

Imprisonment and Labor Market Outcomes: Evidence from a Natural Experiment¹

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Because of racially disproportionate imprisonment rates, the literature on mass incarceration has focused on the labor market consequence of imprisonment and the implications of those effects for racial inequality. Yet, the effects of imprisonment itself, as distinct from conviction, are not well understood. The authors leverage a natural experiment based on the random assignment of judges to felony cases in Michigan to examine the causal effect of being sentenced to prison as compared to probation, stratifying by race and work history. The most widespread effect of imprisonment on employment occurs through incapacitation in prison, both for the initial prison sentence and through the heightened risk of subsequent imprisonment. Negative postrelease effects of imprisonment on employment, employment stability, and employment outside the secondary labor market are concentrated among whites with a presentence work history. Postrelease effects of imprisonment on employment among those with no work history are positive but fade over time.

INTRODUCTION

The rise of mass incarceration in the United States over the last four decades has prompted intense interest among social scientists—and, more recently,

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the public at large—in the consequences of incarceration for the individuals and families who experience it (Western 2006; Alexander 2010; Wakefield and Wildeman 2013; NRC 2014; Kilgore 2015; Turney and Wildeman 2015), for social stratification (Wakefield and Uggen 2010), and for the functioning of local and national institutions (Manza and Uggen 2004; Clear 2007; Weaver 2014). One of the most important questions for understanding the links between mass incarceration and inequality is whether and how being sentenced to prison adversely affects one's future employment and earnings (Pettit and Western 2004; Western 2006; Bushway, Stoll, and Weiman 2007).

Despite the volume of research on imprisonment and labor market outcomes, a recent National Research Council (NRC 2014) report concludes that although “the bulk of the evidence supports the conclusion that incarceration is associated with poor employment outcomes” (p. 258), “current research findings do not make it possible to distinguish among the effects of criminal behavior, criminal conviction, and the experience of incarceration as they relate to subsequent labor market experiences” (p. 256). This weakness in the empirical literature stems primarily from three conceptual and methodological problems in prior research. The first is specifying a proper comparison group of individuals who could have been sentenced to prison but were not. Most prior research relies on survey data or audit studies that conflate in various ways other forms of criminal justice involvement, such as arrest, jail, and conviction, with imprisonment. The second is unobserved confounding. Individuals who are sentenced to prison tend to have very poor labor market prospects even before imprisonment, so it is difficult to convincingly separate individual characteristics from the effect of imprisonment itself. The third is the difficulty of specifying for whom imprisonment affects labor market experiences and how such effects arise. The prior empirical literature has not explored effect heterogeneity or possible proximate causes of the observed poor labor market outcomes, such as incapacitation effects or relegation to the secondary labor market, that have been hypothesized in the current literature.

In this article we revisit the question of how going to prison affects one's subsequent labor market experiences, while addressing these key limitations of prior research. We conduct a quasi-experimental analysis of administrative data on the population of all individuals convicted of a felony in the

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state of Michigan in the years 2003–6 (over 100,000 individuals). Our approach to estimating the causal effect of prison sentences on future labor market outcomes relies on counterfactual comparisons between people convicted of felonies who were sentenced to prison and those who were sentenced to probation. We address the problem of confounding due to selection into prison by leveraging a natural experiment that uses the random assignment of felony cases to judges as instrumental variables, a design that allows us to identify the effect of prison using only variation in sentencing practices between randomly assigned judges. Our large sample size allows us to examine how imprisonment's effects vary by two key structural characteristics of those most at risk of criminal justice involvement, race and prior labor market experience. Finally, our detailed administrative data allow us to separate the short-term incapacitation effects of imprisonment from longer-term effects after release and to provide one of the first empirical tests of a core hypothesis about whether people returning from prison are relegated to the secondary labor market (Western 2002; Weiman, Stoll, and Bushway 2007).

Our results suggest that the effect of imprisonment on employment is far more nuanced than the current conventional wisdom considers. The largest and most widespread effects of being sentenced to prison on employment occur through incapacitation, not only from the period of incarceration connected to the initial sentence but also from being returned to prison in the future on a new sentence or parole revocation, which we term "secondary incapacitation." Furthermore, the postrelease effects of imprisonment on employment vary substantially across subgroups defined by race and work history. Imprisonment is most damaging over the long term for people who had the strongest economic prospects before being convicted of a felony, and it is these individuals who are most at risk of relegation to jobs in the secondary labor market upon release from prison. Our results imply that prison's primary role in expanding and reinforcing racial inequalities comes from removing hundreds of thousands of black men from the labor market at some point in their lives. Our results also suggest that other, even more widespread, forms of criminal justice contact may be just as critical as imprisonment to racial inequalities in employment and earnings.

INCARCERATION, EMPLOYMENT, AND RACIAL INEQUALITY

Since the mid-1970s the United States has experienced an enormous rise in incarceration. Whereas in 1975 the population in jails and prisons on any given day was roughly 400,000 people, by 2003 this number had increased more than fivefold to 2.1 million people (Western 2006). Although the upward trend in incarceration has leveled off in the last several years (Phelps and Pager 2016), the number of individuals in state and federal prisons was over 1.6 million at the end of 2009 (West, Sabol, and Greenman 2010). Com-

pared to other nations and compared to earlier periods in U.S. history, current incarceration rates are unprecedented (Raphael and Stoll 2009), leading to what some have termed the era of mass imprisonment (Garland 2001; Mauer and Chesney-Lind 2002). Sociologists and other social scientists have been documenting the consequences of high rates of incarceration in multiple domains, from health (Binswanger et al. 2007, 2012; Schnittker and John 2007; Binswanger, Krueger, and Steiner 2009; Steadman et al. 2009; Rich, Wakeman, and Dickman 2011; Uggen et al. 2016) to political institutions (Manza and Uggen 2004, 2006) to communities (Clear 2007; Morenoff and Harding 2014) and families (Sabol and Lynch 2003; Turney and Wildeman 2013; Haskins 2014, 2015; Turney 2014, 2015; Turney and Haskins 2014), but a central focus has been on the consequences of incarceration for inequality due to effects of serving time in prison on employment outcomes (Sabol and Lynch 2003; Pettit and Western 2004; Western 2006; Pettit and Lyons 2007; Pettit 2012).

Incarceration is disproportionately experienced by poor minorities, particularly young black men with low levels of education. This, together with the growth in incarceration rates over the last 40 years, means that incarceration policy may play an important role in the creation and maintenance of racial inequality. One in nine African-American men ages 20–34 is in prison on any given day (Pew Center on the States 2008), and among those with less than a high school degree the number is approximately one in three (Western 2006). Over half of African-American men with less than a high school degree go to prison at some time in their lives (Pettit and Western 2004). High rates of incarceration during the late 1990s have also been linked to declining rates of labor force participation among young African-American men at a time when a strong economy pulled other low-skill workers into the labor market (Holzer, Offner, and Sorensen 2005; Weiman et al. 2007). Wacquant (2001) and Alexander (2010) have argued that the prison system now plays the same role in racial domination and exclusion as slavery, Jim Crow, and the ghetto did in previous historical periods, separating African-Americans from whites, tainting blacks with a mark of inferiority, and providing a source of cheap and exploited labor. According to this framework, the black ghetto and the penitentiary are linked, both by high rates of movement between poor black neighborhoods and prisons and by their common symbolic status as locations of exclusion, stigma, and social control.

Approaches to Estimating the Effect of Imprisonment in Employment

Research on the labor market consequences of incarceration appears to be mostly consistent with theoretical accounts that link imprisonment to economic dislocations. In a recent review of this literature, the NRC's Committee on Causes and Consequences of High Rates of Incarceration (NRC 2014)

identified four recent studies of the effects of imprisonment on employment and earnings using survey data (Freeman 1992; Western 2006; Raphael 2007; Apel and Sweeten 2010) and seven that relied on administrative records (Waldfoegel 1994; Grogger 1995; Kling 2006; Pettit and Lyons 2007; Sabol 2007; Lalonde and Cho 2008; Loeffler 2013). All of the survey-based studies they reviewed found that incarceration was negatively associated with employment—with employment declining by 10%–20% after incarceration—and most also found negative effects on earnings. However, only four of the studies based on administrative records found evidence of a negative association between imprisonment and employment, and even in these, the effect size was much smaller, with employment declining by about 5 percentage points after incarceration. Moreover, administrative data studies also typically find a postimprisonment increase in employment or earnings (e.g., Pettit and Lyons 2007; Tyler and Kling 2007; Loeffler 2013), evidence that is inconsistent with large negative effects of imprisonment.

The discrepancy between the survey and administrative data studies may be the result of differences in how the control group is typically specified in the two types of studies. In most administrative data studies, the comparison is between two convicted groups, one of which gets a prison sentence and one of which does not. In contrast, it is difficult in most survey-based studies to specify a well-defined comparison group of individuals who were convicted and plausibly could have served time in prison (but did not) because surveys do not contain the detailed criminal justice contact information needed to construct such comparisons. Even survey-based studies with well-defined comparison groups have other limitations, such as not being able to distinguish between incarceration in jail and prison (e.g., Apel and Sweeten 2010).²

A similar problem occurs in audit studies, where fictional job applicants or applications are sent to real employers in order to determine how the mark of a criminal record (as well as the race of the applicant) affects the likelihood that the applicant will be called back for an interview (Pager 2003, 2007; Pager, Western, and Bonikowski 2009). These studies compare individuals with a record to those without a record, and the incarceration experience is signaled using gaps in the work history and by revealing the conviction on the application, which conflates the effects of conviction and incarceration. Although the findings are often interpreted as representing effects of impris-

² Although jail is also a form of incarceration, we view jail and prison as very different institutions with potentially different effects. Jails hold not only individuals convicted of felonies but also individuals convicted of misdemeanors and individuals held awaiting trial (neither of these groups are in our data). Jails generally offer fewer rehabilitative services, but they tend to be closer to the homes and families of their inmates, since they are run by individual counties or cities. Stays in jails are also generally much shorter than prison stays.

onment per se, they may also reflect the consequences of other forms of criminal justice system involvement, such as arrest or conviction. Recent research has begun to document the consequences of more common but less intense forms of criminal justice involvement (e.g., Kohler-Hausmann 2013; Lara-Millán 2014). Researchers interested in assessing the impact of imprisonment versus precisely specified alternatives like probation have recently turned to detailed administrative data sets of criminal justice involvement linked with administrative data on formal labor market participation (e.g., Loeffler 2013; Mueller-Smith 2015).

Of course, studies with clear counterfactual comparisons still need to control for selection into prison. If those who spend time in prison are systematically different from those who do not in ways we are unable to observe, then estimates of effects will be biased and could lead us to false conclusions about the causal effect (or lack thereof) of imprisonment on employment. Administrative data that offer the possibility of correctly specified comparison groups tend to have fewer control variables than survey data (NRC 2014), so using observable characteristics to control for selection into prison is usually not a feasible strategy with administrative data. Researchers have responded by exploring quasi-experimental methods that exploit the nature of the sentencing process to control for selection bias. The most common is the use of random assignment to judges to capture exogenous variation in outcomes (e.g., Mueller-Smith 2015). Unfortunately, these studies have led to conflicting results, with some finding null effects with large standard errors due to small sample sizes (Kling 2006; Green and Winik 2010; Loeffler 2013) and others finding negative effects (Mueller-Smith 2015).³

As a result of these conceptual and methodological challenges, significant questions remain about the effect of imprisonment on labor market outcomes and its role in racial inequalities in employment and earnings. In other words, the studies that inform the seemingly consensus view that imprisonment is harmful (NRC 2014) do not by themselves provide strong evidence that a movement away from prison sentences and toward alternatives like probation would improve labor market outcomes or reduce racial inequalities.

Incapacitation Effects

The most direct way that incarceration affects employment is by incapacitating people and thereby removing them from the conventional labor mar-

³ Mueller-Smith (2015) used administrative data and random assignment to judges in a county in Texas. That study found negative and significant effects of incarceration on employment, but as in Apel and Sweeten (2010), incarceration was measured imprecisely as either prison or jail as compared to any noncustodial sentence. As a result, Mueller-Smith's results are not directly comparable to those presented here.

ket. Despite the clear link between incarceration and forced removal from the labor market, we are aware of only one prior attempt to measure and quantify this direct impact of incarceration on employment via incapacitation.⁴ Mueller-Smith (2015) finds that being incarcerated in jail or prison reduces the probability of employment in a calendar quarter by 0.32, with such effects concentrated among those with a work history. Although the aggregate consequences of penal incapacitation on lost employment and earnings are potentially large given the scale of imprisonment, particularly among blacks, the magnitude of such an incapacitation effect remains unclear because it depends on the counterfactual labor market outcomes of a well-defined comparison group of individuals who were not sentenced to prison, such as those who received probation sentences for felony offenses. Moreover, such incapacitation effects are likely to vary by presentence work history and race, as those with the best labor market prospects in the absence of imprisonment are most likely to be negatively affected by it.

It is also important to note that incapacitation can be a recurring phenomenon because being sentenced to prison increases one's probability of future imprisonment—the so-called revolving door of prison—in large part because of the greater surveillance and scrutiny that comes with postrelease parole supervision (Harding et al. 2017). Those under parole supervision are at risk of reincarceration not just for new felony crimes but also for technical violations of parole such as curfew violations, absconding, and positive drug or alcohol tests. Because future imprisonment removes former prisoners from the labor market, it will reduce their probability of future employment at least somewhat if some of them would have been employed. Probationers are also potentially subject to imprisonment for technical violations—the “net widening” consequence of probation (Phelps 2013)—but probation supervision is generally less intensive than parole (Petersilia 2003). Moreover, if prison increases crime compared to probation (Nagin, Cullen, and Jonson 2009; Nieuwbeerta, Nagin, and Blokland 2009), that effect will also increase one's risk of future imprisonment relative to probationers.

Relegation to the Secondary Labor Market

One of the literature's primary hypotheses about how imprisonment affects employment is that the stigma of imprisonment relegates former prisoners to the “secondary labor market,” as characterized by jobs with low wages, high turnover, poor working conditions, and few possibilities for upward mobil-

⁴ The study of criminal or penal incapacitation in criminology is sometimes criticized because researchers often assume that individuals do not commit crime in prison (Binder and Notterman 2017). Employment incapacitation is much clearer. Individuals in U.S. prisons do not work for market wages in the free economy, and in-prison employment is not equivalent to employment outside of prison.

ity (Western 2006; Bushway, Stoll, and Weiman 2007). There is some evidence that people returning from prison struggle to find jobs and often must settle for work in the secondary labor market. For example, Western (2002) shows that incarceration does not just reduce employment and earnings in the short term but also lowers earnings growth over the long term, suggesting that former prisoners may be relegated to the secondary labor market as a result of their incarceration. Moreover, prior research shows that an important feature of involvement in the labor market among former prisoners is the high degree of volatility in employment (Cook 1975; Sugie 2016), which could also be explained by employment in industries with high turnover more generally (such as the service sector and temporary labor). To our knowledge, no prior study has attempted to directly test the hypothesis that incarceration in prison relegates individuals to the secondary labor market. Moreover, there are reasons to be skeptical about this hypothesis. For example, the observed concentration of people with prison records in secondary labor market jobs could simply be a reflection of their lack of work experience or education and might have occurred had they not gone to prison. Also, since many black workers already face restriction to the secondary labor market because of racial discrimination, it is not clear whether they face an additional threat of exclusion from the primary labor market if they have a prison record.

Moving beyond the Average Effect of Prison

A firm understanding of how and for whom prison has its effects is critical for analyzing the effects of prison as an institution, interpreting empirical results, and informing policy discussions. In this section, we present a theoretical framework for understanding how the effect of prison on employment varies across individuals defined by race and work history, by considering the principal mechanisms through which prison may affect labor market outcomes after release, both positively and negatively. We emphasize that we are not able to test these mechanisms directly. Rather, this framework motivates our analysis of effect variation by race and work history. We focus on race and prior work history because they are both important predictors of imprisonment and central to wider inequalities in employment (Western 2006). We discuss three sets of theories about the possible consequences of imprisonment, all of which suggest that effects of imprisonment on employment will be less negative for blacks and for those without a preprison work history. There are even some mechanisms that suggest the effect of imprisonment on employment could be positive for some subgroups.

Stigma.—Perhaps the most hypothesized and studied mechanism through which imprisonment may affect employment is stigma. Relegation to the secondary labor market, discussed above, is not the only way that stigma may

affect labor market outcomes. Stigma can operate through formal and informal mechanisms. Formal stigma prevents individuals with a criminal record, or a record of certain types of crimes, from working certain types of jobs or from obtaining the licenses or other certifications needed for whole classes of jobs (Petersilia 2003).⁵ Those with a criminal record are barred from working in a very large number of occupations, even long after they have completed their sentences (Petersilia 2003).⁶ Formal barriers extend to other domains as well, such as housing and public benefits (Travis 2005). One that is particularly important for employment is a driver's license, especially in locations with a weak public transportation infrastructure, such as Michigan.⁷ Federal law requires states to revoke or suspend driver's licenses for those convicted of drug offenses or risk losing federal highway funds (Petersilia 2003).

Informal stigma affects employment prospects when employers prefer not to hire those with felony or prison records. Audit experiments find that having a criminal record reduces one's chance of a call back after applying for a job (Pager 2003, 2007; Pager et al. 2009; Uggen et al. 2014).⁸ A critical aspect of informal stigma is the use of criminal background checks by employers. With the computerization of criminal records and easier access to them online through both private companies and public records searches, use of criminal background checks among employers has been steadily increasing (Holzer, Raphael, and Stoll 2004, 2006, 2007), despite questions about the accuracy of these records, particularly those obtained through private com-

⁵ Another potential barrier to hiring those with criminal records is so-called negligent hiring lawsuits. If an employer is sued over the behavior or actions of an employee, and it is demonstrated that the employer should reasonably have known the employee was likely to engage in such behavior, the employer can be liable for negligent hiring. An employee's past criminal record can be used against the employer in such cases, although only if the crime is directly related to the job responsibilities (Petersilia 2003; see also Holzer et al. 2004). One question is whether employers are actually aware of these legal issues. Pager's (2007) employer surveys suggest most are not.

⁶ According to the American Bar Association, there are over 30,000 state laws, provisions, and exclusions from employment related to criminal records across the United States (NRC 2014). Another estimate suggests that over 800 occupations are closed to those with a criminal record somewhere in the United States (Bushway and Sweeten 2007). In Michigan specifically, there are almost 800 such laws and regulations related to employment, occupational and professional license, and business licenses according to the American Bar Association's (2016) National Inventory of the Collateral Consequences of Conviction.

⁷ For instance, Michigan scored a D+ on the most recent public transportation report card from the American Society of Civil Engineers (2009).

⁸ For an earlier generation of audit studies on the effect of a criminal record, see Schwartz and Skolnick (1964), Buikhuisen and Dijksterhuis (1971), and Boshier and Johnson (1974). These studies were conducted before the large increase in the incarceration that started in the mid-1970s and before technology that allowed for fast and cheap record checks.

panies (Bushway, Stoll, and Weiman 2007), and the inability of a criminal record to predict future criminal behavior in the long term (Blumstein and Nakamura 2009). Such informal stigma may have greater consequences for those with prior work experience, whose better future employment prospects may have more to lose from the stigma of imprisonment. Informal stigma might also have bigger impacts for whites with records than for blacks with records if blacks as a group suffer widespread discrimination in employment (Pager 2003).

In addition to preventing people with criminal records from obtaining jobs, the threat of being stigmatized may discourage former prisoners from actively looking for jobs. For example, Apel and Sweeten (2010) find that the negative effect of incarceration on employment among individuals with only one conviction is entirely due to not searching for work rather than searching but not finding work (see also Smith and Broege 2015). Sugie (2016) finds that some former prisoners observed for the first three months after release, particularly older individuals, stop looking for work after a short period of unsuccessful job searching. Those who already face challenges securing employment, blacks and those with little prior work experience, may be especially discouraged by the stigma of a criminal record.⁹

It is also unclear whether stigma is attached to having a criminal record, prison record, or some additive combination of the two. Audit studies and employer surveys tend to focus on felony criminal records (although see Uggen et al. [2014] on misdemeanors). Recent evidence from qualitative interviews in Chicago suggests that even those who have only been arrested and convicted of very minor crimes experience stigma in the labor market (Ispalanda and Loeffler 2016). One might hypothesize that imprisonment amplifies the impact of a criminal record because it signals a more serious crime, because employers worry about the effects of prison, or because imprisonment makes it harder to conceal a criminal record (because of resume gaps). But, imprisonment may have little additional impact, given that many peo-

⁹ In an effort to remediate stigma, some local governments have passed “ban the box” laws that prohibit employers from asking about a criminal record until a hiring decision has been made. One study found reductions in recidivism after implementation (D’Alesio, Stolzenberg, and Flexon 2015), but the question whether such measures help formerly incarcerated individuals escape the secondary labor market or improve employment more generally is not well understood. There is some evidence that ban-the-box measures in particular harm the overall employment of demographic groups who have high incarceration rates, such as low-skill black men, through “statistical discrimination,” as employers use group stereotypes regarding imprisonment when individual information is no longer available (Doleac and Hansen 2016). If ban-the-box policies harmed blacks without a criminal record or prison record, this might actually decrease estimates of the effect of prison on employment among blacks by reducing the employment prospects of blacks in the comparison group.

ple who suffer an incarceration spell for the first time have at least one conviction before entering prison (Langan and Levin 2002).

Erosion of skills, health, and social relationships.—A second set of mechanisms relates specifically to the social and physical conditions of prison and their potential effects on human capital, mental health, and physical health. As reviewed in Bushway, Stoll, and Weiman (2007), human capital may erode while in prison; the conditions of prison may lead to postrelease problems with physical health, mental health, and substance abuse; and “soft skills” may be damaged by the harsh social environment of prison, what has been termed “prisonization” (Haney 2002). With regard to soft skills, Caputo-Levine (2013) argues that the strategies and interaction styles that men develop to deal with the interpersonal violence of prison life become internalized and persist after release, making it difficult to perform well in job interviews or in a socially demanding work environment. For instance, former prisoners may be more sensitive to confining physical spaces, perceive accidental bodily contact as threatening, resist making small talk, and hesitate to display outward signs of friendliness such as smiling. Those with more human capital, as captured by prior work experience, may have more to lose from imprisonment.

The conditions of confinement in prison may also directly affect health, both mental and physical. First, incarceration can be a very stressful life event, and formerly incarcerated individuals are likely to encounter secondary sources of stress after release, including stigma and discrimination and difficulty finding jobs, housing, transportation and reuniting with family and friends (Massoglia 2008a, 2008b). Second, prisoners could face increased exposure to some infectious diseases (e.g., hepatitis C, influenza, tuberculosis, and HIV/AIDS) because of congregate living environments, poor infection control, and limited access to preventive interventions and could be less resistant to such diseases when under chronically high stress (National Commission on Correctional Health Care 2002; Massoglia 2008b; Johnson and Raphael 2009). Third, incarceration in prison may disrupt ongoing medical treatments for chronic conditions, leading to discontinuity of care at both prison entry and prison release. If these associations reflect causal effects of imprisonment and health problems affect employment, health may be an important mechanism linking incarceration to labor market outcomes.

Incarceration in prison may also affect social capital essential to the job search process, especially for those facing stigma. Prolonged incarceration can separate inmates from family and other social networks that can assist in job search (Braman 2004; Visser et al. 2004) or cause members of those networks to withdraw such assistance out of lack of trust or fear of consequences of vouching for someone who turns out to be a poor employee (Smith 2005, 2007, 2015). Few prisoners move back to the same neighborhoods where they lived before prison (Harding, Morenoff, and Herbert 2013), former prison-

ers experience high rates of residential mobility (Herbert, Morenoff, and Harding 2015), and white former prisoners live in more disadvantaged neighborhoods after prison than before (Massoglia, Firebaugh, and Warner 2013). However, an individual without a work history may have fewer such sources of assistance to lose during imprisonment, and blacks are less able to mobilize social capital in support of a job search (Smith 2007).

Positive transformation.—The above discussion suggests a negative effect of imprisonment on labor market outcomes that may be stronger or weaker depending on race and prior work experience. Yet there are also theoretical perspectives that suggest the potential for positive effects. First, many prisoners leave prison with a strong sense of optimism (Comfort 2012; Harding et al. 2016) that comes from a period of “cooling out” and “drying out.” The opportunity for reflection that prison can provide might prompt new efforts toward desistance, including abstaining from substance use, reconnecting with family, returning to school, or finding employment. Just as prison may separate individuals from positive social networks, it may also provide an opportunity for separation from social relationships that increase the probability of crime. Such effects may be strongest for those without work experience before incarceration. Second, criminologists have long theorized that prison should have a “specific deterrent” effect, by which the experience of incarceration itself should deter future criminal behavior in order to avoid the pains associated with future incarceration. Third, although for many years the conventional wisdom was that prisoner rehabilitation did not work, there is some evidence that some prison programming is effective at reducing recidivism for some prisoners (Petersilia 2003; Visser, Winterfield, and Coggeshall 2005; MacKenzie 2006). Moreover, at least for black men, there is evidence of improvements in health during imprisonment (Patterson 2010, 2013). Prisoners might also experience increased screening and treatment for some health conditions compared to what they would experience in the community, leading to improvements in these conditions (NRC 2014; Uggen et al. 2016). If employment outcomes are also improved through one or more of these processes, then prisoners may fair better than probationers in the longer term. We expect that such rehabilitation is most beneficial for those with little or no prior labor market experience.

METHODOLOGY

Research Design

Our goal is to estimate the causal effect of being sentenced to prison rather than probation on various labor market outcomes. Because of the threat of unobserved differences between individuals sentenced to prison and probation, we rely on a natural experiment based on the random assignment of

judges to criminal cases. Judge identifiers serve as instruments for sentence type. Because they are randomly assigned to criminal cases, they provide a source of exogenous variation in sentence type, or variation that is uncorrelated with individual and offense characteristics that might be predictive of labor market outcomes. The intuition behind an instrumental variables design is to estimate the causal effect of interest (e.g., prison vs. probation sentence) using only the variation in the “treatment” produced by the instrumental variables. This approach also assumes that the variation in treatment assignment provided by the instrument is independent of both observed and unobserved predictors of the outcome. Because we have a large number of cases, we can examine variation in the effects of sentence type by race, sentence length, and work history.

Data

We collected, cleaned, and coded data on all individuals sentenced for felonies in Michigan between 2003 and 2006 from administrative databases at the Michigan Department of Corrections (MDOC). A primary source of data is the presentence investigation reports prepared for judges before sentencing, which provide our presentencing covariates as well as judge identifiers. (In Michigan these reports are prepared by an employee of MDOC for all felony cases, even individuals not sentenced to prison.) We follow our subjects in MDOC records (felony probationers are supervised by MDOC) between the date of their sentencing and the end of October 2013, to see when prisoners are first released from prison and to track all subsequent entries into prison, both for new felony sentences and for technical violations of probation and parole.¹⁰ Crimes for which our subjects were initially sentenced are described in table A6 in the appendix.¹¹

Our analytical sample excludes individuals for whom judges have no discretion in sentencing. This excludes individuals sentenced for first-degree murder or for “flat” sentences, in which the minimum sentence is the same as the maximum sentence and is set by statute (mostly felony firearm crimes).

¹⁰ Missing covariate data are imputed using a hotdeck procedure based on race and gender. The only variable with substantial missing data is education (14% of the sample). Race is missing for 0.2% of the sample, and marital status is missing for 0.3% of the sample.

¹¹ Table A6 shows that once we stratify by presentence work history, there are few patterns by race in the differences in crime type between prisoners and probationers. This suggests that differences in crime types cannot explain different effects of imprisonment by race. Other appendix items include table A4, which shows descriptive statistics on covariates by race, work history, and sentence type. The appendix also describes the covariates included in all models. And table A2 shows descriptive statistics on all outcomes over time since sentence or since release and by race and presentence work history.

We also exclude individuals for whom judges may not have been randomly assigned: those whose cases are handled by specialty courts, individuals who were on probation and were resentenced for a technical probation violation, individuals sentenced by judges who heard fewer than 100 cases, and individuals in counties with only one judge. This leads to a final analytic sample of 111,110 individuals sentenced for a felony between 2003 and 2006, of whom 9,704 were black and sentenced to prison, 20,732 were black and sentenced to probation, 10,067 were white and sentenced to prison, and 22,327 were white and sentenced to probation (the remainder were sentenced to jail or jail followed by probation; these sentences are included in the models but not reported in this article). We estimate these models on all cases in the data and also stratify our analyses by work history and race (white or black). Unfortunately, there are too few Latinos or members of other racial/ethnic groups in Michigan to examine effects for those groups.

We also collected pre- and postsentence employment information from the Michigan Unemployment Insurance (UI) Agency to assess quarterly employment, quarterly earnings, industry, and employer in the formal labor market for our analytic sample between the third quarter of 1997 and the second quarter of 2012. To match individuals with their quarterly employment records, all social security numbers (SSNs) available in MDOC databases were sent to the Michigan UI Agency and Workforce Development Agency. In some cases, more than one SSN and name was available for each subject, because of the use of aliases. We prioritized SSNs that were also listed in Michigan State Police records, to which we also had access. Returned UI records were matched with names from MDOC databases, including aliases, to eliminate incorrect SSNs. If more than one SSN that MDOC had recorded for the same person matched records in the UI data, project staff selected the best match by comparing employer names listed in the UI records with those listed in the MDOC records. Only 1.25% of individuals eligible for our analytical sample did not have sufficient identifying information for matching. These individuals are excluded from the analysis entirely.

The use of unemployment insurance records in prior studies has been criticized on two grounds (NRC 2014). The first is that these records only capture part of the employment experience of those who are formerly incarcerated because they include only formal employment. While this is undoubtedly true, it is also unlikely that informal employment is related to the main mechanisms regarding the effects of incarceration, stigma, and relegation to the secondary labor market. The secondary labor market is part of the formal labor market, and stigma is driven largely by background checks. We know of no evidence that background checks, either formal or informal, are a factor in the informal "off the books" labor market. We would also argue that formal employment, with its many associated social protections, is the most desirable employment and therefore a better measure of integration

into the economy and one's longer-term earnings potential. Nationwide, 96% of formal jobs are covered by the unemployment insurance system, with little variation across states (Bureau of Labor Statistics 1997).

The second criticism is low match rates to unemployment insurance data, a consideration presumably driven by concerns about identifying information used to conduct the match. Prior studies using these records find that individuals who are incarcerated have very low rates of involvement in the formal labor market as measured by unemployment insurance records (Tyler and Kling 2007; Pettit and Lyons 2007; Sabol 2007; Cook et al. 2015), with rates as low as 30% reporting any UI income in the year after release. These rates are, if anything, lower before the incarceration spell (Pettit and Lyons 2007; Sabol 2007; Tyler and Kling 2007; Ramakers et al. 2015). These studies have been questioned because of their relatively low match rates overall, typically around 60% (NRC 2014). However, this low match rate is deceptive, because matches between criminal records and employment records will only occur for people who actually work in the formal sector.¹²

One way to counter this potential problem is to widen the search scope for UI information to include more years, as even one quarter of UI employment over a multiyear period is enough to create a match. In this study, we were able to match 86.5% of our analytic sample to at least one quarterly employer record between 1997 and 2012, which means that we should be able to more reliably observe employment than past studies using administrative data. Another possible source of failure to match in prior work is employment in neighboring states. However, Michigan's labor market is largely self-contained because of its many water boundaries and low population density near its land borders.¹³

We also argue that the strengths of administrative data outweigh these potential weaknesses. Administrative data offer the large samples, quasi-experimental identification strategies (i.e., random assignment to judges), and very precise information about criminal justice involvement that allow for causal estimates of the kinds of processes and outcomes missing from the current literature.

¹² One possible solution is to exclude people without matches from the analysis. Unfortunately, this would exclude an unknown but likely sizable number of people who legitimately have no earnings in the labor market both before and after the sentence. This strategy would be counterproductive because it would understate the negative impact of imprisonment if imprisonment leads to withdrawal from the formal labor market.

¹³ Our own calculations of data from the American Community Survey for 2003–12 provided by the Integrated Public Use Microdata Series (IPUMS; Ruggles et al. 2015) find that only 1.9% of currently working residents of Michigan work outside the state.

Measures

We measure outcomes in time relative to both the sentence date and the release date (see the discussion of risk periods below) at three time points from each date: one year (or the fourth full calendar quarter), three years (or the twelfth full calendar quarter), and five years (or the twentieth full calendar quarter). For MDOC records of entries into prison, we include any move to prison whether for a new felony sentence or a technical violation of parole or probation (parole is community supervision after release from prison and occurs for a varying length of time depending on behavior on parole and the time remaining on one's maximum sentence at release from prison). An individual is coded 1 on this variable at each time point if he or she experienced that event at any time before the time point.

We constructed multiple labor market outcomes from the unemployment insurance records. The most basic is whether the individual had any formal employment in the fourth, twelfth, or twentieth quarter since the start of the risk period (hereafter, focal quarters). To assess employment stability, we constructed a measure of whether the individual was employed in the focal quarter and the two prior quarters and a measure of the proportion of quarters employed out of the focal quarter and all prior quarters.

To examine the hypothesis that incarceration in prison increases the probability of relegation to the secondary labor market, we classified employers as secondary labor market employers on the basis of two-digit North American Industry Classification System (NAICS) industry codes.¹⁴ Employers in Forestry, Fishing, Hunting, and Agriculture Support (NAICS code 11); Retail Trade (44 and 45); Administrative and Support and Waste Management and Remediation Services, which includes temporary labor (56); and Accommodation and Food Services (72) were classified as employers in the secondary labor market. These are the industries that are most strongly associated with precarious employment in prior research (e.g., Kalleberg, Reskin, and Hudson 2000). Together these employers accounted for 49.8% of employed person-quarters in the overall sample between 2003 and the second quarter of 2012.¹⁵ On the basis of this classification, we constructed measures of whether the individual was employed in an industry not associated with the secondary labor market in each of the focal quarters.

¹⁴ Unfortunately the UI data do not contain occupation, so there is potential to misclassify individuals who work in industries associated with the secondary labor market but who themselves are not in the secondary labor market, such as managers of restaurants or retail stores.

¹⁵ Only 0.7% of employed person-quarters in 2003–12 had employers without valid NAICS codes.

Modeling Strategy

To implement our instrumental variables estimator of the effect of incarceration in prison versus probation, we use two-stage least squares estimation (2SLS).¹⁶ Our instruments are a set of dummy variables for the assigned judge plus interactions between judge dummies and presentence characteristics. Although our primary “treatment” in this article is the comparison between prison and probation sentences, there are multiple dimensions to felony sentencing that need be taken into account in the modeling strategy. One is that there are other possible sentences apart from prison and probation, including jail and jail followed by probation. Second, a judge must decide on the minimum sentence length in months for probation and prison sentences (the maximum sentence length is set by statute for the specific crime) or the jail sentence length in the event of a jail sentence. Failure to properly condition on these other aspects of sentencing could lead to biased estimates because these aspects are affected by the judge who is assigned (the instrument) and also has the potential to affect the outcome, so omitting them from the model could lead to a violation of the exclusion restriction. Although we include individuals with jail and probation with jail sentences in the analysis to avoid introducing sample selection bias, we focus on prison versus probation because it provides a comparison between imprisonment and no imprisonment.

Our second-stage models include a set of three binary treatment variables for prison, jail, and jail with probation sentences, with probation as the omitted category. These models also include interactions between prison and prison sentence length, probation and probation sentence length, jail with probation and probation sentence length, and jail with jail sentence length.¹⁷ We instrumented for the three sentence type treatments (prison, jail, jail with probation) and the three sentence length treatments (prison sentence length, probation sentence length, jail sentence length) using the set of all judge dummy variables and their interactions with the presentence characteristics in table A4, resulting in six first-stage equations. All first- and second-stage models also condition on the main effects of the presentence characteristics and county fixed effects (county dummy variables). The county fixed effects are necessary because judges were randomly assigned within counties. They also serve to control for county differences in sentencing practices and out-

¹⁶ Models were estimated using the `ivregress` routine in Stata ver. 14.

¹⁷ All sentence length variables are specified with quadratic functional form. We do not include the interaction between jail with probation and jail sentence length because jail sentence lengths that accompany jail with probation sentences exhibit little variation and are generally only a few months.

comes.¹⁸ Prison and probation sentence length variables are centered at 24 months (the modal sentence length for both sentence types), so coefficients on the prison dummy variable are interpretable as the effect of a 24-month prison sentence compared to a 24-month probation sentence. Both first- and second-stage models are estimated using ordinary least squares (OLS) regression, as is conventional in the instrumental variables literature.¹⁹ Robust standard errors are reported for all effect estimates.

Implementation and Interpretation

A key issue for our analysis is the appropriate “risk” period for measuring outcomes. Probationers will be “at risk” for employment outcomes immediately following sentencing, but those sentenced to prison will not be at risk until their first release (or parole) because of incapacitation in prison. One aspect of both theoretical and policy interest is the total effect of imprisonment, which combines the effect of serving time in prison versus being on probation (i.e., incapacitation) and the effects that persist after the prisoners are released. This effect can be captured by starting the risk period at the sentence date for all cases. Starting the risk period at sentencing to capture the total effect of imprisonment is critical for understanding the potential effects of sentencing reforms that would change the probability of imprisonment for the marginal person convicted of a felony. This approach also provides the cleanest counterfactual comparison between those who receive different types of sentences.

The other option is to start the risk period for prisoners at release. This approach is often used when it is desirable to remove the effects of incapacitation and focus on differences between the experiences of prisoners and probationers when both are in the community, but it also introduces potential problems. First, individuals in the prison and probation groups who were

¹⁸ An additional reason to include county fixed effects is that tighter labor markets and more manufacturing employment are associated with better employment and recidivism outcomes for former prisoners, particularly for individuals who have only been to prison once and those who were employed before prison (Bushway, Stoll, and Weiman 2007; Wang, Mears, and Bales 2010; Bellair and Kowalski 2011; Nguyen, Morenoff, and Harding 2014). Thus, it is important to remove variation in employment outcomes across counties.

¹⁹ Modeling binary outcomes with a linear probability model, as we do here, creates the risk of biased estimates due to violation of assumptions of the linear model. Our use of robust standard errors corrects for violations of the assumption of homoscedasticity of the residuals, and we have assessed the sensitivity of our estimates to the use of linear probability models by comparing our estimates with those that use a probit model for the second stage and then calculating average marginal effects in the probability metric. These produce almost identical estimates. We prefer the linear probability models because of their ease of interpretation and because they allow us to residualize our outcomes by age and year (see below).

sentenced in the same year will start their risk periods in different years, and as a result, differences in sanction type will be conflated with the passage of time, which itself can affect recidivism or employment through either period effects (e.g., changes in the labor market or secular influences on crime) or age effects. Second, when the risk period is started at release for prisoners, treatment effects could be biased because people in the prison group are likely to be older, on average, at the start of the risk period (because of the passage of time). Third, release dates are endogenous because they are determined in part by postsentencing behavior in prison, which introduces another form of bias. Finally, starting the risk period for the treated at their release date will shrink their follow-up period, meaning those with longer prison sentences will not have postrelease outcomes to measure, potentially introducing some sample selection bias into the estimates.²⁰

Because we want to identify the incapacitation mechanism and its contribution to the overall effect, we conduct our analysis both ways. This also facilitates comparison with other studies, which typically measure outcomes from the release date (e.g., Loeffler 2013). To deal with the confounding of age and period discussed above, we residualize all outcomes on the entire sample by age and year.²¹ We have no solution to the endogeneity of release or the sample selection problems, so these “from release” estimates should be interpreted more cautiously. This problem is shared by most studies that use prison release as the starting point for the risk period.

So far, the discussion has assumed that treatment effects are constant across the population, yet it is more reasonable to assume that the effects of prison versus probation vary across individuals (termed “heterogeneous treatment effects” in the literature). When treatment effects are heterogeneous, instrumental variables methods estimate the local average treatment effect (LATE), which means that the estimated effects apply not to the entire population of those actually treated but rather to those whose treatment status is changed by the instrument. Here, this means we are estimating the effects of prison on those whose sentence is influenced by the judge to whom they were assigned. Some individuals, because of their crimes and their histories, would be sentenced to prison by all judges and some to probation by all judges. Our estimates capture the effect of prison among those on the margin, who were sentenced to prison because they were randomly assigned

²⁰ Overall, 22% of prisoners were not released in time to measure employment in the fourth quarter following release, 34% of prisoners were not released in time to measure employment in the twelfth quarter following release, and 57% of prisoners were not released in time to measure employment in the twentieth quarter following release.

²¹ We estimate an OLS regression model for each outcome that includes only dummies for age and year as predictors and then take the residuals from these models as our outcomes in the main analyses. Such residualized outcomes are therefore independent of age and year.

a more punitive judge rather than a more lenient judge. It is not possible to identify these individuals in the data, but we discuss this to be clear about what parameter we are estimating.²²

RESULTS

Employment Pre- and Postsentencing

We begin by describing the presentence and postsentence employment of those sentenced to probation or prison, in order to understand the central underlying patterns in the data, as these will inform our interpretations of model results below and will reveal important insights of their own. Figure 1 shows the proportion of individuals with any employment in the formal labor market at each calendar quarter over the window of time beginning nine years (36 quarters) before and ending nine years after the sampled sentence date, by race and sentence type (prison vs. probation). Lines with open circles represent the employment trajectories of probationers, while those with filled circles represent prisoners. Employment trajectories for blacks are represented by black lines, and those for whites are represented by gray lines. The *X*-axis measures time (in calendar quarters) since sentencing, with negative values representing the presentence period.

We highlight five main empirical observations from figure 1. One is that rates of formal employment before sentencing are low for all groups.²³ None of the groups reached much over 50% employment in the formal labor market, and for the worst-off group, blacks sentenced to prison, the employment rates peaked at only about 28% before sentencing.²⁴ Second, employment rates for blacks are lower than those for whites. Indeed, whites who were sentenced to prison have similar employment rates to blacks who were sentenced to probation. Third, all groups experienced a drop in employment in the quarters immediately preceding sentencing, but the decline is especially sharp for those who were sentenced to prison. We believe this reflects, at

²² When there are more instruments than endogenous regressors—as is the case in our analysis, with multiple judges and interactions between judges and individual characteristics—the LATE interpretation of the treatment effect is as a weighted average of the effects that would be produced by using each instrument individually (Angrist and Pischke 2009).

²³ By employment rate, we mean the proportion of individuals working in the formal labor market divided by all individuals old enough to work (quarters in which an individual is less than 15 years old are excluded). We do not distinguish between whether someone is in the labor market in the way that official unemployment statistics do.

²⁴ Limiting the graph to those who have never been sentenced for a felony—and therefore have never been to prison before—does not improve the employment rates substantially. Only whites who will be sentenced to prison for their first felony offense have better employment records than the white prisoners in fig. 1, and even for them employment rates peak around only 45% (see app. figs. A2 and A3).

Imprisonment and Labor Market Outcomes

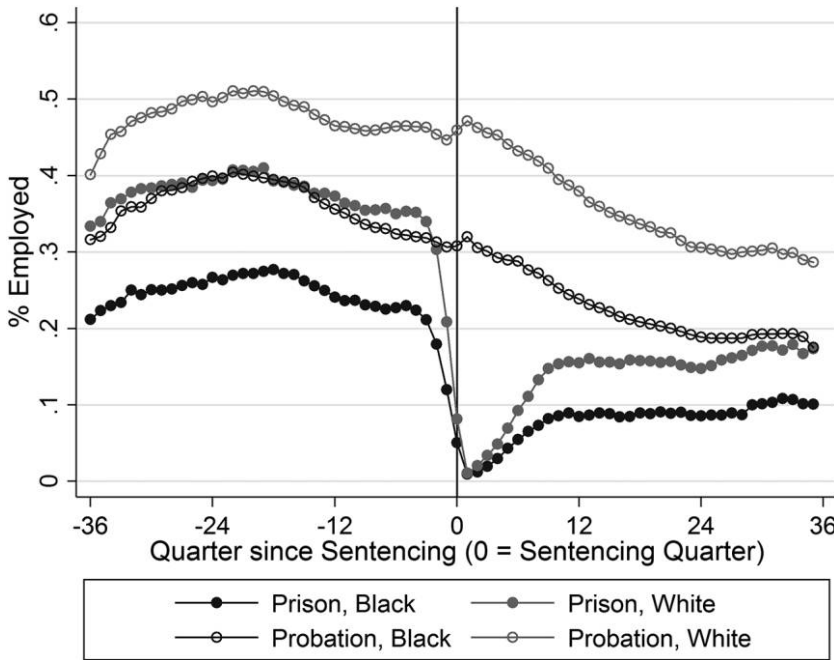


FIG. 1.—Employment relative to sentence date, by sentence type and race.

least in part, pretrial detention, which is more common for those who will be sentenced to prison (see app. table A7).²⁵ Fourth, the graph reveals important differences in the presentence employment trajectories of those sentenced to prison and probation. The employment trajectories of blacks and whites sentenced to prison decline sharply before sentencing, while those of probationers experience a more gradual decline in employment before sentencing.²⁶ A final observation concerns the different postsentencing trajec-

²⁵ All models presented below condition on pretrial detention to ensure that effects of imprisonment are not due to pretrial detention.

²⁶ The divergence in presentence employment rates between eventual prisoners and probationers also informs how we think about appropriate comparisons for estimating the causal effects of imprisonment, and as we select methods for making such comparisons. For example, they violate the assumptions of conventional fixed or random effects estimators for panel data that attempt to capitalize on pre-post differences in employment and that have been used in much of the prior literature. Such models assume parallel trends before treatment (Vaisey and Miles 2017). Moreover, these violations would lead to biased conclusions about the negative impact of prison on employment. The natural experiment approach that we employ here does not rely on such fraught pre-post comparisons. Such divergent trajectories are similar (if not more divergent) among those convicted of their first felony (see app. fig. A2).

ries of those sentenced to prison and probation. Whites and blacks who were sentenced to prison experienced very low rates of employment (below 20% for white prisoners and below 11% for black prisoners at all postsentencing time periods), but employment rates for these groups rose over time as more people exited prison, with most of the growth occurring in the first three years (12 quarters) after sentencing.²⁷ The rate of increase in the employment rate of the prison group after the sentence date was higher for whites than blacks. Despite the rising employment among those in the prison group, their employment rates remained lower than the corresponding rates of the probation group, although this gap narrowed considerably over time.

Figure 2 shows how the employment trajectories of those sentenced to prison and probation compare when we censor the observations of prisoners during the time they are imprisoned for their sampled prison sentence. This method, which eliminates the effects of incapacitation, only affects the trajectories of those sentenced to prison in the period after sentencing/release. Thus, all presentencing trajectories are identical in figures 1 and 2, and the postsentencing trajectories of probationers are also identical in figures 1 and 2. Both white and black prisoners experienced a slight increase in employment after their release, compared to their presentence employment rates. Similar trends have been observed in prior research using UI data to track formal employment among former prisoners in other states (Pettit and Lyons 2007; Tyler and Kling 2007; Loeffler 2013). This initial blip is then followed, however, by a protracted period of declining employment rates for whites and blacks returning from prison, some of which is due to subsequent spells of imprisonment. As a result, the unadjusted gap between prisoners and probationers of the same race narrows only slightly over time. It is also important to note that within the comparison group, people sentenced to probation, postsentence employment rates were much higher for whites than blacks. This racial difference within the comparison group will have important implications for interpreting estimates of effects of imprisonment by race below. The low rate of employment among black probationers means that there is less potential for prison to reduce employment among blacks.

We also examined trajectories of employment rates for groups defined by race and work history. We considered people to have “no work history” if they were not employed in the formal labor market in any of the 12 calendar quarters (three years) before sentencing. Lack of work history was more common among blacks (37%) than whites (24%) and more common among those

²⁷ The median length of prison sentences for whites and blacks was two years (eight quarters). By the end of the twelfth quarter, a little less than half of those sentenced to prison (46% of whites and 49% of blacks) remained in prison, while by the twentieth quarter, only 27% of whites and 33% of blacks who were sentenced to prison remained in prison.

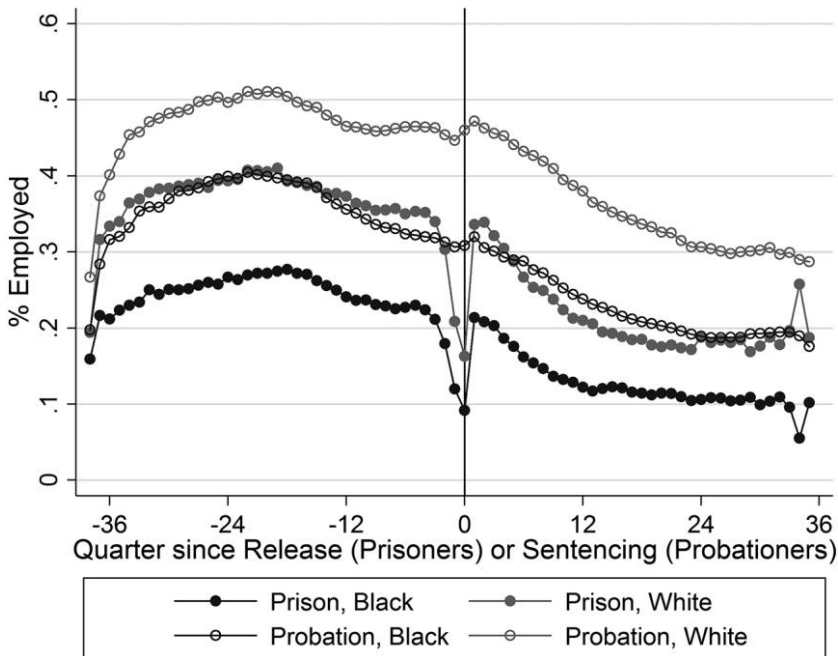


FIG. 2.—Employment relative to release date, by sentence type and race (excludes focal prison).

sentenced to prison (27%) than probation (22%; see app. table A4).²⁸ Further descriptive analysis revealed that the employment rate gap between the probation and prison groups was wider among those with a presentence work history and very small for those with no work history (see app. table A2). This suggests that we are most likely to see effects of imprisonment among those with a work history.

Effects of Prison on Employment

Table 1 shows regression estimates of the effect of being sentenced to prison versus probation for the sample as a whole based on five different estimators. We estimated models for four different employment outcomes: employment in the fourth quarter following sentence and following release and employment in the twelfth quarter following sentence and following release. (The postsentence vs. postrelease distinction is the same as between figs. 1

²⁸ Individuals who are younger at sentencing are of course much less likely to have a presentence work history. Models estimated excluding individuals age 25 or younger at sentencing produce similar estimates to those presented below.

TABLE 1
2SLS ESTIMATES OF EFFECT OF 24-MONTH PRISON SENTENCE VERSUS 24-MONTH PROBATION SENTENCE ON
POSTSENTENCE AND POSTRELEASE LABOR MARKET OUTCOMES FOR THE ENTIRE SAMPLE

	ANY EMPLOYMENT IN QUARTER				FUTURE IMPRISONMENT	
	4th Quarter Postsentence	12th Quarter Postsentence	4th Quarter Postrelease	12th Quarter Postrelease	Within 3 Years after Sentence	Within 3 Years after Release
OLS model:						
OLS without controls	-.34*** (.00)	-.18*** (.00)	-.08*** (.00)	-.08*** (.00)	.06*** (.00)	.21*** (.00)
OLS with controls	-.24*** (.00)	-.09*** (.00)	.03*** (.00)	.02*** (.00)	-.01*** (.00)	.12*** (.00)
Preferred instrumental variables specification: 2SLS models with judge-covariate interactions	-.24*** (.02)	-.09*** (.02)	.02 (.02)	.01 (.02)	.06*** (.01)	.20*** (.01)
Robustness checks—alternative specifications of instruments: 2SLS models excluding judge-covariate interactions	-.31*** (.04)	-.09* (.04)	-.06 (.04)	-.07 (.04)	.22*** (.03)	.28*** (.033)
2SLS models, judge harshness score as instrumental variable	-.27*** (.02)	-.13*** (.02)	-.05 (.03)	-.04 (.03)	.17*** (.02)	.29*** (.02)
Observations	109,636	109,636	105,346	102,890	109,636	105,435

NOTE.—Robust SEs are in parentheses.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

and 2.) These estimates are from linear probability models, so coefficients can be interpreted as the percentage point changes in the probability of the outcome associated with receiving a prison sentence rather than a probation sentence.

The first row of table 1 shows effects from an OLS model without any controls other than county fixed effects. Imprisonment is associated with lower probabilities of employment at all time points, and this negative association is much stronger in the postsentence comparisons, which include the effects of incapacitation. Controlling for covariates in the second row makes the employment associations less negative (even flipping the sign of the coefficients for postrelease employment from negative to positive), as we would expect if those sentenced to prison had systematically worse labor market prospects and greater risk of criminal behavior than those sentenced to probation because of presentence characteristics.

The third row shows estimates from the 2SLS models that leverage the natural experiment provided by random assignment of judges with different sentencing styles to produce plausibly causal effect estimates. Imprisonment is estimated to reduce the probability of employment in the fourth quarter after sentencing by over 24 percentage points for the marginal prisoner, that is, the prisoner for whom the randomly assigned judge made the difference between prison and probation. This effect shrinks to about 9 percentage points by the twelfth quarter, as more and more prisoners are released and able to find work in the formal labor market. The postrelease effects measure prisoners' outcomes starting at their release from prison, so they remove any incapacitation effects from the initial prison sentence. The estimated effects on employment at the fourth and twelfth quarters are very small and not statistically different from zero. This suggests that the post-sentence effects are largely due to incapacitation, at least for the sample as a whole.

As a robustness check, the fourth and fifth rows of table 1 present similar estimates based on two alternative specifications of the instrumental variables. Because of the risk of bias when using many weak instruments (see the appendix), we estimated 2SLS models with specifications with many fewer instruments. The specification in the fourth row reduces the number of instrumental variables by dropping the interactions between the judge dummy variables and individual characteristics, while the one in the fifth row uses only a single instrumental variable constructed as a latent measure of judge harshness.²⁹ Results from these two specifications lead to the same

²⁹ This measure is based on a regression model predicting prison vs. probation sentencing outcomes for all cases with dummies for each judge, controlling for county fixed effects and covariates. The coefficients on the judge dummies are the judge harshness score. In the 2SLS model, we instrument only for prison vs. probation and simply control for other sentence types and sentence length interactions because we only have one instrument.

general conclusions as our preferred specification, although some of the effects of imprisonment are larger in these specifications. However, they have less statistical power (i.e., they have larger standard errors) because the instruments are collectively weaker than those in our preferred specification. For this reason, and because our preferred specification deals with potential threats to the monotonicity assumption of 2SLS (see the appendix), we use our preferred specification from here forward (from table 1 it is clear that our preferred specification also produces more conservative estimates of imprisonment's effects).

One of the reasons that being sentenced to prison could have negative effects on employment that endure beyond one's release from prison is that people who were initially sentenced to prison are often returned in the future and thus experience subsequent dislocations from the labor market. To illustrate this mechanism, table 1 also shows regression estimates of the effect of the sentence type (prison vs. probation) on the probability of entering prison after the initial sentence, within three years of either the sentence date or the release date. A positive coefficient means that prisoners are more likely to be returned to prison than probationers are to enter prison. The 2SLS results from our preferred model show that being sentenced to prison increases one's probability of a subsequent entry into prison by about 6 percentage points when the time period is three years postsentence and by almost 20 percentage points when the time period is three years postrelease. This suggests that there is likely to be a "secondary" incapacitation effect of the initial prison sentence that affects one's chances of employment in the formal labor market through increasing one's risk of future imprisonment.

Effect Variation by Race and Work History

Thus far we have seen that being sentenced to prison has a negative effect on employment and that this effect appears to be largely driven by incapacitation in prison rather than outcomes postrelease. However, these overall estimates mask important variation in prison's labor market effects. Table 2 shows instrumental variables estimates of the effect of imprisonment on employment from our preferred model specification for six different subgroups defined by race and work history.³⁰ The results show that the effect of prison on employment varies by both race and work history.

We first consider the postsentence estimates, which reveal the effects of prison incapacitation. The models that are stratified by race but not work history show that being in prison reduced the probability of employment by a greater margin for whites than blacks. For example, at the fourth quarter fol-

³⁰ Appendix table A5 shows analogous estimates based on OLS regression, as well as analogous OLS estimates for the outcomes discussed in the next section.

Imprisonment and Labor Market Outcomes

TABLE 2
2SLS ESTIMATES OF 24-MONTH PRISON SENTENCE VERSUS 24-MONTH PROBATION
SENTENCE ON EMPLOYMENT AND IMPRISONMENT POSTSENTENCE
AND POSTRELEASE, BY RACE AND WORK HISTORY

	BLACKS			WHITES		
	Overall	No Work History	Work History	Overall	No Work History	Work History
Postsentence:						
Employment 4th quarter . . .	-.16*** (.02)	-.04** (.01)	-.29*** (.02)	-.30*** (.02)	-.09*** (.02)	-.41*** (.02)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
Employment 12th quarter . . .	-.03 (.02)	-.02 (.01)	-.09*** (.02)	-.12*** (.02)	-.03 (.02)	-.20*** (.02)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
Imprisonment 3 years	-.00 (.02)	.00 (.02)	.01 (.02)	.06*** (.02)	.05* (.02)	.05** (.02)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
Postrelease:						
Employment 4th quarter06*** (.02)	.09*** (.02)	-.01 (.03)	-.02 (.02)	.10*** (.02)	-.11*** (.03)
Observations	45,254	16,600	28,654	60,092	14,136	45,956
Employment 12th quarter04* (.02)	.07*** (.02)	-.03 (.02)	.00 (.02)	.05* (.02)	-.07** (.03)
Observations	44,136	16,140	27,996	58,754	13,780	44,974
Imprisonment 3 years14*** (.02)	.19*** (.03)	.13*** (.02)	.19*** (.02)	.20*** (.03)	.17*** (.02)
Observations	45,303	16,618	28,685	60,132	14,146	45,986

NOTE.—Work history = any formal employment in 12 calendar quarters before sentence; robust SEs are in parentheses.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

lowing sentence, the probability of employment was estimated to be 30 percentage points lower for the marginal white person sentenced to prison compared to probation, while the corresponding effect for blacks was roughly half as large (16 percentage points). The reason that the effect of prison incapacitation is stronger among whites is that white probationers had better employment prospects compared to black probationers, as seen in figures 1 and 2. When we further stratify these results by race and work history, we see that the negative effects of prison incapacitation are stronger for those who had a history of working before their sampled felony sentence. Among those with a history of work before being sentenced for the sampled felony sentence, the incapacitation effect is very large and significantly stronger for whites compared to blacks. These effects were strongest in the fourth quarter after sentence, at which point blacks with work history who were sentenced to prison were 29 percentage points less likely to be employed than those sen-

tenced to probation, while the comparable effect among whites with work history was 41 percentage points.³¹ The effect of being incapacitated in prison is much smaller among those without work history, and it is only statistically significant in the fourth quarter after sentence (when effects sizes are -9 percentage points for whites and -4 points for blacks; these coefficients were not significantly different from one another).

We now consider the effects of imprisonment on employment postrelease, which removes the incapacitation effect of incarceration in prison for the initial prison sentence by comparing prisoners postrelease to probationers post-sentence.³² The stratification of the analysis by presentence work history is essential to making sense of the effect of imprisonment on postrelease employment. The overall effects suggest that imprisonment has a moderately positive effect on employment at both four and 12 quarters after release among blacks (with effect sizes of positive 6 percentage points at the fourth quarter and 4 percentage points at the twelfth quarter) and no effect among whites. However, these effects mask significant heterogeneity by work history. Among those with no presentence work history, those who were sentenced to prison were more likely to be employed after release, and this effect was especially strong in the first year (fourth quarter) after release, when the probability of employment was 9 percentage points higher for prisoners compared to probationers among blacks and 10 percentage points higher among whites. These effects suggest some evidence for positive effects of imprisonment for those with the least labor market prospects, especially given their high risk of return to prison, which lowers their chances of employment through subsequent incapacitation (see below). These positive effects fade to statistical insignificance by the twentieth quarter for both blacks and whites (see table 3, which also includes outcomes at the twentieth quarter). These results are consistent with prior studies that find an increase in formal employment after prison that, in most of those studies, fades over time. In contrast, those with a work history experience negative effects of imprisonment, although these effects are only statistically significant among whites (11 percentage points in the fourth quarter and 7 percentage points in the twelfth quarter).

Reimprisonment is one likely contributor to such negative postrelease effects. Table 2 also shows results from 2SLS regressions for each subgroup predicting the effect of being sentenced to prison on the likelihood of going to prison within three years of the sentence date and within three years of

³¹ Note that this is the marginal effect, which is different from the average effect. The mean employment rate among black probationers in the fourth quarter following sentencing is 30%, and the comparable figure for white probationers is 45%.

³² Note that the sample sizes are slightly smaller in the postrelease models. This is because prisoners who have not yet been released are not included in these models. See the methodology section above for discussion of implications.

the release date. In the postsentence models, the effect of being initially sentenced to prison on being sent to prison within three years of the sentence date is statistically significant among whites (6 percentage points), but there was not a statistically significant difference across race groups in the size of these effects. In the postrelease models (which eliminate incapacitation effects), the effects of being sentenced to prison on future imprisonment are significant across all subgroups. The effect sizes are very similar among blacks and whites when we stratify by work history. Those with no work history experience the largest effects of a prison sentence on subsequent imprisonment, an increase of 19 percentage points compared to probationers with no work experience among blacks and 20 percentage points among whites. Among those with presentence work experience, the effect of imprisonment on subsequent entry into prison is also high, 13 percentage points among blacks and 17 percentage points among whites. Together, these findings suggest that the increased risk of subsequent entries into prison among those originally sentenced to prison rather than probation—prison’s revolving door—play an important role in imprisonment effects on employment through a process of “secondary incapacitation.”

Employment Stability and the Secondary Labor Market

We now examine the effects of prison sentences on a wider range of labor market outcomes in table 3. The main hypothesis motivating this part of the analysis is that serving time in prison may relegate formerly imprisoned individuals to the secondary labor market, where job stability is low, turnover is high, career ladders are few, scheduling is irregular, and working conditions are poor. If this hypothesis is correct, we would expect to see that prisoners would be less likely to work in industries not traditionally associated with the secondary labor market, to work less consistently, and to experience less employment stability. In this analysis, we are mainly interested in postrelease outcomes since the hypothesis we are testing is not about the effects of incapacitation on employment but rather about its effect on what type of employment people obtain when they are in the community.

Table 3 shows 2SLS estimates of the effect of prison compared to probation on four postrelease labor market outcomes—being employed at all during a particular quarter, being employed in three consecutive quarters, proportion of quarters employed, and being employed in industries not associated with the secondary labor market (“outside the secondary labor market”)—by race and work history.³³ The top panel shows the same esti-

³³ Descriptive statistics on these outcomes are provided in app. table A2. We have explored additional measures of labor market outcomes, including stability of employment with the same employer, earnings above the poverty line, and stable earnings above the poverty line, with similar results.

TABLE 3
2SLS ESTIMATES OF EFFECT OF 24 MONTH-PRISON SENTENCE VERSUS
24-MONTH PROBATION SENTENCE ON POSTRELEASE LABOR MARKET
OUTCOMES, BY RACE, WORK HISTORY, AND TIME SINCE RELEASE

	ENTIRE SAMPLE	BLACKS			WHITES		
		Overall	No Work History	Work History	Overall	No Work History	Work History
Any employment:							
4th quarter02 (.02)	.06*** (.02)	.09*** (.02)	-.01 (.03)	-.02 (.02)	.10*** (.02)	-.11*** (.03)
12th quarter01 (.02)	.04* (.02)	.07*** (.02)	-.03 (.02)	.00 (.02)	.05* (.02)	-.07** (.03)
20th quarter . . .	-.02 (.02)	.02 (.02)	.05* (.02)	-.04 (.03)	-.05* (.03)	.03 (.02)	-.09** (.03)
Employed 3 consecutive quarters:							
4th quarter01 (.01)	.04** (.02)	.07*** (.01)	-.02 (.02)	-.01 (.02)	.08*** (.02)	-.08*** (.02)
12th quarter . . .	-.00 (.01)	.02 (.02)	.04*** (.01)	-.02 (.02)	-.00 (.02)	.03 (.02)	-.07** (.02)
20th quarter . . .	-.03 (.02)	.01 (.02)	.03* (.02)	-.03 (.02)	-.05* (.02)	.01 (.02)	-.07* (.03)
Proportion quarters employed:							
4th quarter05*** (.01)	.07*** (.01)	.10*** (.01)	-.05 (.03)	.01 (.02)	.12*** (.02)	-.09*** (.02)
12th quarter03* (.01)	.06*** (.01)	.08*** (.01)	-.01 (.02)	.00 (.02)	.09*** (.02)	-.09*** (.02)
20th quarter01 (.01)	.03* (.02)	.07*** (.01)	-.02 (.02)	-.01 (.02)	.05** (.02)	-.07** (.02)
Employed outside secondary labor market:							
4th quarter00 (.01)	.04** (.01)	.04*** (.01)	-.03 (.02)	-.03 (.02)	.05** (.02)	-.08*** (.02)
12th quarter00 (.01)	.02 (.01)	.04*** (.01)	.01 (.02)	.01 (.02)	.02 (.02)	-.04* (.02)
20th quarter . . .	-.01 (.02)	.01 (.02)	.05** (.02)	-.03 (.02)	-.03 (.02)	.03 (.02)	-.07** (.03)
Observations . . .	109,636	47,673	17,740	29,933	61,963	14,718	47,245

NOTE.—Work history = any formal employment in 12 calendar quarters before sentence; robust SEs are in parentheses.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

mates of prison sentences on quarterly employment reported in table 2; they are repeated in table 3 to facilitate comparison with other outcomes. In estimating the effect of prison sentences for the sample as a whole, we found that going to prison had a significant positive effect on the proportion of quarters that one had been employed since the sentence/release date, but

only when this outcome was measured in the fourth quarter (5 percentage points) or twelfth quarter (3 percentage points) after release. Prison was not significantly associated with the probability of being employed for three consecutive quarters or being employed outside the secondary labor market.

Again it is important to examine variation in effects by race and work history. Among blacks, we found small positive effects of prison as compared to probation on being employed for three consecutive quarters (significant only when the outcome was measured at the fourth quarter after release), the proportion of quarters employed (significant at all time periods), and employment outside the secondary labor market (significant only at the fourth quarter after release). The models that stratify by work history show that these positive effects of imprisonment among blacks are limited to those without a work history, for whom effects fade gradually but remain statistically different from zero through the twentieth quarter. For blacks with a work history, we see no statistically significant effects of imprisonment on any of these labor market outcomes, although the coefficients suggest there could be small negative effects that we do not have the power to detect. Among whites overall, we found only one significant result: prison was associated with a lower probability of being employed for three consecutive quarters when the outcome was measured at the twentieth quarter after sentence/release (4.5 percentage points). We find more significant results among whites when we stratify the models by work history. Whites without any work history experience large positive effects of imprisonment on all postrelease labor market outcomes that fade over time. Among whites with a work history, however, the effects of imprisonment on postrelease labor market outcomes are large, negative, and fairly persistent. Not only are they less likely to be employed in any particular quarter, but they experience less employment stability and a lower proportion of quarters employed and are less likely to be employed outside the secondary labor market. These estimates suggest that imprisonment does indeed relegate former prisoners to the secondary labor market, but only among whites with work history in the formal labor market before sentencing.

CONCLUSION

The rise in incarceration since the 1970s has prompted intense research interest in the consequences of imprisonment, particularly with regard to labor market outcomes and racial inequality. Despite the proliferation of research on this question, the NRC report (2014) finds some fundamental weaknesses in the overall body of research, weaknesses that do not make “it possible to distinguish among the effects of criminal behavior, criminal conviction, and the experience of incarceration as they relate to subsequent labor market ex-

periences" (p. 256). In this article, we respond to the challenge of the NRC report and examine the effect of incarceration in prison on individual labor market outcomes, using administrative data and a natural experiment based on the random assignment of judges to estimate plausibly causal effects of the imprisonment experience itself. Collectively, our results suggest that imprisonment's role in the exacerbation of black-white inequalities in employment outcomes occurs primarily through incapacitation.

Our analysis improves on prior empirical work both methodologically and substantively. First, we employed a natural experiment that addresses selection bias via the random assignment of judges and poses a precise and policy-relevant counterfactual comparison between being sentenced to prison or probation, allowing us to isolate the effect of imprisonment from other possible effects of the criminal justice system, such as the mark of a felony conviction. Although prior studies have leveraged similar natural experiments, they have been hampered by sample sizes too small to detect effects of reasonable size, by inattention to race and presentence work history, or by imprecise comparisons between types of sentences.

Second, we explicitly considered incapacitation effects, both during the original prison term and later in time, because of the increased risk of future imprisonment faced by individuals sentenced to prison. Although it may seem intuitively obvious that there is an incapacitation effect on employment, how large it is relative to other effects of imprisonment and how it varies by race and work history had not been previously investigated. Our results suggest that for the sample as a whole, imprisonment reduces the probability of employment by 24 percentage points in the fourth quarter after sentencing, when about 87% of people sentenced to prison were still incarcerated. In contrast, when we removed the effect of incapacitation by comparing people sentenced to prison and probation at similar lengths of time in the community, we found that the probability of finding work was actually higher among those who went to prison, with the effects for the overall sample ranging from 4 percentage points in the twelfth quarter after sentencing/release to 14 percentage points in the twentieth quarter. These results challenge the dominant account that prison sentences have lasting negative consequences for employment trajectories. At the same time, the low rates of employment among both prisoners and probationers in our sample illustrate how difficult it is for anyone with a felony conviction to find work in the formal labor market.

We also found that being sentenced to prison exposes individuals to a so-called secondary incapacitation effect, meaning that they face a greater risk of going to prison again in the future, either for a new felony conviction or a technical violation, leading to further loss of work. By three years after release, an individual sentenced to prison was 20 percentage points more likely than one sentenced to probation to go to prison for a new spell.

Third, our large sample of felony sentences allowed us to investigate effect heterogeneity by two key presentence characteristics, race and work history, which proved critical to understanding imprisonment's effects. The short-term negative effects of prison that operate via incapacitation were stronger among people who had a history of working in the formal labor market before their sentence. For example, in the fourth quarter after sentencing, prison reduced the probability of employment by 29 percentage points among blacks with a presentence work history and 41 percentage points among whites with a work history. The larger size of this effect among whites reflects the comparatively stronger employment prospects among whites compared to blacks sentenced to probation. Yet given that blacks face much higher rates of imprisonment than whites, the aggregate consequences of these incapacitation effects, in terms of overall "lost" employment, are arguably more profound for black men as a group, even though the effect size is smaller among blacks.

The longer-term effects of prison sentences, estimated by comparing people sentenced to prison to those on probation after the former were released, also varied substantially across subgroups defined by race and work history. When we removed the incapacitation effect of imprisonment by analyzing postrelease employment outcomes, we only found significant negative effects of prison among whites with presentence work histories, who presumably would have had the best prospects in the labor market had they not been imprisoned. We found no long-term effect of prison sentences on employment among blacks with work histories. This suggests that the stigma of conviction—which affects probationers as well—may be more consequential than the stigma of imprisonment, a hypothesis that is also consistent with prior research finding that employers do not have access to information about imprisonment (Bushway, Briggs et al. 2007) and rarely ask about it on applications (Vuolo, Lageson, and Uggen 2017).

Among whites and blacks with no presentence work history, the effect of being sentenced to prison on employment was positive and significant. This effect faded over time, in part because of higher rates of secondary incapacitation among those in the prison group. These results are also consistent with other studies on employment in the formal labor market, including evidence from multiple states that employment increases after release from prison relative to preprison levels but shrinks over time, as well as prior natural experiment studies that find negligible or null effects of prison on employment after release. Such effects may be due to postprison services and programs, such as work programs, or to the effect of parole supervision: important topics for further research. The positive effects of prison on employment among those with no presentence work history also reflect the poor employment prospects among comparable individuals on probation.

Fourth, we examined the hypothesis that incarceration in prison relegates former prisoners to the secondary labor market, characterized by high employment instability, low wages, and few prospects for upward mobility. We only found evidence supporting this hypothesis in one subgroup, whites with a presentence work history. In this group, prison sentences not only reduced the probability of employment over the long term but also decreased employment stability and the probability of working in industries outside of the secondary labor market. Among blacks and whites without presentence work histories, our results contradict the secondary labor market hypothesis, since those sentenced to prison in these subgroups experienced more positive employment outcomes (more likely to be employed for three consecutive quarters, be employed for a higher proportion of quarters, and be employed in nonsecondary labor market jobs) at the fourth quarter after their release compared to probationers, but these effects faded over time. Again, we believe this reflects the poor labor market prospects of the comparison group, probationers without any work history. Together these results suggest that imprisonment's role in exacerbating racial inequalities in the labor market is primarily in its incapacitation effects.

Our ability to specify a well-defined comparison group and to disentangle imprisonment effects on employment by race and work history allows us to make sense of some of the conflicting empirical findings in the prior literature. One such disagreement is the difference between effects estimated from survey data and those from administrative data. Our estimates of negative effects for those with a work history are more consistent with the estimated negative effects from survey data, while our positive effects of imprisonment for those without a work history are consistent with evidence from administrative data that employment increases after release from prison. If surveys disproportionately miss individuals with no work history in their initial sampling or are more likely to lose track of such individuals over time, survey-based estimates will be biased toward negative effects.

A second disagreement exists between natural-experiment studies using administrative data, some of which find negative effects and some of which find null effects of imprisonment on employment. Our estimates suggest that different sample compositions and an inability to stratify by race and work history could account for some discrepancies. For example, Loeffler (2013) finds slightly positive but insignificant employment effects with a sample that is 79% black, and he is unable to stratify by work history. Kling (2006) finds no effects on employment in a Florida sample that is 54% black but of whom only 31% have any formal work history. These are both samples that in the study presented here would also likely generate null effects. In contrast, Mueller-Smith (2015) finds negative effects of incarceration in a Texas county where 46% of felony defendants are black (and effect differences by

race are unreported). If many of the whites in Mueller-Smith's sample have a work history, his estimates would be consistent with ours.

We remind the reader of the limitations of this study. First, we have focused only on the effects of imprisonment relative to probation and only on the effects in the labor market. Imprisonment can have impacts in many different life domains (e.g., Binswanger et al. 2007; Schnittker, Massoglia, and Uggen 2012) and can affect families (e.g., Braman 2004; Turney and Wildeman 2013, 2015) and communities (e.g., Clear 2007; Morenoff and Harding 2014) in addition to those who are imprisoned. Moreover, criminal justice involvement is much broader than just imprisonment, and the finding that imprisonment itself does not have universally negative effects on employment does not mean that conviction or arrest does not have serious negative effects. Indeed, our finding that those who receive probation sentences have very poor labor market outcomes could be the direct result of large and negative consequences of felony arrest or conviction.

Second, our results come from a single state and may not be generalizable to other states with different labor markets and different criminal justice systems. Third, we are unable to examine racial groups other than whites and blacks because of low numbers of Latinos and Asians in Michigan. Fourth, our data do not allow for long-term follow-up of subjects or the assessment of long-term effects. Fifth, our labor market outcomes are derived from administrative records from the unemployment insurance system and are therefore limited to formal employment among individuals who can be matched to such records. To the extent that there are differences in formal versus informal employment or the ability to accurately match to administrative records by race, prior work history, or sentence type, the group differences in labor market outcomes we examine here could be over- or understated.

Sixth, causal effect estimates from an instrumental variables analysis are LATE. This means we are estimating the effect of incarceration in prison as compared to probation among individuals for whom the judge assigned made the difference between prison and probation. Those are individuals who are on the margin between prison and probation. Our estimates do not provide average treatment effects for all individuals sentenced to prison in Michigan. As a result, they should not be interpreted as informative regarding radical policy changes such as decarceration on a massive scale, which would surely involve individuals who are far from the margin on which the effects in this article are estimated. Moreover, such a policy change would likely affect the mechanisms through which incarceration has its effects. For example, the stigma attached to incarceration in prison might change if incarceration in prison became rarer.

Nevertheless, our findings suggest two avenues for future research on the role of the criminal justice system in generating and perpetuating racial in-

equalities. The first is to examine more carefully the effects of other forms of involvement in the criminal justice system, such as arrest or conviction. One possible explanation for the apparent disagreement between our results and those from prior research is that much prior research has not been able to adequately distinguish between various forms of criminal justice system involvement, and estimated effects that were attributed to imprisonment are actually due to arrest or conviction. By comparing prisoners to probationers, the estimates in this article focus precisely on the effect of imprisonment and remove any effect of arrest or conviction. Future research should examine the effects of these forms of criminal justice system involvement. Moreover, future research should also examine specifically the features of probation—one of the least studied aspects of the criminal justice system (Phelps 2016)—that may affect labor market outcomes either positively or negatively, as well as the effects of incarceration in jail.

The second avenue is to examine specific mechanisms or processes by which incarceration and other forms of criminal justice system involvement have their effects on labor market outcomes. Our estimates of positive effects of incarceration in prison among those without a work history suggest that some mechanisms generating positive effects are at work, that countervailing mechanisms may be present, and that mechanisms may operate differently for those with different labor market prospects absent imprisonment.

We also note a number of possible policy implications. The initially positive effects of prison on employment and other labor market outcomes among those without a work history suggest that the period immediately after release from prison may be a particularly important moment for policy intervention to reinforce and prolong this improvement in employment outcomes. Prior research suggests that the period immediately after release from prison may be a moment of optimism and commitment to desistance (Comfort 2012; Harding et al. 2016). The fading of these effects, however, is also consistent with prior research on the challenges of labor market success for former prisoners, due to both the difficulties of the current low-skill labor market and the human capital and social capital deficits of former prisoners (Bushway, Stoll, and Weiman 2007), who have trouble maintaining short-term successes in the face of these structural headwinds.

Our findings also have possible policy implications for efforts to shrink the criminal justice system and to reduce its effects on racial inequalities. If, as we discuss above, our mix of negative, positive, and null effects of imprisonment mean that negative effects of criminal justice system contact are actually more closely associated with arrest and conviction rather than incarceration for some subgroups, efforts to reduce imprisonment by simply sentencing fewer individuals convicted of a felony to prison may create fewer benefits than expected, at least with regard to labor market outcomes. Instead, strategies involving changing felonies to misdemeanors or allowing more

people to shield or expunge their criminal records might do more to improve the labor market prospects of those involved in the criminal justice system. In addition, policies that allow individuals to mitigate the stigma of a felony conviction by signaling their commitment to reform might also prove effective (Bushway and Apel 2012).

APPENDIX

Instrumental Variables Assumptions

A valid instrument must meet two conditions (for a review, see Angrist and Pischke 2009, chap. 4). First, it must affect the causal variable of interest or the “treatment” (here, sentence to prison rather than probation). This is often referred to as the “relevance” condition. Second, it can only be correlated with the outcome through the treatment. In other words, the instrument’s effect on the treatment must be the only pathway through which the instrument affects the outcome, and there are no other unobserved variables that create an association between the instrument and the outcome. This second condition is known as the “exclusion restriction.” While the first condition can be examined empirically, the validity of the exclusion restriction must be argued based on theory or knowledge of the institutional rules that generate the instrument.

The Relevance Condition and Strength of the First Stage

The relevance condition is based on the idea that judges have considerable discretion in sentencing and that different judges systematically sentence more or less harshly than other others. Although Michigan does have sentencing guidelines, these guidelines are advisory only and leave considerable room for judicial discretion. We can examine the relevance condition by examining how the probability of sentencing to prison varies by judge within county. Figure A1 graphs judge variation in the probability of a prison vs. a probation sentence (fig. A1a), in prison minimum sentence lengths (fig. A1b) and in probation sentence lengths (fig. A1c). Each vertical bar in each graph represents one judge, and the height of the bar is the deviation of that judge’s mean sentence from the mean sentence of the other judges in his or her county, after controlling for characteristics of the defendants. The variation across judges within counties in sentencing is readily apparent in all three graphs.

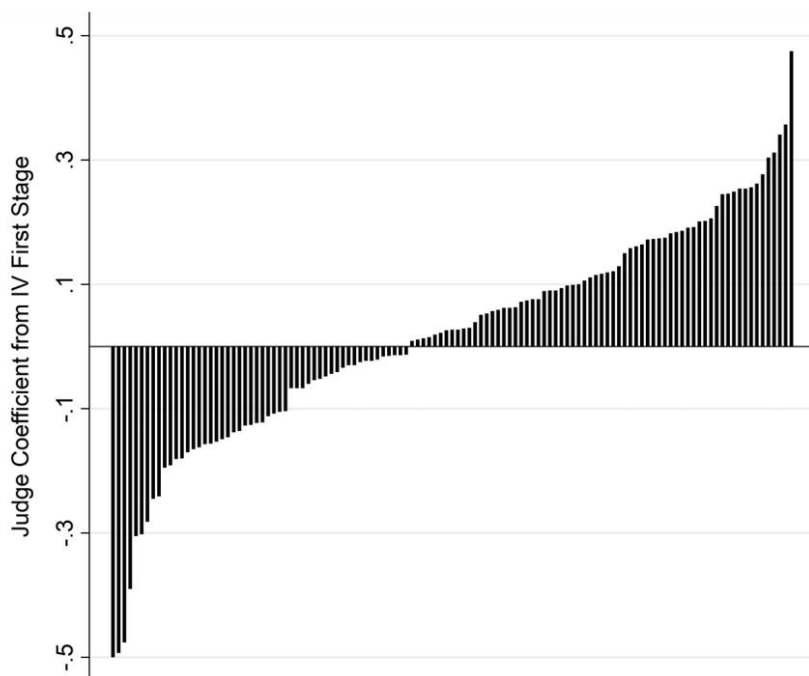


FIG. A1a.—Judge fixed effects from IV first stage, prison versus probation (each line represents one judge).

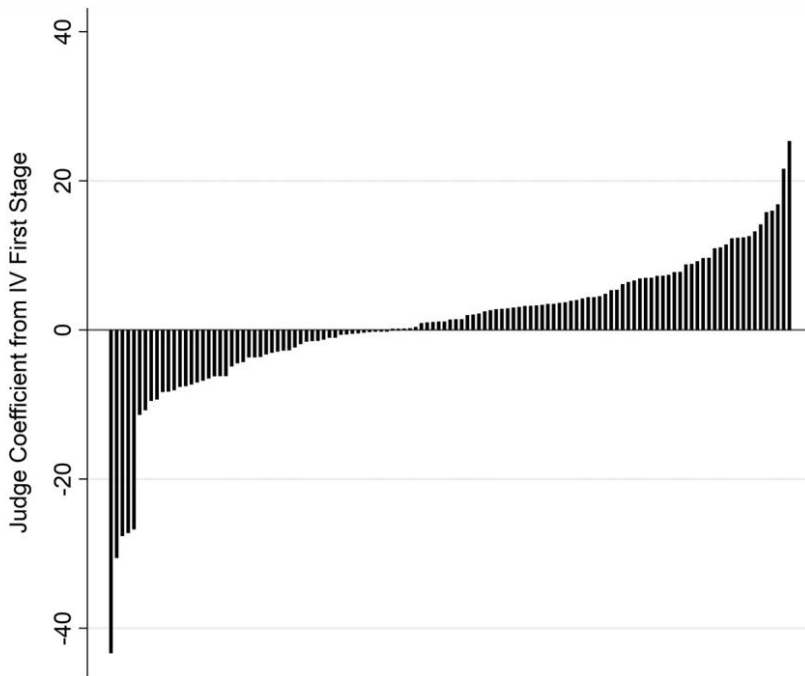


FIG. A1b.—Judge fixed effects from IV first stage, prison length (each line represents one judge).

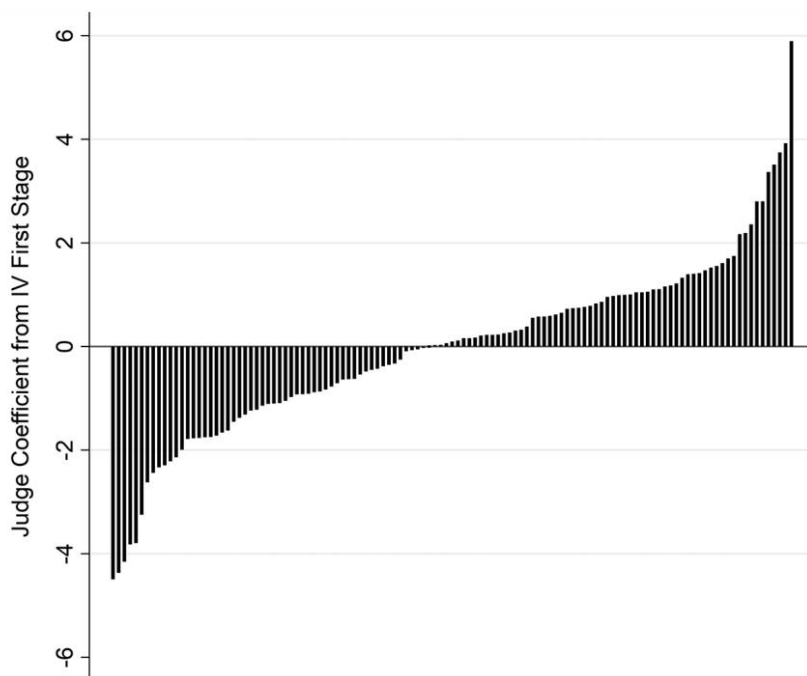


FIG. A1c.—Judge fixed effects from IV first stage, probation length (each line represents one judge).

Even when there is variation in the treatment across instruments, estimates from an instrumental variables design can be inconsistent when the instruments are weak, in other words, when they are only slightly correlated with treatment (Bound, Jaeger, and Baker 1995). The danger is that chance relationships in a sample can be mistaken for true correlations in the population and IV estimates may be no better than OLS regression (Angrist and Pischke 2009). This can be a problem especially when there are many instruments relative to endogenous treatment variables, as is the case here. As suggested by Bollen (2012), we examine Shea's partial R -squared, which shows the proportion of the variation in each treatment variable independently explained by the instruments, that is, once associations with the covariates are partialled out (Shea 1997). Appendix table A1 shows Shea's partial R -squared values for the treatments examined in this analysis (prison vs. probation, probation sentence length, prison sentence length), both when only judge identifiers are used as instruments and when judge identifiers

TABLE A1
FIRST-STAGE DIAGNOSTICS, BY RACE AND WORK HISTORY

	BLACKS						WHITES					
ENTIRE SAMPLE	Overall			Work History			Overall			No Work History		
	JO	J1	JO	J1	JO	J1	JO	J1	JO	JO	J1	JO
Shea's partial R-squared												
Prison01	.08	.02	.10	.02	.17	.02	.13	.02	.09	.21	.01
Prison length00	.05	.01	.08	.01	.15	.01	.10	.00	.06	.01	.00
Prison length ²00	.04	.00	.05	.01	.11	.00	.08	.00	.01	.27	.00
Probation length02	.07	.03	.12	.03	.16	.03	.15	.01	.07	.19	.01
Probation length ²00	.03	.01	.06	.02	.13	.01	.10	.00	.04	.19	.00
Partial F-test												
Prison	32.71	4.75	18.75	2.98	7.49	1.86	12.57	2.44	14.83	3.02	1.62	11.51
Prison length	13.02	3.36	8.19	2.38	4.37	1.71	5.11	2.04	3.64	1.81	1.58	2.86
Prison length ²	4.65	2.09	2.90	1.42	1.72	1.11	2.64	1.45	2.03	2.20	1.71	1.54
Probation length	29.36	4.01	19.96	2.93	7.53	1.62	13.68	2.43	11.41	2.33	1.48	9.04
Probation length ²	4.70	1.40	3.65	1.36	3.26	1.30	2.38	1.46	2.85	1.33	1.47	2.04
Sample size	109,636		47,673		17,740		29,933		61,963		14,718	47,245

NOTE.—JO = judge only; JI = judge and interactions.

and their interactions are used as instruments. The latter set of instruments is used in all analyses presented in this article (see below for further justification of instruments). These values show that the first stage explains a substantial portion of the variation in treatment across all treatments and all subgroups, giving us confidence that the instruments are sufficiently strong.

The traditional test for weak instruments is to estimate the “first-stage” equation and perform an F -test for the joint significance of the instruments (here, the judge identifiers and their interactions with the covariates). Typically, F -statistics above 10 are considered ideal, but F -statistics less than 10 are not always indicative of a problem; they merely alert the researcher of the potential for a problem (Angrist and Pischke 2009; Staiger and Stock 1997). Moreover, in the context of a study in which there are many instruments and a large sample size (as was the case here), F -statistics may not be particularly informative regarding instrument strength because the number of instruments and the sample size are included directly in the calculation of the F -statistic. This is evident in the second panel of table A1, which shows the relevant F -statistics, which were large for the overall sample for the prison, linear prison length, and linear probation length treatments when we used only the judge identifiers as instruments. However, even though we see large values of Shea’s partial R -squared, both reductions in sample size as the sample is stratified and increases in the number of instruments from judges only to judges and covariate interactions reduced the F -statistics considerably.

An additional concern with regard to the first stage is “over fitting,” a finite sample bias that leads to the misestimation of the first stage when there are many instruments that are weak individually even if they collectively explain a substantial portion of the variation in treatment (Bound et al. 1995). As discussed in the main text, to assess whether the number of instruments we use has resulted in biased coefficients, we reestimated our models in a number of ways that use far fewer instruments. These included (a) using only the judge dummy variables as instruments; (b) constructing a single instrument that is a judge harshness score based on individual judge coefficients from a linear probability model of prison versus probation for all cases and conditioning on presentence characteristics and county identifiers (table A2). These models produce estimates that are of the same direction and magnitude as our preferred model, which has greater statistical power due to its stronger first stage and more effectively accounts for the monotonicity assumption (see below).³⁴

³⁴ Note also that the proportion of the variance in treatment explained by the instrument in the Angrist paper (that Bound et al 1995 re-evaluates) is orders of magnitude smaller than that here (partial $R^2 \approx 0.001$ in tables 1 and 2 in the Bound et al vs. 0.066 to 0.163 in this article for the prison vs. probation comparison in our table A1). This suggests that our instruments are explaining a substantial part of the variation in treatment.

The Exclusion Restriction and Randomization Tests

The exclusion restriction has two requirements: (1) the instruments are as good as randomly assigned, and (2) the instruments are correlated with the outcome only through the treatment. We are confident that judges are randomly assigned in Michigan. Criminal cases are assigned to judges when cases are initially filed (at indictment), which means that initial charges are filed before the prosecutor knows which judge will be assigned. Michigan's Administrative Rules of Court specify in section 8.111(B) that judges be assigned to all cases "by lot," but the chief judge of each court is responsible for issuing orders on the exact procedures. All felony cases in Michigan are handled by circuit courts, and all circuit courts have computerized case management systems that assign cases at filing to judges using a random number generator. This procedure assigns cases at random based on the proportion of cases a judge is supposed to receive over the course of a year, rather than when the case is filed. Our conversations with both prosecutors and defense attorneys indicate that random assignment of judges when charges are filed is taken extremely seriously as a core tenant of fair and just operations of the court. While experienced attorneys are typically aware of the sentencing styles of particular judges—and one might imagine that some degree of "judge shopping" occurs for high profile or extremely serious cases—circumventing the computerized random number generator sounds implausible on its face, and moreover it is hard to believe that such efforts, if even possible, would be taken in the more routine cases that make up the vast majority of felony cases.

While we cannot empirically verify that judge assignment is random with respect to unobserved variables, we can check that the covariates we observe are uncorrelated with judge assignment. Table A3 shows that this is indeed the case. The F -tests of the joint significance of the instruments in predicting the covariates net of county fixed effects are statistically significant due to our very large sample size (picking up chance variation in individual defendant characteristics across judges), but the differences are small in magnitude and have small F -statistics, given the size of the sample. The F -statistics also shrink substantially when we stratify by race and sentence type. Nonetheless, we control for these covariates in all models to adjust for chance differences across judges in individual defendant characteristics.

The exclusion restriction also requires us to assume that the instruments are correlated with the outcome only through the treatment. For this analysis, this means that the judge to whom one is assigned only affects employment through the sentence the judge imposes. This assumption would be violated, for example, if judges who sentenced more harshly also treated defendants more harshly in court, leading defendants to question the legitimacy of the criminal justice system, which might make them more likely to return to crime. However, this would only be a violation of the assumption if the

TABLE A2
MEAN OUTCOMES POSTSENTENCE AND POSTRELEASE, BY SENTENCE TYPE, WORK HISTORY, AND RACE

	BLACKS										WHITES											
	ENTIRE SAMPLE			Overall			No Work History			Work History			Overall			No Work History			Work History			
	Prison Prob.	Diff		Prison Prob.	Diff		Prison Prob.	Diff		Prison Prob.	Diff		Prison Prob.	Diff		Prison Prob.	Diff		Prison Prob.	Diff		
Postsentence:																						
Any employment:																						
4th quarter																						
12th quarter																						
20th quarter																						
Employed 3 consecutive quarters:																						
4th quarter																						
12th quarter																						
20th quarter																						
Proportion quarter employed:																						
4th quarter																						
12th quarter																						
20th quarter																						
Employed outside SLM:																						
4th quarter																						
12th quarter																						
20th quarter																						
Entered prison at least once:																						
1 year																						
3 years																						
5 years																						

Postrelease:																			
Any employment:																			
4th quarter25	.38	-.13	.19	.29	-.11	.09	.07	.01	.25	.41	-.16	.30	.45	-.15	.14	.13	.01	.36
12th quarter17	.31	-.14	.12	.24	-.12	.06	.07	-.01	.17	.33	-.16	.21	.38	-.17	.09	.12	-.04	.25
20th quarter15	.27	-.12	.11	.20	-.09	.06	.06	.00	.15	.28	-.13	.18	.33	-.15	.08	.11	-.03	.20
Employed 3 consecutive quarters:																			
4th quarter16	.26	-.10	.11	.19	-.09	.04	.02	.02	.15	.28	-.13	.21	.33	-.12	.08	.06	.02	.25
12th quarter11	.23	-.12	.08	.17	-.09	.04	.04	.00	.10	.24	-.14	.14	.29	-.15	.05	.08	-.02	.17
20th quarter09	.20	-.11	.07	.15	-.08	.04	.04	.00	.09	.21	-.12	.12	.25	-.14	.05	.08	-.03	.14
Proportion quarters employed:																			
4th quarter27	.39	-.12	.20	.31	-.10	.10	.07	.03	.28	.43	-.15	.33	.46	-.13	.15	.12	.03	.39
12th quarter22	.36	-.13	.17	.28	-.11	.08	.07	.01	.22	.39	-.16	.28	.43	-.15	.12	.12	.00	.33
20th quarter20	.33	-.12	.15	.25	-.10	.08	.07	.01	.20	.35	-.15	.25	.40	-.14	.11	.12	-.01	.30
Employed outside SLM:																			
4th quarter13	.19	-.06	.08	.14	-.06	.04	.03	.02	.10	.20	-.10	.17	.24	-.06	.07	.06	.01	.21
12th quarter09	.17	-.08	.06	.13	-.07	.03	.03	.00	.08	.18	-.09	.12	.21	-.09	.05	.06	-.01	.14
20th quarter08	.15	-.07	.06	.11	-.05	.04	.03	.01	.07	.15	-.08	.10	.19	-.08	.06	.06	.00	.12
Entered prison at least once:																			
1 year13	.02	.11	.14	.03	.11	.16	.04	.12	.13	.02	.11	.12	.02	.10	.12	.02	.10	.12
3 years32	.07	.25	.35	.09	.26	.37	.11	.26	.34	.08	.26	.29	.06	.24	.29	.06	.23	.29
5 years40	.10	.29	.43	.13	.30	.46	.17	.29	.42	.12	.30	.36	.08	.28	.36	.09	.27	.36

NOTE.—SLM = secondary labor market; work history = any formal employment in 12 calendar quarters before sentence.

TABLE A3
RANDOMIZATION CHECKS: GLOBAL *F*-TESTS (Joint Significance Test of Judge Dummies)

	BLACKS						WHITES					
	ENTIRE SAMPLE			Overall			No Work History			Overall		
	<i>F</i>	<i>p</i>		<i>F</i>	<i>p</i>		<i>F</i>	<i>p</i>		<i>F</i>	<i>p</i>	
First time felony offender . . .	5.16	.0000		4.24	.0000		2.09	.0000		3.11	.0000	
0–4 prior arrests	6.01	.0000		5.61	.0000		2.87	.0000		3.70	.0000	
5–9 prior arrests	1.38	.0039		1.65	.0000		1.24	.0387		1.52	.0002	
10+ prior arrests	5.73	.0000		4.70	.0000		2.71	.0000		3.06	.0000	
Age at sentence	3.00	.0000		2.71	.0000		1.85	.0000		1.94	.0000	
Female	4.32	.0000		5.22	.0000		2.12	.0000		4.06	.0000	
Black	9.61	.0000										
Less than high school	1.89	.0000		1.48	.0006		1.18	.0883		1.37	.0047	
GED	1.67	.0000		1.24	.0407		1.19	.0800		1.22	.0528	
High school	1.64	.0000		1.36	.0059		1.02	.4347		1.15	.1238	
More than high school	1.95	.0000		1.56	.0001		1.20	.0693		1.47	.0006	
Presentence employment	4.06	.0000		2.63	.0000		1.03	.3890		2.00	.0000	
Nonsingle	2.03	.0000		1.71	.0000		1.23	.0475		1.50	.0004	
Mental illness history	2.33	.0000		1.34	.0079		1.13	.1598		1.00	.4854	
Ever used alcohol	2.58	.0000		1.15	.1224		.90	.7803		1.05	.3259	
Ever used marijuana	2.98	.0000		2.18	.0000		1.26	.0274		1.98	.0000	
Ever used stimulants	2.87	.0000		1.93	.0000		1.24	.0380		1.51	.0003	
Ever used opioids	1.66	.0000		1.31	.0143		1.05	.3255		1.18	.0882	
Ever used other drugs	3.11	.0000		2.35	.0000		1.57	.0001		1.80	.0000	
Pretrial detention	5.68	.0000		4.68	.0000		2.44	.0000		3.07	.0000	

NOTE.—Each regression includes judge IDs, county fixed effects, and sentence year dummies.

legitimacy of the system is undermined by the judge's actions beyond sentencing. Given the very small amount of time that a given defendant actually interacts with "his" judge, such effects seem unlikely to be consequential. The sheer volume of cases that judges handle is the most direct evidence of the small amount of time a typical criminal defendant spends in the presence of the judge. Data provided by the Michigan State Court Administrative Office shows that Michigan circuit courts handled over 80,000 criminal cases in various stages of court processing per year between 2003 and 2006 (Michigan Supreme Court 2016), or over 500 cases per judge per year on average. Another possible violation of the exclusion restriction could occur through pretrial detention (Dobbie et al. 2017). If judges vary in their assignment of pretrial detention and if pretrial detention affects employment, that will create an association between the instrument and the outcome that occurs through a pathway other than imprisonment. However, in Michigan bail and pretrial detention are not initially determined by the circuit court judges we use on our analysis, although such judges can change bail and pretrial detention decisions once they receive a case. Judge identifiers are only slightly correlated with pretrial detention conditional on race and work history (see table A3), and we condition on pretrial detention in all models.

A violation of the exclusion restriction might also occur if prosecutors react to the selection of a more or less punitive judge by changing the crimes for which they pursue prosecution or by changing their plea bargaining behavior. For example, a prosecutor on a case that is assigned to a more punitive judge might be content with a plea to a lesser crime, knowing that the harsh judge will sentence at the higher end of the range provided in the guidelines. Assuming that the prosecutor is basing such decisions on her prediction about the defendant's likelihood of recidivism or employment, this scenario could result in defendants who otherwise appear comparable but have different probabilities of the outcome receiving different sentences as a result of the judge that was assigned.

A similar violation of the exclusion restriction might also occur due to the selection of cases into our data set, which only includes cases that result in conviction and sentencing. We do not observe defendants who are indicted but not convicted. National data from the 75 largest U.S. counties in 2009 suggest that this could be a common occurrence (Reaves 2013). Within one year of a felony arrest, only 54% of the cases had been resolved through a felony conviction, and an additional 12% had been resolved through a misdemeanor conviction. Almost all cases are eventually resolved through a guilty plea, and about 75% of the cases not resolved through conviction are dismissed.

While many individuals whose cases do not result in conviction are unlikely to be comparable to those who are convicted (based on actual guilt, e.g.), defendant plea bargaining could be influenced by the harshness of the

judge to whom the case is assigned. For example, a defendant who draws a harsh judge may be more willing to plea bargain knowing that a conviction at trial will result in a more severe sentence, while an otherwise similar defendant who draws a lenient judge may be more willing to risk a trial knowing there is some chance of acquittal and, in the event of a guilty verdict, a less severe sentence. One might also suspect that a judge's sentencing practices would be positively correlated with probability of conviction at trial. Under these scenarios, defendants who appeared before lenient judges would be less likely to appear in the data, since more will go to trial and some of those will be acquitted. The result could be that, among the cases in our data set, more lenient judges have sentenced cases with individuals more likely to recidivate and less likely to find employment, a correlation between judge harshness and the outcome that is not due to sentence type. This would introduce a downward bias into the effect of incarceration. One way to examine whether these scenarios occur is to see if more lenient judges have more trials and fewer plea bargains among the cases that appear in our data. Fortunately, this does not seem to be the case, and, as a result, we do not believe that this problem represents a serious threat to our strategy.

Monotonicity Assumption

The LATE interpretation of IV requires an additional assumption, which is termed "monotonicity." This means that the instrument only affects the treatment in one direction—the harshness of judges always affects the treatment in the same direction. In other words, a judge who imposes more punitive sentences than her colleagues to some individuals does not also impose more lenient sentences than her colleagues to others. (This assumption is also sometimes referred to as "no defiers" in the IV literature [Angrist, Imbens, and Rubin 1997].) This might occur if a judge treats some types of offenses, say drug offenses, more harshly than her colleagues, but other types of offenses, such as property offenses, less harshly than her colleagues. Following Mueller-Smith (2015), we relax this assumption by interacting judge dummies with presentencing individual characteristics and also treating those interactions as instruments.

Covariate Measures

Covariate measures are derived from the "presentencing investigation" conducted by the MDOC staff for the judge in each criminal case prior to sentencing. Data in these reports are collected from criminal records, interviews with defendants and in some cases their family members, police reports, and prior presentencing investigation reports. Where we encountered discrep-

ancies or missing data, we looked at other data records available and considered the most common value, such as MDOC administrative records or arrest records from the Michigan State Police.

Age at Sentence.—Age in years on the sentencing date, calculated based on the birth date and sentence date.

Black (vs. White).—Whether the individual was identified as black or African-American. The small number of individuals who are neither black nor white are not included in the analysis.

Female.—Whether the individual was identified as female or male.

Education.—The individual's highest level of education at the time of sentencing. Categories include less than high school, GED, high school, and more than high school.

Presentence employment.—Whether the individual had any record of employment in the formal labor market in the data from the unemployment insurance system in the 23 calendar quarters before the sentence.

Not single (vs. single).—Marital status at the time of sentence.

Presentence substance use.—Self or family reported history of substance abuse, including a set of nonmutually exclusive dummy variables indicating ever used alcohol, ever used marijuana, ever used stimulants, ever used opioids, ever used other drugs.

Mental health illness history.—An indicator for whether there was any history of mental illness prior to sentencing, based on prior records, self-reports, and family reports. Reports of prescription drugs or mental health treatment or hospitalization for mental illness are the most common reason mental illness was recorded, but this could also indicate diagnosis of untreated mental health problems or descriptions of symptoms.

First time felony offender.—Whether the individual had ever been sentenced for a felony before.

Number of prior arrests.—Number of arrests recorded by the Michigan State Police prior to the current sentence date, categorized as: 0–4 prior arrests, 5–9 prior arrests, 10 or More prior arrests.

Year of sentence.—Calendar year in which the sentence date fell: 2003, 2004, 2005, or 2006.

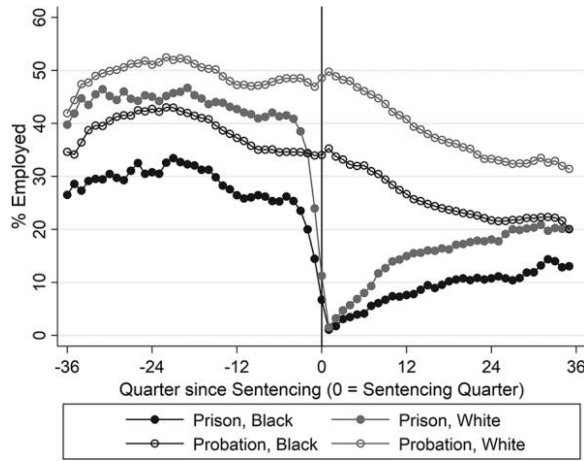


FIG. A2.—Employment relative to sentence date among individuals sentenced for their first felony, by sentence type and race

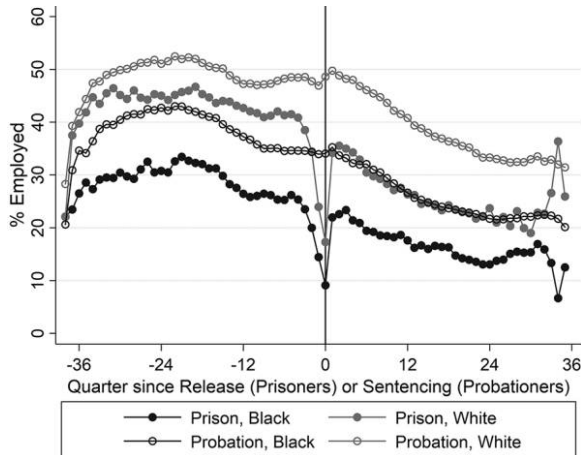


FIG. A3.—Employment relative to release date among Individuals sentenced for their first felony, by sentence type and race (excludes focal prison term)

TABLE A4
COVARIATE MEANS, BY RACE, SENTENCE TYPE, AND WORK HISTORY

	BLACKS						WHITES					
	Prison			Probation			Prison			Probation		
	No Work History			No Work History			No Work History			No Work History		
	Overall	Work History	Overall	Work History	Overall	Work History	Overall	Work History	Overall	Work History	Overall	Work History
ENTIRE SAMPLE												
Prison	33.77	30.58	33.23	33.44	33.08	30.21	30.69	29.96	33.13	30.93	34.58	29.91
Age at sentence49	.48	.07	.06	.07	.22	.16	.25	.07	.24	.22	.25
Black41	.41	.46	.51	.42	.46	.59	.40	.35	.36	.44	.34
Female23	.10	.23	.23	.23	.11	.11	.11	.24	.10	.09	.10
Less than high school29	.38	.25	.21	.28	.34	.25	.39	.33	.42	.37	.44
GED07	.10	.06	.05	.07	.09	.06	.10	.08	.12	.10	.13
High school34	.30	.25	.22	.27	.23	.21	.24	.42	.38	.43	.36
More than high school29	.39	.23	.05	.36	.33	.04	.48	.46	.44	.06	.55
Presentence employment66	.63	.58	.56	.59	.54	.52	.56	.76	.71	.67	.73
Nonsingle62	.61	.61	.61	.61	.61	.63	.60	.64	.62	.58	.63
Ever used alcohol41	.27	.38	.38	.39	.20	.22	.19	.44	.34	.34	.34
Ever used marijuana14	.09	.10	.12	.09	.05	.06	.04	.17	.13	.14	.13
Ever used stimulants27	.25	.24	.24	.24	.24	.23	.25	.30	.26	.27	.26
Ever used opioids19	.18	.12	.13	.11	.11	.14	.09	.24	.24	.26	.23
Ever used other drugs												
Mental health illness history												

TABLE A4 (Continued)

		BLACKS						WHITES					
		Prison			Probation			Prison			Probation		
		ENTIRE SAMPLE		No	Work	Overall	No	Work	Overall	No	Work	Overall	No
		Prison	Probation	History	History	History	History	History	History	History	History	History	History
First time felony offender33	.73	.28	.28	.28	.67	.63	.69	.39	.36	.79	.75	.81
0–4 prior arrests24	.63	.22	.22	.23	.61	.56	.63	.25	.26	.64	.60	.66
5–9 prior arrests30	.26	.31	.29	.32	.27	.29	.26	.30	.27	.31	.26	.24
10+ prior arrests46	.12	.47	.49	.45	.13	.15	.11	.45	.48	.45	.11	.10
Pretrial detention69	.93	.62	.57	.64	.93	.93	.93	.76	.73	.77	.93	.92
Sentenced in 200329	.24	.29	.25	.32	.25	.20	.28	.29	.23	.31	.24	.25
Sentenced in 200426	.24	.26	.25	.26	.24	.24	.24	.25	.27	.25	.24	.25
Sentenced in 200523	.25	.23	.25	.22	.25	.26	.24	.23	.25	.22	.25	.25
Sentenced in 200622	.26	.22	.25	.20	.26	.29	.24	.22	.25	.21	.26	.26
Sample size	19,771	43,059	9,704	4,083	5,621	20,732	7,303	13,577	10,067	2,687	7,380	22,327	17,451

NOTE.—Presentence employment = ever employed in 23 quarters before sentence.

TABLE A5
OLS MODELS BY RACE AND WORK HISTORY (Three-Year Follow-up, Select Coefficients)

	BLACKS			WHITES			
	ENTIRE SAMPLE	Overall	No Work History	Work History	Overall	No Work History	Work History
Postsentence:							
Employment in the 12th quarter:							
Prison vs. probation	-.090*** (.004)	-.062*** (.005)	-.006 (.006)	-.093*** (.007)	-.110*** (.006)	-.029** (.009)	-.137*** (.007)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared183	.174	.022	.138	.172	.053	.130
Employed 3 consecutive quarters: ^{a,b}							
Prison vs. probation	-.077*** (.003)	-.054*** (.004)	-.007 (.004)	-.079*** (.006)	-.095*** (.005)	-.026*** (.007)	-.117*** (.006)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared169	.160	.015	.137	.160	.034	.131
Proportion quarters employed: ^{a,c}							
Prison vs. probation	-.183*** (.003)	-.134*** (.003)	-.031*** (.003)	-.199*** (.005)	-.224*** (.004)	-.062*** (.005)	-.280*** (.004)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared383	.377	.057	.319	.367	.106	.308
Employed outside SLM in 12th quarter:							
Prison vs. probation	-.059*** (.003)	-.041*** (.004)	-.005 (.004)	-.063*** (.006)	-.072*** (.005)	-.015* (.007)	-.091*** (.006)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared110	.109	.013	.093	.102	.029	.082
Imprisonment within 3 years:							
Prison vs. probation	-.003 (.003)	-.017*** (.005)	-.018* (.009)	-.017** (.006)	.007 (.004)	.021* (.009)	.004 (.005)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared072	.074	.073	.077	.072	.075	.076

TABLE A5 (Continued)

	ENTIRE SAMPLE	BLACKS			WHITES		
		Overall	No Work History	Work History	Overall	No Work History	Work History
Postrelease:							
Employment in the 12th quarter:							
Prison vs. probation016*** (.004)	.037*** (.006)	.078*** (.006)	.016 (.008)	.001 (.006)	.050*** (.010)	-.019* (.008)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared151	.159	.069	.123	.139	.054	.100
Employed 3 consecutive quarters: ^a							
Prison vs. probation010* (.004)	.026*** (.005)	.059*** (.005)	.010 (.007)	-.004 (.006)	.039*** (.008)	-.019*** (.007)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared145	.148	.074	.126	.139	.050	.111
Proportion quarters employed: ^a							
Prison vs. probation019*** (.003)	.033*** (.004)	.076*** (.005)	.008 (.006)	.010* (.005)	.075*** (.007)	-.015*** (.006)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared314	.336	.151	.257	.291	.098	.210
Employed outside SLM in 12th quarter:							
Prison vs. probation007* (.004)	.020*** (.004)	.042*** (.004)	.008 (.007)	-.002 (.005)	.029*** (.008)	-.014* (.007)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared094	.101	.050	.085	.089	.046	.068
Imprisonment within 3 years:							
Prison vs. probation127*** (.004)	.129*** (.007)	.141*** (.011)	.119*** (.008)	.121*** (.006)	.140*** (.012)	.113*** (.007)
Observations	105,435	45,303	16,618	28,685	60,132	14,146	45,986
R-squared105	.107	.101	.114	.104	.103	.109

NOTE.—Robust SEs are in parentheses; models include county fixed effects and covariates in table A2; SLM = secondary labor market.
^a Through 12th quarter.
 * $P < .05$.
 ** $P < .01$.
 *** $P < .001$.

Imprisonment and Labor Market Outcomes

TABLE A6
FOCAL CRIME BY SENTENCE TYPE

	NO WORK HISTORY				ANY WORK HISTORY			
	Prison		Probation		Prison		Probation	
	Freq.	%	Freq.	%	Freq.	%	Freq.	%
Black:								
Controlled substance	851	21	2,756	39	1031	18	4,255	31
Person	1,983	49	1,088	15	3073	55	2,228	16
Property	700	17	1,947	27	898	16	4,470	33
Public order.	49	1	256	4	61	1	465	3
Public safety	489	12	1,085	15	546	10	2,065	15
Other crimes	11	0	23	0	12	0	94	1
White:								
Controlled substance	329	12	1,684	35	752	10	6,185	35
Person	1,290	48	892	18	3,859	52	3,037	17
Property	518	19	1,408	29	1,453	20	5,642	32
Public order.	76	3	465	10	150	2	969	6
Public safety	450	17	398	8	1,090	15	1,476	8
Other crimes	24	1	29	1	76	1	142	1

TABLE A7
PRETRIAL DETENTION BY RACE, PRESENTENCE WORK HISTORY, AND SENTENCE TYPE

	NO WORK HISTORY				WORK HISTORY			
	Prison		Probation		Prison		Probation	
	Freq.	%	Freq.	%	Freq.	%	Freq.	%
Black:								
No detention	1,738	43	483	7	1,998	36	895	7
Any detention	2,345	57	6,672	93	3,623	64	12,682	93
White:								
No detention	718	27	362	7	1,697	23	1,424	8
Any detention	1,969	73	4,514	93	5,683	77	16,027	92
Black:								
0–7 days in detention	1,922	47	4,118	58	2,381	42	9,299	68
7+ days in detention. . . .	2,161	53	3,037	42	3,240	58	4,278	32
White:								
0–7 days in detention	907	34	3409	70	2,523	34	13,497	77
7+ days in detention. . . .	1,780	66	1467	30	4,857	66	3,954	23

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Juvenile Jails: A Path to the Straight and Narrow or to Hardened Criminality?

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Abstract

Juvenile justice systems throughout the United States have become increasingly punitive since the 1970s. Most states have passed legislation making it easier to transfer juveniles to the criminal courts. Supporters of this “get tough” movement argue, in part, that juvenile courts are ineffective in deterring young offenders. This claim, however, is based primarily on poorly designed evaluations that do not account for the nonrandom nature of sentencing. This paper demonstrates how the institutional features of the justice system can be exploited to identify causality when true random assignment is not feasible. In particular, I capitalize on discontinuities in punishment that arise in Washington State’s juvenile sentencing guidelines to identify the effect of incarceration on the postrelease criminal behavior of juveniles. The results indicate that incarcerated individuals have lower propensities to be reconvicted of a crime. This deterrent effect is also observed for older, criminally experienced, and/or violent youths.

1. Introduction

Juvenile justice systems throughout the United States have become increasingly punitive since the 1970s (Feld 1998). Forty-five states have passed laws making it easier to transfer juvenile offenders to the criminal justice system; this legislation has largely targeted youths who commit serious violent crimes or who are almost adults in the eyes of the court (Bishop 2000). At the extreme, three

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states since 1992 (Wyoming, New Hampshire, and Wisconsin) have lowered the upper age under which the juvenile court has original jurisdiction; as of 2004, 13 states had an upper age under 17. Consequently, the number of youths under age 18 in adult jails quadrupled between 1990 and 1999 (Snyder and Sickmund 2006). Legislation has also been passed that has increased sentence severity in the juvenile courts. The number of convicted juveniles committed to out-of-home placement increased by 44 percent between 1985 and 2002; more dramatically, the number of juvenile drug offense and crimes-against-person cases resulting in outside placement increased by 179 and 109 percent, respectively (Stahl et al. 2005).

This “get tough” movement was spurred on in the late 1980s by public concern about the increasing numbers of violent crimes, particularly homicides and gun crimes. Homicide arrest rates of juveniles ages 10–17 more than doubled between 1984 and 1991, and the use of guns in male youth homicides increased by about 17 percentage points from 1985 to 1993 (National Research Council and Institute of Medicine 2001; Cook and Laub 2001). Supporters of this movement and, in particular, of increased transfers to the criminal courts maintained that the persistently high juvenile violent crime rates were an indication of the ineffectiveness of the juvenile courts in curtailing crime (Fagan and Deschenes 1990). They based this belief, that essentially nothing done by the juvenile courts works, on a number of evaluations of juvenile treatment programs in the 1970s (see, for instance, Robinson and Smith 1971; Lipton, Martinson, and Wilks 1975; Wright and Dixon 1975; Sechrest, White, and Brown 1979). Advocates of the juvenile courts, however, argue that this assessment is based on poor evaluations and/or programs of low quality (Fagan 1990; Palmer 1991); in particular, much of this research does not properly account for the fact that youths are not randomly assigned to treatment programs or sanctions. In addition, recent research indicates that transferring juveniles to the criminal courts does not deter the general population and may, in fact, increase recidivism (Jensen and Metsger 1994; Singer and McDowall 1988; Bishop et al. 1996; Fagan 1996; Winner et al. 1997).

This paper makes a number of contributions to this debate. First, it provides empirical evidence that juvenile court sanctions can be effective in reducing recidivism, contrary to the belief of those advocating transfers to the criminal courts. Second, it improves on the analytical weaknesses of many earlier studies by demonstrating how the institutional features of the justice system can be exploited to identify a causal effect in a context in which true random assignment is not feasible.¹ Third, it evaluates the effectiveness of a large-scale residential placement program in Washington State rather than a specific program feature or small-scale alternative treatment.

¹ Economists studying crime are increasingly utilizing such research designs to answer other questions. For instance, Chen and Shapiro (2007) study the effect of prison security level on recidivism using a regression discontinuity design. Kuziemko (2007) takes advantage of cutoffs in parole board guidelines and finds that longer sentence lengths decrease recidivism.

Specifically, this paper utilizes an administrative data set of more than 20,000 adjudicated (or convicted) juveniles from the Washington State juvenile courts and Washington's post-July 1998 juvenile sentencing guidelines. These guidelines consist of a sentencing grid that determines an individual's punishment on the basis of the severity of his or her current offense and criminal history score. An individual is sentenced to a state facility for a minimum of 15 weeks if he or she falls above a prespecified cutoff in the grid; otherwise, he or she receives a more minor sanction, such as a fine or probation. Even in the absence of random assignment to residential placement, a causal effect can be identified by comparing the recidivism behavior of individuals on either side of the prespecified cutoffs, only one group of which was incarcerated.

Underlying this identification strategy is the assumption that unobservable characteristics of the youths do not vary discontinuously at the cutoffs in the grid. The fact that I do not find any systematic variation in a large set of observable characteristics that is correlated with recidivism and that varies discontinuously around the cutoffs supports this assumption. In fact, the estimated treatment effect does not change when controlling for a wide array of variables. There is also no evidence of two behaviors that could potentially undermine this identification strategy—court and juvenile gaming of the sentencing system.

Contrary to the opinion of those advocating increased juvenile transfers, this study indicates that incarceration in juvenile facilities can be an effective means of combating juvenile crime. In fact, incarcerated individuals have a daily hazard rate of recidivating that is more than 35 percent lower than that of nonincarcerated individuals. This deterrence effect is observed even for individuals with histories of violent crimes, youths of all ages, and youths who have and have not been previously incarcerated in a juvenile facility. The fact that deterrence is not isolated to younger, inexperienced, nonviolent individuals has important policy implications, given that (1) many arguments in favor of transferring juveniles to the criminal courts are prefaced on the belief that the juvenile justice system cannot adequately deal with these older, criminally experienced, and/or violent juveniles and (2) recent research, such as Fagan (1996), suggests that youths incarcerated via the criminal courts are actually more likely to recidivate.

The remainder of the paper is organized as follows. Section 2 provides background information on the previous literature and Washington's juvenile sentencing guidelines and describes the data. Section 3 presents the empirical specification, identifying assumptions, and evidence that these identifying assumptions are satisfied. Section 4 presents the results, and Section 5 concludes.

2. Background and Data

2.1. *Previous Literature*

There are few studies of the specific deterrence effects of juvenile incarceration, that is, whether there is a decrease in the postrelease criminal behavior of the

incarcerated youth. The studies that do exist yield conflicting findings and generally do not take into account unobserved heterogeneity.^{2,3} For instance, McCorkle, Elias, and Bixby (1957) and Stevenson and Scarpitti (1974) find higher recidivism rates for juveniles in traditional correctional institutions than those receiving somewhat more experimental community sanctions. As is the case in most sentencing decisions, individuals are likely to be selected into the experimental program on the basis of characteristics observable to the judge but not to the researcher; this casts doubt on whether one can interpret the findings as causal. In contrast, Murray and Cox (1979) find that juveniles were arrested less frequently after a number of interventions, including incarceration. Once again, assignment to the treatment programs is not random, and the authors do not control for any characteristics other than treatment. In addition, Maltz (1980) argues that mean reverting behavior could also account for Murray and Cox's (1979) findings.⁴

Manski and Nagin (1998) show that unobserved heterogeneity can explain the mixed findings in the literature. Specifically, they find that confinement exacerbates recidivism if they assume that judges assign treatment to minimize recidivism. But when they assume that the members of a high-risk group receive treatment, they find a deterrent effect. Thus, one of the goals of the current paper is estimate the effect of juvenile sanctions in a manner that adequately deals with the issue of unobserved heterogeneity.

2.2. Washington State's Juvenile Sentencing Guidelines

Because juvenile justice systems are state specific and vary greatly across states, it is very difficult to evaluate the effect of juvenile sanctions at a national level. Thus, this paper focuses on the state of Washington, which has been a leader in reforming the juvenile justice system; in fact, it is the only state to have enacted sentencing guidelines for juvenile offenders (Boerner and Lieb 2001). My empirical analysis is based on these guidelines. Specifically, Washington State implemented the Juvenile Justice Act of 1977 (Wash. Rev. Code sec. 13.40), which included a presumptive, determinate sentencing system, to address grow-

² The economics literature focuses on estimating the general deterrent effects of incarceration or the effect of incarceration on the criminal activity of unincarcerated individuals (National Research Council 1978). See Kessler and Levitt (1999), Lee and McCrary (2005), Levitt (1998), and Levitt (1996); only Levitt (1998) focuses on juvenile behavior. Nagin (1978) reviews the early deterrence literature; see Levitt (2004) for a more recent review.

³ See Giguere (2005) for a review of the juvenile-specific deterrence literature in criminology. She indicates that much of the specific deterrence research does not look at recidivism as the dependent variable and often focuses on the effects of sanctions other than incarceration. Giguere's conclusion is that the empirical research on the effect of juvenile incarceration on recidivism is inconclusive.

⁴ Because of the sensitive nature of the question, few large-scale random-assignment experiments are done. One exception is the Lerman (1975) study of the Community Treatment Project in California that began in 1961, which randomly assigned youths to the California Youth Authority (institutionalization) or intensive supervision for 8 months and then limited supervision thereafter. He does not find an impact on recidivism behavior.

A+	180 weeks to Age 21 for all category A+ offenses				
A	103 - 129 weeks for all category A offenses				
A-	15 - 36 Except 30 - 40 weeks for 15 to 17 year olds.	52 - 65	80 - 100	103 - 129	103 - 129
B+	15 - 36	15 - 36	52 - 65	80 - 100	103 - 129
B			15 - 36	15 - 36	52 - 65
C+				15 - 36	15 - 36
C					15 - 36
D+					
D					
E					
	0	1	2	3	4 or more

Rounded Adjudication Score

Figure 1. Post-July 1998 sentencing grid

ing concerns that juveniles arrested for like crimes were being disparately prosecuted and sanctioned within and across jurisdictions.^{5,6}

Juveniles in Washington against whom a formal petition is filed and who either plead or are adjudicated guilty receive a disposition in accordance with the current sentencing guidelines. The 1977 guidelines determined sentences on the basis of the seriousness of the current offense, age, criminal history, and time between offenses. In 1997, the guidelines were significantly amended, such that punishment depends only on the current offense and criminal history.

This paper focuses on the sentencing grid that is effective for any offense committed on or after July 1, 1998 (see Figure 1). The *x*-axis indicates the individual's prior adjudication score, which is calculated by assigning one point to each previously adjudicated felony and one-quarter point to each violation

⁵ Presumptive sentencing is characterized by the following: (1) the appropriate sentence for an offender is presumed to fall within a range of sentences authorized by sentencing guidelines that are adopted by a legislatively created sentencing body, (2) judges are expected to sentence within the range or provide written justification for departure, and (3) the guidelines provide for some review of the departure. Determinate sentencing implies that an offender sentenced to incarceration is given a fixed term that may be reduced by good or earned time (Bureau of Justice Assistance 1996). Thus, the nature of these guidelines also addresses an additional criticism made by advocates of transferring juveniles to the adult courts, namely, that punishment is less certain in juvenile courts (Fagan and Deschenes 1990).

⁶ Focusing on one state, however, does raise the question of generalizability. Thus, it is important to point out that Washington is not an outlier in many dimensions. According to the Federal Bureau of Investigation's *Uniform Crime Reports*, violent crime rates in Washington tend to be lower than national rates, while property crime rates tend to be higher.

or misdemeanor. The total number of points is rounded down and determines the appropriate column of the grid. The y -axis indicates the current offense class, ranging from the least serious gross misdemeanor (class E) to the most serious felony (class A+); crime classes C through A are felonies. The cell corresponding to the intersection between the current offense class and rounded adjudication score indicates the punishment. There are two types of punishments: local sanctions and incarceration in a state detention facility. Local sanctions, indicated by the shaded cells, can include any combination of 0–30 days in a local detention facility, 0–12 months of community supervision, 0–150 hours of community service, and a fine of \$0–\$500. For sentences of state incarceration, the cell also denotes the sentencing range; the minimum is 15 weeks.

The most relevant feature of the grid for this paper is the discontinuous nature of sentencing within the rows and columns. For instance, youths with rounded adjudication scores of zero and a current offense class of B or lower are sentenced to a local sanction, while those with offense classes of B+ or higher are sentenced to 15–36 weeks of incarceration. Similarly, if there are two individuals with a current offense class of C+ and adjudication scores of $2\frac{3}{4}$ and 3, then only the latter individual will be sentenced to state incarceration.

There are three legitimate reasons to depart from the standard range of the guidelines, including the Chemical Dependency Disposition Alternative and the Special Sex Offender Disposition Alternative (SSODA). The latter is reserved for nonserious violent sex offenders who do not have a sex offense history. The judge can also reduce (increase) the sentence and declare a manifest injustice downward (upward) if he or she believes that the standard sentence is too harsh (lenient).

2.3. Data Description and Summary Statistics

Case-level administrative data were provided by the Washington Juvenile Courts for the years 1981–2000. A case is defined as a youth processed by the court on a new referral. Although each referral can contain multiple offenses, only the three most serious are observed; however, fewer than 20 percent of the cases referred have three referral reasons. Each case record includes demographic information (zip code, county of residence, sex, birth date, and ethnicity); the types of offenses referred; the dates of the offense, referral, and disposition; whether each offense is valid for criminal history; and the three most serious dispositions.⁷ A unique identification number allows one to track a juvenile's criminal history and recidivism. I then created a data set of 20,542 youths who were adjudicated of at least one offense, through formal court procedures, on

⁷ An offense is valid for criminal history if the individual was found guilty at trial or plead guilty to the offense without a trial or if the offense was diverted.

or after July 1, 1998. Only youths adjudicated of one or two offenses, only one of which is punishable by state incarceration, were included in the analysis.⁸

I define each individual's current offense to be the first formally handled, adjudicated offense after July 1, 1998. If the individual is adjudicated of two offenses on the same date, the current offense is defined to be the more serious of the two. Past offenses are defined as those adjudicated prior to the current offense date. An individual's current offense is characterized with 13 offense type dummy variables (assault, sex crime, theft, robbery, and so forth) and offense class dummies, ranging from E to A+.⁹ A parallel set of variables characterize an individual's past offenses. (See Table A1 for variable names and definitions.) Because the data set does not include the actual adjudication history scores, I calculate the actual and rounded scores using the above-described formula. I then assign the juvenile to a cell in the grid, which determines whether he or she ought to be incarcerated by the Department of Juvenile Rehabilitation (DJR) and the minimum and maximum sentence lengths.

Finally, the first subsequent adjudicated offense is used to characterize recidivism. Two important data-related issues arise when measuring recidivism. First, there are two types of censoring in the data: (1) individuals age out of the data at age 18, and (2) only offenses that are disposed of on or before December 31, 2000, are observed. Censoring will empirically be accounted for in a hazard framework. Second, the data do not include release dates; thus, one must use the expected sentence length (based on the grid) to determine when the individual becomes at risk of recidivating. Throughout the paper, I use the disposition date plus the minimum sentence and calculate the time to recidivism accordingly. An advantage of using this date rather than just the disposition date, for instance, is that it helps to isolate deterrence from incapacitation, that is, those crimes the youth cannot commit because he is isolated from society.¹⁰ Using the minimum sentence date does imply, however, that two youths who were the same age at the time of disposition are different ages when they become at risk of recidivating. This issue will be discussed in more detail when interpreting the results.

Table 1 provides selected summary statistics for the sample as a whole, the 1,147 individuals sentenced to state incarceration (DJR) for their current offense

⁸ This results in the exclusion of approximately 950 individuals. If the individual is adjudicated of multiple offenses, the data set does not allow me to determine which dispositions correspond to which crimes. If only one offense is adjudicated, then all punishments are attributable to that crime. If two offenses are adjudicated but only one can result in incarceration, I can deduce to which offense the sentence of incarceration corresponds. Excluding these individuals raises the issue of sample selection; that is, I am selecting individuals who are slightly less serious criminals. While this should not bias the estimated treatment effects, it may reduce the generalizability of the results.

⁹ These offense categories and classes are based on the classification scheme presented in the Sentencing Guidelines Commission's *Juvenile Disposition Manuals* (http://www.sgc.wa.gov/PUBS/Juvenile/Juvenile_Manual_title_Page.htm).

¹⁰ The results presented throughout this paper are qualitatively identical if individuals are at risk of recidivism as of the disposition date plus the maximum sentence or just the disposition date; in the latter case, effect sizes appear larger because incapacitation is not accounted for.

Table 1
Summary Statistics

	Full Sample		Sentenced to State Incarceration		Not Sentenced to State Incarceration	
	Mean	SD	Mean	SD	Mean	SD
Demographic:						
Male	.76	.43	.88	.32	.75	.43
Black	.12	.33	.18	.39	.12	.32
Hispanic	.097	.30	.13	.34	.095	.29
Native American	.039	.19	.036	.19	.039	.19
Age_off	15.57	1.56	15.82	1.41	15.6	1.6
Current offense:						
Arson	.085	.28	.037	.19	.088	.28
Assault	.18	.39	.19	.39	.18	.39
Burglary	.12	.32	.15	.36	.11	.32
Drugs	.16	.36	.088	.28	.16	.37
Firearm	.023	.15	.028	.16	.022	.15
Homicide	.00049	.022	.0070	.083	.00010	.010
Motor	.016	.12	.012	.11	.016	.12
Kidnap	.00054	.023	.0017	.042	.00046	.022
Obstruction	.016	.13	.0061	.078	.017	.13
Public	.011	.11	.00087	.030	.012	.11
Theft	.31	.46	.28	.45	.31	.46
Sex	.021	.14	.13	.34	.014	.12
Other	.060	.24	.061	.24	.060	.24
A+	.000049	.0070	.00087	.030	0	0
A	.0019	.044	.024	.15	.00067	.026
A-	.011	.11	.093	.29	.0066	.081
B+	.029	.17	.35	.48	.010	.099
B	.095	.29	.20	.40	.089	.28
C+	.027	.16	.050	.22	.025	.16
C	.16	.36	.19	.39	.15	.36
D+	.16	.36	.035	.18	.16	.37
D	.33	.47	.049	.22	.35	.48
E	.19	.39	.011	.11	.20	.40
Past offense:						
Pany_arson	.12	.33	.24	.43	.11	.32
Pany_ass	.17	.38	.30	.46	.16	.37
Pany_burg	.13	.34	.34	.47	.12	.32
Pany_drug	.13	.33	.17	.38	.12	.33
Pany_farm	.032	.18	.076	.26	.030	.17
Pany_hom	.00015	.012	.0017	.042	.000052	.0072
Pany_motor	.031	.17	.064	.24	.029	.17
Pany_kid	.00058	.024	.00087	.030	.00057	.024
Pany_obstr	.022	.15	.056	.23	.020	.14
Pany_public	.013	.11	.022	.15	.013	.11
Pany_theft	.34	.47	.52	.50	.33	.47
Pany_sex	.013	.11	.042	.20	.011	.11
Pany_other	.057	.23	.13	.34	.052	.22
Rnd_score = 0	.78	.41	.46	.50	.80	.40
Rnd_score = 1	.11	.32	.13	.34	.11	.32
Rnd_score = 2	.046	.21	.11	.31	.042	.20
Rnd_score = 3	.026	.16	.10	.30	.022	.15
Rnd_score = 4	.031	.17	.20	.40	.021	.14
Recidivism offense: recidivate	.27	.44	.19	.39	.27	.44
N	20,542		1,147		19,395	

Note. Approximately 1,250 individuals are missing information concerning their race; so as to not lose these observations, I include a dummy variable indicating that this information is missing.

and the 19,395 nonincarcerated individuals. Seventy-six percent of the sample is male and 12, 9.7, and 3.9 percent are black, Hispanic, and Native American, respectively. For the entire sample, the most common current offense categories are arson, assault, burglary, drugs, and theft. The incarcerated sample, however, is composed of fewer arson and drug offenses but many more sex offenses. Incarcerated individuals also have more serious histories; more than 40 percent of the DJR individuals have an adjudication history score greater than or equal to 2, while less than 9 percent of the non-DJR individuals have such a score. Finally, approximately 27 percent of the entire sample recidivates, while only 19 percent of the incarcerated sample recidivates; note that this recidivism measure does not yet take censoring into account.¹¹

3. Empirical Methodology and Identification

3.1. Empirical Specification

The basic specification is presented in equation (1). It is estimated with a Cox proportional hazard model to utilize survival time information in the data and to control for the two sources of censoring:

$$\lambda(t; \mathbf{x}) = \lambda_0(t) \exp [\mathbf{X}_i \beta + \text{Class}_i \gamma + \lambda_1 f(\text{Act_adj_score}_i) + \alpha D_{\text{above_cutoff}_i}]. \quad (1)$$

Failure, or recidivism, is defined to be an adjudicated referral back to the juvenile courts; $\lambda(t; \mathbf{x})$ is the hazard rate of recidivism conditional on covariates \mathbf{x} , while $\lambda_0(t)$ is the baseline hazard. This baseline hazard rate of failure is common to all units in the population; individual hazard functions differ proportionately on the basis of a function of observable characteristics.

Specifically, Class is a vector of current offense class dummy variables or fixed effects; in other words, the analysis focuses on identifying the effect of incarceration within a row of the sentencing grid.¹² The term $f(\text{Act_adj_score})$ is a second-order polynomial of the actual adjudication score, and $D_{\text{above_cutoff}}$ is a dummy variable indicating whether an individual falls in a cell of the grid that prescribes state incarceration.¹³ The intuition underlying this identification

¹¹ The proportion of the sample censored because of reaching the end of the sample period is fairly constant across the incarcerated and nonincarcerated samples, while the proportion censored because of reaching age 18 is greater in the incarcerated sample. Additional analyses indicate that the main results are not sensitive to excluding individuals who turn 18 before December 31, 2000.

¹² While discontinuities in sentencing also exist within columns, current offense classes do not translate directly into a numerical representation. Although one can create such a representation, the distance between each class would be somewhat arbitrary. Thus, the analysis emphasizes the discontinuities within a row as the exogenous source of treatment variation; that is, current offense class dummies are included instead of assigning an arbitrary functional form to the score on the y -axis. The results are completely robust, however, to such a specification.

¹³ This specification identifies only the effect of a 15- to 36-week sentence. Unfortunately, data limitations make it infeasible to include dummy variables for additional grid cutoffs, such as those between 15- to 36- and 52- to 65-week sentences.

strategy parallels that of a regression discontinuity design.¹⁴ Identification is based on the idea that the sample of individuals within a very small interval around the cutoff are very similar to a randomized experiment at the cutoff—they have essentially the same assignment score. In the current context, the assignment score is the actual adjudication score. Because this underlying score is not perfectly continuous and moves in one-quarter-point intervals, individuals on either side of the cutoff could be systematically different. The equation controls for such differences by using the entire sample and including a polynomial of the underlying score, $f(\text{Act_adj_score})$. The term \mathbf{X} is a vector of other observable characteristics that may also influence recidivism.

3.2. Identification

The coefficient on D_above_cutoff , α , provides an unbiased estimate of the effect of state incarceration if (1) treatment (incarceration) varies discontinuously at this cutoff, (2) unobservables do not vary discontinuously at the cutoff, and (3) the assignment mechanism is perfectly followed. This section addresses each of these conditions in turn.

Figures 2–6 and 7–11 provide evidence that discontinuities in treatment exist within rows and columns of the grid, respectively. Given that the equation includes current offense class dummies, the existence of the former are most relevant. Specifically, Figures 2–6 hold the current offense class constant and plot the percentage of individuals incarcerated for each actual adjudication score in that row.¹⁵ Figures 7–11 hold the rounded adjudication score constant and plot the percentage of individuals incarcerated in each cell of that column. The vertical lines indicate where a discontinuity in treatment ought to occur. For instance, Figures 2 and 6 consider those individuals with B+ and D+ current offenses, respectively. As these rows do not contain any cutoffs, it is reassuring that there are no discontinuities in the percentage of individuals incarcerated. In contrast, Figure 3 indicates a jump in the percentage incarcerated from 25 to 59 percent when the actual adjudication score moves from 1.75 to 2.0 for those with a class B current offense. Similar jumps are observed within columns; Figure 7 indicates that 3 percent of those with a rounded score of zero and a B offense are incarcerated, compared with 62 percent of those with a B+ offense. Overall, Figures 2–11 indicate that discontinuities in treatment exist at the cutoffs but that the assignment mechanism is not perfectly followed.

Second, all unobserved correlates of recidivism must vary continuously with the score. While this cannot be tested directly, I can examine any observable

¹⁴ Regression discontinuity designs have been increasingly used in economics. See Thistlethwaite and Campbell (1960), Seaver and Quarton (1976), Trochim (1984), Angrist and Lavy (1999), van der Klaauw (2002), and Jacob and Lefgren (2004). Papers on crime that utilize this design include Berk and Rauma (1983), Berk and de Leeuw (1999), Chen and Shapiro (2007), and Kuziemko (2007).

¹⁵ The only exception is for the C+ row of the grid, as there are just 28 and 16 individuals in the rounded cells surrounding the cutoff. Thus, the graph for the C+ row is presented by a rounded adjudication score.

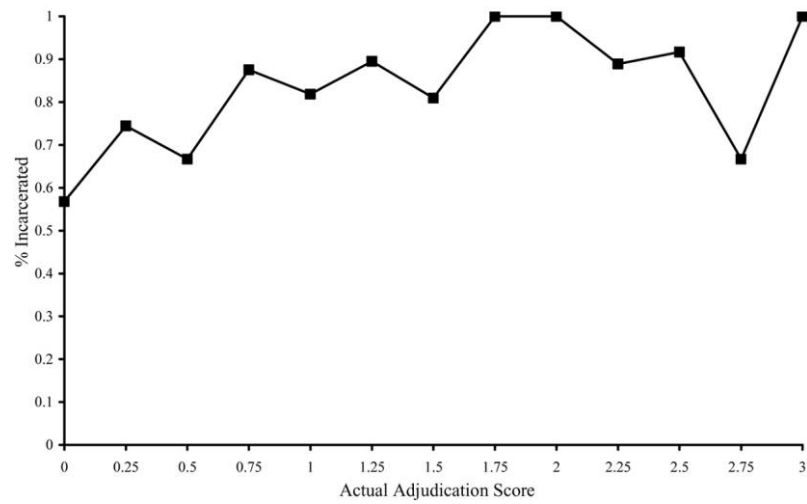


Figure 2. Discontinuities in treatment for current offense class B+

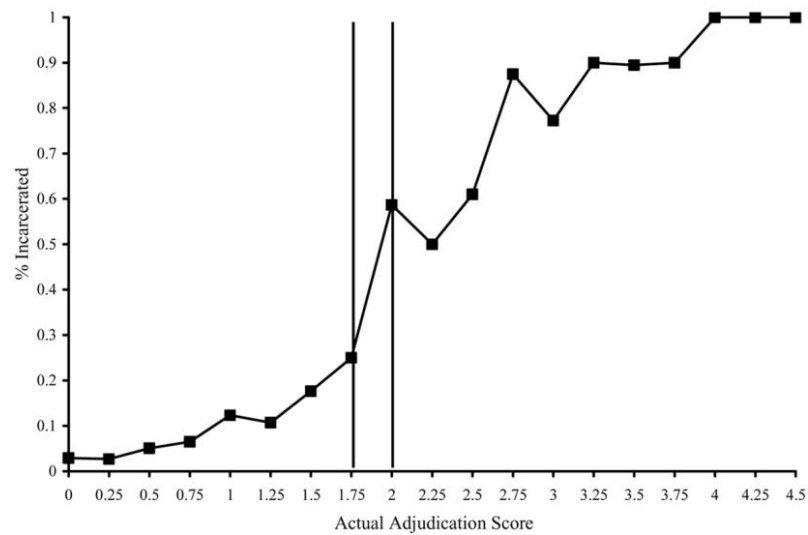


Figure 3. Discontinuities in treatment for current offense class B

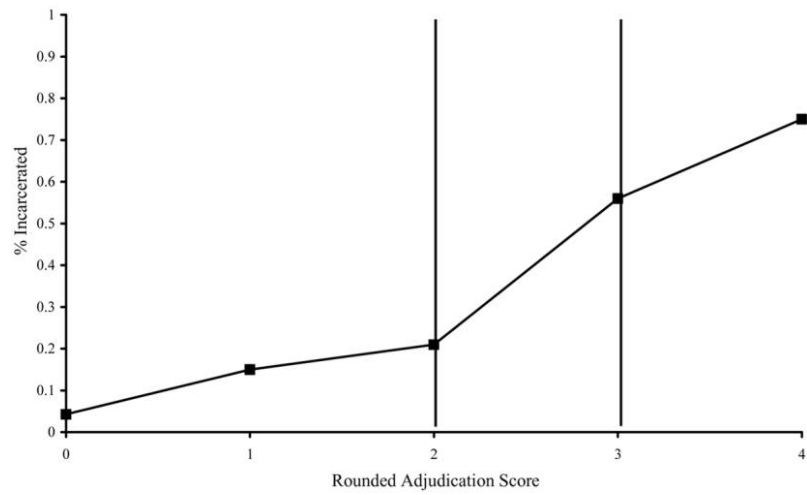


Figure 4. Discontinuities in treatment for current offense class C+

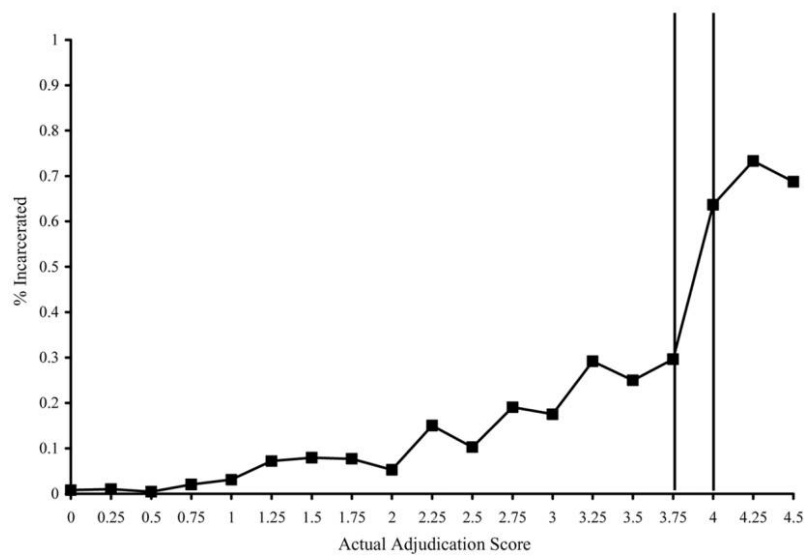


Figure 5. Discontinuities in treatment for current offense class C

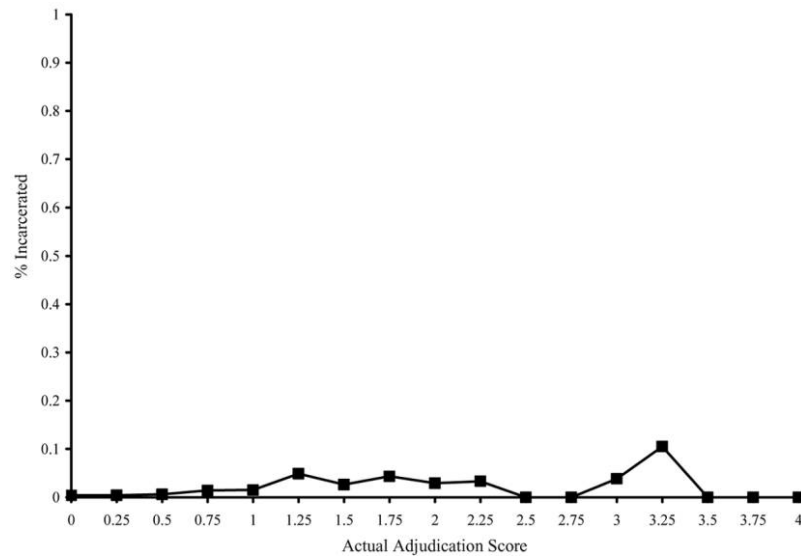


Figure 6. Discontinuities in treatment for current offense class D+

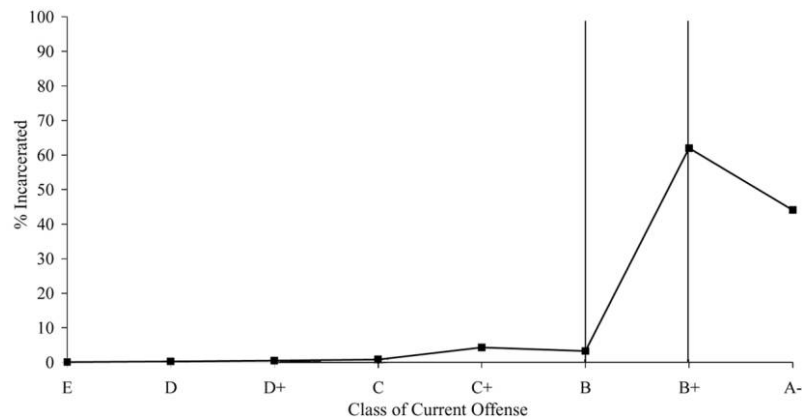


Figure 7. Discontinuities in treatment when holding the rounded adjudication score equal to 0

correlates of recidivism. Specifically, I focus on the variation in the observables that is related to recidivism by first estimating a Cox proportional hazard model where the only covariates are a subset of X_i and then calculating the estimated hazard ratio, $\hat{\lambda}$, which is then regressed on D_above_cutoff , a second-order polynomial of the actual adjudication score, and a set of current offense class dummies. An insignificant coefficient on D_above_cutoff in the latter regression

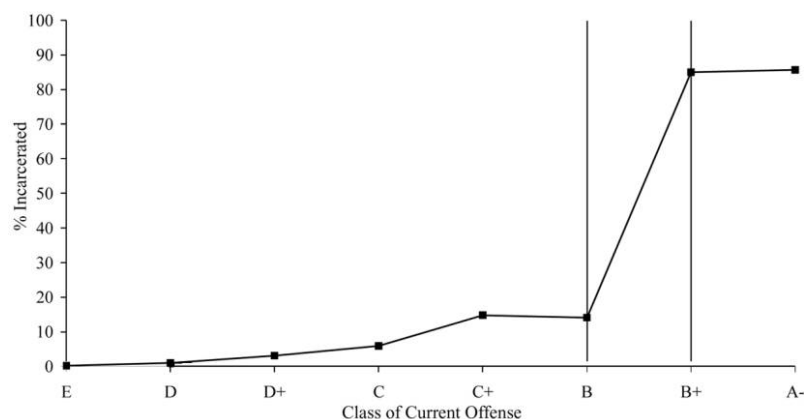


Figure 8. Discontinuities in treatment when holding the rounded adjudication score equal to 1

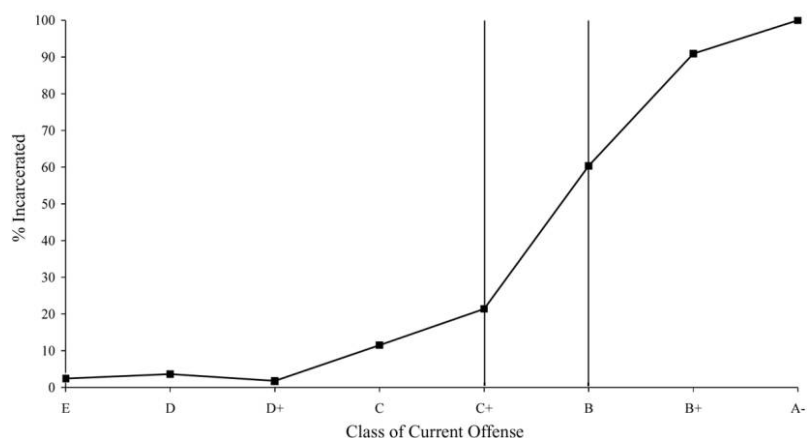


Figure 9. Discontinuities in treatment when holding the rounded adjudication score equal to 2

implies that the variation in the observables that explains an individual's daily hazard rate of recidivating does not vary discontinuously at the cutoff. Column 1 of Table 2 displays these results when X includes only demographic characteristics, while columns 2 and 3 add to X dummies for past and current experience, respectively, in each of the 13 crime categories. The coefficient on D_above_cutoff is insignificant and equal to zero to two digits in each specification.

Finally, whether the assignment mechanism is perfectly followed affects the interpretation of α . When there are individuals around the cutoff who do not

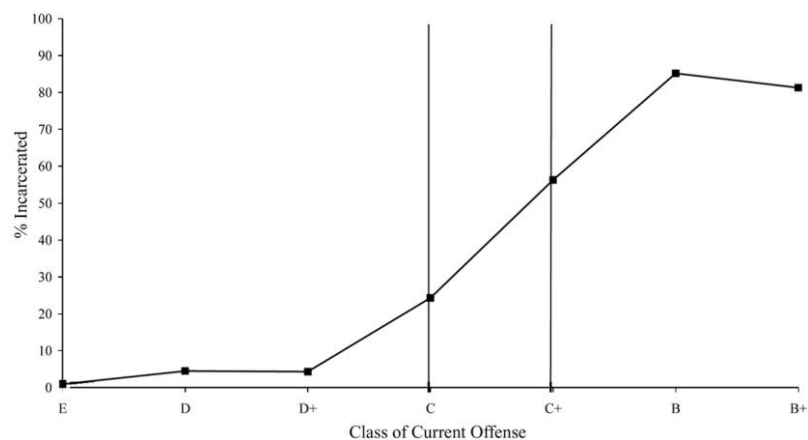


Figure 10. Discontinuities in treatment when holding the rounded adjudication score equal to 3

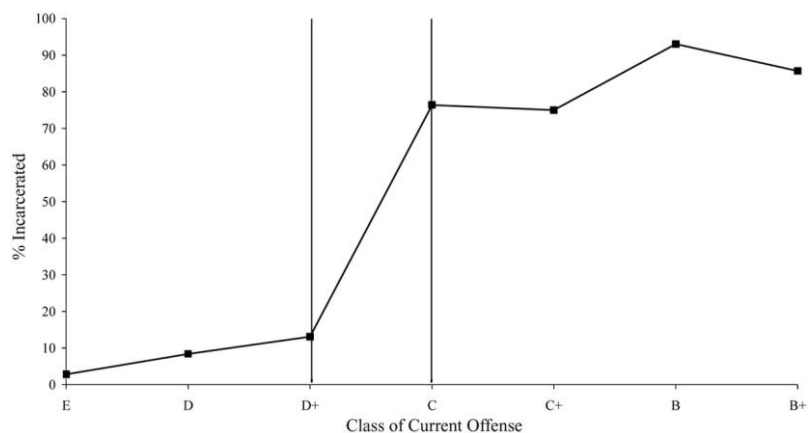


Figure 11. Discontinuities in treatment when holding the rounded adjudication score equal to 4

receive the punishment prescribed by the grid, α cannot be interpreted as a treatment effect. Basically, this mimics the intent-to-treat analysis in an experimental design and yields diluted estimates of the treatment effect. Figures 2 and 3 indicate that the assignment mechanism is not perfectly followed. There are 424 youths who are not sentenced to DJR but whom I place in a cell prescribing incarceration, while 314 youths are sentenced to DJR but are not placed in such a cell. Such misassignment can arise in two ways: I am assigning individuals to the correct cell but the courts depart from the prescribed sentence, or I am incorrectly assigning individuals to the grid.

Table 2
Do Observable Characteristics Vary Discontinuously at the Cutoff?

	$\hat{\lambda}(\mathbf{x}_1)$ (1)	$\hat{\lambda}(\mathbf{x}, \mathbf{x}_1, \mathbf{x}_2)$ (2)	$\hat{\lambda}(\mathbf{x}_1, \mathbf{x}_2, \mathbf{x}_3)$ (3)	$\hat{\lambda}(\mathbf{x}_1, \mathbf{x}_2, \mathbf{x}_3, \mathbf{x}_4)$ (4)
D_above_cutoff	-.00 (.01)	-.01 (.01)	-.00 (.01)	-.01 (.01)
Act_adj_score	-.00 (.00)	.07** (.00)	.07** (.00)	.07** (.00)
Act_adj_score ²	.00 ⁺ (.00)	-.01** (.00)	-.01** (.00)	-.00** (.00)
Constant	.41** (.00)	.19** (.00)	.17** (.00)	.16** (.00)
R ²	.01	.33	.33	.32

Note. The term \mathbf{x}_1 includes race, gender, and age variables; \mathbf{x}_2 includes past offense type dummy variables; \mathbf{x}_3 includes current offense type dummy variables; and \mathbf{x}_4 includes whether the individual was sentenced to the Department of Juvenile Rehabilitation in the past, the number of past referrals invalid for criminal history, and the age at first referral. Standard errors are in parentheses. All regressions include row dummies. $N = 18,616$.

⁺ Significant at the 10% level.

** Significant at the 1% level.

To assess the relevance of the first mechanism, I regress whether individuals are misassigned to a punishment that is either too harsh or too lenient on demographic characteristics, current and past offense characteristics, and legitimate reasons for departure.¹⁶ There is very little systematic misassignment on the basis of these variables. One exception is that sex offenders are approximately 30 percent more likely to be assigned a too-harsh punishment, even when controlling for SSODA, which is a legitimate departure from the grid. This could imply that the use of SSODA is not accurately denoted in the data.¹⁷ Similarly, manifest injustice downward and upward dispositions are significantly associated with punishments that are too harsh and too lenient, respectively. Eligibility for a declination hearing to the criminal courts also increases the chance of a too-harsh punishment.¹⁸ These are legitimate departures from the grid that, as evidenced by the case for sex offenses, may not be accurately recorded in the data; this reporting issue could arise from the fact that only three dispositions are included. With regard to the second potential source of misassignment, it is certainly possible that I miscalculate adjudication history scores given that only the three most serious referral reasons are observed. But controlling for whether an individual ever had a case with three referrals increases the chance of assignment to a too-lenient punishment by only .8 percent.

The remainder of the analysis is therefore based on those individuals who follow the assignment mechanism; the 738 youths whose sentences do not appear

¹⁶ Regression results are available from the author on request.

¹⁷ There is some evidence of this, as the data indicate that only 67 individuals received a sentence of Special Sex Offender Disposition Alternative (SSODA), while more than 350 individuals were eligible. In addition, there is evidence that either the actual use of SSODA or the reporting of such use varies across counties. Thirty-three counties contain SSODA-eligible individuals. The use of SSODA is reported for none who are eligible in 21 counties, all who are eligible in three counties, and 10–55 percent of those eligible in nine counties. However, county dummies are not significantly related to a too-harsh or too-lenient punishment.

¹⁸ See Wash. Rev. Code sec. 13.40.110 for transfer eligibility criteria.

to follow this mechanism are dropped from the sample. Treatment in the remaining sample is perfectly assigned by the rules of the grid, and α can be interpreted as the effect of incarceration; in other words, D_{above_cutoff} is equivalent to DJR. Because there is very little systematic evidence of misassignment and because that which I can identify appears to be legitimate departures from the grid, I do not expect the omission of individuals who do not follow the assignment mechanism to bias the results. In addition, it is important to note that 432 of the omitted individuals are actually in the first column of the sentencing grid. These youths do not fall immediately on either side of a cutoff within a row; therefore, the effect of incarceration would not be directly identified from these individuals (even if they were included in the analysis).^{19,20}

3.3. Discretion or Gaming by the Courts

The identifying assumptions would not be satisfied if prosecutors could use discretion to systematically charge juveniles with crimes that place them on either side of a cutoff. This could occur around the cutoffs within either a column or a row. In the former case, prosecutors may exercise discretion in deciding whether to impose a charge that will result in incarceration today. However, as identification is based on discontinuities within the rows, it is the latter case that is of more concern. Such discretion would imply that prosecutors decide today's charges with the juvenile's future score in mind. This type of discretion is much more complicated and highly implausible in the current context. Because the current offense is defined as the first post-July 1998 offense, the adjudication history scores were completely determined under the previous guidelines. The earlier guidelines were dramatically different than the post-1998 guidelines and did not even use the same scoring scale.²¹ Thus, prosecutorial discretion could not have played a role in whether criminal history scores fall to either side of a cutoff in the revised grid.

Empirical tests provide further evidence that prosecutorial discretion is not a concern. Such discretion could occur on the basis of individual characteristics

¹⁹ The author has also estimated a two-stage model as an alternative to omitting those individuals who do not follow the rules. The first stage regresses whether or not the individual was incarcerated on whether he or she fell above the cutoff, while the second stage replaces whether he or she fell above the cutoff in the equation with the predicted values from the first stage. The qualitative results from the two-stage model are quite comparable to those presented in the current paper and, if anything, generally find slightly larger deterrence effects. The two-stage estimates are available from the author on request.

²⁰ The results are also robust to the exclusion of an additional 1,000 individuals who were assigned the correct punishment but who are characterized by correlates of misassignment, such as sex offenders.

²¹ First, prior grids were not two-dimensional; rather, sanctions were initially based on four variables. Second, all delinquents were not subject to the same sentencing grid; rather, the judge first classified the offender as a serious offender, middle offender, or minor offender to determine the appropriate sentencing grid. Third, the original scoring system was considerably different, even within each class of offender. A youth's age and current offense established a base point number (ranging from 4 to 375), which was multiplied by an increase factor determined by the youth's criminal history and how far in the past the previous offenses occurred.

that are not captured by the grid. However, this would imply discontinuities in observables around the cutoffs, which Table 2 demonstrates not to be the case. Column 4 of Table 2 repeats this analysis for additional variables on which a prosecutor may base a discretionary decision: age at first referral, number of previous offenses that were not valid for criminal history, and previous incarceration. The coefficient on *D_above_cutoff* is again insignificant.^{22,23}

A similar concern is that judges do not sentence juveniles according to the sentencing grid but rather according to their perceptions of how amenable the juveniles would be to treatment. If judges were able to selectively incarcerate those juveniles who are most likely to be deterred by such a sentence, then the estimation results presented here would be biased in favor of finding evidence of deterrence. As the identification strategy is based on the sentencing discontinuity within a given row of the grid, this is equivalent to a concern that judges can systematically assign a juvenile an adjudication history score that places him or her exactly at a cutoff. Table 3 provides an informal test of this hypothesis. Specifically, conditional on each current offense class or row in which a treatment discontinuity exists, it compares the probability of being assigned a criminal history score that places the juvenile exactly at the cutoff (for instance, 2.0 conditional on a class B current offense) with the probability of being assigned a criminal history score that places the juvenile just to the left of a cutoff (1.75 conditional on a class B current offense). As seen in Table 3, the probabilities of receiving a score of 2.0 and 1.75 conditional on having a class B current offense do not significantly differ. The same holds true when comparing the conditional probabilities of receiving scores exactly at and just below the cutoffs in the class C+ and C rows.

Anecdotal evidence also minimizes the possibility that such behavior on the part of the judges exists. First, adjudication history scores (that is, the scores on the *x*-axis of the grid) are generally not calculated by the judge. Second, all parties, including the defense attorney, are typically provided the criminal history scores and the opportunity to review them. The party responsible for calculating the criminal history scores varies somewhat across counties. For instance, in Spokane County, the Court Investigation Unit is responsible for calculating and confirming offender history scores. This office then forwards the scores to both the prosecutor and public defender and makes dispositional recommendations

²² Of course, additional unobservables may play a role in the prosecutor's decision regarding charges, such as aggravating circumstances, degree of remorse, and school record. However, if these variables vary discontinuously at the cutoff, their omission would likely result in underestimating the deterrence effect. For instance, the prosecutor may be inclined to give an individual who regularly attends school a break, as opposed to a dropout. Such discretion would imply that the control group contains "better" youths who are also less likely to recidivate, while the treatment group contains "bad" individuals. Thus, such discretion could give the appearance that incarceration exacerbates crime.

²³ Prosecutorial discretion may also play a role in whether a juvenile pleads guilty. Consistent with national patterns, almost 90 percent of the sample pleads guilty. The results are robust to controlling for whether an individual pleads.

Table 3
Are Individuals Systematically Charged with Offenses That Place Them Exactly on a Cutoff?

Offense Class (Z)	Actual Adjudication Score		Pr(Act_adj_Sc ₁ Z)	Pr(Act_adj_Sc ₂ Z)	Pr(Act_adj_Sc ₁ Z) – Pr (Act_adj_Sc ₂ Z)	t-Statistic
	On a Cutoff (Act_adj_Sc ₁)	To the Left of a Cutoff (Act_adj_Sc ₂)				
B	2.0	1.75	.012	.010	.002	.61
C+	3.0	2.75	.007	.009	–.002	.33
C	4.0	3.75	.007	.008	–.002	.72

Note. An individual placed exactly at the cutoff receives a sentence of incarceration; an individual just to the left of each cutoff receives a local sanction. If the courts are gaming the system in this way, one would expect that $\Pr(\text{Act_adj_Sc}_1 | Z) > \Pr(\text{Act_adj_Sc}_2 | Z)$.

to the judge on the basis of the sentencing guidelines. Similarly, in Skagit County, the probation staff submit their opinion as to the appropriate adjudication history score. According to a Skagit County probation manager, all parties (the probation staff, prosecutor, and defense counsel) agree on this score 99 percent of the time.

Third, judges can utilize the manifest-injustice sentence to impose sentences outside the range prescribed by the grid. However, statistical reports indicate that manifest-injustice sentences are not used very regularly and vary little over time. In 2000, for instance, judges sentenced offenders within the grid's standards 97 percent of the time (Aos 2002). In 2005, just over 3 percent of the total dispositions were for a manifest injustice; about 75 percent of these were for a manifest injustice upward (Sentencing Guidelines Commission 2005). The use of a manifest injustice disposition by the courts must be documented and justified with clear and convincing evidence. In 2005, the most common reasons cited for imposing a manifest injustice upward included a recent criminal history, a failure to comply with a diversion agreement, and the fact that the standard range was too lenient considering priors. These reasons are not particularly consistent with the idea of judges selectively sentencing "amenable" juveniles to incarceration. Furthermore, it is important to recall that those individuals who receive a manifest-injustice sentence are excluded from the analysis.

3.4. *Juvenile Gaming*

The assumptions of the design would also be violated if individuals control the value of their score (Lee 2008). For juveniles to game the Washington grid, they must know when the grid is applied, the punishment in each cell, and how to classify offenses and calculate scores. As this analysis is based on a new grid, youths are likely to have minimal, if any, knowledge of it. Such information could be obtained through their peers, which may be especially relevant for gang members, or through local media sources. However, the relatively short time span of the analysis likely does not allow sufficient time for such interactions and, according to a Lexis-Nexis search, there was little mention of the reforms and no description of the new sentencing grid in four major Washington newspapers from January 1, 1997, to December 31, 1998.

Table 4 provides additional indirect evidence that juveniles are not gaming the system. It tests whether individuals immediately to the left of the horizontal cutoff have the same probability of committing an offense with a class that places the group 1 individuals in a cell immediately below the vertical cutoff. For instance, youths with a score of 2.0 are in the third column of the grid; a C+ offense would place them in a cell immediately below the cutoff. If they are gaming the system, then one would expect those with a score of 2.0 to have a higher probability of committing a C+ offense than those with a score of 1.75, for whom C+ is not immediately below the cutoff. But, as seen in Table 4, both

Table 4
Do Individuals Systematically Choose Their Current Offense So That It Is Right below the Cutoff?

Actual Score of Group 1 (Act_adj_Sc ₁)	Offense Class Right below Cutoff for Group 1 (Z)	Pr (Z Act_adj_Sc ₁)	Actual Score of Group 2 (Act_adj_Sc ₂)	Pr (Z Act_adj_Sc ₂)	Pr (Z Act_adj_Sc ₁) – Pr (Z Act_adj_Sc ₂)	t-Statistic
2.0	C+	.024	1.75	.024	.000	.00
3.0	C	.223	2.75	.266	–.042	.90
4.0	D+	.129	3.75	.104	.025	.53

Note. Group 1 is the sample of individuals with an actual score on the horizontal cutoff, while group 2 is the sample of individuals whose actual score is right below the horizontal cutoff. If individuals are gaming the system, one would expect that $\Pr(Z|\text{Act_adj_Sc}_1) > \Pr(Z|\text{Act_adj_Sc}_2)$.

groups are equally likely to be referred for a C+ offense. Similar patterns are observed in other columns of the grid.²⁴

4. Results

4.1. Main Results

Table 5 presents the results of estimating the equation for the sample for which treatment (incarceration) is perfectly assigned by the grid. Each specification includes a second-order polynomial of the actual adjudication history score and current offense class (row) dummies. All coefficients are exponentiated such that they have the interpretation of a hazard ratio; that is, values less than one imply a deterrent effect. Column 1 presents the results with no additional controls. The coefficient on *D_above_cutoff* indicates that incarcerated youths have a daily hazard rate of recidivating that is approximately 37 percent lower than that of nonincarcerated youths. In other words, there is strong evidence of a specific deterrence effect.

Recall that individuals become at risk of recidivating as of the disposition date plus the minimum sentence length. This raises the concern that individuals in the treatment and control groups become at risk of recidivating at different dates. If aging is related to a juvenile's propensity to recidivate, then the estimated treatment effect may in part be due to aging. Let us assume that individuals are less likely to recidivate as they get older.²⁵ How would this influence the interpretation of the estimated deterrence effect? Depending on one's assumptions about where an individual falls on his or her crime trajectory after incarceration, aging could in theory lead to either an under- or overstatement of the deterrence effect. If the clock stops while a youth is incarcerated, then the incarcerated youth is essentially set back 15 weeks compared with an individual in the control group. The control group would be at a less active stage of their criminal careers, and the deterrence effect would be understated. However, if the clock does not stop, then incarcerated youths will become at risk of recidivating at a time in their age-crime trajectory when they have a lower propensity to recidivate than

²⁴ Alternatively, individuals may choose their current offense with their future placement on the grid in mind. For individuals with a history score of 1.75, either a misdemeanor or felony today will result in a score in the third column if he or she recidivates. But an individual with a score of 1.5 will be in the third column next period only if he or she commits a felony this period; otherwise, he or she remains in the second column. Thus, if individuals game the system, then those with a score of 1.5 should have a higher probability of committing a misdemeanor than those with a score of 1.75, as the expected punishment associated with the third column is greater than that for the second. This is not the case, however, and similar patterns are seen for scores of 2.75 and 2.5 as well as 3.75 and 3.5. Results are available from the author.

²⁵ This is a reasonable assumption, as the estimated hazard ratio associated with age when controls are included in the regression is significantly less than one. Note that this may seem to contradict the fact that the age-crime curve is commonly accepted to be upward sloping in the juvenile years; however, it is the aggregate age-crime curve that is upward sloping. If individuals are less likely to be rearrested as they get older, then more youths must be entering the crime market as they get older.

Table 5
Main Estimation Results

	(1)	(2)	(3)	(4)
D_above_cutoff	.6337** (.0814)	.6185** (.0794)	.6167** (.0801)	.6395** (.0902)
Act_adj_score	1.4925** (.0392)	1.5383** (.0422)	1.2468** (.0520)	1.5118** (.1368)
Act_adj_score ²	.9558** (.0050)	.9512** (.0051)	.9721** (.0058)	.9646** (.0081)
Demographic controls	No	Yes	Yes	Yes
Past offense types	No	No	Yes	Yes
Current offense types	No	No	Yes	Yes
County dummies	No	No	Yes	Yes
Future rounded score controls	No	No	No	Yes

Note. Values are hazard ratios that result from estimating a Cox proportional hazard model with the covariates noted; a value greater than one indicates an exacerbation effect, and a value less than one indicates a deterrent effect. Standard errors are in parentheses. Each specification includes a set of row dummies, that is, a dummy variable for each current offense class. The start date, or date at which the individual becomes at risk of recidivating, is the disposition date plus the minimum sentence. $N = 18,616$.

** Significantly different from one at the 1% level.

the control group. Deterrence would then be overestimated. Given that theory does not clearly sign the impact of aging, I assess its importance empirically. As seen in column 2 of Table 5, controlling for demographic characteristics, including age, gender, and ethnicity, has minimal impact. This estimated deterrence effect is also not affected when controlling for age more flexibly with age dummies or an age polynomial.

Column 3 indicates that the estimated deterrence effect is completely robust to expanding the set of controls to include past and current offense types as well as county dummies. This provides further evidence that unobservables do not vary discontinuously at the cutoffs in such a way that would bias the treatment effect.

An additional concern is that the youth is deterred by the prospect of being in a higher column of the grid upon recidivating rather than by the current incarceration sentence. For instance, two individuals who commit the same offense today and who are on either side of a cutoff today will also be in different columns tomorrow. I therefore expand the equation to include dummy variables that indicate a youth's rounded adjudication score in the next period. According to the grid, the expected probability and severity of incarceration increase as the rounded score increases. The results of this specification are presented in column 4 of Table 5. Despite the fact that individuals with higher future scores (that is, they are in higher columns of the grid if they recidivate) are significantly less likely to recidivate, there is little impact on the estimated treatment effect.²⁶

Thus, the results presented in Table 5 indicate that incarcerated individuals

²⁶ The results presented in Table 5 are also robust to a variety of alternative specifications and functional forms, including the inclusion of third- and fourth-order polynomials of the actual score and the assumption that the underlying hazard function has either a Weibull or exponential distribution.

have a daily hazard rate of recidivating that is approximately 37 percent lower. This implies that incarcerated individuals are 11 percent less likely to recidivate after 1 year and approximately 13 percent less likely to recidivate after 1.5 years. These estimates are roughly consistent with the 8-percentage-point difference in recidivism behavior observed in the raw data in Table 1.

Although Table 5 provides strong evidence that placement in juvenile residential facilities decreases the postrelease criminal activity of the incarcerated youth, it is important to keep in mind that theory is ambiguous regarding the direction of this relationship. The existence of effective treatment programs as well as Becker's (1968) economic model of crime imply a deterrence effect, while labeling theory (Lemert 1967; Schwartz and Skolnick 1962), peer effects (Taylor 1996; Bayer, Hjalmarsson, and Pozen 2009), and poor prison conditions (Chen and Shapiro 2007; Katz, Levitt, and Shustorovich 2003) may result in an exacerbation of criminal behavior. The evidence of deterrence presented in Table 5 thus indicates that, on average, the former mechanisms dominate the latter. This does not imply that those mechanisms that exacerbate criminality are nonexistent—simply that their effect is small relative to those that can yield deterrence. It may even be the case that these exacerbating forces dominate for certain subpopulations. It is also important to keep in mind that this analysis is based on a single state that has been a leader in juvenile justice reform. It is certainly feasible that incarceration has an exacerbating effect in states other than Washington, which have, for instance, worse prison conditions or educational programs.

4.2. *Heterogeneity*

Table 6 explores whether the treatment effect is heterogeneous across demographic and criminal history characteristics by estimating the equation for different subsamples of the data; additional controls, X , are not included. Each row presents the hazard ratio associated with D_above_cutoff . The results for the full sample, or the baseline results, were displayed in column 1 of Table 5. This effect is driven by males, which is likely due to the fact that more than 85 percent of the females have a rounded score of zero and that individuals in the first column of the grid are not used in identifying the treatment effect.

While the estimated effect is fairly homogeneous across ethnicity, the table indicates that the deterrent effect is larger for youths who are 14 or younger than for those who are 15–16 or 17–18. One potential explanation is that the youngest individuals update their beliefs about future punishment the most. However, it is important to point out that individuals of all age groups, including those older than 16, are deterred. Results for the criminal history characteristics indicate that the deterrence effect is observed for both those who have and those who have not been previously incarcerated. Finally, the effect is fairly homogeneous across individuals with various past offense types, including assault, firearms, sex offenses, burglary, theft, and drug offenses.

Table 6
Heterogeneity in the Treatment Effect

Sample Restrictions	Hazard Ratio	N
Full sample	.6337** (.0814)	18,616
Male	.6319** (.0837)	13,999
Female	1.3663 (.7638)	4,613
Black	.4873* (.1717)	2,091
Hispanic	.5653 ⁺ (.1859)	1,628
Native American	.3487 (.3647)	689
Age at offense ≤ 14	.2164* (.1574)	3,415
Age at offense > 14 and ≤ 16	.6418* (.1151)	7,387
Age at offense > 16	.6466* (.1264)	7,814
No previous sentences to DJR	.6267* (.1185)	17,965
At least one previous sentence to DJR	.6538 ⁺ (.1662)	651
Any past assault referrals	.5448** (.1117)	3,125
Any past firearms referrals	.5637 ⁺ (.1965)	569
Any past sex offense referrals	.8415 (.7221)	221
Any past burglary referrals	.6576* (.1102)	2,285
Any past theft referrals	.6147** (.0869)	6,141
Any past drug referrals	.6421 ⁺ (.1615)	2,244

Note. The hazard ratio is that associated with *D_above_cutoff*, which is equivalent to being sentenced to a residential placement facility. Each specification includes second-order polynomials of the actual score as well as current offense class (row) dummies. A hazard ratio greater than one implies an exacerbation effect and that a hazard ratio less than one implies a deterrence effect. Standard errors are in parentheses. DJR = Department of Juvenile Rehabilitation.

⁺ Significantly different from one at the 10% level.

* Significantly different from one at the 5% level.

** Significantly different from one at the 1% level.

The results presented in Table 6 therefore indicate that a deterrence effect is not isolated to young, less experienced, nonviolent offenders. This is an important finding, given that advocates of juvenile transfer laws argue that the juvenile courts are especially ineffective and inappropriate for certain older, criminally experienced, violent youths.

5. Conclusion

This paper analyzes the impact of juvenile incarceration on recidivism behavior by taking advantage of the institutional features of Washington State's juvenile justice system. In particular, it bypasses the analytical weaknesses of previous evaluations by focusing on the pseudorandom variation in sentencing that exists around the cutoffs in Washington's juvenile sentencing grid. There are two key findings. First, youths sentenced to state incarceration for 15–36 weeks have a daily hazard rate of recidivating that is approximately 37 percent lower than that of youths sentenced to a local sanction. This contradicts assertions that juvenile courts are ineffective at combating crime made by advocates of the “get tough” attitude toward juvenile crime. Second, this deterrent effect is observed for a wide range of youths, including those who are older, criminally experienced, and charged with a violent offense. It is exactly this group of individuals that is

most affected in many states by legislative changes that make it easier to transfer juveniles to the criminal courts.

The finding that juvenile incarceration reduces postrelease criminal behavior is not sufficient to make public policy recommendations. Rather, it is important to assess how the social benefits of juvenile incarceration compare with the social costs. According to Aos et al. (2001), the operating and capital costs of incarcerating a juvenile for 1 year in a state facility in Washington in 2000 were \$30,300 and \$5,690, respectively. Thus, a 15-week sentence (the focus of this analysis) costs just over \$10,000. Unfortunately, other costs to society, such as the social cost to the incarcerated juvenile's family or the productivity loss to society due to an incarcerated juvenile's reduced education (Hjalmarsson 2008) and employment opportunities (Freeman 1992), are much harder to quantify.

Similarly, there are numerous potential social benefits of juvenile incarceration. The empirical results presented in this analysis, however, can contribute directly only to our knowledge of the social benefits of reducing an individual's post-release criminal behavior. I find that incarcerated juveniles are, on average, 13 percent less likely to recidivate within a year and a half. It is not trivial, however, to translate this finding into monetary terms. First, this paper identifies only the short-term effects of juvenile incarceration on the recidivism behavior of the incarcerated youth; it cannot make any statements regarding whether this deterrence effect will still be observed 5 or 10 years postrelease. Second, social costs vary greatly across crime type, and making a statement about the crimes averted for the average incarcerated juvenile could therefore be misleading. With these caveats in mind, estimates of the social costs of crime by Miller, Cohen, and Wiersema (1996) imply that the expected benefit of preventing one offense is approximately equal to \$20,000.²⁷ Therefore, on the basis of the 13-percentage-point reduction in recidivism and the assumption that the recidivism offense consists of just one offense, the expected benefit of deterring one offender in the short term is \$2,600. By itself, this benefit is only about one-fourth of the fiscal cost of juvenile incarceration for 15 weeks. But one must keep in mind that this estimate does not take into account numerous other social benefits of juvenile incarceration, including potential long-term effects, incapacitation, and general deterrence, which this paper does not measure. Nor does it account for the heterogeneity across juvenile offenders. Perhaps more telling is the Cohen (1998) estimate that the typical career criminal causes \$1.3–\$1.5 million in external costs. If just a few juveniles are prevented from becoming lifetime criminals, then the social benefits could easily outweigh the fiscal costs of juvenile incarceration. Clearly, further research regarding the long-term impacts of juvenile incarceration on postrelease criminal behavior is needed to fully answer this question.

²⁷ This estimate assumes that the distribution of recidivism offense types is the same as the distribution of current offenses for the entire sample.

Appendix A

Table A1
Variable Definitions

	Definition
D_above_cutoff	Indicates whether the individual is in an unshaded cell of the sentencing grid, that is, above the cutoff
DJR	Equals one if the individual is sentenced to the Department of Juvenile Rehabilitation (a sentence of incarceration for at least 15 weeks to a state juvenile detention facility); DJR and D_above_cutoff are equivalent when the sample of misassigned individuals is dropped
Act_adj_score	Actual prior adjudication score
Rnd_adj_score	Rounded prior adjudication score; the actual score is rounded down to the nearest whole number
Class	Vector of current offense class or row dummy variables for the 10 offense classes
Male	Equals one if the youth is male
Black	Equals one if the youth is black
Hispanic	Equals one if the youth is Hispanic
Native	Equals one if the youth is Native American
Age_off	Age at the time of the offense
Arson	Equals one if the current offense is "arson and malicious mischief"
Assault	Equals one if the current offense is "assault other crimes involving physical harm"
Burglary	Equals one if the current offense is "burglary and trespass"
Drugs	Equals one if the current offense is "drugs"
Firearm	Equals one if the current offense is "firearms and weapons"
Homicide	Equals one if the current offense is "homicide"
Motor	Equals one if the current offense is "motor-vehicle-related crimes" (not including auto theft)
Kidnap	Equals one if the guidelines categorize the individual's current offense is "kidnapping"
Obstruction	Equals one if the current offense is "obstructing governmental operation"
Public	Equals one if the current offense is "public disturbance"
Theft	Equals one if the current offense is "theft, robbery, extortion, and forgery"
Sex	Equals one if the current offense is "sex crimes"
Other	Equals one if the current offense is "other"
Pany_arson	Equals one if the guidelines categorize any of the individual's past offenses as "arson and malicious mischief"
Pany_ass	Equals one if past offenses include "assault other crimes involving physical harm"
Pany_burg	Equals one if past offenses include "burglary and trespass"
Pany_drug	Equals one if past offenses include "drugs"
Pany_frarm	Equals one if past offenses include "firearms and weapons"
Pany_hom	Equals one if past offenses include "homicide"
Pany_motor	Equals one if past offenses include "motor-vehicle-related crimes" (not including auto theft)
Pany_kid	Equals one if past offenses include "kidnapping"
Pany_obstr	Equals one if past offenses include "obstructing governmental operation"
Pany_public	Equals one if past offenses include "public disturbance"

Table A1 (Continued)

	Definition
Pany_theft	Equals one if past offenses include “theft, robbery, extortion, and forgery”
Pany_sex	Equals one if past offenses include “sex crimes”
Pany_other	Equals one if past offenses include “other”
SSODA	Equals one if disposed under the Special Sex Offender Disposition Alternative
Manifest up	Equals one if an upward manifest injustice was received
Manifest down	Equals one if a downward manifest injustice was received
Drug treatment	Equals one if disposition included drug treatment
DJR_local	Equals one if sentenced to state incarceration (at least 15 weeks) but served in a local facility
Plead	Equals one if the individual pled guilty
Decline eligible ^a	Equals one if eligible for a declination hearing (transfer to criminal court)
3ref_past	Equals one if the individual has any past cases that included three referral reasons
Any past DJR?	Equals one if previously sentenced to state incarceration
Number invalid for history past referrals	Number of previous referral reasons that are invalid for criminal history
Age at first referral	Age first referred to the court system, regardless of whether the referral was valid or invalid for criminal history

Note. Offense types are from the Washington State Sentencing Guidelines Commission’s *Juvenile Disposition Sentencing Standards*.

^aDetermined by consulting the Revised Code of Washington, sec. 13.40.110.

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