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Title

Imprisonment and Labor Market Outcomes: Evidence from a Natural Experiment

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Abstract: Due to 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. We 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 post-release effects of imprisonment on employment, employment stability, and employment outside the secondary labor market are concentrated among whites with a pre-sentence work history. Post-release effects of imprisonment on employment among those with no work history are positive but fade over time.

Imprisonment and Labor Market Outcomes: Evidence from a Natural Experiment

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, the public at large – in the
consequences of incarceration for the individuals and families who experience it (Alexander
2010; Kilgore 2015; National Research Council 2014; Turney and Wildeman 2015; Wakefield
and Wildeman 2013; Western 2006), for social stratification (Wakefield and Uggen 2010) and
the functioning of local and national institutions (Clear 2007; Manza and Uggen 2004; 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 (Bushway, Stoll and Weiman 2007; Western 2006; Pettit
and Western 2004).

Despite the volume of research on imprisonment and labor market outcomes, a recent
National Research Council (NRC) report (2014) concludes that although "the bulk of the
evidence supports the conclusion that incarceration is associated with poor employment
outcomes" (p. 258), "(c)urrent 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 paper 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 state of Michigan in the years 2003-2006 (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 which 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 (Weiman, Stoll and Bushway 2007; Western 2002).

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 post-release 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. They also suggests 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). Compared to other nations and compared to earlier periods in US 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, 2009, 2011; Rich et al. 2011, Schnittker and John 2007; Steadman et al. 2009; 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 2014, 2015, Turney and Haskins 2014, Turney and Wildeman 2013, Haskins 2014, 2015), but a central focus has been on the consequences of incarceration for inequality due to effects of serving time in prison on employment outcomes (Pettit 2012; Pettit and Western 2004; Pettit and Lyons 2007; Sabol and Lynch 2003; Western 2006).

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 30 years, means that incarceration policy may play an important role in the creation and maintenance of racial inequality. One in nine African-American men age 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 et al 2005; Weiman et al. 2007). Alexander (2010) and Wacquant (2001) 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

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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 National Research Council's Committee on Causes and Consequences of High Rates of Incarceration (National Research Council 2014) identified four recent studies of the effects of imprisonment on employment and earnings using survey data (Apel and Sweeten 2010; Freeman 1992; Raphael 2007; Western 2006) and seven that relied on administrative records (Grogger 1995; Kling 2006; Lalonde and Cho 2008; Loeffler 2013; Pettit and Lyons 2007; Sabol 2007; Waldfogel 1994). All of the survey-based studies they reviewed found that incarceration was negatively associated with employment – with employment declining by 10 to 20 percent 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 five percentage points after incarceration. Moreover, administrative data studies also typically find a post-imprisonment increase in employment or earnings (e.g. Tyler and Kling 2007, Pettit and Lyons 2007, Loeffler 2013), evidence which 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 that have 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).

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 et al 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 imprisonment *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 datasets 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 (National Research Council 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, Loeffler 2013, Green and Winik 2010) and others finding negative effects (Mueller-Smith 2015).[1]

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 towards alternatives like probation would improve labor market outcomes or reduce racial inequalities.

Incapacitation Effects

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[1] 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 non-custodial sentence. As a result, Mueller-Smith's results are not directly comparable to those presented here.

The most direct way that incarceration affects employment is by incapacitating people and thereby removing them from the conventional labor market. 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.[2] 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 pre-sentence 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 post-release parole supervision (Harding et al 2017). Those under parole supervision are at risk of re-incarceration 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

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[2] 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.

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 et al 2009; Nieuwbeerta et al. 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 mobility (Bushway, Stoll, and Weiman 2007, Western 2006). 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

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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 due to 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 pre-prison 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).[3] 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).[4] 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.[5] 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, Western, and Bonkowski

2009, Uggen et al 2014).[6] A critical aspect of informal stigma is the use of criminal background

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[3] 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, though only if the crime is directly related to the job responsibilities (Petersilia 2003, see also Holzer et al 2002). One question is whether employers are actually aware of these legal issues. Pager's (2007) employer surveys suggest most are not.

[4] According to the American Bar Association, there are over 30,000 state laws, provisions, and exclusions from employment related to criminal records across the US (National Research Council 2014). Another estimate suggests that over 800 occupations are closed to those with a criminal record somewhere in the US (Bushway and Sweeten 2007). In Michigan specifically, there are almost 600 such laws and regulations related to employment, occupational and professional license, and business licenses according to the American Bar Association's National Clearinghouse of the Collateral Consequences of Conviction (2016).

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

[6] 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.

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 et al 2004, 2006, 2007), despite questions about the accuracy of these records, particularly those obtained through private companies (Bushway et al 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.[7]

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[7] 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'Alessio, Stolzenberg, and Flexon 2015), but the question of 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

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 (though 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 (Ispa-Landa 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 (due to resume gaps). On the other hand, imprisonment may have little additional impact, given that many people who suffer an incarceration spell for the first time have at least one conviction prior to 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 post-release 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 social demanding work environment. For instance, former prisoners may be more sensitive to

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.

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 in finding jobs, housing, transportation, and reuniting with family

and friends (Massoglia 2008a,b). Second, prisoners could face increased exposure to some

infectious diseases (e.g., hepatitis C, influenza, tuberculosis, and HIV/AIDS) due to congregate

living environments, poor infection control, and limited access to preventive interventions, and

be less resistant to such diseases when under chronically high stress (Johnson and Raphael 2009;

Massoglia 2008a; National Commission on Correctional Health Care 2002). Third, incarceration

in prison may disrupt ongoing medical treatments for chronic conditions, leading to disruption in

continuity of care at both prison entry and prison release. If these associations reflect causal

effects if 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 (Visher et. al. 2004, Braman 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 prisoners 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, Dobson, Wyse, and Morenoff 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 prior to 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 (MacKenzie 2006, Petersilia 2003, Visher et al 2005). 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 (National Research Council 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. Due to 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. See the online appendix for discussion of assumptions in the instrumental variables design.

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Data

We collected, cleaned, and coded data on all individuals sentenced for felonies in

Michigan between 2003 and 2006 based on administrative databases at the Michigan Department

of Corrections (MDOC). A primary source of data is the pre-sentence investigation reports

prepared for judges before sentencing, which provide our pre-sentencing 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.[8] Crimes for which our subjects were initially sentenced are described in

Appendix Table A6.[9]

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). 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 re-sentenced for a technical probation violation, individuals sentenced by

judges who heard less than 100 cases, and individuals in counties with only one judge. This leads

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[8] Missing covariate data are imputed using a hotdeck procedure based on race and gender. The only variable with substantial missing data is education (14 percent of the sample). Race is missing for 0.2 percent of the sample, and marital status is missing for 0.3% of the sample.
[9] This table shows that once we stratify by pre-sentence 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.

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, sentences included in the models but not reported in this paper). 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 post-sentence 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 Unemployment Insurance Agency and Workforce Development Agency for matching. In some cases, more than one SSN and name were available for each subject, due to 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 percent 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 (National Research Council 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 percent 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 concern 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 (Cook et al. 2015, Sabol 2007, Pettit and Lyons 2007, Kling and Tyler 2007), with rates as low as 30% reporting any UI income in the year after release. These rates are, if anything, lower prior to the incarceration spell (Tyler and Kling 2007; Pettit and Lyons 2007; Ramakers et al. 2014; Sabol 2007). These studies have been questioned because of their relatively low match rates overall, typically around 60 percent (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.[10]

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 multi-year period is enough to create a match. In this study, we were able to match 86.5 percent 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.[11]

We also argue that the strengths of administrative date 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.

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: 1 year (or the 4th full calendar quarter), 3 years (or the 12th full calendar quarter), and 5 years (or the 20th full calendar

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[10] One possible solution is to exclude people without matches from the analysis. Unfortunately, this would exclude an unknown but likely sizeable 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.

[11] Our own calculations of data from the American Community Survey for 2003-2012 provided by IPUMS (Ruggles et al 2015) find that only 1.9% of currently working residents of Michigan work outside the state.

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 recorded as a 1 on this variable at each time point if he or she experienced that event at any time prior to 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 4th, 12th, or 20th quarter since the start of the risk period (hereafter, focal quarters). To measure 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 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 based on two-digit NAICS industry codes.[12] Employers classified as 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

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[12] 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.

2003 and the second quarter of 2012.[13] Based on this classification, we constructed measures of whether or not the individual was employed in an industry *not* associated with the secondary labor market in each of the focal quarters. Appendix Table A1 (see online appendix) shows descriptive statistics on covariates by race, work history, and sentence type. The appendix also describes the covariates included in all models. Appendix Table A2 shows descriptive statistics on all outcomes over time since sentence or since release and by race and pre-sentence work history.

Modeling Strategy

To implement our instrumental variables estimator of the effect of incarceration in prison vs. probation, we use two stage least squares estimation (2SLS).[14] Our instruments are a set of dummy variables for the assigned judge plus interactions between judge dummies and pre-sentence characteristics. Although our primary "treatment" in this paper 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 besides 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 based on 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 other aspects of sentencing 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 (discussed

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[13] Only 0.7% of employed person-quarters 2003-2012 had employers without valid NAICS codes.
[14] Models were estimated using the ivregress routine in Stata version 14.

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more broadly in the online appendix). Although we include individuals with jail and probation with jail sentences in the analysis to avoid introducing sample selection bias, we focus on prison vs. probation because it provides a comparison between imprisonment and no imprisonment.[15]

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.[16] 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 pre-sentence characteristics in Table A1, resulting in six "first-stage" equations. All first-and second-stage models also condition on the main effects of the pre-sentence 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 outcomes. [17] 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

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[15] 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 whom we have 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.

[16] 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.

[17] 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 et al. 2010, Bellair and Kowalski 2011, Nguyen et al. 2014). Thus it is important to remove variation in employment outcomes across counties.

Least Squares (OLS) regression, as is conventional in the instrumental variables literature.[18]

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) due to incapacitation in prison. One effect of both theoretical and policy interest is the total effect of imprisonment, which combines the effect of serving time in prison vs. 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 sentenced in the same year will start their risk periods in different years, and as a result,

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[18] 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 homoskedasticity 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 calculate 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).

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 (due to the passage of time). Third, release dates

are endogenous because they are determined in part by post-sentencing 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

post-release outcomes to measure, potentially introducing some sample selection bias into the

estimates.[19]

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.[20] 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 vs. probation vary across

individuals (termed "heterogeneous treatment effects" in the literature). When treatment effects

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[19] Overall, 21.7 percent of prisoners were not released in time to measure employment in the 4th quarter following release, 34.1 percent of prisoners were not released in time to measure employment in the 12th quarter following release, and 56.7 percent of prisoners were not released in time to measure employment in the 20th quarter following release.

[20] 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.

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, based on 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 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.[21]

Results

Employment Pre- and Post-Sentencing

We begin by describing the pre-sentence and post-sentence 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

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[21] 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).

blacks are represented by black lines and those for whites are represented by grey lines. The x-axis measures time (in calendar quarters) since sentencing, with negative values representing the pre-sentence period.

We highlight five main empirical observations from Figure 1. One is that rates of formal employment before sentencing are low for all groups.[22] None of the groups reached much over 50 percent employment in the formal labor market, and for the worst-off group, blacks sentenced to prison, the employment rates peaked at only about 28 percent prior to sentencing.[23] 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 least in part, pre-trial detention, which is more common for those who will be sentenced to prison (see Appendix Table A7).[24] Fourth, the graph reveals important differences in the pre-sentence employment trajectories of those sentenced to prison and probation. The employment trajectories of blacks and whites sentenced to prison decline sharply prior to sentencing, while those of probationers experience a more gradual decline in employment prior to sentencing.[25] A final

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[22] 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 or not in the way that official unemployment statistics do.

[23] 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 Figure 1, and even for them employment rates peak around only 45 percent (see Appendix Figures A2 and A3).

[24] All models presented below condition on pre-trial detention to ensure effects of imprisonment are not due to pre-trial detention.

[25] The divergence in pre-sentence employment rates between eventual prisoners and probationers also inform 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 prior to treatment (Vaisey and Miles 2017). Moreover, these violations would lead to biased conclusions about the negative impact of prison on employment.

observation concerns the different post-sentencing trajectories of those sentenced to prison and probation. Whites and blacks who were sentenced to prison experienced very low rates of employment (below 20 percent for white prisoners and below 11 percent for black prisoners at all post-sentencing 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.[26] 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 pre-sentencing trajectories are identical in Figures 1 and 2, and the post-sentencing 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 pre-sentence employment rates. Similar trends have been observed in prior research using UI data to track formal employment among former prisoners in other states (Tyler and Kling 2007, Pettit and Lyons 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

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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 Appendix Figure A2).

[26] The median length of prison sentences for whites and blacks was two years (eight quarters). By the end of the 12th quarter, a little less than half of those sentenced to prison (45.5 percent of whites and 49 percent of blacks) remained in prison, while by the 20th quarter, only 27.4 percent of whites 32.5 percent of blacks who were sentenced to prison remained in prison.

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, post-sentence 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 (3 years) before sentencing. Lack of work history was more common among blacks (37 percent) than whites (24 percent) and more common among those sentenced to prison (27 percent) than probation (22 percent; see Appendix Table A1).[27] Further descriptive analysis revealed that the employment rate gap between the probation and prison groups was wider among those with a pre-sentence work history and very small for those with no work history (see Appendix Table A2). This suggests 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 vs. 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 12th quarter following sentence and following release.

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[27] Individuals who are younger at sentencing are of course much less likely to have a pre-sentence work history. Models estimated excluding individuals age 25 or younger at sentencing produce similar estimates to those presented below.

(The post-sentence vs. post-release distinction is the same as between Figures 1 and 2 above.)

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 post-sentence 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 post-

release 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 due to pre-sentence 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 12th

quarter, as more and more prisoners are released and able to find work in the formal labor

market. The post-release 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 4th and 12th 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 row four reduces the number of instrumental variables by dropping the interactions between the judge dummy variables and individual characteristics, while the one in row five uses only a single instrumental variable constructed as a latent measure of judge harshness.[28] Results from these two specifications lead to the same 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 appendix), we use our preferred specification from here forward (from Table 1 it is also 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

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[28] 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.

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 six percentage points when the time period is three years post-sentence and by almost 20 percentage points when the time period is three years post-release. 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 post-release. 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. [29] The results show that the effect of prison on employment varies by both race and work history.

We first consider the post-sentence 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 following sentence, the probability of employment was estimated to be 30.4 percentage points lower for the marginal white person sentenced to prison compared

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[29] Appendix Table A5 shows analogous estimates based on ordinary least squares regression (OLS), as well as analogous OLS estimates for the outcomes discussed in the next section.

to probation, while the corresponding effect for blacks was roughly half as large (16.3 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 prior to 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 4th quarter after sentence, at which point blacks with work history who were sentenced to prison were 28.5 percentage points less likely to be employed than those sentenced to probation, while the comparable effect among whites with work history was 41.4 percentage points.[30] The effect of being incapacitated in prison is much smaller among those without work history, and it is only statistically significant in the 4th 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 post-release, which removes the incapacitation effect of incarceration in prison for the initial prison sentence by comparing prisoners post-release to probationers post-sentence.[31] The stratification of the analysis by pre-sentence work history is essential to making sense of the effect of imprisonment on post-release 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

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[30] Note that this is the marginal effect, which is different from the average effect. The mean employment rate among black probationers in the 4th quarter following sentencing is 30 percent, and the comparable figure for white probationers is 45 percent.

[31] Note that the sample sizes are slightly smaller in the post-release models. This is because prisoners who have not yet been released are not included in these models. See the methods section above for discussion of implications.

effect sizes of positive 6 percentage points at the 4th quarter and 4 percentage points at the 12th quarter) and no effect among whites. However, these effects mask significant heterogeneity by work history. Among those with no pre-sentence 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 (4th quarter) after release, when the probability of employment was 9.2 percentage points higher for prisoners compared to probationers among blacks, and 10.1 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 20th quarter for both blacks and whites (see Table 3 below, which also includes outcomes at the 20th 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 (10 percentage points in the 4th quarter and 7 percentage points in the 12th quarter).

Re-imprisonment is one likely contributor to such negative post-release effects. Table 2 also shows results from 2SLS regressions for each sub-group 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 the release date. In the post-sentence 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 post-release models (which eliminate incapacitation effects), the effects of being sentenced to prison on future

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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 18.9 percentage points compared to probationers with no work experience among blacks and 19.9 percentage points among whites. Among those with pre-sentence work experience, the effect of imprisonment on subsequent entry into prison is also high, 12.9 percentage points among blacks and 17.2 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 post-release outcomes since the hypothesis we are testing is not about the effects of incapacitation on employment but rather 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 post-release 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.[32] The top panel shows the same estimates 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 4th quarter (4.5 percentage points) or 12th quarter (2.7 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 4th quarter after release), the proportion of quarters employed (significant at all time periods), and employment outside the secondary labor market (significant only at the 4th 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 20th 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

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[32] Descriptive statistics on these outcomes are provided in Appendix Table A2. We have also explored additional measures of labor market outcomes, including stability of employment with the same employer, earnings, earnings above the poverty line, and stable earnings above the poverty line, with similar results.

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 20th 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 post-release labor market outcomes that fade over time. Among whites with a work history, however, the effects of imprisonment on post-release 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, 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 National Research Council 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 experiences" (p. 256).   In this paper, 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 either by sample sizes too small to detect effects of reasonable size, by inattention to race and pre-sentence 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 due to 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 24.4 percentage points in the fourth quarter after sentencing, when about 87 percent 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 4.4 percentage points in the 12th quarter after sentencing/release to 14.1 percentage points in the 20th 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 19.6 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 pre-sentence 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 28.5 percentage points among blacks with a pre-sentence work history and 41.4 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 post-release employment outcomes, we only found significant negative effects of prison among whites with pre-sentence 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 et al. 2017).

Among whites and blacks with no pre-sentence 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 pre-prison 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 post-prison 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 pre-sentence 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

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hypothesis in one subgroup, whites with a pre-sentence 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 pre-sentence 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 non-secondary 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 effect 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 percent black, and he is unable to stratify by work history. Kling (2006) finds no effects on employment in a Florida sample that is 54 percent black but of whom only 31 percent 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 percent 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 impact families (e.g. Braman 2004, Turney and Wildeman 2014) 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 and/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 due to 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 vs. 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 "local average treatment effects." 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 paper 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 inequalities. 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 paper 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

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fading of these effects, however, is also consistent with prior research on the challenges of labor market success for former prisoners, due both to the difficulties of the current low-skill labor market and the human capital and social capital deficits of former prisoners (Bushway et al 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 2011).

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Figure Legends

Figure 1: Employment Relative to Sentence Date, by Sentence Type and Race

Figure 2: Employment Relative to Release Date, by Sentence Type and Race (Excludes Focal Prison Term)

% Employed plotted against *Quarter since Sentencing (0 = Sentencing Quarter)*

Legend:
- Prison, Black
- Prison, White
- Probation, Black
- Probation, White
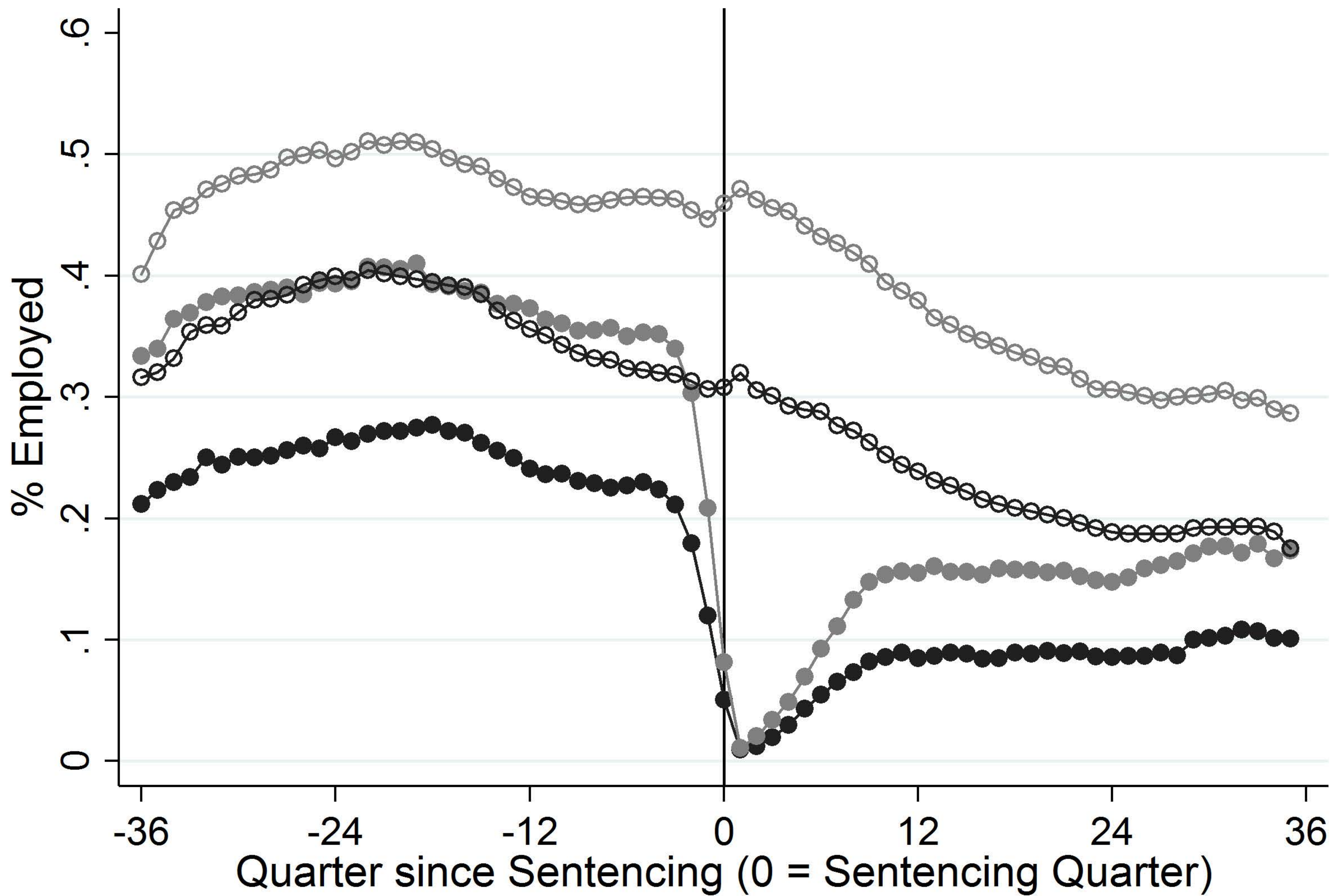
Table 1: 2SLS Estimates of Effect of 24 Month Prison Sentence vs. 24 Month Probation Sentence on Post-Sentence and Post-Release Labor Market Outcomes for the Entire Sample

	Any Employment in Quarter				Future Imprisonment	
	4th Quarter post sentence	12th Quarter post sentence	4th Quarter post release	12th Quarter post release	Within 3 years after sentence	Within 3 years after release
OLS Models						
OLS without controls	-0.343***	-0.182***	-0.080***	-0.083***	0.056***	0.212***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
OLS with controls	-0.241***	-0.089***	0.026***	0.018***	-0.013***	0.115***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Preferred Instrumental Variables Specification						
2SLS Models with Judge-Covariate Interactions	-0.244***	-0.089***	0.023	0.010	0.061***	0.196***
	(0.014)	(0.015)	(0.016)	(0.015)	(0.012)	(0.014)
Robustness Checks: Alternative Specifications of Instruments						
2SLS Models Excluding Judge-Covariate Interactions	-0.305***	-0.092*	-0.062	-0.067	0.222***	0.276***
	(0.038)	(0.038)	(0.037)	(0.037)	(0.033)	(0.033)
2SLS Models, Judge Harshness Score as IV	-0.269***	-0.129***	-0.051	-0.042	0.165***	0.288***
	(0.024)	(0.024)	(0.027)	(0.028)	(0.018)	(0.021)
Observations	109,636	109,636	105,346	102,890	109,636	105,435

Robust standard errors in parentheses; *** p<0.001, ** p<0.01, * p<0.05

Table 2: 2SLS Estimates of 24 Month Prison Sentence vs. 24 Month Probation Sentence on Employment and Imprisonment Post-Sentence and Post-Release, By Race and Work History

	Post-Sentence					
	Blacks			**Whites**		
	Overall	**No Work History**	**Work History**	**Overall**	**No Work History**	**Work History**
Employment 4th Quarter	-0.163***	-0.037**	-0.285***	-0.304***	-0.087***	-0.414***
	(0.016)	(0.014)	(0.022)	(0.018)	(0.017)	(0.022)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
Employment 12th Quarter	-0.031	-0.016	-0.085***	-0.121***	-0.034	-0.198***
	(0.017)	(0.014)	(0.022)	(0.019)	(0.018)	(0.024)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
Imprisonment 3 Years	-0.004	0.001	0.009	0.061***	0.048*	0.053**
	(0.016)	(0.021)	(0.017)	(0.015)	(0.020)	(0.016)
Observations	47,673	17,740	29,933	61,963	14,718	47,245
	Post-Release					
	Blacks			**Whites**		
	Overall	**No Work History**	**Work History**	**Overall**	**No Work History**	**Work History**
Employment 4th Quarter	0.060***	0.092***	-0.013	-0.022	0.101***	-0.105***
	(0.018)	(0.016)	(0.025)	(0.020)	(0.021)	(0.025)
Observations	45,254	16,600	28,654	60,092	14,136	45,956
Employment 12th Quarter	0.044*	0.071***	-0.028	0.001	0.045*	-0.073**
	(0.017)	(0.015)	(0.023)	(0.021)	(0.020)	(0.025)
Observations	44,136	16,140	27,996	58,754	13,780	44,974
Imprisonment 3 Years	0.141***	0.189***	0.129***	0.189***	0.199***	0.172***
	(0.018)	(0.025)	(0.021)	(0.017)	(0.025)	(0.019)
Observations	45,303	16,618	28,685	60,132	14,146	45,986

Work History = Any formal employment in 12 calendar quarters before sentence; Robust standard errors in parentheses; *** $p<0.001$, ** $p<0.01$, * $p<0.05$
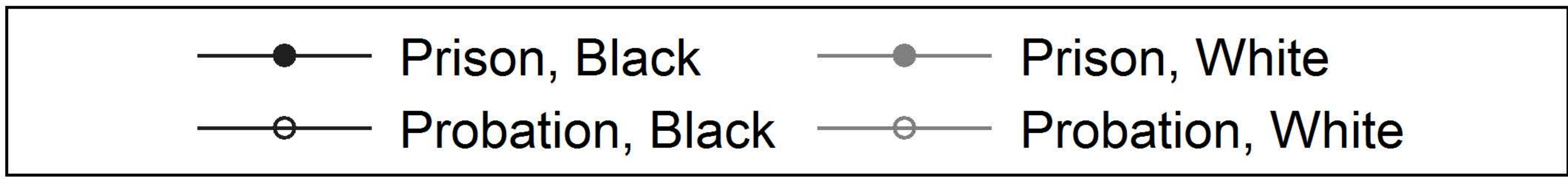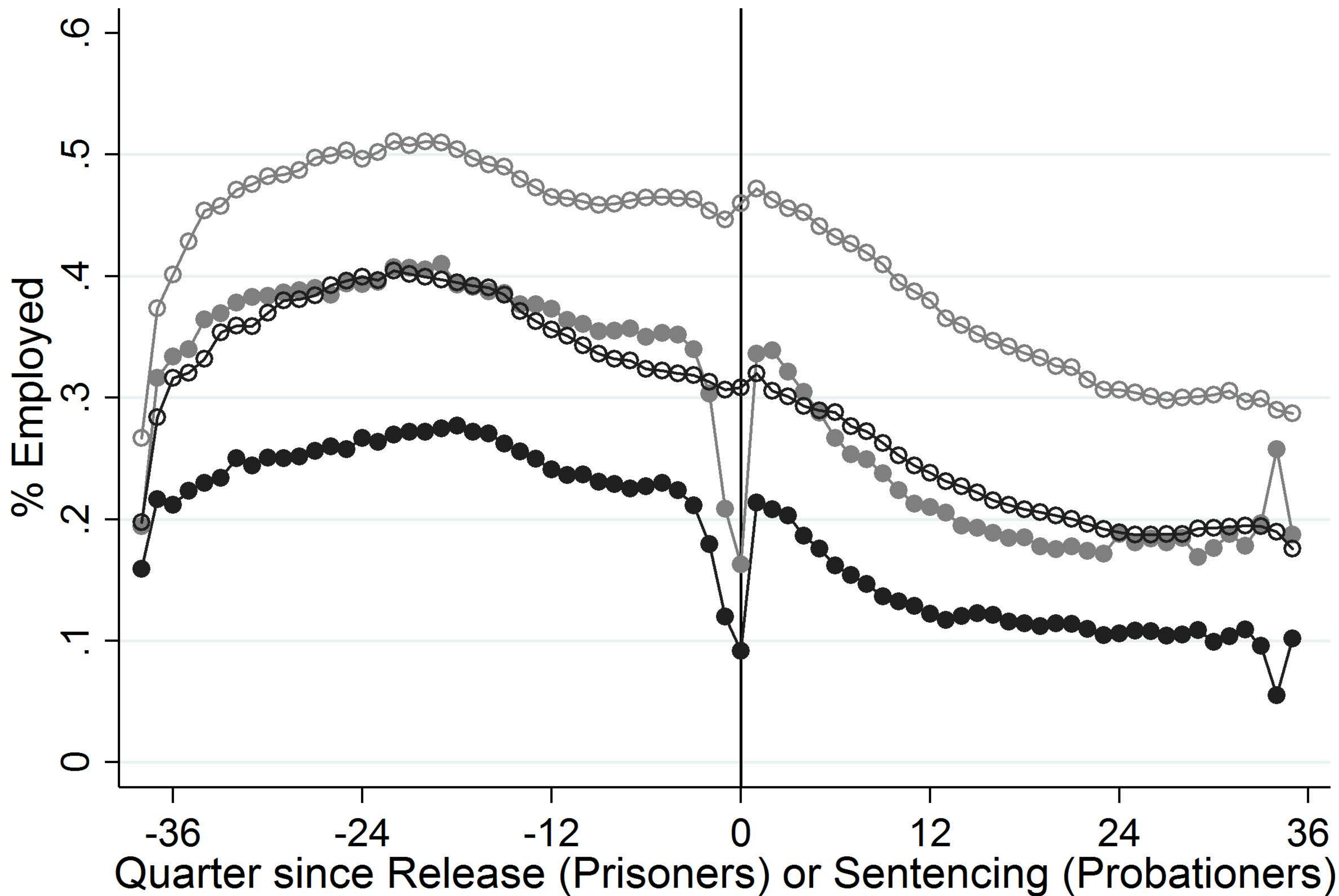
Table 3: 2SLS Estimates of Effect of 24 Month Prison Sentence vs. 24 Month Probation Sentence on Post-Release Labor Market Outcomes, by Race, Work History, and Time Since Release

	Entire Sample	Blacks			Whites		
		Overall	No Work History	Work History	Overall	No Work History	Work History
Any Employment							
4th Quarter	0.023	0.060***	0.092***	-0.013	-0.022	0.101***	-0.105***
	(0.016)	(0.018)	(0.016)	(0.025)	(0.020)	(0.021)	(0.025)
12th Quarter	0.010	0.044*	0.071***	-0.028	0.001	0.045*	-0.073**
	(0.015)	(0.017)	(0.015)	(0.023)	(0.021)	(0.020)	(0.025)
20th Quarter	-0.021	0.019	0.047*	-0.040	-0.050*	0.033	-0.094**
	(0.020)	(0.023)	(0.021)	(0.029)	(0.025)	(0.023)	(0.030)
Employed 3 Consecutive Quarters							
4th Quarter	0.014	0.043**	0.069***	-0.018	-0.008	0.080***	-0.084***
	(0.013)	(0.015)	(0.011)	(0.021)	(0.018)	(0.016)	(0.023)
12th Quarter	-0.002	0.022	0.044***	-0.018	-0.004	0.029	-0.065**
	(0.014)	(0.015)	(0.011)	(0.021)	(0.019)	(0.016)	(0.023)
20th Quarter	-0.034	0.007	0.034*	-0.032	-0.045*	0.009	-0.069*
	(0.018)	(0.019)	(0.016)	(0.020)	(0.022)	(0.018)	(0.027)
Proportion Quarters Employed							
4th Quarter	0.045***	0.070***	0.099***	-0.045	0.011	0.118***	-0.087***
	(0.012)	(0.014)	(0.013)	(0.025)	(0.016)	(0.017)	(0.020)
12th Quarter	0.027*	0.055***	0.079***	-0.009	0.001	0.087***	-0.087***
	(0.011)	(0.013)	(0.011)	(0.020)	(0.015)	(0.015)	(0.019)
20th Quarter	0.011	0.034*	0.067***	-0.017	-0.009	0.048**	-0.072**
	(0.014)	(0.016)	(0.014)	(0.018)	(0.017)	(0.017)	(0.022)
Employed Outside Secondary Labor Market							
4th Quarter	0.004	0.038**	0.042***	-0.029	-0.029	0.052**	-0.080***
	(0.013)	(0.014)	(0.011)	(0.022)	(0.018)	(0.016)	(0.022)
12th Quarter	0.004	0.017	0.037***	0.008	0.006	0.018	-0.042*
	(0.012)	(0.014)	(0.010)	(0.019)	(0.018)	(0.015)	(0.021)
20th Quarter	-0.008	0.010	0.045**	-0.027	-0.028	0.031	-0.072**
	(0.016)	(0.018)	(0.016)	(0.019)	(0.021)	(0.019)	(0.025)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245

Work History = Any formal employment in 12 calendar quarters before sentence; Robust standard errors in parentheses; *** $p<0.001$, ** $p<0.01$, * $p<0.05$

Online Appendix for

Imprisonment and Labor Market Outcomes: Evidence from a Natural Experiment

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Instrumental Variables Assumptions

  A valid instrument must meet two conditions (for a review, see Angrist and Pischke 2009, Ch. 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 (Figure A1a), in prison minimum sentence lengths (Figure A1b) and in probation sentence lengths (Figure 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 in Figure A1.

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 ordinary least squares 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 partialed out (Shea 1997). Appendix Table A3 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 and their interactions are used as instruments. The latter set of instruments is used in all analyses presented in this paper (see below). 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). F-statistics above 10 are typically considered ideal, but lower F-statistics 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 A3, which shows the relevant F-statistics. F-statistics 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 mis-estimation 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 re-estimated 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 vs. probation for all cases and conditioning on pre-sentence characteristics and county identifiers (Appendix 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).[1]

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[1] 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 R2≈0.001 in Tables 1 and 2 in the Bound et al vs. 0.066 to 0.163 in our paper for the prison vs. probation comparison in our Table A2). 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) that the instruments are as good as randomly assigned, and (2) that 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 a computerized case management system that assigns 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. Appendix Table A4 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 them 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 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 pre-trial detention (Dobbie et al 2017). If judges vary in their assignment of pre-trial detention and pre-trial 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 pre-trial detention are not initially determined by the circuit court judges we use on our analysis, although such judges can change bail and pre-trial detention decisions once they receive a case. Judge identifiers are only slightly correlated with pre-trial detention conditional on race and work history (see Appendix Table A4), and we condition on pre-trial 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 dataset, 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 counties in the U.S. in 2009 suggest that this could be a common occurrence (Reaves 2013). Within 1 year of a felony arrest, only 54% of the cases have been resolved through a felony conviction, and an additional 12% of been resolved through a misdemeanor conviction. Almost all cases are 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, for example), 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 dataset, 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 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 pre-sentencing individual characteristics and also treating those interactions as instruments.

Covariate Measures

Covariate measures are derived from the "Pre-sentencing investigation" conducted by 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 pre-sentencing investigation reports. Where we encountered discrepancies 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.

Pre-Sentence 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.

Pre-sentence Substance Use: Self or family reported history of substance abuse, including a set of non-mutually exclusive dummy variables indicating Ever Used Alcohol, Ever Used Marijuana, Ever Used Stimulants, Ever Used Opoids, 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 fel:.2003, 2004, 2005, or 2006.

Appendix References

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Figure A1a

Figure A1b

Figure A1c

Judge Fixed Effects from IV First Stage
Probation Length

Note: Each bar represents one judge.
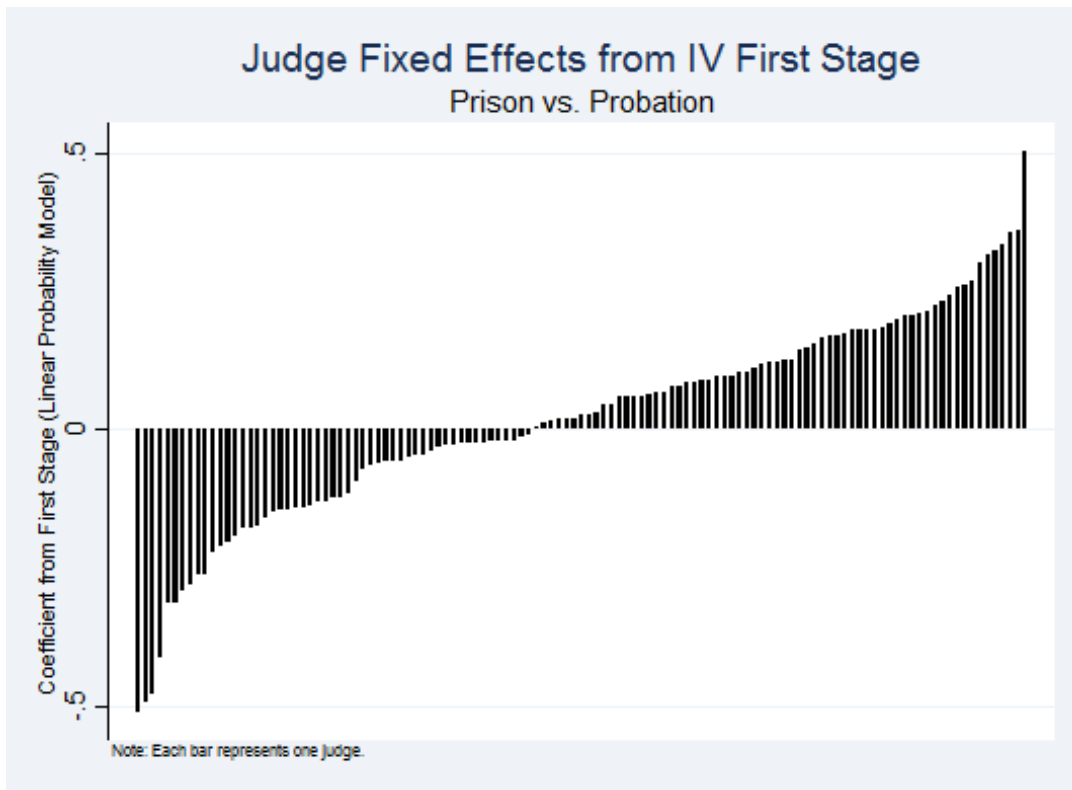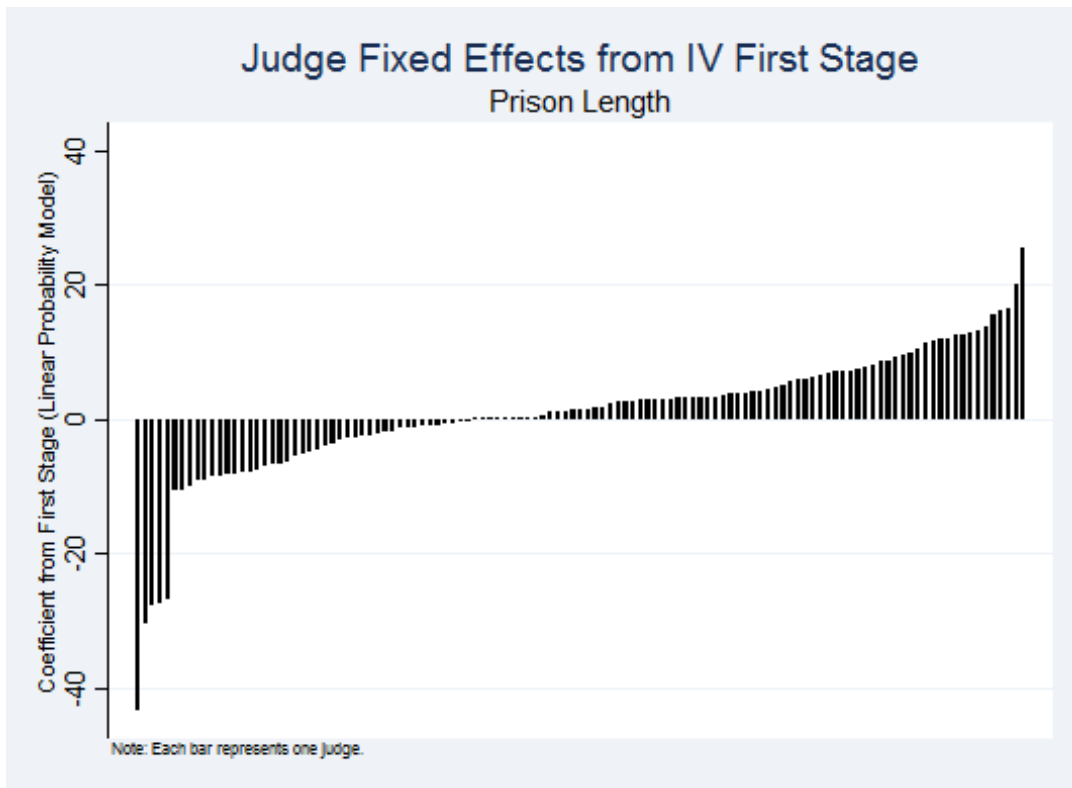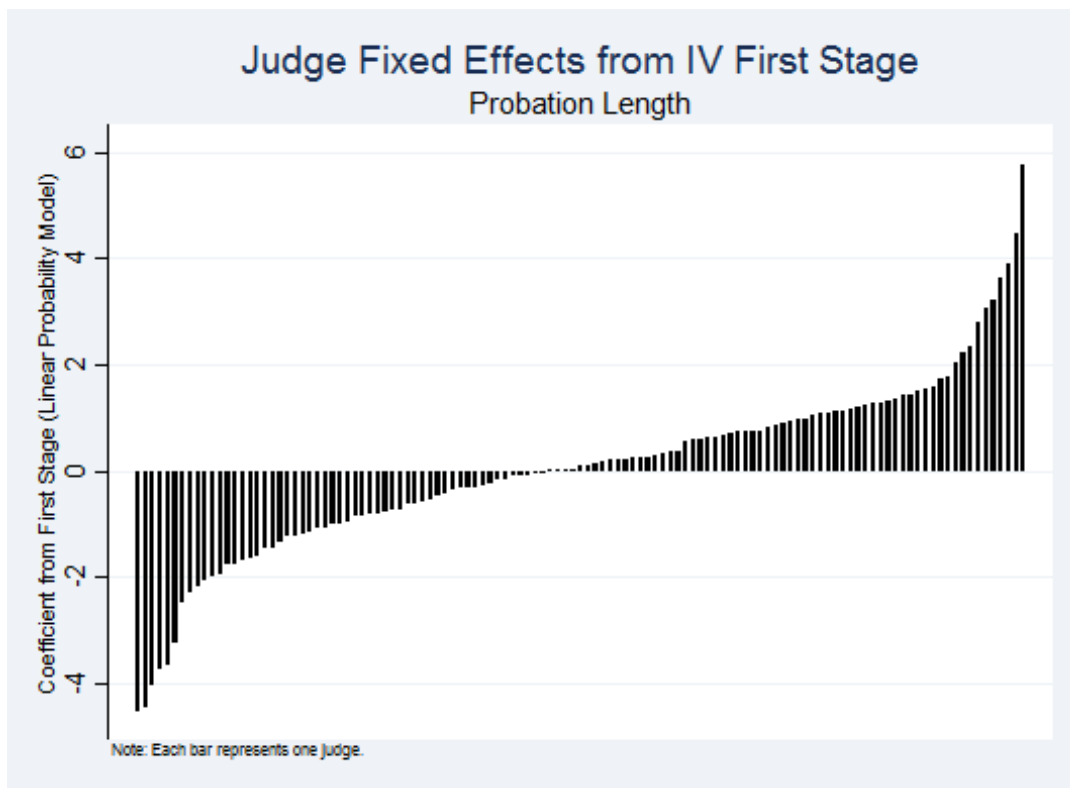
Figure A2: Employment Relative to Sentence Date among Individuals Sentenced for Their First Felony, by Sentence Type and Race
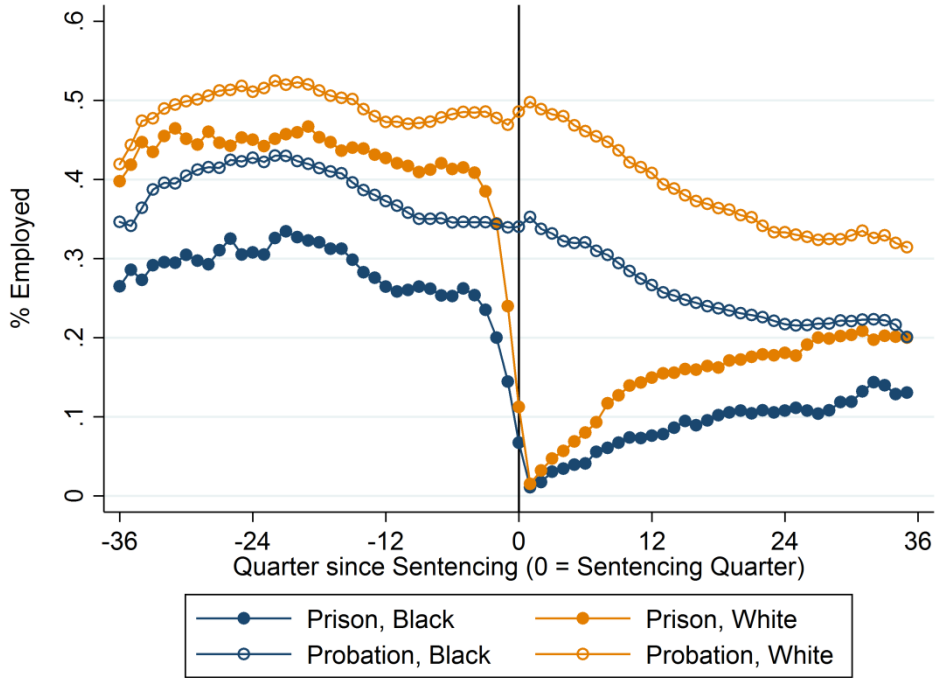
Figure A3: Employment Relative to Release Date among Individuals Sentenced for Their First Felony, by Sentence Type and Race (Excludes Focal Prison Term)
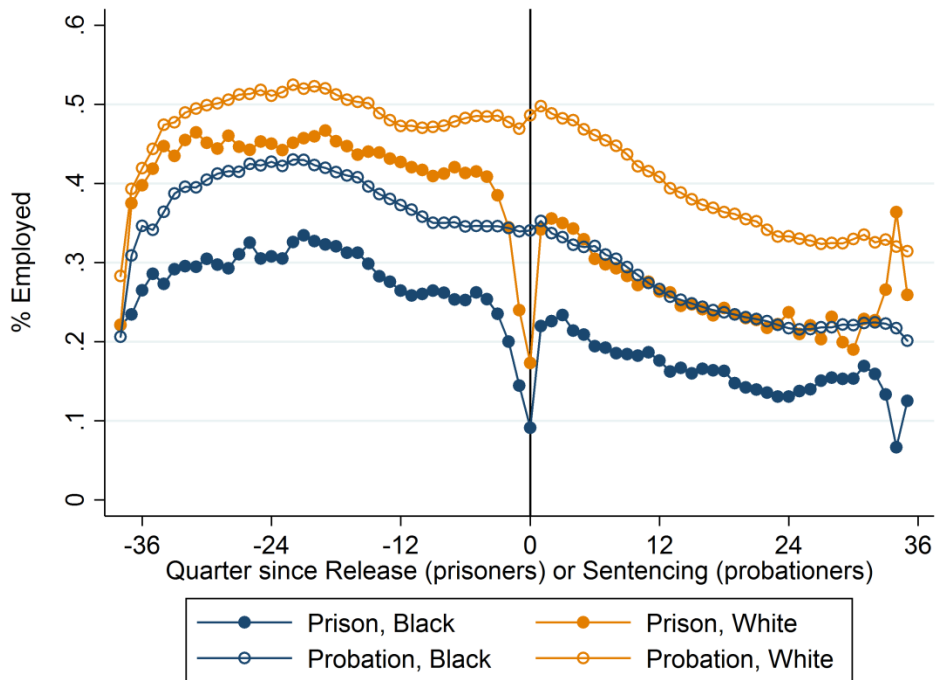
Table A1: Covariate Means, By Race, Sentence Type, and Work History

	Entire Sample, Prison	Entire Sample, Probation	Blacks, Prison			Blacks, Probation			Whites, Prison			Whites, Probation		
			Overall	No Work History	Any Work History	Overall	No Work History	Any Work History	Overall	No Work History	Any Work History	Overall	No Work History	Any Work History
Age at Sentence	33.77	30.58	33.23	33.44	33.08	30.21	30.69	29.96	34.29	37.47	33.13	30.93	34.58	29.91
Black	0.49	0.48	-	-	-	-	-	-	-	-	-	-	-	-
Female	0.07	0.23	0.07	0.06	0.07	0.22	0.16	0.25	0.07	0.07	0.07	0.24	0.22	0.25
Less Than High School	0.41	0.41	0.46	0.51	0.42	0.46	0.59	0.40	0.36	0.40	0.35	0.36	0.44	0.34
GED	0.23	0.10	0.23	0.23	0.23	0.11	0.11	0.11	0.24	0.24	0.24	0.10	0.09	0.10
High School	0.29	0.38	0.25	0.21	0.28	0.34	0.25	0.39	0.32	0.28	0.33	0.42	0.37	0.44
More Than High School	0.07	0.10	0.06	0.05	0.07	0.09	0.06	0.10	0.08	0.07	0.08	0.12	0.10	0.13
Pre-Sentence Employment*	0.34	0.30	0.25	0.22	0.27	0.23	0.21	0.24	0.43	0.46	0.42	0.38	0.43	0.36
Not Single	0.29	0.39	0.23	0.05	0.36	0.33	0.04	0.48	0.36	0.06	0.46	0.44	0.06	0.55
Ever Used Alcohol	0.66	0.63	0.58	0.56	0.59	0.54	0.52	0.56	0.74	0.70	0.76	0.71	0.67	0.73
Ever Used Marijuana	0.62	0.61	0.61	0.61	0.61	0.61	0.63	0.60	0.63	0.58	0.64	0.62	0.58	0.63
Ever Used Stimulants	0.41	0.27	0.38	0.38	0.39	0.20	0.22	0.19	0.44	0.43	0.44	0.34	0.34	0.34
Ever Used Opoids	0.14	0.09	0.10	0.12	0.09	0.05	0.06	0.04	0.17	0.19	0.17	0.13	0.14	0.13
Ever Used Other Drugs	0.27	0.25	0.24	0.24	0.24	0.24	0.23	0.25	0.30	0.31	0.30	0.26	0.27	0.26
Mental Health Illness History	0.19	0.18	0.12	0.13	0.11	0.11	0.14	0.09	0.25	0.26	0.24	0.24	0.26	0.23
First Time Felony Offender	0.33	0.73	0.28	0.28	0.28	0.67	0.63	0.69	0.39	0.36	0.39	0.79	0.75	0.81
0-4 Prior Arrests	0.24	0.63	0.22	0.22	0.23	0.61	0.56	0.63	0.25	0.26	0.25	0.64	0.60	0.66
5-9 Prior Arrests	0.30	0.26	0.31	0.29	0.32	0.27	0.29	0.26	0.30	0.27	0.31	0.24	0.26	5-24
10+ Prior Arrests	0.46	0.12	0.47	0.49	0.45	0.13	0.15	0.11	0.45	0.48	0.45	0.11	0.14	0.10
Pre-Trial Detention	0.69	0.93	0.62	0.57	0.64	0.93	0.93	0.93	0.76	0.73	0.77	0.92	0.93	0.92
Sentenced in 2003	0.29	0.24	0.29	0.25	0.32	0.25	0.20	0.28	0.29	0.23	0.31	0.24	0.21	0.25
Sentenced in 2004	0.26	0.24	0.26	0.25	0.26	0.24	0.24	0.24	0.25	0.27	0.25	0.25	0.24	0.25
Sentenced in 2005	0.23	0.25	0.23	0.25	0.22	0.25	0.26	0.24	0.23	0.25	0.22	0.25	0.26	0.25
Sentenced in 2006	0.22	0.26	0.22	0.25	0.20	0.26	0.29	0.24	0.22	0.25	0.21	0.26	0.29	0.26
sample size	19771	43059	9704	4083	5621	20732	7303	13577	10067	2687	7380	22327	4876	17451

*Pre-Sentence Employment = Ever Employed in 23 Quarters before Sentence

Table A2: Mean Outcomes Post-Sentence and Post-Release, by Sentence Type, Work History, and Race

	Entire Sample			Blacks Overall			Blacks No Work History			Blacks Work History			Whites Overall			Whites No Work History			Whites Work History		
	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*	Prison	Prob.	*Diff*
Any Employment																					
4th Quarter	0.04	0.38	*-0.34*	0.03	0.29	*-0.26*	0.02	0.07	*-0.06*	0.04	0.41	*-0.37*	0.05	0.45	*-0.40*	0.02	0.13	*-0.11*	0.06	0.54	*-0.48*
12th Quarter	0.12	0.31	*-0.19*	0.08	0.24	*-0.15*	0.04	0.07	*-0.03*	0.12	0.33	*-0.21*	0.16	0.38	*-0.22*	0.07	0.12	*-0.05*	0.19	0.45	*-0.27*
20th Quarter	0.12	0.27	*-0.14*	0.09	0.20	*-0.11*	0.04	0.06	*-0.02*	0.13	0.28	*-0.15*	0.16	0.33	*-0.17*	0.06	0.11	*-0.05*	0.19	0.39	*-0.20*
Employed 3 Consecutive Q's																					
4th Quarter	0.01	0.26	*-0.25*	0.01	0.19	*-0.19*	0.00	0.02	*-0.02*	0.01	0.28	*-0.27*	0.01	0.33	*-0.32*	0.00	0.06	*-0.06*	0.01	0.40	*-0.39*
12th Quarter	0.07	0.23	*-0.16*	0.04	0.17	*-0.13*	0.02	0.04	*-0.02*	0.06	0.24	*-0.18*	0.09	0.29	*-0.20*	0.04	0.08	*-0.04*	0.11	0.35	*-0.24*
20th Quarter	0.07	0.20	*-0.13*	0.05	0.15	*-0.10*	0.03	0.04	*-0.01*	0.07	0.21	*-0.14*	0.10	0.25	*-0.16*	0.03	0.08	*-0.05*	0.12	0.30	*-0.18*
Proportion Quarters Employed																					
4th Quarter	0.02	0.39	*-0.36*	0.02	0.31	*-0.29*	0.01	0.07	*-0.06*	0.02	0.43	*-0.41*	0.03	0.46	*-0.43*	0.01	0.12	*-0.10*	0.04	0.56	*-0.52*
12th Quarter	0.07	0.36	*-0.28*	0.05	0.28	*-0.23*	0.02	0.07	*-0.05*	0.08	0.39	*-0.31*	0.09	0.43	*-0.33*	0.04	0.12	*-0.08*	0.11	0.51	*-0.40*
20th Quarter	0.09	0.33	*-0.23*	0.07	0.25	*-0.19*	0.03	0.07	*-0.04*	0.09	0.35	*-0.26*	0.12	0.39	*-0.28*	0.05	0.12	*-0.07*	0.14	0.47	*-0.33*
Employed Outside SLM																					
4th Quarter	0.02	0.19	*-0.17*	0.01	0.14	*-0.13*	0.01	0.03	*-0.02*	0.01	0.20	*-0.19*	0.03	0.24	*-0.21*	0.01	0.06	*-0.05*	0.03	0.29	*-0.25*
12th Quarter	0.06	0.17	*-0.11*	0.04	0.13	*-0.09*	0.02	0.03	*-0.01*	0.05	0.18	*-0.12*	0.08	0.21	*-0.13*	0.04	0.06	*-0.02*	0.10	0.25	*-0.15*
20th Quarter	0.07	0.15	*-0.08*	0.04	0.11	*-0.07*	0.02	0.03	*-0.01*	0.05	0.15	*-0.10*	0.09	0.19	*-0.10*	0.04	0.06	*-0.02*	0.11	0.22	*-0.11*
Entered Prison at Least Once																					
1 Year	0.00	0.02	*-0.02*	0.00	0.03	*-0.02*	0.00	0.04	*-0.03*	0.00	0.02	*-0.02*	0.00	0.02	*-0.01*	0.00	0.02	*-0.02*	0.00	0.02	*-0.02*
3 Years	0.10	0.07	*0.03*	0.11	0.09	*0.02*	0.11	0.11	*0.00*	0.11	0.08	*0.03*	0.09	0.06	*0.04*	0.09	0.06	*0.03*	0.07	0.05	*0.02*
5 Years	0.21	0.10	*0.10*	0.21	0.13	*0.08*	0.22	0.17	*0.05*	0.21	0.12	*0.09*	0.20	0.08	*0.12*	0.18	0.09	*0.10*	0.18	0.08	*0.10*

Post-Sentence

Table A2 Continued

	Entire Sample			Post-Release Blacks Overall			Blacks No Work History			Blacks Work History			Whites Overall			Whites No Work History			Whites Work History		
	Prison	Prob.	Diff	Prison	Prob.	Diff	Prison	Prob.	Diff	Prison	Prob.	Diff	Prison	Prob.	Diff	Prison	Prob.	Diff	Prison	Prob.	Diff
Any Employment																					
4th Quarter	0.25	0.38	-0.13	0.19	0.29	-0.11	0.09	0.07	0.01	0.25	0.41	-0.16	0.30	0.45	-0.15	0.14	0.13	0.01	0.36	0.54	-0.18
12th Quarter	0.17	0.31	-0.14	0.12	0.24	-0.12	0.06	0.07	-0.01	0.17	0.33	-0.16	0.21	0.38	-0.17	0.09	0.12	-0.04	0.25	0.45	-0.20
20th Quarter	0.15	0.27	-0.12	0.11	0.20	-0.09	0.06	0.06	0.00	0.15	0.28	-0.13	0.18	0.33	-0.15	0.08	0.11	-0.03	0.20	0.39	-0.18
Employed 3 Consecutive Q's																					
4th Quarter	0.16	0.26	-0.10	0.11	0.19	-0.09	0.04	0.02	0.02	0.15	0.28	-0.13	0.21	0.33	-0.12	0.08	0.06	0.02	0.25	0.40	-0.15
12th Quarter	0.11	0.23	-0.12	0.08	0.17	-0.09	0.04	0.04	0.00	0.10	0.24	-0.14	0.14	0.29	-0.15	0.05	0.08	-0.02	0.17	0.35	-0.18
20th Quarter	0.09	0.20	-0.11	0.07	0.15	-0.08	0.04	0.04	0.00	0.09	0.21	-0.12	0.12	0.25	-0.14	0.05	0.08	-0.03	0.14	0.30	-0.17
Proportion Quarters Employed																					
4th Quarter	0.27	0.39	-0.12	0.20	0.31	-0.10	0.10	0.07	0.03	0.28	0.43	-0.15	0.33	0.46	-0.13	0.15	0.12	0.03	0.39	0.56	-0.17
12th Quarter	0.22	0.36	-0.13	0.17	0.28	-0.11	0.08	0.07	0.01	0.22	0.39	-0.16	0.28	0.43	-0.15	0.12	0.12	0.00	0.33	0.51	-0.19
20th Quarter	0.20	0.33	-0.12	0.15	0.25	-0.10	0.08	0.07	0.01	0.20	0.35	-0.15	0.25	0.40	-0.14	0.11	0.12	-0.01	0.30	0.47	-0.18
Employed Outside SLM																					
4th Quarter	0.13	0.19	-0.06	0.08	0.14	-0.06	0.04	0.03	0.02	0.10	0.20	-0.10	0.17	0.24	-0.06	0.07	0.06	0.01	0.21	0.29	-0.08
12th Quarter	0.09	0.17	-0.08	0.06	0.13	-0.07	0.03	0.03	0.00	0.08	0.18	-0.09	0.12	0.21	-0.09	0.05	0.06	-0.01	0.14	0.25	-0.11
20th Quarter	0.08	0.15	-0.07	0.06	0.11	-0.05	0.04	0.03	0.01	0.07	0.15	-0.08	0.10	0.19	-0.08	0.06	0.06	0.00	0.12	0.22	-0.10
Entered Prison at Least Once																					
1 Year	0.13	0.02	0.11	0.14	0.03	0.11	0.16	0.04	0.12	0.13	0.02	0.11	0.12	0.02	0.10	0.12	0.02	0.10	0.12	0.02	0.10
3 Years	0.32	0.07	0.25	0.35	0.09	0.26	0.37	0.11	0.26	0.34	0.08	0.26	0.29	0.06	0.24	0.29	0.06	0.23	0.29	0.05	0.24
5 Years	0.40	0.10	0.29	0.43	0.13	0.30	0.46	0.17	0.29	0.42	0.12	0.30	0.36	0.08	0.28	0.36	0.09	0.27	0.36	0.08	0.29

SLM= Secondary Labor Market; Work History = Any Formal Employment in 12 Calendar Quarters before Sentence

Table A3: First Stage Diagnostics, By Race and Work History

	Entire Sample		Blacks						Whites					
			Overall		No Work History		Any Work History		Overall		No Work History		Any Work History	
	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions	Judge Only	Judge & Interactions
A: Shea's Partial R-Squared														
Prison	0.012	0.076	0.017	0.105	0.019	0.167	0.020	0.130	0.012	0.093	0.023	0.212	0.015	0.103
Prison Length	0.004	0.051	0.007	0.078	0.012	0.149	0.007	0.104	0.004	0.063	0.013	0.199	0.004	0.071
Prison Length Squared	0.002	0.037	0.003	0.054	0.007	0.109	0.006	0.083	0.004	0.080	0.014	0.269	0.003	0.076
Probation Length	0.018	0.070	0.028	0.116	0.033	0.160	0.032	0.149	0.012	0.074	0.023	0.186	0.014	0.088
Probation Length Squared	0.003	0.027	0.006	0.060	0.015	0.130	0.007	0.100	0.004	0.045	0.021	0.189	0.004	0.057
B: Partial F-Test														
Prison	32.71	4.75	18.75	2.98	7.49	1.86	12.57	2.44	14.83	3.02	4.72	1.62	11.51	2.62
Prison Length	13.02	3.36	8.19	2.38	4.37	1.71	5.11	2.04	3.64	1.81	2.09	1.58	2.86	1.82
Prison Length Squared	4.65	2.09	2.90	1.42	1.72	1.11	2.64	1.45	2.03	2.20	1.71	2.25	1.54	1.86
Probation Length	29.36	4.01	19.96	2.93	7.53	1.62	13.68	2.43	11.41	2.33	3.40	1.48	9.04	2.15
Probation Length Squared	4.70	1.40	3.65	1.36	3.26	1.30	2.38	1.46	2.85	1.33	2.98	1.47	2.04	1.30
Sample size	109,636		47,673		17,740		29,933		61,963		14,718		47,245	

Table A4: Randomization Checks - Global F-tests (Joint Significance Test of Judge Dummies)

	Entire Sample		Blacks						Whites					
			Overall		No Work History		Any Work History		Overall		No Work History		Any Work History	
	F	p	F	p	F	p	F	p	F	p	F	p	F	p
First Time Felony Offender	5.16	0.0000	4.24	0.0000	2.09	0.0000	3.11	0.0000	2.55	0.0000	1.31	0.013	2.27	0.0000
0-4 Prior Arrests	6.01	0.0000	5.61	0.0000	2.87	0.0000	3.70	0.0000	2.20	0.0000	1.22	0.055	1.89	0.0000
5-9 Prior Arrests	1.38	0.0039	1.65	0.0000	1.24	0.0387	1.52	0.0002	0.95	0.6283	0.92	0.715	1.09	0.2356
10+ Prior Arrests	5.73	0.0000	4.70	0.0000	2.71	0.0000	3.06	0.0000	2.35	0.0000	1.37	0.004	1.88	0.0000
Age at Sentence	3.00	0.0000	2.71	0.0000	1.85	0.0000	1.94	0.0000	1.33	0.0102	1.35	0.006	1.12	0.1749
Female	4.32	0.0000	5.22	0.0000	2.12	0.0000	4.06	0.0000	1.33	0.0098	1.19	0.075	1.15	0.1301
Black	9.61	0.0000									--	--	--	--
Less Than High School	1.89	0.0000	1.48	0.0006	1.18	0.0883	1.37	0.0047	1.03	0.3855	1.56	0.000	1.00	0.4887
GED	1.67	0.0000	1.24	0.0407	1.19	0.0800	1.22	0.0528	1.21	0.0619	1.04	0.378	1.16	0.1084
High School	1.64	0.0000	1.36	0.0059	1.02	0.4347	1.15	0.1238	1.11	0.1913	1.28	0.023	1.09	0.2367
More Than High School	1.95	0.0000	1.56	0.0001	1.20	0.0693	1.47	0.0006	1.54	0.0001	1.02	0.437	1.56	0.0001
Pre-Sentence Employment	4.06	0.0000	2.63	0.0000	1.03	0.3890	2.00	0.0000	1.99	0.0000	0.96	0.613	1.71	0.0000
Non-Single	2.03	0.0000	1.71	0.0000	1.23	0.0475	1.50	0.0004	1.00	0.4898	1.06	0.315	1.15	0.1312
Mental Illness History	2.33	0.0000	1.34	0.0079	1.13	0.1598	1.00	0.4854	1.59	0.0000	1.37	0.005	1.29	0.0188
Ever Used Alcohol	2.58	0.0000	1.15	0.1224	0.90	0.7803	1.05	0.3259	2.01	0.0000	1.19	0.081	1.73	0.0000
Ever Used Marijuana	2.98	0.0000	2.18	0.0000	1.26	0.0274	1.98	0.0000	1.86	0.0000	1.45	0.001	1.66	0.0000
Ever Used Stimulants	2.87	0.0000	1.93	0.0000	1.24	0.0380	1.51	0.0003	2.03	0.0000	1.31	0.013	1.95	0.0000
Ever Used Opoids	1.66	0.0000	1.31	0.0143	1.05	0.3255	1.18	0.0882	1.47	0.0007	0.89	0.794	1.45	0.0009
Ever Used Other Drugs	3.11	0.0000	2.35	0.0000	1.57	0.0001	1.80	0.0000	1.90	0.0000	1.24	0.038	1.77	0.0000
Pre-Trial Detention	5.68	0.0000	4.68	0.0000	2.44	0.0000	3.07	0.0000	2.28	0.0000	1.45	0.0011	1.92	0.0000

Note: Each regression includes judge IDs, county fixed effects, and sentence year dummies.

Table A5: OLS Models by Race and Work History (3 Year Follow-Up, selected coefficients)

		Post-Sentence					
		Blacks			Whites		
	Entire Sample	Overall	No Work History	Any Work History	Overall	No Work History	Any Work History
Employment in the 12th Quarter							
Prison vs. Probation	-0.090***	-0.062***	-0.006	-0.093***	-0.110***	-0.029**	-0.137***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.009)	(0.007)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared	0.183	0.174	0.022	0.138	0.172	0.053	0.130
Employed 3 Consecutive Quarters through the 12th Quarter							
Prison vs. Probation	-0.077***	-0.054***	-0.007	-0.079***	-0.095***	-0.026***	-0.117***
	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)	(0.007)	(0.006)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared	0.169	0.160	0.015	0.137	0.160	0.034	0.131
Proportion Quarters Employed through the 12 Quarter							
Prison vs. Probation	-0.183***	-0.134***	-0.031***	-0.199***	-0.224***	-0.062***	-0.280***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared	0.383	0.377	0.057	0.319	0.367	0.106	0.308
Employed Outside the SLM in the 12 Quarter							
Prison vs. Probation	-0.059***	-0.041***	-0.005	-0.063***	-0.072***	-0.015*	-0.091***
	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)	(0.007)	(0.006)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared	0.110	0.109	0.013	0.093	0.102	0.029	0.082
Imprisonment within 3 Years							
Prison vs. Probation	-0.003	-0.017***	-0.018*	-0.017**	0.007	0.021*	0.004
	(0.003)	(0.005)	(0.009)	(0.006)	(0.004)	(0.009)	(0.005)
Observations	109,636	47,673	17,740	29,933	61,963	14,718	47,245
R-squared	0.072	0.074	0.073	0.077	0.072	0.075	0.076

Table A5 continued

| | Post-Release | | | | | | |
| | Entire Sample | Blacks | | | Whites | | |
		Overall	No Work History	Any Work History	Overall	No Work History	Any Work History
Employment in the 12th Quarter							
Prison vs. Probation	0.016***	0.037***	0.078***	0.016	0.001	0.050***	-0.019*
	(0.004)	(0.006)	(0.006)	(0.008)	(0.006)	(0.010)	(0.008)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared	0.151	0.159	0.069	0.123	0.139	0.054	0.100
Employed 3 Consecutive Quarters through the 12th Quarter							
Prison vs. Probation	0.010*	0.026***	0.059***	0.010	-0.004	0.039***	-0.019**
	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)	(0.008)	(0.007)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared	0.145	0.148	0.074	0.126	0.139	0.050	0.111
Proportion Quarters Employed through the 12 Quarter							
Prison vs. Probation	0.019***	0.033***	0.076***	0.008	0.010*	0.075***	-0.015**
	(0.003)	(0.004)	(0.005)	(0.006)	(0.005)	(0.007)	(0.006)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared	0.314	0.336	0.151	0.257	0.291	0.098	0.210
Employed Outside the SLM in the 12 Quarter							
Prison vs. Probation	0.007*	0.020***	0.042***	0.008	-0.002	0.029***	-0.014*
	(0.004)	(0.004)	(0.004)	(0.007)	(0.005)	(0.008)	(0.007)
Observations	102,890	44,136	16,140	27,996	58,754	13,780	44,974
R-squared	0.094	0.101	0.050	0.085	0.089	0.046	0.068
Imprisonment within 3 Years							
Prison vs. Probation	0.127***	0.129***	0.141***	0.119***	0.121***	0.140***	0.113***
	(0.004)	(0.007)	(0.011)	(0.008)	(0.006)	(0.012)	(0.007)
Observations	105,435	45,303	16,618	28,685	60,132	14,146	45,986
R-squared	0.105	0.107	0.101	0.114	0.104	0.103	0.109

Robust standard errors in parentheses; *** $p<0.001$, ** $p<0.01$, * $p<0.05$; Includes county fixed effects and covariates in Table A1

Table A6: Focal Crime Type by Race, Pre-Sentence Work History, and Sentence Type

	Black, No Work, Prison		Black, No Work, Probation		Black, Any Work, Prison		Black, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
Controlled Substance	851	21%	2756	39%	1031	18%	4255	31%
Person	1983	49%	1088	15%	3073	55%	2228	16%
Property	700	17%	1947	27%	898	16%	4470	33%
Public Order	49	1%	256	4%	61	1%	465	3%
Public Safety	489	12%	1085	15%	546	10%	2065	15%
Other Crimes	11	0%	23	0%	12	0%	94	1%

	White, No Work, Prison		White, No Work, Probation		White, Any Work, Prison		White, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
Controlled Substance	329	12%	1684	35%	752	10%	6185	35%
Person	1290	48%	892	18%	3859	52%	3037	17%
Property	518	19%	1408	29%	1453	20%	5642	32%
Public Order	76	3%	465	10%	150	2%	969	6%
Public Safety	450	17%	398	8%	1090	15%	1476	8%
Other Crimes	24	1%	29	1%	76	1%	142	1%

Focal Crime = Most Serious Crime at Initial Sentencing

Table A7: Pre-Trial Detention by Race, Pre-Sentence Work History, and Sentence Type

No Detention vs. Any Detention

	Black, No Work, Prison		Black, No Work, Probation		Black, Any Work, Prison		Black, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
No Detention	1738	43%	483	7%	1998	36%	895	7%
Any Detention	2345	57%	6672	93%	3623	64%	12682	93%

	White, No Work, Prison		White, No Work, Probation		White, Any Work, Prison		White, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
No Detention	718	27%	362	7%	1697	23%	1424	8%
Any Detention	1969	73%	4514	93%	5683	77%	16027	92%

< 7 Days Detention vs. 7+ Days Detention

	Black, No Work, Prison		Black, No Work, Probation		Black, Any Work, Prison		Black, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
0-7 Days in Detention	1922	47%	4118	58%	2381	42%	9299	68%
7+ Days in Detention	2161	53%	3037	42%	3240	58%	4278	32%

	White, No Work, Prison		White, No Work, Probation		White, Any Work, Prison		White, Any Work, Probation	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
0-7 Days in Detention	907	34%	3409	70%	2523	34%	13497	77%
7+ Days in Detention	1780	66%	1467	30%	4857	66%	3954	23%