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Custodial Parole Sanctions and Earnings after Release from Prison

David J. Harding,

UC Berkeley

Jonah A. Siegel, and

Michigan Indigent Defense Commission

Jeffrey D. Morenoff

University of Michigan

Abstract

Although the labor market consequences of incarceration in prison have been central to the literature on mass incarceration, punishment, and inequality, other components of the growing criminal justice system have received less attention from sociologists. In particular, the rise of mass incarceration was accompanied by an even larger increase in community supervision. In this paper, we examine the labor market effects of one frequently experienced aspect of post-prison parole, short-term custody for parole violations. Although such sanctions are viewed as an alternative to returning parole violators to prison, they have the potential to affect labor market outcomes in ways similar to imprisonment, including both adverse and positive effects on earnings. We estimate that parolees lost approximately 37 percent of their earnings in quarters during which they were in short-term custody. Although their earnings tended to increase in the quarter immediately following short-term custody—consistent with the stated intentions of such sanctions—parolees experienced further earnings loss over the longer term after such sanctions. In the third quarter following a short-term custody sanction, earnings are lowered by about 13 percent. These associations are larger for those who were employed in the formal labor market before their initial incarceration.

Since the early 1970s, the rate of imprisonment has more than quintupled, with over 2.2 million individuals currently behind bars (National Research Council 2014). Prior research shows that gainful employment, a critical aspect of prisoner reintegration, is elusive for many former prisoners (National Research Council 2007), as imprisonment creates significant barriers that result in bleak rates of employment and low earnings in this population. The lower rates of employment and wages experienced by former prisoners in comparison to their counterparts who have never been incarcerated result in a disparity in annual earnings of up to 40 percent (Western 2006). Imprisonment also has consequences

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Direct correspondence to David J. Harding, Department of Sociology, 410 Barrows Hall, UC Berkeley, Berkeley CA 94720; telephone: +510-642-2707; fax: +510-642-0659; dharding@berkeley.edu.

Supplementary Material

Supplementary material is available at *Social Forces* online.

beyond the individual incarcerated, affecting the health, development, and economic stability of family members, particularly children and romantic partners (Turney and Haskins 2014; Wakefield and Wildeman 2013; Wildeman, Schnittker, and Turney 2012), as well as the social cohesion and economic stability of communities most affected by mass incarceration (Clear 2007).

Yet imprisonment is not the only part of the long-term growth of the criminal justice system with important implications for scholarship on inequality, punishment, and crime. The growth of the correctional system brought not only a significant increase in the prison population but also a corresponding rise in the number of people under community supervision on either parole or probation (Hughes and Wilson 2002; Kaeble, Maruschak, and Bonczar 2015). With this rise came a concurrent increase in the use of custodial sanctions for parole and probation violators. Reliance on custodial sanctions has become particularly pervasive in the parole system, with one estimate indicating that almost half of parolees spend time in jail and one-quarter are mandated to other custodial sanctions within two years of release from prison (Harding, Morenoff, and Herbert 2013).¹ Given that 64 percent of former prisoners are released onto parole or other forms of community supervision (Carson 2015), experiencing custodial sanctions after release is now a common occurrence among former prisoners.

With regard to inequality, an important limitation of the extensive literature on incarceration is that it focuses almost exclusively on the effects of imprisonment (Lyons and Pettit 2011; National Research Council 2014; Western 2006), neglecting the potential effects of related custodial sanctions—short-term jail spells, technical rule violator centers, or custodial reentry programs—which are alternatives to prison for parole or probation violators. For example, brief custodial sanctions for probation violators are one of the key features of the much touted but still controversial Project HOPE program (Cullen, Manchak, and Duriez 2014; Kleiman, Kilmer, and Fisher 2014), but the labor market consequences of such sanctions have not been investigated. More generally, given the high rates at which former prisoners cycle in and out of short-term custody, identifying whether custodial alternatives to prison interfere with the ability of individuals to find and maintain work is critical to understanding how criminal justice system involvement shapes labor market outcomes. Moreover, because most prisoners are released onto parole, custodial parole sanctions may be one of the mechanisms through which imprisonment affects labor market outcomes.

With regard to the sociology of punishment, the growth of the parole system and the corresponding increase in custodial sanctions for parole violators represent an expansion of the carceral state's capacity to surveil, control, and punish poor and minority populations (Alexander 2010; Wacquant 2001), the potential consequences of which have not been investigated. Similar to the collateral consequences of conviction due to formal and informal stigmatization, custodial parole sanctions may represent a form of what Uggen and Stewart

¹Although we are not aware of any national figures on the frequency of custodial sanctions among parolees or probationers, many other states use custodial sanctions. For example, under California's "Realignment," technical parole violators are sent to jail rather than returned to prison (Martin and Grattet 2015). States including but not limited to Michigan, Nebraska, Oregon, Arkansas, Colorado, New York, Tennessee, Mississippi, and Pennsylvania have custodial programs for parole violators that do not involve a return to prison.

(2015) call “piling on” of punishment. With regard to criminology, the link between employment and desistance from crime (Sampson and Laub 1993) suggests that custodial parole sanctions have the potential to interfere with the desistance process if they negatively affect employment. As a result, increased risk of exposure to such sanctions may be important to understanding the potentially criminogenic effects of imprisonment. Moreover, the frequency of custodial sanctions suggests that they are central to the role of the criminal justice system in prisoner reentry and reintegration more generally.

This paper investigates the effects of short-term custodial sanctions on earnings in the formal labor market among former prisoners. Using a unique longitudinal dataset on a cohort of prisoners released onto parole in Michigan in 2003, we analyze the effect of a short-term custody spell on earnings in the calendar quarter during which custody was experienced and its effects over subsequent quarters. We estimate that parolees lost approximately 37 percent of their earnings in quarters during which they were in short-term custody. Although parolees’ earnings tended to increase in the quarter following short-term custody—consistent with the stated intentions of such sanctions—they experienced further loss of earnings over the longer term following a custodial sanction. In the third quarter following a short-term custody sanction, earnings are about 13 percent lower than they would have been had the individual not experienced the sanction. The effects were larger for those with the best labor market prospects: those who were employed in the formal labor market before their initial incarceration.

The Expansion of Community Supervision, Net-Widening, and Custodial Sanctions

As a result of the boom in community corrections, the number of individuals on parole and probation has increased dramatically over the past four decades, with current estimates revealing that one of every 31 American adults is either on probation, parole, or in prison or jail on any given day (Pew Center on the States 2009). We focus in this study on one aspect of punishment and social control related to community supervision: custodial sanctions, which remove parolees from the community for short-term spells of custody in jails and specialized facilities for parolees and probationers who have violated the conditions of their community supervision (Burke and Tonry 2006). Although such institutions go by different names in different states, they are often called residential reentry centers, correctional centers, detention centers, or technical rule violator centers.

When a parolee is found to have committed a new crime or violated the terms of supervision, the judge (in the case of a new felony) or the parole board (in the case of a technical violation or misdemeanor) can decide to revoke the terms of parole and return the person to prison. Alternatively, a sanction can be imposed without the revocation of parole. In some cases, such sanctions involve a short period of custody. Such sanctions are often used to punish minor offenses, such as drug use or petty theft, or technical rule violations such as consuming alcohol, breaking curfew, or failing to report to one’s parole officer. Custodial sanctions are intended to stop such behavior from escalating to more serious offenses. For example, a parolee who fails to report to his parole officer and is suspected of

using drugs may be sent to a technical rule violation center for a week or two in order to “detox.”² In other cases, the sanction may be non-custodial (Harding, Morenoff, and Herbert 2013). A wide range of non-custodial sanctions are possible, ranging from a mere warning, to electronic monitoring or more frequent drug tests or required contact with the parole officer, to more restrictive curfews, to requirements to complete substance abuse or mental health treatment programs. The exact non-custodial sanction is typically tailored to the violation and the individual’s prior criminal offenses. For example, a violation for a positive drug test might result in more frequent testing and a requirement to attend drug treatment, or a violation for a brief period of absconding might result in electronic monitoring and more stringent parole conditions related to curfew. An individual with a history of drunken driving may be prohibited from driving if he tests positive for alcohol.

Custodial sanctions can be understood as part of a larger shift in the institution of parole that coincided with the rise of mass incarceration and a greater emphasis on punishment, deterrence, and surveillance (Burke and Tonry 2006; Simon 1993). Parole was originally intended to both rehabilitate and control former prisoners and also later served to help relieve prison overcrowding (Petersilia 2011; Simon 1993). In the late 1970s, the rehabilitative goals of parole came under question in the “Martinson Report,” which argued that “nothing works” in offender rehabilitation (Lipton, Martinson, and Wilks 1975) and helped usher in an era of more punitive criminal justice policies in which parole came to be seen as a “soft” approach to crime (Burke and Tonry 2006).

The shift toward punishment and surveillance in parole was institutionalized in the late 1980s and early 1990s as part of a broader set of changes that Feeley and Simon (1992) have called “the new penology.” They involved more efficient management of growing caseloads, actuarial methods of risk assessment, and a focus on preventing offending rather than rehabilitation or reintegration. This era also brought corresponding changes in the role of parole officers, whose primary responsibilities were reframed as managing risks to public safety (Lynch 2000; Simon 1993). The benchmarks by which parole officers’ work is evaluated shifted toward parole violations and revocations resulting from detection of prohibited behavior (Feeley and Simon 1992; Simon 1993). The use of violations and sanctions concomitantly expanded (Burke and Tonry 2006). The custodial sanctions we examine in this paper are one understudied aspect of the contemporary focus on punishment during parole.

Custodial Sanctions and Back-End Net-Widening

The term “back-end net widening” (Phelps 2013) refers to the possibility that parole or probation sanctions, which are often touted as alternatives to prison, may actually contribute to higher rates of incarceration, thus widening the reach of the criminal justice system (Austin and Krisberg 1981; Tonry and Lynch 1996). Phelps (2013) argues that back-end net-widening is more likely when community supervision focuses on monitoring and neglects rehabilitation. Supervision programs with onerous rules can hamper individuals’ ability to secure work and live law-abiding lives, and stringent reporting and testing requirements

²We choose this example because half of our sample has a history of drug or alcohol abuse, surely an underestimate because it is based on self-reports at sentencing.

increase the chances that minor offenses and technical violations will be detected and sanctioned.³

Another way custodial sanctions could be implicated in net-widening is that risk-averse community corrections officers could use them to incarcerate offenders who might otherwise have received non-custodial sanctions (Tonry and Lynch 1996). Evidence consistent with this idea was found in a study of frontend sentencing, where such alternatives to prison are typically called “intermediate sanctions.” Homant and DeMercurio (2009) found that the intermediate sanction of intensive probation was more often being used as an alternative to probation than a diversion from prison.

Although there have been almost no studies of back-end net-widening in the parole population (White et al. 2011), the same theoretical argument applies: the use of custodial sanctions for parole violations—especially for minor technical violations—could widen the net of penal control by increasing the probability of imprisonment for subsequent violations and expanding the population ensnared in this net to include parolees at low risk of recidivating. Moreover, facilities and programs intended to divert technical violators from prison may expand once created, and the availability of such facilities and programs can then create additional demand for them, sweeping into the widening net individuals who would have previously received non-custodial sanctions.⁴ Prior studies of the parole revocation process suggest there is a considerable role for discretion by parole officers in the application of sanctions (Steen et al. 2013) and considerable contextual variation across parole jurisdictions in the use of parole revocation (Lin, Grattet, and Petersilia 2010).

Custodial Sanctions and Labor Market Outcomes

We suggest four mechanisms through which a custodial sanction can shape future labor market outcomes. We note that these mechanisms are similar to those discussed in the prior literature with regard to the effects of imprisonment on labor market outcomes (e.g., Bushway et al. 2007; Western 2006), except that custodial sanctions are far shorter than periods of imprisonment (see table 2), suggesting that custodial sanctions may have smaller effects. The first mechanism is simply incapacitation. Custodial sanctions preclude employment in the formal labor market.

Second, short-term custodial sanctions may undermine efforts to overcome the labor market stigma associated with a criminal record. Prior research suggests that criminal records send “negative signals” to employers about the skills or trustworthiness of job candidates (Holzer 1996; Holzer, Raphael, and Stoll 2007). For employers who have previously hired a former prisoner, even short spells of custody may heighten concerns about an employee’s trustworthiness or reliability. In addition, employers may be unwilling or unable to tolerate repeated, extended, or unexplained absences that result from even short custody spells

³An often-cited example of an intermediate sanction that leads to net-widening is intensive probation supervision, which is intended to divert people from prison by providing a community-based alternative with more stringent conditions than traditional probation (Petersilia 1999) but ends up increasing technical violations and incarceration compared to routine supervision (Lin, Grattet, and Petersilia 2010).

⁴Cost savings from diverting technical parole violators from prison is the explicit motivation for Michigan’s technical rule violator centers, one of its custodial sanctions for parolees (MDOC 1998).

(Grogger 1995). Thus, parolees who manage to find employment but experience a custodial sanction may struggle to retain that employment or find new employment afterward.

More generally, the inconsistent engagement in the work force that results from repeated custody spells may raise employer concerns about lack of experience or unpredictability (Holzer 1996; Holzer, Raphael, and Stoll 2007). Employer perceptions of applicants undoubtedly contain a critical racial component. Among non-offenders, black applicants are already significantly less likely to receive job offers (Bendick, Jackson, and Reinoso 1994); when criminal involvement is added, employers demonstrate even more hesitancy (Pager 2003). Custodial sanctions may reinforce beliefs about former prisoners and their suitability for employment and cast doubt upon claims of desistance from crime, a process of “stigma amplification.”

Third, custodial sanctions may exacerbate the loss of human and social capital that parolees have already suffered from their time in prison (Apel and Sweeten 2010). For example, custodial sanctions can interfere with education and place additional strain on already-weakened social networks. Relatedly, brief periods of confinement in jail or prison appear to disrupt cohabiting unions in the short term and decrease marriage in the long term (Apel 2016). Moreover, high rates of residential mobility following custodial sanctions may further weaken social ties and create spatial mismatches between old job networks and new residential communities. Relocation may also generate contextual challenges for parolees, whose post-sanction neighborhoods tend to be characterized by higher poverty rates than their pre-sanction neighborhoods (Harding, Morenoff, and Herbert 2013).

Fourth, custodial sanctions may discourage former prisoners from seeking new jobs by decreasing their attachment to the formal work force and educational programs (Western, Kling, and Weiman 2001). Indeed, recent research suggests that much of the non-employment of former prisoners results not from unemployment but from labor force nonparticipation, potentially due to either discouragement or more immediate or accessible employment in the underground economy (Apel and Sweeten 2010). It is possible that the experience of a custodial sanction—particularly for minor violations of parole—perpetuates nonparticipation through feelings of unfair treatment and the development of legal cynicism (Sampson and Bartusch 1998).

However, custodial sanctions also have the potential to improve labor market outcomes. The intended purpose of such sanctions is to prevent more serious recidivism, and many sanction facilities include rehabilitative programming or drug treatment intended to accomplish such goals. Custodial sanctions may also serve a “cooling out” function, in which a former prisoner who has returned to substance use or petty crime is temporarily removed from the community to disrupt a downward spiral. Finally, a custodial sanction can separate an individual from criminogenic peer networks, signal to family and friends that their loved one is in need of social support, or serve as a form of specific deterrence, making real the potential for more serious sanctions. In short, a custodial sanction has the potential to re-orient the former prisoner toward a more conventional lifestyle, at least in the short term.

We also expect that the effects of custodial sanctions on labor market outcomes will vary depending on one's labor market prospects because the above mechanisms may operate with greater or lesser strength depending on individual characteristics. Parolees with the best labor market prospects, including those previously employed and those with the most human and social capital, have the most to lose from being temporarily removed from the work force. Those who lack work experience and possess little human or social capital have weak prospects of finding a job even without the disruptive effects of custodial sanctions (Pager, Wager, and Bonikowski 2009; Smith 2005). Similarly, whatever additional stigma might arise from custodial sanctions could have less impact on the job prospects of parolees who are already heavily stigmatized in the low-wage labor market due to their racial identity or criminal record, and those who have already developed strong feelings of legal cynicism are unlikely to be further hardened by a custodial sanction. Conversely, parolees with the worst labor market prospects might have the most to gain from whatever positive influences custodial sanctions might exert on labor market trajectories by offering programs or services, a period for cooling out, or the removal of criminogenic influences. To test these hypotheses in the analysis below, we examine whether the effects of short-term custody vary by pre-prison employment, race, and number of times in prison. If the "more to lose" hypothesis is correct, we expect to see that those with pre-prison employment, whites, and those released from their first prison term will experience the most negative effects of custodial sanctions.

Methodology

The current study investigates the impact of custodial sanctions on labor market outcomes through an examination of quarterly earnings in the formal labor market among parolees. Although our primary goal is estimating the effects of custodial sanctions, examining these effects also requires that we take into account reimprisonment. Estimates of the effects of reimprisonment also provide a contrast for any effects of short-term custodial sanctions. We expect that any effects of custodial sanctions will depend on labor market prospects, so we also examine effect heterogeneity by pre-prison employment, race, and the number of terms an individual has served in prison.

Data

This study draws on data from parolees released from Michigan prisons in 2003 to a residence within the state and tracked for six years (24 calendar quarters following the release quarter). From the original 2003 parole cohort ($N = 11,064$), a one-third random sample was selected, resulting in an initial sample size of 3,689. The analytic sample was reduced to 3,673 after dropping 11 individuals for whom employment information was not available and five individuals who died in the quarter of release from prison.

The dataset combines records from four sources. First, quarterly earnings information was obtained from Michigan Unemployment Insurance (UI) records, which provided data on all formal employment reported to the state's unemployment insurance system by employers. Gross earnings for each individual were retrieved through a process that matched Social Security numbers and names between MDOC and UI records (see the appendix). Second, administrative data was retrieved from MDOC databases that contained measures spanning

the length of time each offender was on parole and also tracked all new entries into prison and new periods of parole or felony probation. These databases also provided longitudinal records of parole violations and sanctions for individuals under supervision. Third, arrest records were obtained from the Michigan State Police, which collects records on arrests from all police agencies in the state. Finally, county unemployment rates were retrieved from the Michigan Department of Technology, Management, and Budget (2009).

The data were formatted as a person-period dataset that tracks each parolee in the quarter of release from prison and for 24 quarters thereafter. Parolees who died were removed from the data starting in the quarter of death. Deaths were only recorded by MDOC when a parolee died while under MDOC custody or supervision, totaling 100 people (approximately 2.8 percent of the sample) during the observation period. The analytic sample was composed of 3,673 parolees who were observed for a total of 90,418 quarters.

Key Dependent and Independent Variables

Quarterly Earnings—We use total earnings from all employers in the formal labor market for all individuals in each of the 25 observed quarters. Values were adjusted for inflation to 2010 dollars.⁵ We focus on earnings rather than employment because our preferred models require a continuous outcome (see below) and because an earnings measure provides more variation than the binary employment measure.⁶

Custody—Custodial sanctions are defined as any spell in a jail or custodial facility while on parole, other than imprisonment. Different states have different names for such sanctions. In addition to jail stays, here we are analyzing what MDOC calls correctional centers, technical rule violator centers, detention centers, and residential reentry programs.⁷ All of these custodial sanctions have in common both their use as a punishment for prohibited behavior while on community supervision after prison and their custodial nature—confinement of the individual to the facility. As we discuss below, these sanctions are far shorter than imprisonment, ranging from a few days to a few months (a few weeks is most typical). We combine jail and other short-term custodial sanctions because jail is often used in place of other sanctions when such facilities are either full or not geographically proximate. Our conversations with MDOC staff suggest that even when a jail stay results from police arrest, it is often understood as a sufficient sanction by parole officers when a return to prison is not warranted, whether or not a violation is formally written up. Serious violations of parole that warrant re-imprisonment would result in an eventual transfer from jail to a state prison. Stays in jail that immediately precede imprisonment are coded as part of that prison term because almost everyone who is re-imprisoned is placed in jail beforehand, and therefore such jail stays are not short-term custodial sanctions. We have explored models that separate jail and non-jail short-term custodial sanctions. Because such

⁵As is conventional when analyzing earnings, we take the natural log of earnings to reduce the influence of high earners. To deal with person-quarters with zero earnings, we set such quarters to one dollar before logging. Our main results are not sensitive to this decision, as setting them to a higher value produces substantively similar results, despite the large number of quarters with zero earnings (table 1).

⁶Specifically, model 5 in table 3 and all the models in table 4 require a continuous outcome. We also estimated models 1–4 in table 3 as logit models of quarterly employment with substantively similar results.

⁷Periods of incarceration in county jails that did not occur during a period in which an individual was on parole or probation are not recorded in our data, but going to jail while not on community supervision cannot be a custodial sanction as we have defined it.

models produce effect estimates for jail and non-jail sanctions that are not statistically different from one another, our results below present models that combine the two into a single measure of short-term custodial sanctions. We also include in our models a separate measure of custody in state or federal prison.⁸

Control Variables

The analysis controls for time-varying characteristics that vary within individuals. *Quarter since first parole* captures the number of quarters that have passed since the original release from prison, where the first full quarter following release is coded as 1. This variable both controls for amount of time in the community and also acts as an indirect proxy for year. To account for seasonality in employment, we include dummies for *calendar quarters*, where quarter 1 (January–March) is the reference category. *Local unemployment* measures the time-varying unemployment rate of the county in which each parolee resided in each quarter. Five time-varying measures control for confounders of the relationship between custody and employment related to parole supervision, substance use, and offending behavior. These are measured in the lagged quarter in order to capture circumstances present before the measurement of earnings. *Number of positive substance abuse tests* is a count variable identifying the number of times a parolee tested positive for drugs or alcohol in each quarter. *Absconding status* is an indicator that identifies whether a parolee was reported as absconding at any point in the quarter. *Number of arrests* is a count variable identifying the number of arrests in each quarter. *Parole violation* indicates whether parolees received a violation in the quarter. A final time-varying measure captures each individual's *discharge status* at each quarter. Table 1 shows descriptive statistics for all variables.

Analytic Strategy

Our goal is to estimate the effect of custody in a short-term sanction facility on log quarterly earnings. The primary challenge in estimating such effects is confounding variables that produce a non-causal association between our explanatory variables and our outcome. Because we are working with panel data (repeated measures over time for each person of our explanatory variables and our outcomes), there are two types of confounders, time constant and time varying. Time-constant confounders are baseline differences across individuals who do and do not experience short-term custody that affect earnings, such as criminal history, experiences in prison, race, gender, substance abuse history, and so on. Time-varying confounders are post-release characteristics and experiences that affect both earnings and whether an individual experiences a custodial sanction as well as whether the same individual experiences them in one quarter rather than another. Examples are post-release substance use, technical violations, absconding, and arrests.

To control for time-constant confounders, we employ a fixed-effects estimation strategy that conditions on all time-constant person characteristics, both observed and unobserved, by removing all variation in the explanatory variables that is across individuals, leaving only within-person variation over time (Allison 2009). We estimate what Allison (2009) calls a

⁸We have also estimated models that measure the number of days in the calendar quarter during which an individual was in prison or in short-term custody. These models show similar patterns of associations between such custody and labor market earnings.

“hybrid” fixed-effects model. The hybrid model provides fixed-effects estimates by including in the model two versions of each time-varying variable, the person-mean across the whole panel and the deviation from the person-mean in each time period. The intuition is that the person-mean removes all time-constant between-person variation in the predictor, so that the coefficients on the mean-deviation variables can be interpreted as if they came from a conventional fixed-effects model that leverages within-person variation over time. A person-level random effect is also included in the model to account for the clustering of time periods within individuals and produce correct standard errors. The model can be written as follows:

$$Y_{it} = \beta_0 + \beta_1(X_{1i} - \bar{X}_{1i}) + \beta'_1\bar{X}_{1i} + \dots + \beta_k(X_{ki} - \bar{X}_{ki}) + \beta'_k\bar{X}_{ki} + v_i + \varepsilon_{it}, \quad (1)$$

where i indexes individuals, t indexes calendar quarters since release, k indexes time-varying X predictors, v_i is the person random effect, and ε_{it} is a time-varying error term capturing unexplained variance. Each predictor X is entered into the model twice, once as a person-mean across the panel (\bar{X}_{ki}) and once as a quarter-specific deviation from its mean ($X_{ki} - \bar{X}_{ki}$). There are therefore two different versions of each coefficient, β_k and β'_k . Our primary interest is in the coefficients on the quarter-specific deviations, β_k , which represent the association between each predictor, X_k , and earnings, after removing all confounders that do not vary over time. These are the coefficients displayed in the tables in the main text. Full models are provided in the appendix. Our model also includes controls for the passage of time entered in a cubic functional form, since the risk of experiencing imprisonment or short-term custody varies over time since release.⁹

Because the fixed-effects model only deals with time-constant confounding, we must also adjust for all time-varying confounders directly in the model in order to interpret β_k coefficients as causal. As discussed above, we control for events and experiences after release from prison that are the primary reasons for reincarceration or a custodial sanction. We emphasize that a key assumption of our modeling strategy is that we have controlled for all time-varying confounders; a time-varying common cause of both short-term custody and earnings that is not controlled will bias coefficients that estimate the effects of short-term custody.

Prior labor market success can also be a source of time-varying confounding because people with less stable employment histories and who had lower earnings when working may be more likely to engage in behavior that is a parole violation and may be more likely to receive custodial sanctions (or be sent back to prison) for a parole violation. Failing to control for prior earnings could negatively bias our effects of interest (assuming lagged earnings is positively correlated with current earnings but negatively correlated with custody), making the effects of short-term custody spells appear to be more negative than they really are. However, controlling for a lagged dependent variable in a fixed-effects model presents a

⁹The person-mean versions of the time variables also serve to control for differential attrition from the data due to death. Individuals who die (or die earlier than others) will have lower values on these variables.

special set of statistical problems. One cannot simply add a lagged dependent variable into a fixed-effects model as one would any other regressor because doing so violates the assumption that the regressor (lagged Y) is uncorrelated with the errors (Allison 2015; Angrist and Pischke 2009; Halaby 2004; Nickell 1981). To avoid this problem when controlling for lagged earnings, we implement a model developed by Bhargava and Sargan (1983) using a Stata implementation by Kripfganz (2015). This model relaxes the assumption of no correlation between the lagged outcome and the error term.¹⁰

Given that our models control for all time-constant confounding and the major sources of time-varying selection into short-term custodial sanctions, it is important to consider the potential sources of variation in these experiences that are driving the variation that our models leverage to estimate their effects. Based on our knowledge of parole supervision in Michigan, we believe that variation is being driven by three factors: (a) parole officer (supervisor) discretion in formally writing up a parolee for a violation and in determining a recommended sanction (see also Lin, Grattet, and Petersilia [2010]; Steen et al. [2013]), (b) geographic and temporal variation in the availability of different forms of sanctions for parole violations, and (c) geographic and temporal variability in law enforcement capacity to detect unlawful or technical violation behavior among parolees. Our modeling strategy assumes that these sources of variation are independent of an individual parolee's earnings in one quarter versus another, conditional on the controls in the model.

In addition to the assumption of no uncontrolled time-varying confounding, fixed-effects models also make what is called the "common trends assumption." This means that the temporal trajectories of the outcome among those who receive the treatment (custodial sanction) would be the same absent treatment as those of the subjects who do not receive the treatment. This is equivalent to saying that the individual fixed effects are constant over time. Sometimes conditioning on time-varying covariates is sufficient to make this assumption reasonable, but here we worry that those who experience a custodial sanction were on a downward earnings trajectory before they experienced these sanctions. Following Vaisey and Miles (2017), we relax this assumption by allowing the individual fixed effects to vary over time. In the context of our hybrid fixed-effects model, this is accomplished by interacting the person-specific means discussed above with the time variable itself (including its quadratic and cubic terms). As these interactions are not of direct substantive interest, they are displayed in the full models in the appendix.

Finally, in estimating the effects of custodial sanctions, we must distinguish between prison or custody sanction quarters and the quarters following these experiences. We must also allow the effects of these experiences to vary over time after release. Accordingly, we include four indicator variables, one for prison or short-term custodial sanction in the current

¹⁰Panel models with lagged dependent variables are termed "Dynamic Panel Data Models" in econometrics. Two other estimation strategies have been proposed for such models. One approach is to instrument for the lagged dependent variable using prior lags (Arellano and Bond 1991). In our case, the prior lags are unlikely to be valid instruments, as we would have to assume that earnings in one period are directly affected only by earnings in the prior period, denying the importance of longer-term work experience. Another solution estimates the fixed-effects model in a structural equation model framework, in which the correlation between the fixed effects and the lagged dependent variable can be estimated rather than constrained (i.e., assumed) to be zero (Bollen and Brand 2010). We experimented with the latter method, but due to the number of time periods and parameters, estimation was not computationally feasible.

quarter (quarter t) and lagged versions of this indicator to capture effects in the first, second, and third quarters following return to the community (referred to as prison or custody lag in quarter $t-1$, $t-2$, $t-3$).¹¹ An additional complication arises because an individual can experience both prison and short-term custody in the same quarter. In order to isolate the effects of one type of sanction (prison or short-term custody) during quarters in which the individual was not experiencing the other type, we include interaction terms for prison in quarter t with the short-term custody lags and interaction terms for short-term custody in time t and the prison lags. The presence of these interactions in the models makes the base terms interpretable as the effect of experiencing one type of punishment compared to experiencing neither punishment. This will also allow us to compare the effects of re-incarceration in prison with the effects of short-term custodial sanctions. Because the interaction terms are not of substantive interest, they are displayed in the full models in the appendix.

Results

Table 2 shows descriptive statistics on spells of custodial sanctions and new prison spells. Forty-five percent of our sample experienced a custodial sanction other than jail in the six years after their initial release from prison, with the mean individual in the sample experiencing 1.14 such custody spells. Sixty-three percent experienced a jail sanction spell while on parole, with the mean individual experiencing 1.96 jail spells. The durations of non-jail and jail custodial sanctions were similar, although non-jail sanctions can be longer. While the median for both is 14 days and the 25th percentile for both is 5 days, the 75th percentile is higher for non-jail sanctions (64 days) than for jail sanctions (38 days). These similarities help justify collapsing jail and non-jail custodial sanctions into a single short-term custody category for the remainder of the analysis. Seventy-two percent of the sample experienced either a jail or non-jail custodial sanction (or both). By comparison, half of the sample returned to prison at some point in the six years after release, with the mean individual returning 0.72 times. Prison stays were considerably longer, with the median spell being 499 days.

Figure 1 shows trends in mean quarterly earnings alongside trends in the proportion of parolees in prison and short-term custodial (STC) sanctions from the date of release through the 24th quarter after release. (Quarters with zero earnings are included in the mean.) Consistent with prior research on post-prison employment using administrative data (e.g., Pettit and Lyons 2007; Sabol 2007), mean earnings start out very low after prison but increased over the first year and a half after release, peaking at \$1,187 in the sixth quarter following release and declining thereafter, reaching \$690 at the 24th quarter after release. Short-term custodial sanctions also spike early in the period after release, with over 20 percent of our sample experiencing a custodial sanction in the quarter of release. Rates increase to 28 percent in the second full quarter after the release quarter and decline gradually after that. This early prevalence and peak of STC is consistent with STC being used as a sanction of first resort when technical violations occur. Incarceration in prison

¹¹When an individual is in prison or short-term custody for more than one consecutive quarter, the indicator for current prison or custody is 1 for all such quarters.

increases more gradually, with rates of incarceration peaking at about 28 percent at 2.5 years (10 quarters) following release. In sum, figure 1 shows (a) the poor labor market prospects of former prisoners and their decline following an initial peak, (b) high rates of experiencing STC, especially early in the period after release, and (c) high rates of return to prison, which tend to occur later.

Table 3 shows models of log quarterly earnings (here we show key coefficients—full models are in the appendix). Model 1 is a random-effects model with only our key independent variables: indicators for STC and three period lags. We also include parallel indicators for prison, which serve as a benchmark against which to calibrate the impacts of custodial sanctions. This model provides a baseline description of how both custodial sanctions and prison spells are associated with earnings. Because the outcome is logged quarterly earnings, the coefficients can be interpreted roughly as percent changes in earnings associated with unit changes in the independent variables. Hence, the coefficients on the prison and STC variables and their lags represent percent losses or gains in earnings relative to what earnings would have been had the individual not been in either prison or STC in that quarter or the three prior quarters.

Not surprisingly, experiencing prison or STC in the current quarter is associated with dramatically lower earnings. This association is larger for prison than STC, which probably reflects the difference in the amount of time parolees spent in prison versus short-term custody within a given quarter. In calendar quarters when parolees were in prison, they spent an average of 82 days in prison (median = 90 days), whereas in calendar quarters when parolees were in STC, they only spent an average of 38 days in STC (median = 30 days), leaving considerable time to potentially work in the formal labor market. The negative association between short-term custodial sanctions and earnings is also apparent in the first quarter after release from custody, when those recently released from short-term custody earned 17.9 percent less than those who had not been in any form of custody in the prior quarter. This gap diminishes and becomes non-significant in the second and third quarters after release. In contrast, there is no significant association between being re-imprisoned and earnings in the quarters following release from that re-imprisonment. In fact, in the second quarter after release, those who previously were re-imprisoned have 18 percent higher earnings compared to those who were not.

Models 2 through 5 in table 3 progressively add complexity to the model in our attempt to estimate more plausibly causal effects of re-incarceration in prison and short-term custody on earnings. Model 2 adds individual person fixed effects using the hybrid fixed-effects specification presented in equation 1. This model controls for all time-constant differences between people who do or do not experience re-incarceration in prison and do or do not experience short-term custody. Adding these fixed effects changes the coefficients only slightly, dampening the effect of prison in the current quarter, STC in the current and prior quarter, and slightly elevating the increase in earnings two quarters out from a return to prison.

Model 3 controls for the observed time-varying covariates that are predictors of experiencing STC.¹² Adding these controls does not change any of the results, with one exception: it

increases the negative association between having been released from prison in the prior quarter and earnings in the current quarter. However, this association becomes non-significant in model 4, which adds interactions between the fixed effects and time, relaxing the common trends assumption.

Model 5 is our preferred model because it controls for lagged log earnings, which we believe is an important source of selection into STC. In other words, association between custodial sanctions and earnings could be due not to effects of STC itself but to the fact that those with lower prior earnings are more likely to experience custody. Controlling for lagged earnings produces multiple important changes to the estimates. First, adjusting for prior earnings reduces the magnitude of the coefficients for prison in the current quarter and short-term custody in the current quarter. This reduction implies that people who experienced these two forms of custody would have performed relatively poorly in the labor market even had they not been in custody at that point in time. According to these estimates, prison is associated with a reduction in earnings of about 75 percent, and short-term custody is associated with a reduction in earnings of about 39 percent in quarters in which they are experienced. While the finding that prison and custodial sanctions reduce earnings should not be surprising, the magnitude of this association is interesting because it is implicitly based on what individuals would have earned had they not been in custody.

We see even larger changes in our estimates of the lagged effects of prison and short-term custody. The adjustment for prior earnings in model 5 reveals that compared to time periods when a person was not incarcerated, earnings were 57 percent greater in the quarter immediately after release from prison and 13 percent greater in the quarter after release from short-term custody. As discussed above, these positive but short-term impacts of custody might reflect the “cooling out” or motivational effects of custody, which might interrupt periods of intense drug use, pretty crime, or failure to comply with parole regulations, or might be interpreted by the parolee as a signal of the seriousness of parole supervision. Indeed, this is consistent with the purported purposes of short-term custody as a sanction. In the case of prison, such positive short-term effects could also be due to prisoner reentry programs.

Although there are no significant coefficients for prison in the second or third quarter after release, we find evidence that short-term custodial sanctions have a lagged effect: individuals who were in short-term custody experienced 12 percent lower earnings in their third quarter after release compared to time periods when they were not in custody. Since mean quarterly earnings (for all parolees and time periods in the sample) was about \$1,000 per month, this translates into an estimated earnings loss of roughly \$120 per quarter, a modest but potentially significant loss for a population with extremely low incomes. These associations also suggest that episodes of short-term custody for parole violations play a disruptive role in post-prison labor market outcomes beyond the incapacitation effect of the

¹²The negative association between discharge from parole and earnings may be surprising to some. We interpret this as a supervision effect stemming from monitoring of employment. Parolees may, for example, remain employed in low-wage or otherwise undesirable jobs in order to comply with parole regulations and earn discharge from parole (which in Michigan is usually granted at two years after release from prison for someone with no serious violations).

period of custody itself. If these estimates capture causal effects, any positive effects in the short term are counteracted by longer-term negative effects of such parole sanctions.

We conducted a supplementary analysis in which we added dummy variables to model 5 that mark one, two, and three quarters before a custodial sanction to check whether earnings are systematically lower in quarters leading up to a sanction (appendix table A3). Two of these dummies had positive coefficients, and none were statistically significant. Moreover, their addition did not change the primary coefficients in the model. This indicates that individuals who experienced a custodial sanction were not on a downward earnings trajectory before the custodial sanction that is not already being accounted for in the model.

Subgroup Analyses

Table 4 repeats this analysis stratifying by pre-prison employment, race, and whether the individual was initially released from a first prison spell or had been in prison multiple times before (first-time vs. repeat prisoners). We present our preferred model for each subgroup (model 5 from table 3). All subgroups exhibit significantly lower earnings in the quarter they were in custody (prison or STC) and significantly higher earnings in the quarter following prison. Our hypothesis that those with the best labor market prospects would be most negatively affected by STC was only consistently supported when stratifying by the most direct measure of labor market prospects, employment in the year prior to the initial prison spell. Those who were not employed prior to the prison spell are estimated to have experienced a large and statistically significant gain in earnings in the quarter following STC but a small and statistically insignificant earnings loss in the third quarter following STC. In contrast, those with pre-prison employment are estimated to have experienced no earnings gain in the first quarter following STC and a large earnings loss in the third quarter following STC. Both of these differences by pre-prison employment are statistically significant. However, none of the black-white differences or the differences between firsttime and repeat prisoners are statistically significant, suggesting that one's criminal history and race are not as salient as prior employment in determining who has the most to lose from STC in the long term.

Discussion and Conclusion

Although the labor market consequences of imprisonment have been central to the literature on punishment and inequality, other components of the criminal justice system have received less attention from sociologists. In particular, the rise of mass incarceration was accompanied by an even larger increase in community supervision. Given the high rates of parole after release from prison, any effects of incarceration in prison and its collateral consequences for families and communities may be closely tied to the experience of parole. Furthermore, understanding the consequences of the growth of punishment and formal social control through the carceral state requires understanding the effects of more than just imprisonment. We have examined the labor market effects of one frequently experienced aspect of parole, the high risk of being placed in short-term custody. Although such custodial sanctions are viewed as an alternative to returning parole violators to prison, they

also have the potential to adversely affect earnings through stigma amplification, disruption of employment, fraying of human and social capital, incapacitation, and legal cynicism.

We estimate that experiencing a short-term custodial sanction lowers earnings in the formal labor market by approximately 37 percent in the quarter of custody. Although our estimates suggest earnings tend to increase in the quarter immediately following the sanction—consistent with the stated intentions of such sanctions—the associations turn negative in the longer term. In the third quarter following a sanction, earnings are lower by about 13 percent than they would have been had the individual not experienced the sanction. The loss of earnings attributable to short-term custody was more acute among parolees employed in the formal labor market before their initial incarceration. We caution that although these effects are statistically significant and large in percentage terms, they represent relatively small changes to earnings in absolute terms, since mean quarterly earnings were slightly under \$1,000. These earnings differences could be driven by either lower probability of employment or lower pay conditional on employment (or both), mechanisms that should be addressed in future research.

In addition, our analysis examines the labor market effects of re-incarceration in prison for parolees, estimating similar but larger incapacitation effects and positive effects on earnings in the quarter after re-release. However, we see no evidence for longer-term earnings consequences of re-incarceration in prison, a finding that we interpret as a product of the higher levels of criminal involvement and generally lower labor market prospects of individuals subject to return to prison. We caution that these prison estimates are not directly comparable to previous estimates of the effects of incarceration on employment or earnings in the existing literature. First, we are only estimating the effects of incarceration on those who have been incarcerated before and then re-incarcerated, a more selective group that may have particularly poor labor market prospects even in the absence of imprisonment. Second, we are unable to follow our sample for as long as prior research using longitudinal survey data, and we are only able to capture formal employment as recorded in the unemployment insurance system. This shorter follow-up means we are also only examining the post-release effects of shorter prison terms. Those with longer prison terms following a return to prison in our data are not followed long enough to contribute to our estimates of earnings after re-release.

Why might custodial sanctions have longer-term detrimental effects on earnings while re-incarceration in prison seems to have no long-term effect? One possible explanation is that different individuals are experiencing these two types of custody, with higher-risk or more serious parole violators being returned to prison. This suggests that those assigned to short-term custody would have had higher earnings in the absence of their sanction. In other words, they had more to lose by being placed in custody. This explanation would be consistent with a possible net-widening effect of parole sanctions in which the availability of such sanctions provided opportunities for officials to apply them to individuals who might otherwise not have received any form of custody.

We remind the reader of the limitations of this study. First, our data come from a single state, and criminal justice policies and institutions vary between states and over time. Second, in

order to ensure adequate statistical power, our analysis collapsed different types of short-term custodial sanctions, potentially glossing over differences of either magnitude or direction in the effects of different types of sanctions. Third, as a non-experimental study, we must be cautious in making strong causal statements. If there are important time-varying covariates that predict both the experience of a short-term custodial sanction and future earnings, such as motivation to work or illegal earnings, then our estimates have the potential to either over- or understate the effects of such sanctions. Fourth, our use of UI data to measure earnings requires us to use a fairly course unit of time in our analysis, calendar quarter, and limits the analysis to formal employment. Custodial sanctions may affect informal and formal earnings differently. Earnings of formerly incarcerated people calculated from UI data are considerably lower than that from self-reported survey data (Grogger 1995). If informal earnings were less affected by custodial sanctions, then focusing on formal earnings would bias our results negatively. On the other hand, earnings in the formal labor market are an important marker of integration into mainstream institutions, a focal concern of research on prisoner reentry. Fifth, our data do not allow us to measure non-custodial sanctions, probably the ideal comparison for custodial sanctions. Future research should examine non-custodial sanctions in comparison to custodial sanctions. Sixth, we are unable to directly assess the mechanisms through which our estimated effects operate, an important topic for future research. Finally, our window of post-release observation is relatively short. Future research should examine the long-term consequences of custodial parole sanctions.

Nevertheless, we believe our study demonstrates the importance of examining experiences during the period after release for understanding the labor market outcomes of former prisoners, and—more broadly—those involved in the wider criminal justice system. Their outcomes are not a product of simply their preprison disadvantages or imprisonment itself but also of the contexts and institutions in which they find themselves enmeshed after prison. For those on parole, those institutions may be particularly focused on social control, surveillance, and further punishment. Our study examines only one such institution, parole and its system of custodial sanctions, for one outcome, earnings, but our findings suggest that future research should focus greater attention on the post-release period. For example, non-custodial sanctions such as house arrest, electronic monitoring, or more intensive supervision are also important topics for future research, and effects of custodial sanctions in other domains, such as health, social support, and housing stability, should also be investigated. Our study also suggests that productive points of intervention to improve the life prospects of those involved in the criminal justice system through policy changes, social welfare, or social services interventions may be found not just in the prison itself but also after release. In particular, our results suggest that more judicious use of short-term custodial sanctions could improve the labor market outcomes of some former prisoners.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Biographies

David J. Harding is Associate Professor of Sociology at the University of California, Berkeley. His research interests include poverty and inequality, culture, neighborhood effects, causal inference, mixed methods, prisoner reentry, and the effects of criminal justice contact. His recent work has appeared in the *Annual Review of Sociology*, *American Journal of Cultural Sociology*, and *Social Science Research*.

Jonah Siegel is the Research Director of the Michigan Indigent Defense Commission, a statewide agency tasked with reforming indigent defense, where he is responsible for identifying research priorities, overseeing data collection and analysis, and translating findings into best practices. His research interests revolve around the accessibility of justice and local policy development. Siegel's recent research has appeared in the *Ohio State Journal of Criminal Law* and *Journal of Substance Abuse Treatment*.

Jeffrey D. Morenoff is Professor of Sociology and Public Policy, Research Professor at the Institute for Social Research, and Director of the Population Studies Center at the University of Michigan. His research interests include urban sociology, health, criminology, criminal justice, and social stratification. His recent work has appeared in the *Annual Review of Sociology*, *American Journal of Epidemiology*, and *Russell Sage Foundation Journal of the Social Sciences*.

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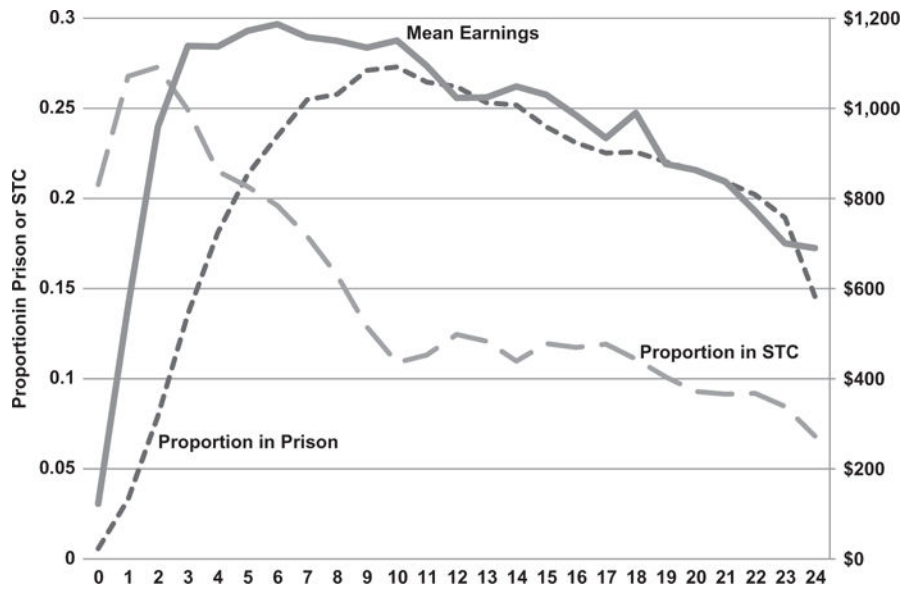


Figure 1. Mean earnings and proportion in prison and STC by quarter since release

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Table 1Descriptive Statistics ($n = 90,418$ person-quarters)

	Mean	SD	Min	Max
Employed	0.23	0.42	0	1
Earnings	949.40	2,695.92	0	150,978.00
Ln Earnings	1.75	3.29	0	11.9249
Custody variables				
Prison at quarter t	0.20	0.40	0	1
Prison at quarter $t-1$	0.20	0.40	0	1
Prison at quarter $t-2$	0.02	0.13	0	1
Prison at quarter $t-3$	0.02	0.12	0	1
STC at quarter t	0.15	0.35	0	1
STC at quarter $t-1$	0.06	0.25	0	1
STC at quarter $t-2$	0.05	0.22	0	1
STC at quarter $t-3$	0.04	0.21	0	1
Time-varying variables				
Quarter 1 (Jan–March)	0.25	0.43	0	1
Quarter 2 (April–June)	0.25	0.43	0	1
Quarter 3 (Jul–Sep)	0.25	0.43	0	1
Quarter 4 (Oct–Dec)	0.25	0.43	0	1
County unemployment rate	8.12	2.49	2.6	26.3
Abscond lag	0.10	0.29	0	1
Positive substance abuse test lag	0.04	0.19	0	1
Arrested lag	0.05	0.21	0	1
Discharged	0.36	0.48	0	1
Parole violation lag	0.10	0.30	0	1

Descriptive Statistics on Custodial Sanctions and New Prison Spells

Table 2

	Ever experienced	Mean number of custody spells	Custody duration (days)		
			Median	25th pctile	75th pctile
Custodial sanctions other than jail	45 percent	1.14	14	5	64
Jail custodial sanctions	63 percent	1.96	14	5	38
All custodial sanctions	72 percent	3.09	14	5	46
Prison	50 percent	0.72	499	276	849

Table 3

Panel Models of Ln Quarterly Earnings ($n = 3,673$ individuals, 90,418 person-quarters)

	(1)	(2)	(3)	(4)	(5)
	Basic RE	Hybrid FE	M2 + time-varying	M3 + time interactions	M4 + lagged DV
Custody variables					
Prison at quarter t	-1.712*** (0.057)	-1.646*** (0.060)	-1.862*** (0.063)	-1.682*** (0.066)	-0.749*** (0.035)
Prison at quarter $t-1$	-0.090 (0.086)	-0.033 (0.086)	-0.256** (0.086)	-0.097 (0.089)	0.574*** (0.075)
Prison at quarter $t-2$	0.182* (0.091)	0.233* (0.090)	0.047 (0.090)	0.131 (0.090)	0.119 (0.073)
Prison at quarter $t-3$	0.043 (0.090)	0.089 (0.089)	-0.076 (0.088)	-0.014 (0.087)	-0.017 (0.077)
STC at quarter t	-0.730*** (0.043)	-0.674*** (0.045)	-0.655*** (0.046)	-0.603*** (0.046)	-0.386*** (0.032)
STC at quarter $t-1$	-0.179*** (0.054)	-0.131* (0.054)	-0.176** (0.056)	-0.128* (0.056)	0.132** (0.044)
STC at quarter $t-2$	-0.035 (0.060)	0.009 (0.061)	-0.090 (0.060)	-0.056 (0.060)	-0.012 (0.048)
STC at quarter $t-3$	-0.074 (0.064)	-0.034 (0.064)	-0.109 (0.063)	-0.097 (0.063)	-0.121* (0.052)
Time-varying variables					
Quarter 2 (April-June)			-0.077*** (0.019)	-0.074*** (0.019)	0.109*** (0.022)
Quarter 3 (Jul-Sep)			0.138*** (0.020)	0.139*** (0.020)	0.192*** (0.022)
Quarter 4 (Oct-Dec)			0.044* (0.021)	0.050* (0.021)	0.166*** (0.023)
County unemployment rate (centered)			-0.095*** (0.010)	-0.093*** (0.010)	-0.024*** (0.006)
Absconding lag			-0.637*** (0.042)	-0.586*** (0.041)	-0.255*** (0.030)
Positive substance abuse lag			0.066 (0.050)	0.057 (0.050)	-0.067 (0.043)

	(1)	(2)	(3)	(4)	(5)
	Basic RE	Hybrid FE	M2 + time-varying	M3 + time interactions	M4 + lagged DV
Arrest lag			-0.038 (0.041)	-0.034 (0.041)	-0.104** (0.037)
Discharged			-0.597*** (0.056)	-0.566*** (0.060)	-0.337*** (0.035)
Parole violation lag			0.015 (0.034)	0.006 (0.034)	-0.292*** (0.032)
Lagged Ln quarterly earnings					0.559*** (0.006)

Note:

*** $p < 0.001$

** $p < 0.01$

* $p < 0.05$;

robust standard errors in parentheses; Full model results reported in the appendix.

Table 4
 Panel Models of Ln Quarterly Earnings, Stratified by Pre-Prison Employment, Race, and First Time in Prison

	Pre-prison employment			Race			Times in prison		
	No	Yes		White	Black		First	>First	
Custody variables									
Prison at quarter t	-0.657*** (0.040)	-1.067*** (0.104)		-0.992*** (0.059)	-0.563*** (0.041)		-0.700*** (0.060)	-0.755*** (0.044)	
Prison at quarter $t-1$	0.640*** (0.087)	0.440* (0.211)		0.890*** (0.132)	0.328*** (0.087)		0.598*** (0.140)	0.565*** (0.089)	
Prison at quarter $t-2$	0.120 (0.084)	0.234 (0.213)		0.205 (0.130)	0.038 (0.085)		0.113 (0.129)	0.126 (0.089)	
Prison at quarter $t-3$	0.025 (0.088)	-0.049 (0.194)		-0.148 (0.131)	0.101 (0.092)		-0.128 (0.121)	0.044 (0.098)	
STC at quarter t	-0.324*** (0.036)	-0.568*** (0.091)		-0.454*** (0.057)	-0.339*** (0.037)		-0.400*** (0.049)	-0.373*** (0.041)	
STC at quarter $t-1$	0.247*** (0.050)	-0.190 (0.130)		0.191* (0.078)	0.087 (0.053)		0.115 (0.066)	0.149* (0.060)	
STC at quarter $t-2$	0.012 (0.053)	0.007 (0.155)		0.034 (0.082)	-0.060 (0.058)		-0.008 (0.069)	-0.012 (0.068)	
STC at quarter $t-3$	-0.063 (0.060)	-0.303* (0.141)		-0.084 (0.088)	-0.150* (0.064)		-0.106 (0.072)	-0.134 (0.077)	
Time-varying variables									
Quarter 2 (April-June)	0.088*** (0.025)	0.072 (0.062)		0.100*** (0.037)	0.100*** (0.027)		0.142*** (0.035)	0.078** (0.028)	
Quarter 3 (Jul-Sep)	0.180*** (0.025)	0.127* (0.063)		0.191*** (0.036)	0.171*** (0.027)		0.213*** (0.035)	0.174*** (0.028)	
Quarter 4 (Oct-Dec)	0.148*** (0.026)	0.124 (0.068)		0.169*** (0.038)	0.152*** (0.029)		0.183*** (0.035)	0.150*** (0.030)	
County unemployment rate (centered)	-0.024*** (0.007)	-0.018 (0.016)		-0.035*** (0.009)	-0.015 (0.008)		-0.031*** (0.009)	-0.018* (0.008)	
Abscending lag	-0.245*** (0.033)	-0.350*** (0.091)		-0.325*** (0.059)	-0.227*** (0.034)		-0.266*** (0.052)	-0.251*** (0.037)	
Positive substance abuse lag	-0.084 (0.050)	0.048 (0.124)		0.016 (0.082)	-0.096 (0.050)		-0.121 (0.073)	-0.032 (0.053)	

	Pre-prison employment		Race			Times in prison	
	No	Yes	White	Black	First	>First	
Arrest lag	-0.115** (0.041)	-0.161 (0.114)	-0.176* (0.069)	-0.046 (0.041)	-0.075 (0.059)	-0.124** (0.047)	
Discharged	-0.289*** (0.040)	-0.462*** (0.104)	-0.369*** (0.055)	-0.301*** (0.045)	-0.333*** (0.048)	-0.345*** (0.050)	
Parole violation lag	-0.274*** (0.036)	-0.376*** (0.088)	-0.429*** (0.058)	-0.181*** (0.036)	-0.264*** (0.058)	-0.311*** (0.037)	
Lagged Ln quarterly earnings	0.548*** (0.007)	0.558*** (0.014)	0.560*** (0.008)	0.557*** (0.009)	0.558*** (0.008)	0.561*** (0.009)	

Note:

*** $p < 0.001$

** $p < 0.01$

* $p < 0.05$;

robust standard errors in parentheses; Full model results reported in the appendix.