposing "race-neutral" policies that instead focus on something like the number of prior arrests or prison sentences are not race-neutral in practice, because structural racism as manifested in our criminal justice system has led to people of color, especially Black people, having more extensive records.

Identifying the important role of macro-level context, whether state or county, also shows that it is imperative to consider how the adoption of policies and practices that have desirable results in one jurisdiction may result in unintended consequences in another jurisdiction. Including a process evaluation as part of policy implementation will help decisionmakers better understand how and why policies have certain results and course-correct, especially if outcomes differ from implementation in other locales.

Lastly, increasing the political power and representation of communities of color may result in policies that reduce recidivism and racial disparities. Yet a larger nonwhite population may not necessarily result in more political representation. Policies that disproportionately disenfranchise people of color, like strict voter identification card laws and laws that bar people with felony convictions from voting may suppress calls from the community for change to policies in the criminal justice system. In addition to opposing these types of policies, intentional engagement of nonwhite voters may be a fruitful avenue for positive change.

Directions for Future Research

The NCRP data I use throughout this dissertation is a rich source of large-scale longitudinal data. As these studies show, there are many ways to utilize different aspects of the data set to answer highly varied research questions and it has the potential to answer many more. In today's world of ever-increasing access to big data, I expect the door will open to investigate the impact of many more prison policies.

Linking prison, jail, or arrest data with other variables measuring political representation may help to understand how macro-level factors affect racial disparities in recidivism. Such variables may include the proportion of nonwhite elected officials or levels of disenfranchisement through laws that bar people with felony convictions from voting. To further investigate questions regarding the role of technical violations in creating a pathway to incarceration, linking the NCRP to jail and probation data is crucial. In many states, technical violations would rarely result in prison time and more commonly result in jail stays, and some states, like Washington State, rely heavily on probation while parole is highly uncommon. Lastly, to better assess the effect of drug courts on the composition of people sent to prison for drug possession, data on pre-incarceration income and measures of average fees in each court jurisdiction-year would be useful.

As scholars continue to study criminal justice policies, it will be key to focus attention on policies that may exacerbate racial and economic disparities and how we can mitigate such negative impacts. One area of the criminal justice system that has received less attention than prisons is community supervision. The population on probation or parole at any given time numbers in the millions in the U.S. (Kaeble 2018a). As my dissertation shows, community supervision can present another pathway to prison. Research on how jurisdictions across the country implement community supervision policies in ways that actually reduce the prison population and support healthy communities may present a possible path forward to reform.

However, this dissertation also raises questions about existing efforts to reduce the prison population and rehabilitate people in the community. Programs like drug court may seem like a viable alternative to help people with substance abuse issues while avoiding incarceration. Future scholars should consider not just how such programs and policies may affect a particular target group, but also the implications of excluding people and potential spillover effects that may result in entrenchment of existing racial and economic disparities.

Research on the criminal justice system is especially relevant now, particularly when thinking about macro-level characteristics and not just individual-level characteristics. Between fears of coronavirus spreading through prisons and racism in policing, people across the country are asking many questions about the role of institutions in the racial disparities we see. It will be important for researchers to not focus solely on evaluating the effectiveness of popular policies but to ask bigger picture questions of how such policies situate themselves in the larger institutional setting and interact with and are a product of structural racism. Chapter Three demonstrated the types of questions and research design researchers can consider in evaluating an immensely popular program's potential impact on socioeconomic equity. This is a direction that some research in public policy already appears to be headed. Program evaluators are increasingly asking for whom certain programs are helpful and what impacts programs may have on a variety of communities.

In criminal justice research, the focus has shifted from concentrating on recidivism in the traditional sense to studies of the process of desistance from a life course perspective. This framing has helped to move the focus from the correlates of individual failure to an examination of larger social processes and institutions that shape long-term change (Kruttschnitt, Uggen, and Shelton 2000; Laub and Sampson 2001). This dissertation contributes to the conceptualization of macro-level determinants of individual-level criminal justice contact. However, many interventions are still focused on changing individual behavior as opposed to policies that change institutions. More work is necessary to push the focus to research that identifies such policies and their impact on communities.

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APPENDIX A: APPENDIX TO CHAPTER 1

Appendix Table A.1. Individual-Level Characteristics by Race

	Percent of Individuals			
	White	Black	Native American	Hispanic
Individual max prison terms				
1	57.54	50.71	51.22	62.64
2	23.02	25.97	23.75	20.09
3	10.65	13.00	14.17	9.27
4	5.11	6.06	5.72	4.62
5+	3.67	4.27	5.13	3.38
Age at prison admission*				
<18	0.49	1.29	0.78	0.99
18-24	27.29	37.42	28.1	33.99
25-39	46.24	42.34	48.39	47.61
40-64	25.51	18.78	22.53	17.09
65-100	0.47	0.17	0.20	0.32
Male	84.84	89.75	83.24	93.31
Conviction offense of prison sentence*				
Violent	24.45	28.27	28.01	28.09
Property	34.52	21.29	24.63	20.54
Drug	25.04	39.16	17.99	36.18
Public Order	15.99	11.28	29.37	15.20
Total	64520	58722	2046	17212

Source: Author’s analysis of National Corrections Reporting Program 2000-2016 data.
Notes:
Race categories refer to individuals identified as non-Hispanic or Hispanic ethnicity unknown unless otherwise noted. “Hispanic” refers to individuals identified as Hispanic, regardless of response in race item. “Native American” refers to the Census Bureau’s American Indian/Alaskan Native designation.
* At first prison term admission.
** Examples of public order offenses include weapons, drunk driving, court offenses, commercialized vice, morals and decency charges, and liquor law violations.

Appendix Table A.2. Percent of Sample by State of Jurisdiction

	% Observations	% Individuals
State of jurisdiction		
Arizona	7.25	7.00
Colorado	3.48	2.90
Florida	11.26	13.43
Georgia	6.09	6.82
Illinois	10.96	9.36
Kentucky	5.44	5.44
Michigan	4.21	4.74
Minnesota	1.73	1.45
Missouri	7.76	6.25
Nebraska	0.7	1.26
New Jersey	8.8	8.40
New York	7.52	7.89
Oklahoma	3.73	4.44
Pennsylvania	5.43	4.65
South Carolina	5.79	6.24
Tennessee	5.76	5.46
Utah	1.25	1.03
Washington	2.84	3.23
Total	258512	142500

Source: Author’s analysis of National Corrections Reporting Program 2000-2016 data.

Appendix Table A.3. Results of Prentice-Williams-Peterson Conditional Gap-Time Models Estimating Log Hazard Rates of Time to Prison Readmission

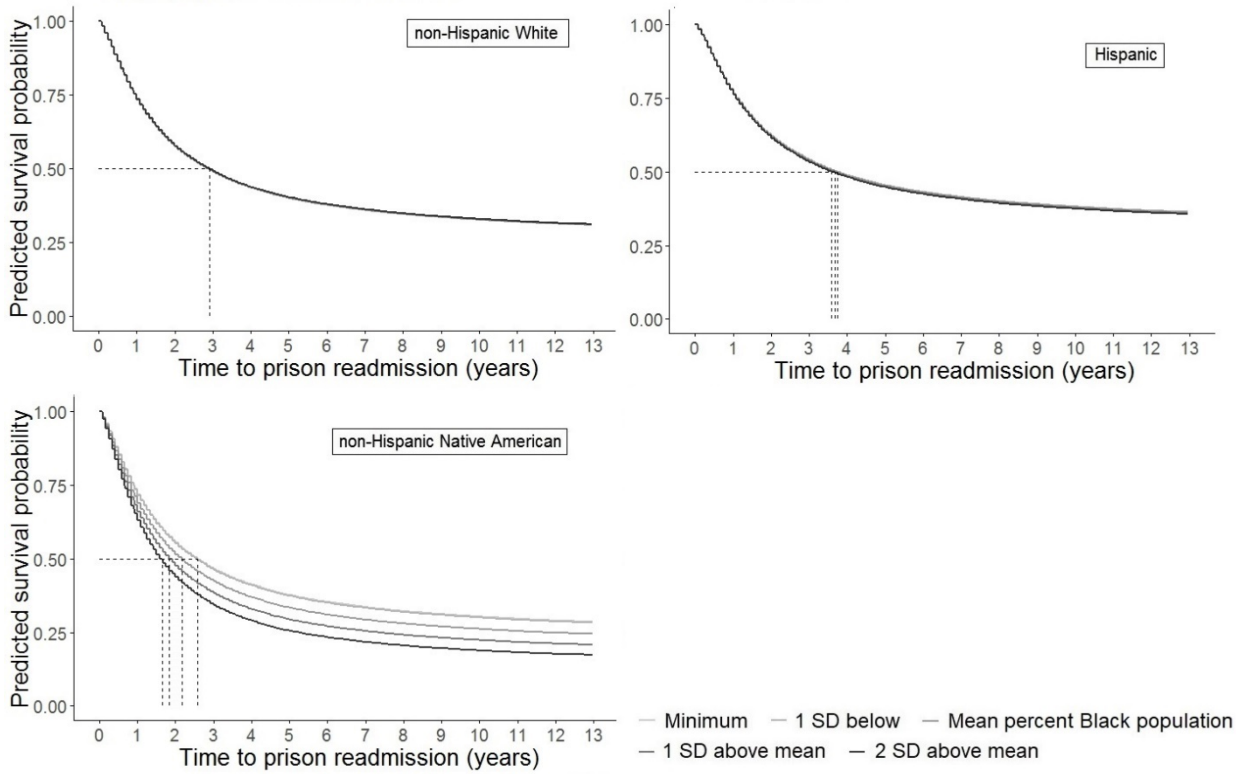
	Model 1		Model 2	
	Coef.	95% CI	Coef.	95% CI
Prison admit age				
<18	0.215	0.146, 0.285	0.212	0.143, 0.282
25-39	-0.311	-0.324, -0.298	-0.310	-0.323, -0.297
40-64	-0.652	-0.669, -0.635	-0.652	-0.669, -0.635
65-100	-1.603	-1.802, -1.405	-1.604	-1.802, -1.406
Male	0.296	0.276, 0.317	0.296	0.276, 0.317
Race†				
Black	0.162	0.148, 0.176	0.110	0.081, 0.139
Native American	0.089	0.043, 0.135	0.060	-0.059, 0.178
Hispanic	-0.120	-0.143, -0.098	-0.442	-0.524, -0.360
Conviction offense				
Violent	-0.218	-0.234, -0.202	-0.218	-0.234, -0.202
Drug	-0.219	-0.233, -0.205	-0.220	-0.234, -0.206
Public Order	-0.201	-0.219, -0.184	-0.201	-0.219, -0.184
Days in custody	0.000	0.000, 0.000	0.000	0.000, 0.000
% Black pop††	-0.001	-0.002, -0.001	0.000	-0.001, 0.001
Log (% Hispanic pop)	-0.043	-0.050, -0.036	-0.073	-0.083, -0.064
Unemployment rate	-0.013	-0.016, -0.010	-0.014	-0.017, -0.011
Deep South state	-0.375	-0.392, -0.358	-0.383	-0.400, -0.365
Determinate sentencing state	-0.010	-0.023, 0.004	-0.005	-0.019, 0.009
Truth-in-sentencing state	0.059	0.023, 0.095	0.045	0.009, 0.081
Habitual offender law state	0.186	0.171, 0.200	0.201	0.185, 0.216
Race† X % Black pop††				
Black			-0.002	-0.003, -0.001

(*continued on to next page)*

Appendix Table A.3 *Continued*

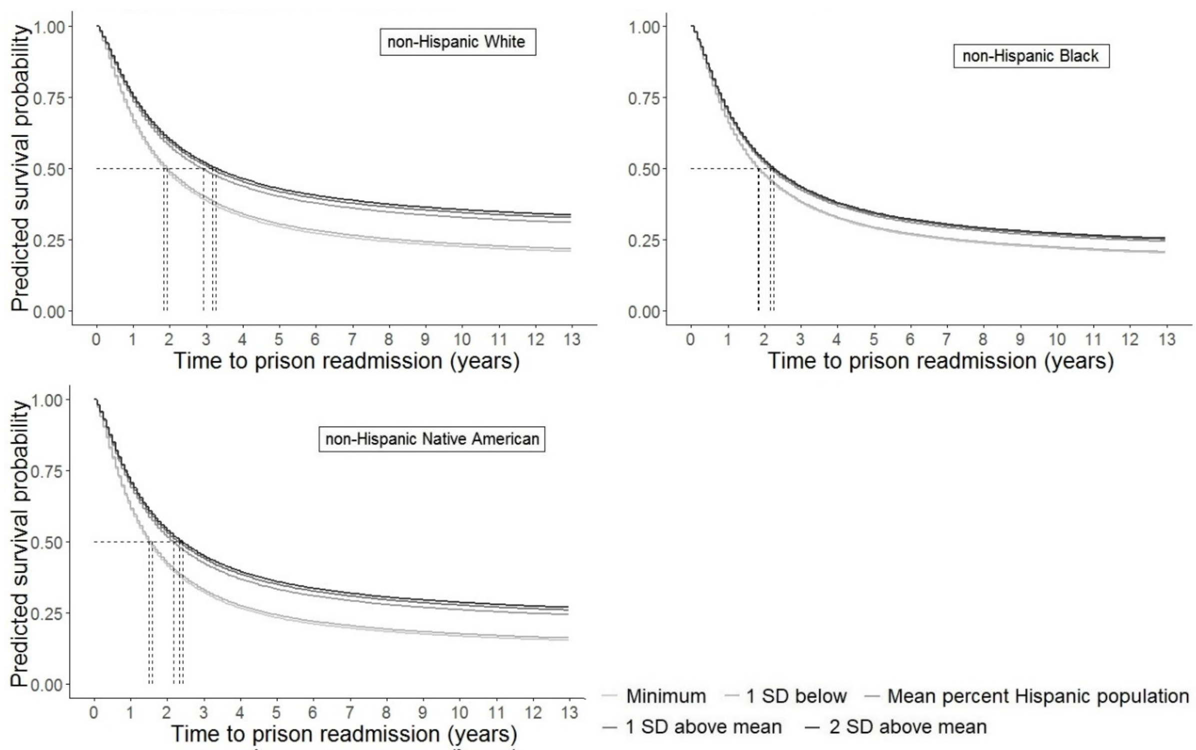
Native American			0.008	0.001, 0.015
Hispanic			0.001	-0.001, 0.003
Race† X log (% Hispanic pop)				
Black			0.043	0.031, 0.056
Native American			0.004	-0.042, 0.050
Hispanic			0.117	0.090, 0.144
BIC	2802558		2802844	

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: N = 258,512, N events (returns to prison) = 129,709; Pop. = population; Coef. = coefficient; CI = confidence interval; BIC = Bayesian Information Criteria
Reference categories: prison admit age 18-24, female, White, property crime
† Race categories refer to individuals identified as non-Hispanic or Hispanic ethnicity unknown, unless otherwise noted. "Hispanic" refers to individuals identified as Hispanic, regardless of response in race item. "Native American" refers to the Census Bureau's American Indian/Alaskan Native category.
†† % Black pop refers to the percent of the county population that is non-Hispanic Black.



Appendix Figure A.1. Predicted survival curves for time to readmission by race and percent non-Hispanic Black population from Model 2 estimates.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: Plot of predicted survival probabilities of returning to prison after first release by race, for a male admitted to prison at 18 to 24 years old for a property offense conviction, having served mean custody length, in a non-Deep South state in a county without determinate sentencing, with truth-in-sentencing, and with habitual offender laws in 2004, with mean unemployment rate, mean percent non-Hispanic Black population, and mean percent Hispanic population. Dotted lines represent median survival.



Appendix Figure A.2. Predicted survival curves for time to readmission by race and percent Hispanic population from Model 2 estimates.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Note: Plot of predicted survival probabilities of returning to prison after first release by county percent Hispanic population, for a male, admitted to prison at 18 to 24 years old for a property offense conviction, having served mean custody length, in a non-Deep South state in a county without determinate sentencing, with truth-in-sentencing, and with habitual offender laws in 2004, with mean unemployment rate, and mean percent non-Hispanic Black population. Dotted lines represent median survival.

APPENDIX B: APPENDIX TO CHAPTER 2

Appendix Table B.1. Descriptive Statistics for Model 3 by Outcome

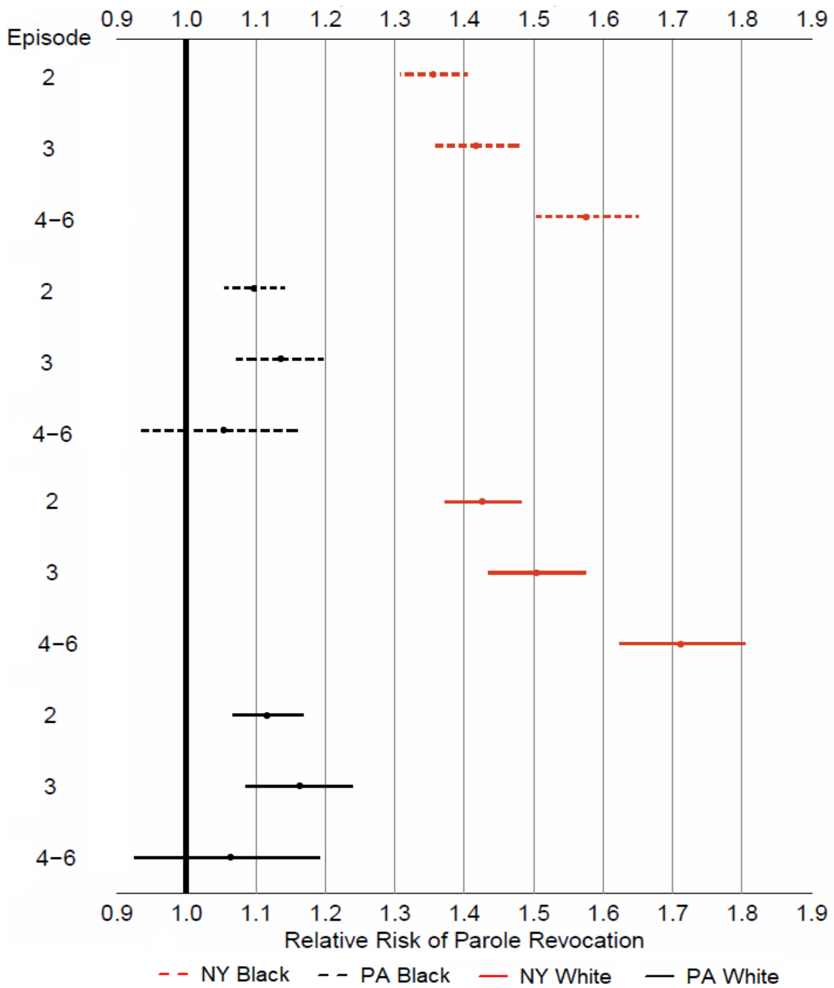
Outcome	Stay on parole	Completed	Revocation NS	Revocation NNS
New York (%)	49.37	70.06	39.84	71.73
Conviction offense [a] (%)				
Property offense	16.93	17.51	22.82	22.98
Drug	36.67	39.98	35.91	30.69
Violent	25.76	28.4	29.14	34.89
Public Order	14.33	13.02	10.95	10.66
Unspecified	6.31	1.11	1.17	0.78
Male (%)	94.06	89.28	95.31	93.79
Race [b] (%)				
White	33.78	38.56	32.91	35.34
Black	49.81	45.67	56.14	53.11
Other	16.41	15.78	10.95	11.55
Age at parole entry (%)				
Under 18	0.00	0.04	0.05	0.08
18-24	1.34	16.94	25.57	22.14
25-39	60.06	52.12	57.26	55.47
40-64	37.64	29.93	17.01	22.12
65-100	0.97	0.97	0.10	0.18
Total (N)	1347	10579	1963	6165

Source: Author's analysis of the National Corrections Reporting Program 2000-2015.
Notes:
a. First conviction for which individual went to prison and was subsequently released to parole
b. Other race refers to individuals identified as Native American, Asian, Pacific Islander, Multiracial, or Other

Appendix Table B.2. Risk of NNS Revocation, NS Revocation, or Parole Completion of Any Parole Term by US State and Race across First Five Years after Parole Release

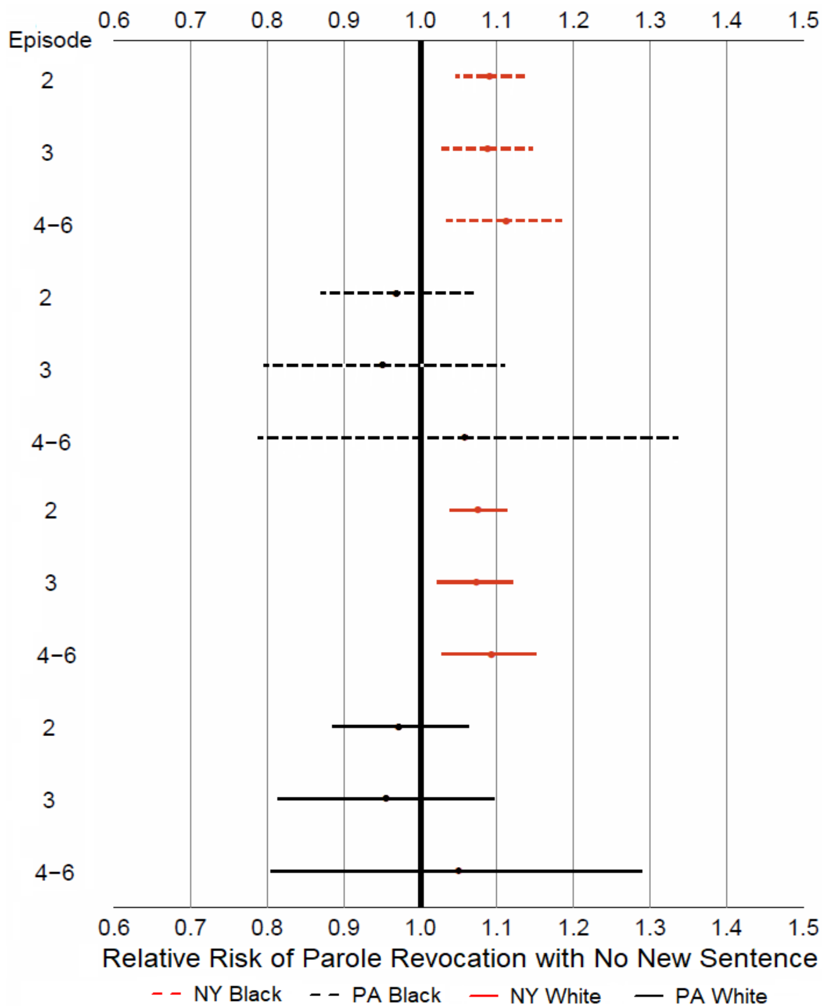
	Year	1	2	3	4	5
Pennsylvania White						
	NNS	22.1	30.3	33.1	34.1	34.5
	NS	13.4	24.3	28.5	30.0	30.7
	Completion	10.6	18.4	25.7	29.9	32.1
Pennsylvania Black						
	NNS	24.5	32.5	34.8	35.6	35.8
	NS	18.4	31.5	35.9	37.4	38.0
	Completion	9.3	15.5	20.6	23.3	24.5
New York White						
	NNS	27.8	38.2	41.7	42.9	43.3
	NS	4.2	7.6	8.9	9.3	9.5
	Completion	15.1	26.5	37.0	42.5	45.2
New York Black						
	NNS	31.3	42.2	45.6	46.8	47.1
	NS	5.8	10.3	11.9	12.5	12.7
	Completion	13.7	23.4	32.0	36.4	38.5

Source: Author's analysis of the National Corrections Reporting Program 2000-2015.
Notes: NNS: No new sentence, NS: New sentence.
The table shows the risk of a revocation with no new sentence, revocation with a new sentence, or parole completion of any parole term, by US state and race, for a male aged 18-24 at parole entry, convicted of a property offense. Estimates are from a competing risks model computed with an Aalen-Johansen estimator.



Appendix Figure B.1. Model 1 results: Relative risk of parole revocation by state, race, and parole episode (compared to episode 1) among males, convicted of property offense, aged 18-24, having served mean prison term.

Source: Author's analysis of the National Corrections Reporting Program 2000-2015.
Notes: Bars show 95% confidence intervals using the variance-covariance matrix from one imputed data set; results are the same across all data sets.



Appendix Figure B.2. Model 2 results: Relative risk of NNS parole revocation by state, race, and parole episode (compared to episode 1) among males, convicted of property offense, aged 18-24, having served mean prison term.

Source: Author's analysis of the National Corrections Reporting Program 2000-2015.
Notes: Bars show 95% confidence intervals using the variance-covariance matrix from one imputed data set; results are the same across all data sets.

APPENDIX C: APPENDIX TO CHAPTER 3

Appendix Table C.1. Summary Statistics: Prison Admission and Drug Court Jurisdiction Data Excluding Populous Early Implementer Jurisdiction

	Pre-DC	Post-DC	No DC
Prison admission characteristics			
% drug possession admits with less than HS	63.52	59.15	63.08
(SD)	12.95	13.87	17.88
% violent offense admits with less than HS	68.67	68.26	68.80
(SD)	9.73	11.00	11.18
% of all admits for drug possession	14.98	11.56	11.00
(SD)	6.37	6.12	4.62
Number of drug possession admissions	40.19	41.42	19.33
(SD)	45.43	59.30	16.06
Number of prison admissions	253.59	333.12	165.91
(SD)	230.18	300.26	96.93
Drug court jurisdiction characteristics			
% over age 25 with less than HS	22.56	19.05	25.14
(SD)	7.13	6.22	6.04
% female-headed households	23.25	27.22	26.04
(SD)	9.26	9.93	5.74
Sworn officers per capita	0.26	0.30	0.24
(SD)	0.10	0.09	0.08
% Black population	25.22	27.90	25.72
(SD)	15.49	18.99	9.28
% below poverty line	15.48	17.93	20.12
(SD)	6.06	5.66	5.05
Unemployment rate	5.73	7.16	6.80
(SD)	2.33	2.44	2.51
Number of court-years	738	432	338
Number of courts	45	45	13

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: DC = drug court. No DC = jurisdictions that did not have a drug court during the period of observation 1991-2016. Less than HS degree = Less than high school degree or GED. % female-headed households = percent of households with children that are female headed with no husband present.

Appendix Table C.2. Robustness Checks: Results of Two-Way Fixed Effects Analyses of Drug Court Implementation on Percent of Violent Offense Prison Admissions with Less than a High School Degree/GED

	Model 1		Model 2	
	Coef.	95% CI	Coef.	95% CI
DC implemented	2.475	0.243, 4.707	1.234	-1.220, 3.688
% pop over age 25 with less than HS	0.750	0.244, 1.256	0.498	0.073, 0.924
% female-headed households	-0.274	-1.432, 0.884	0.002	-0.472, 0.476
Sworn officers per capita	0.262	-0.005, 0.529	-2.075	-3.665, -0.486
% Black population	4.379	-0.584, 9.342	-0.063	-0.467, 0.341
Unemployment rate	0.155	-0.506, 0.816	0.180	-0.582, 0.943
% below poverty line	-0.633	-1.715, 0.449	1.000	-0.441, 0.641

Source: Author's analysis of National Corrections Reporting Program, 2000-2016 data.
Notes: Number of courts = 58. Number of court-year periods = 1508. DC = drug court. Coef = coefficient. CI = confidence interval. Estimation uses weighted least squares weighted by total number of admissions for drug possession. Standard errors are clustered on drug court jurisdiction and use bias-reduced linearization. Model 1 includes year and drug court jurisdiction fixed effects and Model 2 adds jurisdiction-specific linear time trends.

Appendix Table C.3. Robustness Checks: Aggregated Group-Time Average Treatment Effect Estimates of Drug Court Implementation on Violent Offense Prison Admissions with Less than a High School Degree/GED

	Unconditional		Conditional	
	Coef.	95% CI	Coef.	95% CI
Simple weighted average	3.392	-1.751, 5.399	1.757	-2.31, 7.123
Selective treatment timing	3.273	-1.543, 4.759	3.197	-1.967, 6.064
Dynamic treatment timing	8.241	-2.239, 6.902	1.624	-4.858, 14.978
Selectivity and dynamics (e = 1)	1.075	-1.212, 3.738	0.732	-1.55, 4.779
Selectivity and dynamics (e = 3)	1.902	-1.442, 4.446	1.602	-1.705, 5.256
Selectivity and dynamics (e = 5)	1.211	-1.261, 3.887	0.393	-1.784, 5.499

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Coef = coefficient. CI = confidence interval. e = exposure years. Number of court-years = 1508. Number of courts = 58. Standard errors are bootstrapped and clustered on drug court jurisdiction. Covariates used for conditional parallel trends = percent of population over age 25 with less than high school degree or GED, percent of households with children that are female headed with no husband present, and sworn officers per capita.

Appendix Table C.4. Robustness Checks: Results of Two-Way Fixed Effects Analyses of Fake Drug Court Implementation Timing on Percent of Drug Possession Prison Admissions with Less than a High School Degree/GED – One-Year Lead

	Model 1		Model 2	
	Coef.	95% CI	Coef.	95% CI
DC implemented	0.542	-1.188, 2.272	-0.865	-2.350, 0.621
% pop over age 25 with less than HS	0.291	-0.059, 0.641	0.026	-0.404, 0.455
% female-headed households	0.183	-0.258, 0.624	0.581	0.208, 0.953
Sworn officers per capita	2.753	-2.813, 8.319	-0.093	-6.361, 6.175
% Black population	0.104	-0.082, 0.290	-0.210	-0.547, 0.127
Unemployment rate	0.468	-0.438, 1.374	0.345	-0.488, 1.178
% below poverty line	-0.124	-0.600, 0.352	-0.069	-0.388, 0.251

Source: Author's analysis of National Corrections Reporting Program, 2000-2016 data.
Notes: Number of courts = 58. Number of court-year periods = 1508. DC = drug court. Coef = coefficient. CI = confidence interval. Estimation uses weighted least squares weighted by total number of admissions for drug possession. Standard errors are clustered on drug court jurisdiction and use bias-reduced linearization. Model 1 includes year and drug court jurisdiction fixed effects and Model 2 adds jurisdiction-specific linear time trends.

Appendix Table C.5. Robustness checks: Results of Two-Way Fixed Effects Analyses of Fake Drug Court Implementation Timing on Percent of Drug Possession Prison Admissions with Less than a High School Degree/GED– Two-Year Lead

	Model 1		Model 2	
	Coef.	95% CI	Coef.	95% CI
DC implemented	0.940	-0.884, 2.764	0.000	-1.639, 1.638
% pop over age 25 with less than HS	0.288	-0.054, 0.630	0.023	-0.414, 0.406
% female-headed households	0.189	-0.254, 0.632	0.577	0.204, 0.949
Sworn officers per capita	2.570	-3.105, 8.245	0.024	-6.411, 6.459
% Black population	0.104	-0.081, 0.289	-0.174	-0.494, 0.145
Unemployment rate	0.459	-0.447, 1.365	0.343	-0.490, 1.176
% below poverty line	-0.128	-0.610, 0.354	-0.074	-0.399, 0.252

Source: Author’s analysis of National Corrections Reporting Program, 2000-2016 data.
Notes: Number of courts = 58. Number of court-year periods = 1508. DC = drug court. Coef = coefficient. CI = confidence interval. Estimation uses weighted least squares weighted by total number of admissions for drug possession. Standard errors are clustered on drug court jurisdiction and use bias-reduced linearization. Model 1 includes year and drug court jurisdiction fixed effects and Model 2 adds jurisdiction-specific linear time trends.

Drug Court A

Participant Program Costs.

Participants are required to pay an orientation fee of $50.00 and $150 per month for the amount of time you are in the program. In order to phase up, ALL participants {NO EXCEPTIONS} are required to have a $0 balance to include the month of phase up. The example listed below is based on the minimum 24-month program schedule - phasing through the program on time with no delays. [There is a $150.00 monthly charge for every month over the 24-month minimum participation.]

Phase	Phase Length	Fees Charged	Monthly Fee	Phase Balance
1	3 Months	Orientation Fee	$50.00	$50.00
		January 2018	$150.00	$200.00
		February 2018	$150.00	$350.00
		March 2018	$150.00	
		Total to phase to 2 [not including restitution]		$500.00

Phase	Phase Length	Fees Charged	Monthly Fee	Phase Balance
2	4 Months	April 2018	$150.00	$150.00
		May 2018	$150.00	$300.00
		June 2018	$150.00	$450.00
		July 2018	$150.00	
		Total to phase to 3 [not including restitution]		$600.00

Phase	Phase Length	Fees Charged	Monthly Fee	Phase Balance
3	5 Months	August 2018	$150.00	$150.00
		September 2018	$150.00	$300.00
		October 2018	$150.00	$450.00
		November 2018	$150.00	$600.00
		December 2018	$150.00	
		Total to phase to 4 [not including restitution]		$750.00

Special Notice Regarding Phases 4 & 5

If you are in phase 4 or 5 and do not qualify to be an all-star, you are required to attend court weekly. Once you have paid your fees to date or resolve whatever issue that prevents you from being an all-star you will be permitted to attend the phase required weeks. There will be no exceptions.

Phase	Phase Length	Fees Charged	Monthly Fee	Phase Balance
4	6 Months	January 2019	$150.00	$150.00
		February 2019	$150.00	$300.00
		March 2019	$150.00	$450.00
		April 2019	$150.00	$600.00
		May 2019	$150.00	$750.00
		June 2019	$150.00	
		Total to phase to 5 [not including restitution]		$900.00

Phase	Phase Length	Fees Charged	Monthly Fee	Phase Balance
5	6 Months	July 2019	$150.00	$150.00
		August 2019	$150.00	$300.00
		September 2019	$150.00	$450.00
		October 2019	$150.00	$600.00
		November 2019	$150.00	$750.00
		December 2019	$150.00	
		Total to phase to 5 [not including restitution]		$900.00
		Total scheduled program fee for participants		$3,650.00

Drug Court B

Program Fees

All program participants are expected to pay a total of $300 in Phase I and $586 in each of the remaining Phases (II-IV) Fees will partially cover the cost of treatment. A participant cannot advance to the next Phase without paying the Program Fees. The fees assessed are used initially to pay for the participant's treatment and later in the program for supplies and materials needed to maintain the Drug Court Program. Arrangements must be made with the Drug Court Coordinator for the payment of fees. It will be the responsibility of each participant to ensure the Program Fees are paid. Although the participant will not receive any fine, the participant is also responsible for the payment of any court costs and probation fees through the Probation Office.

Drug Court C

Phases of Drug Court

Drug Court is a 21 to 27 month program divided into five phases, including a transition phase. An individual participant must successfully complete each phase before transitioning on to the next. Each phase has a key concept or focus different from the other phases.

Phase I – Duration: four months minimum

Drug Court attendance weekly
(At least) two random urine tests per week
Three group sessions per week (1.5 hours each session)
Minimum of three Self-Help meetings per week
Meet with Case Manager in office within 2 weeks of entering program

Phase II – Duration: four months minimum

Drug Court attendance every other week
(At least) two random urine tests per week
Three group sessions per week (1.5 hours each session)
Three Self-Help meetings per week

Phase III – Duration: five months minimum

Drug Court attendance every three weeks
(At least) two random urine tests per week
Two group sessions per week (1.5 hours each session)
Five Self-Help meetings per week

Phase IV – Duration: five months minimum

Drug Court attendance every four weeks
(At least) Two random urine tests per week
One group session per week (1.5 hours per week)
Five Self-Help meetings per week (including at least one alumni meeting)
You must be clean and sober for a period of a minimum of six months prior to aftercare

Phase V – Duration: three months minimum

Attend two alumni meetings per month
Random urine test
Mentor a Phase 1 or Phase 2 participant (meeting at least 1x per month)

Drug Court D

General Requirements of Each Phase of Treatment

Orientation Phase:
1. Remain drug and alcohol free for at least 12 weeks
2. Attend 4 group counseling sessions per week
3. Attend 1 individual counseling session per week
4. Attend court every week
5. Attend at least 3 community support meetings per week
6. Complete a written assignment

Phase I:
1. Remain drug and alcohol free
2. Attend 4 group counseling sessions per week
3. Attend 1 individual counseling session per month
4. Attend court every week
5. Attend at least 3 community support meetings per week
6. Pay at least $250.00 toward Drug Court fee
7. Complete a written exercise as assigned
8. Complete 20 hours of community service

Phase II:
1. Remain drug and alcohol free
2. Attend 3 group counseling sessions per week
3. Attend 1 individual counseling session per month
4. Attend court every week
5. Attend at least 3 community support meetings per week
6. Pay at least $250.00 toward Drug Court fee. ($500.00 total paid)
7. Complete a written exercise as assigned
8. Complete 20 hours of community service

Phase III:
1. Remain drug and alcohol free
2. Attend 3 group counseling sessions per week
3. Attend 1 individual counseling session per month
4. Attend court every other week
5. Attend at least 3 community support meetings per week
6. Pay at least $250.00 toward Drug Court fee. ($750.00 total paid)
7. Enroll in GED program
8. Complete a written exercise as assigned
9. Complete 20 hours of community service

Phase IV:
1. Remain drug and alcohol free
2. Attend 2 group counseling sessions per week
3. Attend 2 individual counseling sessions per month
4. Attend court once per month
5. Attend at least 3 community support meetings per week
6. Pay remaining balance of Drug Court fee. ($1000.00 total paid)
7. Complete GED program and provide documentation
8. Obtain and maintain full-time employment
9. Prepare and present graduation speech
10. Complete 20 hours of community service for total of 80 hours
11. Complete a written exercise as assigned

Appendix Figure C.1. Example of fees and phases in four Georgia drug courts.

Drug Court E

Failure to pay program fees will result in sanctions. If your fees are over $150 on the day of court, you have until 2:00 pm to pay your fees and MUST provide proof of payment to the drug court office by that time or you will be ordered community service. If your outstanding **fees exceed $150**, you will be required to complete and provide proof of **eight(8) hours** of community service that will be due by Friday with proof in the Drug Court office. Faxes will not be accepted. Failure to complete your community service will result in your community service being doubled.

If your outstanding fees exceed $300, you need to pay below $300 before the start of court session or you could be incarcerated until your fee balance is paid to zero. You will also be required to attend court weekly until your fees are below $300.

All Drug Court program fees must be paid in full before 2 pm in order to be eligible for Phase transition, vacation, and graduation from the program.

Appendix Figure C.2. Sanctions for nonpayment – Example from Georgia drug court.

Drug Court F

10. ______ I understand that I will be required to pay a total of $200.00 per month, for payment of my drug treatment counseling and supervision fee. I further understand that I will be required to pay a one-time fee of $175.00 for the initial treatment assessment.

Drug Court G

DRUG COURT PARTICIPATION REQUIREMENTS

You are voluntarily entering the drug court program. The following terms are conditions of continuing participation in the program.

1. ______ I am not allowed to take drugs or drink alcohol.

2. ______| I have a monthly fee $150 of which I am required to keep up-to date in order to progress through the program and graduate.

3. ______ I will pay a one-time orientation fee of $50.00 upon admission to the Drug Court.

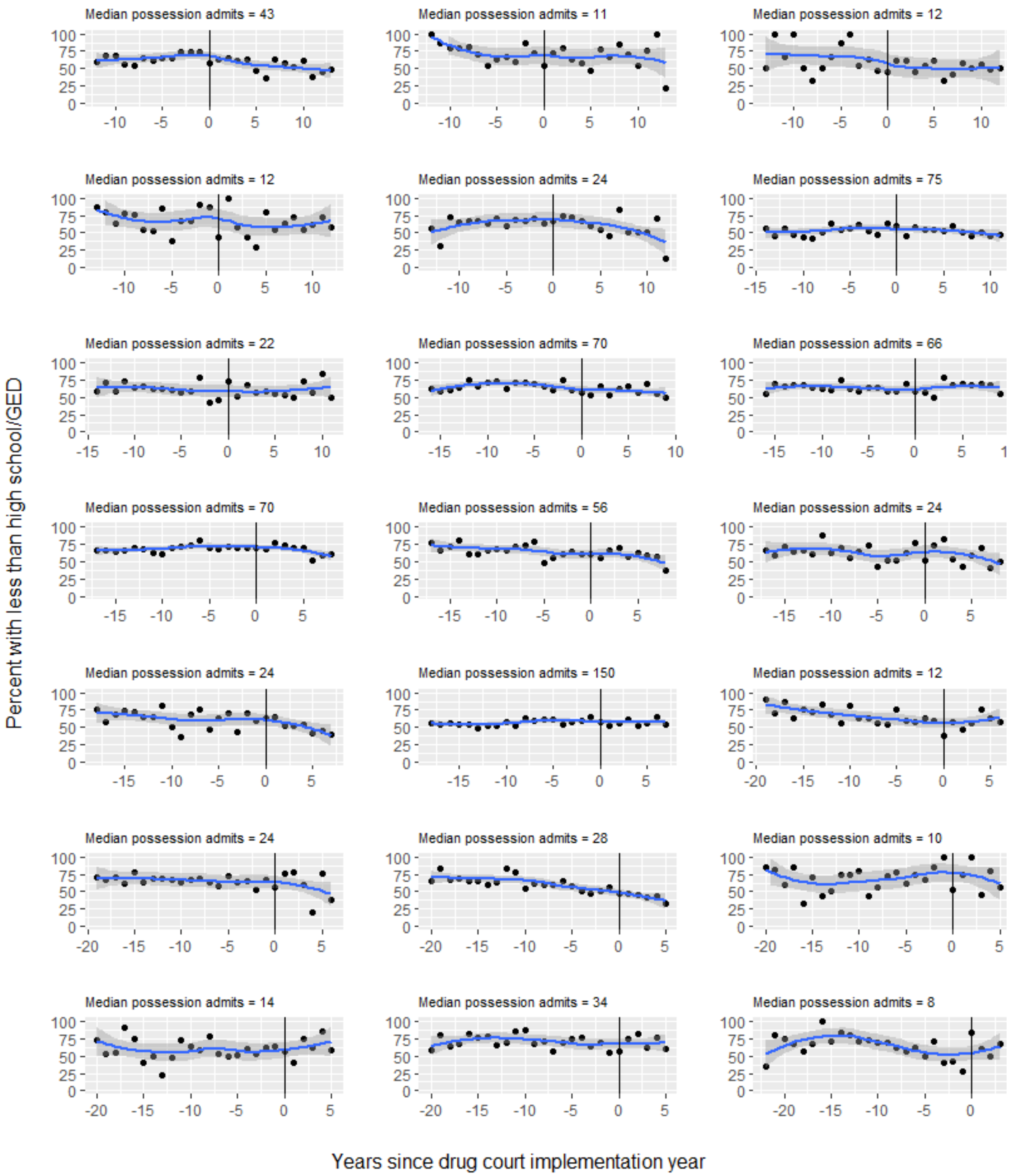
29. ______ I understand that my participation in the Augusta Judicial Circuit Drug Court is in addition to my probation and I must continue reporting to my probation officer and paying restitution as directed. The Court will instruct me as to the necessity of continuing to pay fines and probation fees. My participation in the Drug Court in no way automatically relieves me of my probation responsibilities.

30. ______ I understand that as part of my plea agreement I agree to pay the restitution owed to the victim in my case(s) that is set forth in my restitution. Further, I understand that I hereby waive my right to have a restitution hearing challenging the amount of restitution. I further understand that I may be held responsible for the full amount set forth in my restitution order even though I may be found jointly

and severally liable for the restitution with a co-defendant. I also agree to have no contact with the victim or the victim's property as ordered by the Court.

31. ______ I further understand that I must pay my restitution as a condition of progressing through the phases of the Drug Court program. At a minimum, I understand that I must pay twenty (20) percent of the total restitution owed before I can advance from one phase to the next. I understand that I will be required to have paid twenty percent to advance to phase two, a minimum of forty percent to advance to phase three, sixty percent to advance to phase four, eighty percent

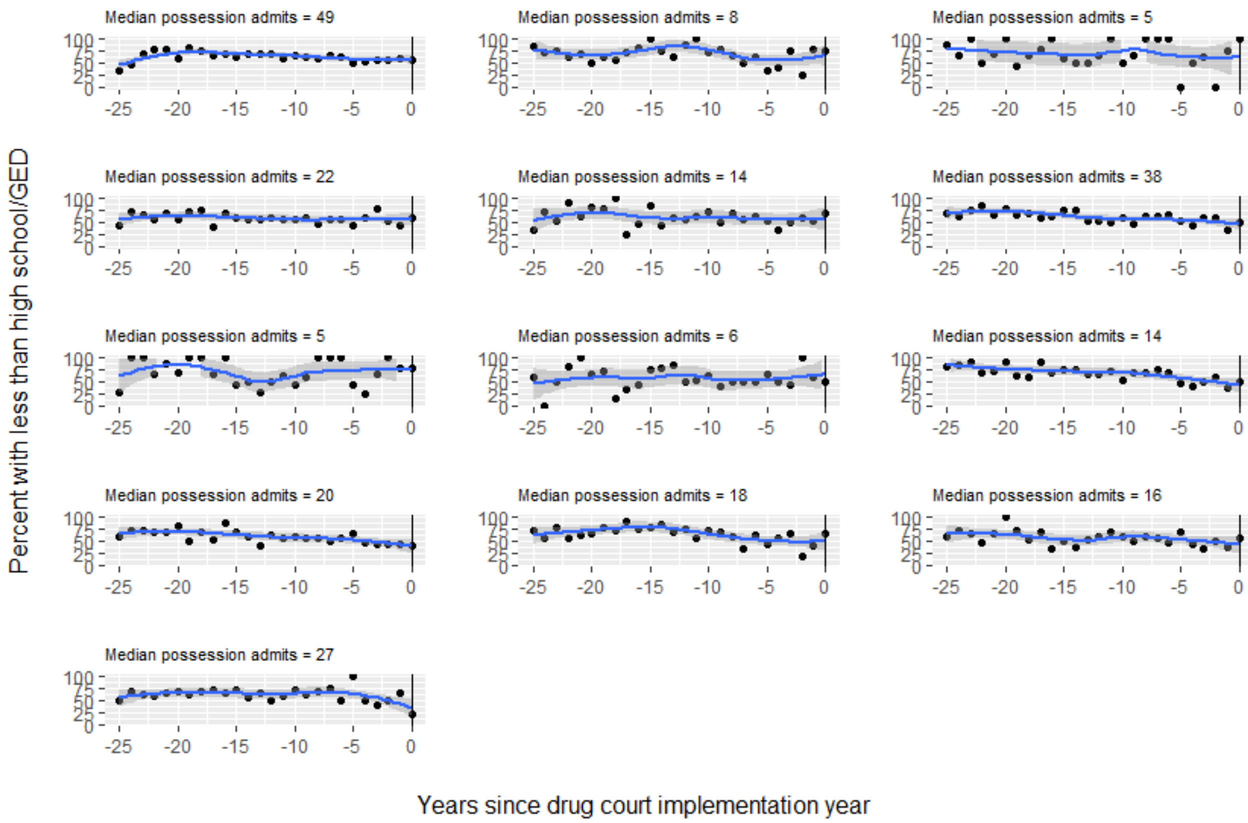
Appendix Figure C.3. Program contract: Pay fees on time – Examples from two Georgia drug courts.



Appendix Figure C.4. Georgia drug courts 1991-2016: Educational attainment of people admitted to prison for drug possession by drug court implementation year – Middle adopter courts.

Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Vertical line represents timing of drug court implementation. Smoother is a loess curve.

Appendix Figure C.5. Georgia Drug Courts 1991-2016: Educational attainment of people admitted to prison for drug possession by drug court implementation year – Never adopter courts.



Source: Author's analysis of National Corrections Reporting Program 2000-2016 data.
Notes: Vertical line represents timing of drug court implementation. Smoother is a loess curve.

VITA

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