Criminal Justice Contact, Labor Force Participation, and Employment Among Young Adults

Naomi F. Sugie
University of California, Irvine

Kristine Chan
University of California, Irvine

Please direct all correspondence to: Naomi F. Sugie
nsugie@uci.edu

In the United States, the criminal justice system's expansive reach has made it a consequential labor market institution (Western and Beckett 1999). Prior scholarship has demonstrated that the criminal justice system—and specifically, incarceration in prison—influences the labor market in two ways. First, the removal of people from labor force estimates impacts the calculation of unemployment rates and the perceived health of the US's labor market (Pettit 2012; Western and Pettit 2005). Second, incarceration in prison is related to long-term unemployment, lower wages, and lower lifetime earnings after release (Holzer et al. 2005; Western 2002). Because criminal justice contact is concentrated among people with low income and racial/ethnic minorities, the dual roles of the criminal justice system diminish the appearance of labor market inequalities in the short term while exacerbating long-term disparities (Western and Beckett 1999).

Although the expansion of incarceration in prison has received the most scholarly attention (e.g., see discussion in Turney and Wakefield 2019), particularly with respect to its labor market consequences, prison incarceration is not the most frequent type of criminal justice contact nor is it necessarily the most long-lasting form of criminal justice interaction. As of 2016, an estimated 1.5 million adults were in prison and 4.5 million adults were under community supervision (e.g., probation and parole) (Kaeble and Cowhig 2018). At year-end 2016, approximately 740,000 adults were in local jails; however, this number does not capture the total annual admissions into local jails and it is estimated that over 12 million people are admitted to jail annually (Subramanian et al. 2015; Turney and Conner 2018). The prevalence of arrest is also high. Recent statistics indicate that 12.2 million individuals are arrested annually (U.S. Department of Justice 2013), and by age 23, an estimated 30 to 41% of individuals will have experienced at least one arrest (Brame et al. 2012). Given the expansive reach of these various forms of criminal justice contact, focusing on incarceration in prison very likely underestimates the full implications of the criminal justice system for labor market outcomes.

Using contemporary, nationally representative longitudinal survey data from the National Longitudinal Survey of Youth 1997, with sequence analysis and regression methods, this paper examines how various forms of criminal justice contact, including arrest and incarceration in jail and prison, are associated with labor market participation and a variety of employment outcomes, including weeks worked, wage, job satisfaction, and shift scheduling. Specifically, the paper examines the stability of labor market participation and employment over time using sequence analysis methods. It estimates monthly and yearly associations among arrest, incarceration in jails and prisons, labor market nonparticipation, and various employment outcomes using fixed-effects regression analyses. It also uses propensity score matching models to estimate longer-term associations (from the late 20s to mid 30s) between criminal justice contact, labor market participation, and employment outcomes. The findings contribute to growing scholarship on lower-level forms of criminal justice contact (e.g., arrest) and employment by examining associations at various timeframes (e.g., monthly, yearly, and cumulative measures from the late 20s to mid 30s) and by broadening the scope of inquiry to consider labor force participation and aspects of employment (e.g., job satisfaction, shift scheduling) that reflect contemporary secular trends toward labor market polarization, precariousness, and poor quality employment (Kalleberg 2011).

CRIMINAL JUSTICE CONTACT AND EMPLOYMENT

Incarceration in Prison

Most research on criminal justice contact and employment focuses on the consequences of incarceration in prison. Studies based on survey data and administrative records generally find that employment, wages, and earnings in the formal market all decline after incarceration (see National Research Council 2014 for a review; exceptions to these findings include Kling 2006; Lalonde and Cho 2008; Loeffler 2013). Rates of formal employment after incarceration are low, ranging from 40 percent to 64 percent in the first year after release from prison (Pettit and Lyons 2007; Sabol 2007; Tyler and Kling 2007). These rates are based on any formal labor market participation; when duration and stability are considered, the employment situation of individuals recently released from prison is particularly dire. A reentry survey study found that only 10 percent of individuals were employed at both three and eight months after release from prison (Visher and Kachnowski 2007), and a recent reentry study using smartphones to capture self-reported employment (including off the books and self employment) found that less than 20 percent of individuals worked at least five or more consecutive days within the first three months after release from prison (Sugie 2018).

Why is incarceration in prison associated with lower rates of labor market participation and poor employment outcomes after release? In addition to the labor market obstacles that poor and less educated (e.g., less than a high school degree) individuals confront more generally, such as spatial segregation, changing industries, and low educational attainment, individuals face a variety of employment barriers related to their incarceration and conviction record. These include state-level restrictions on entry into certain occupations and professions (Stafford 2006) and intense stigma against hiring by employers (Holzer et al. 2006; Pager 2007; Pager et al. 2009; Stoll and Bushway 2008). Individuals with recent incarcerations in prison also may experience "transformative effects," or the erosion of job skills due to their time incarcerated (Pager 2007; Western 2002). All of these reasons are thought to contribute to poor employment outcomes for people recently incarcerated in prison.

Other Forms of Criminal Justice Contact

Like incarceration in prison, other types of criminal justice contact—specifically, arrest and incarceration in jail[1]—are likely consequential for both short-term and long-term *labor force nonparticipation* and *labor market outcomes*. In terms of nonparticipation, prior research has examined the labor force consequences of removing people from labor force counts due to incapacitation in prison (Pettit 2012; Western and Beckett 1999). However, people reentering their communities from jail also must contend with employer aversion related to their record (whether an arrest record or a conviction), which might lead to discouragement and premature exit from the labor force due to frustration with job search (Pager 2007; Pager et al. 2009; Uggen et al. 2014). Individuals who have been arrested—but not incarcerated or under supervision—may also be more likely to leave the labor force, even if temporarily, due to mental health issues (Sugie and Turney 2017), bureaucratic requirements of attending court and fulfilling other court-imposed obligations such as community service and specialized programs (Kohler-Hausmann 2013; 2018; Sullivan 1989), and anticipatory stress, or uncertainty related to any future criminal justice sanction, such as time in prison (Kohler-Hausmann 2013; Sugie and Turney 2017). For example, Sullivan finds that some young people decide to leave the labor force temporarily in order to attend court hearings and complete other bureaucratic obligations (1989), and Kohler-Hausmann describes how court-imposed sanctions following an arrest can impede a person's ability to search for work and maintain employment (2018).

Arrest and incarceration in jail might also affect longer-term labor market outcomes, such as stable employment and job quality. As with labor market nonparticipation, the stigma of an arrest record makes it difficult to find employment, leading job seekers to take on more

[1] Supervision on probation and parole is also related to labor force participation and employment; however, we are unable to focus on supervision in this paper due to data limitations in the NLSY97.

precarious and poorer quality work (Uggen et al. 2014). Because an arrest can also result in short term nonparticipation, as mentioned, individuals may have spotty work histories with unexplained gaps of time without employment, making them less competitive for jobs in the longer term. Empirically, a study by Grogger (1995) examined the labor market consequences of arrest on employment rates and earnings. Using longitudinal administrative data from California on arrests and unemployment insurance (UI) earnings, Grogger found that an arrest is associated with a modest loss of earnings, corresponding to a decrease of 4 percent in the time period concurrent to arrest, and 2 to 3 percent for approximately a year following arrest. More recently, Apel and Powell (2019) used NLSY97 data to estimate longer-term associations between arrest and wages. Specifically, they examine whether an arrest was related to wages in the last observed survey round for respondents, which typically occurred 13 years after the arrest. Using this timeframe, along with a research design that compares siblings, they find no associations between arrest and wages.

Although the experience of jail incarceration is often very different from prison incarceration (Turney and Conner 2018), incarceration in jail can entail many of the same employment obstacles that affect people recently incarcerated in prison, both in the short and long term. Leaving jail and reentering the community can be a stressful transition, in which people need to not only find employment but also locate housing, address health and substance use issues, and reestablish relationships with friends and family. The disruption in employment history and the need to find new work both also contribute to poorer employment outcomes in the longer term, similar to the consequences of arrest described above. A survey study of young men and women who were incarcerated in New York City's jails found that only 34 percent of young men and 27 percent of young women were employed in the formal labor market one year after release (Freudenberg et al. 2005). Another study, using administrative records and exploiting random assignment to judges, found that pretrial detention is related to lower formal labor market employment rates and less income among the very poor (Dobbie et al. 2018). Because spells of jail incarceration are generally shorter than prison incarceration, it may be that associations between jail and labor market outcomes are weaker, even if consequential, compared to those incarcerated in prison (Turney and Conner 2018).

SELECTION INTO CRIMINAL JUSTICE CONTACT

Because criminal justice contact is not random, any observed relationships between contact and labor market outcomes may result from unobserved factors associated with both criminal justice contact and employment. It is well documented that people who experience incarceration in prison often have not completed high school, score lower on literacy and cognitive tests, and are more likely to have impulsive behavior than those who have not experienced incarceration in prison (National Research Council 2014; Travis 2005). They also come from and return to neighborhoods with high rates of poverty, high rates of concentrated disadvantage, and scarce local job opportunities (Crutchfield 2014; Harding et al. 2013; Sampson and Loeffler 2010; Wilson 1996). All of these factors are correlated with incarceration in prison, and are likely similarly related to other forms of criminal justice contact. They are also associated with labor force participation and employment outcomes, leading to a potentially spurious relationship between criminal justice contact, labor force participation, and employment. To help address selection issues, we use fixed-effects regression models with longitudinal panel data, which improves upon cross sectional methods, by controlling for observed time-varying measures and unobserved time-stable factors. We also estimate a series of propensity score matching models, in order to predict the likelihood of experiencing arrest or incarceration based on a set of observed covariates.

In addition to selection issues, it is possible that any documented association is due to reverse causality, such that labor force nonparticipation and poor employment outcomes result in criminal justice contact (e.g., Crutchfield 2014; Fagan and Freeman 1999). Our use of short-term fixed effects analyses that consider monthly and yearly measures of criminal justice contact and employment helps with this concern about time ordering, but does not eliminate the possibility of reverse causality. Moreover, our propensity score matching approach is based on explicitly defined time periods, in which the covariates to predict arrest or incarceration predate the treatment (arrest/incarceration) and the labor force and employment outcomes occur in time periods following the treatment.

RESEARCH QUESTIONS

Using nationally representative, longitudinal survey data of young adults in the United States, we present results from sequence analysis methods, fixed-effects regression models, and propensity score matching models to answer the following questions:

R1. What are the experiences of criminal justice contact (arrest and incarceration in jail and prison), labor force participation, and employment over time among young adults ages 18 to 36?

R2. How is criminal justice contact associated with monthly and yearly changes in labor force participation and labor market experiences?

R3. How is criminal justice contact in early life (by age 27) associated with subsequent labor force participation and labor market experiences from age 28 to 36?

DATA, MEASURES, AND METHODS

Data

The National Longitudinal Survey of Youth 1997 (NLSY97) is a panel dataset of 8,984 youth between the ages of 12 and 16 on December 31, 1996. The data include a nationally representative sample of 6,748 youth and an oversample of 2,236 Hispanic and non-Hispanic black youth. Beginning in 1997, participants were interviewed annually, and starting in 2011, participants were interviewed every two years. Our primary analytic sample considers information for respondents who are 18 years old until the last observed survey round, in which the oldest respondents are 36 years old. In the models that we present in this paper, we exclude cases that are missing information on the key dependent variables and we multiply impute missing data in the covariates using the Amelia package in R (Honaker, King, and Blackwell 2011). This corresponds to an analytic sample of 8,818 respondents (or 98% of the original NLSY97 cohort).

The NLSY97 is well suited to answer questions about criminal justice contact, labor force participation, and labor market outcomes for a variety of reasons. First, the NLSY97 is the only nationally representative dataset with detailed (weekly and monthly) information on labor force status, employment, and criminal justice contact. Second, it is a longitudinal dataset, which facilitates precise time ordering and the use of fixed-effects models that take into account within-person changes in criminal justice contact and employment. Third, it is a relatively contemporary dataset, which reflects the US context of criminal justice system expansion and increasing precariousness in the labor market.

Measures

Labor force nonparticipation. Weekly information on labor force nonparticipation is collected for each respondent. These data are aggregated to monthly measures for some analyses (e.g., research questions R1 and R2 above) to capture any period of labor force nonparticipation in that month. In other analyses (e.g., R3), they are aggregated to measure the total months that the respondent experienced any labor force nonparticipation.

For some analyses (e.g., R1), we further distinguish labor force nonparticipation by whether the respondent was enrolled in school during the month. This enables us to differentiate nonparticipation related to school enrollment and nonparticipation that reflects the lack of engagement with both employment and school.

To capture instability in labor force nonparticipation, we also measure variation in weekly labor force nonparticipation for a given month by constructing the standard deviation of weekly nonparticipation. In some analyses (e.g., R3), we take the mean of monthly measures of variation in nonparticipation to measure instability in nonparticipation over time.

Employment outcomes. Weekly information on employment status is collected for each respondent. People who report that they are working for an employer, freelancers, self-employed, and in the military are coded as working. These data are aggregated into monthly measures to measure the average number of weeks employed in the given month. In some analyses (R3), we calculate the total of the average monthly measures to determine total average number of weeks employed over time.

For those that are working for an employer, additional information is collected in the NLSY97 to measure wage, job satisfaction, and shift schedule, among other job characteristics. We aggregate the weekly information on wage to calculate an average monthly wage. In terms of job satisfaction, respondents report how they feel about each job that they worked in the past year (dislike it very much = 1, like it very much = 5), where higher values indicate higher job satisfaction. This information is then combined with weekly reports about whether they worked that particular job in the focal week and we take the monthly average of these weekly measures. For shift schedule, respondents are asked what type of shift they work for each job (e.g., regular day shift, regular other shift, irregular shift, and other/weekends). We combine these answers with weekly reports about whether they worked that job in the focal week and we code people as working a regular day shift, as opposed to all of the other categories, if they worked at least one regular day shift job in the month. In some analyses (e.g., R3), we take an average of these monthly measures of wage, job satisfaction, and shift schedule over time.

Criminal justice contact. Monthly information on criminal justice contact is collected for arrest and incarceration in jail or prison post-conviction. Beginning in 2004, NLSY97 surveyors also reported whether the interviewee was in jail or prison at the time of the survey. Consequently, starting in 2004, the incarceration measure reflects both imprisonments after conviction and incarcerations (including pretrial) that occur during an interview.

Other covariates. We include a variety of covariates, in order to adjust for time-varying factors that are associated with selection into criminal justice contact, labor force participation, and labor market outcomes. At a monthly level, these include demographic information (e.g., marital status, number of children), school enrollment (=1 if enrolled in the focal month), and receipt of welfare benefits (including unemployment, cash assistance, food stamps, WIC, and other benefits; we take the sum of these benefit types). Models based on person-year data use the monthly covariates, including a school enrollment variable that is the sum of months in school, as well as a variable measuring educational attainment (e.g., less than high school, high school degree/GED, some college, and 4-year college degree and above). All of these characteristics are related to labor force participation and labor market outcomes, as well as criminal justice contact. For example, being married or cohabiting is not only positively related to employment but it is also negatively related to the likelihood of criminal offending (e.g., Gottlieb and Sugie 2018; Sampson and Laub 1995; Skardhamar et al. 2015). School enrollment is likely to be positively associated with labor force nonparticipation and negatively associated with being employed, as well as negatively related to criminal justice contact. Receipt of welfare benefits, such as unemployment insurance benefits and cash assistance is also likely to be positively related to labor force nonparticipation and negatively related to being employed. Because fixed effects regression models consider only time-varying covariates, we do not include time-stable characteristics in the models (see Methods below for more details).

In some analyses (R3), we use a more extensive set of covariates to predict whether a person experiences an arrest or incarceration by age 27 (see Methods below). These variables are all measured at age 18 or earlier, in order to ensure that they are measured prior to the possible occurrence of the treatment (in this case, arrest or incarceration from age 19 through age 27). These variables include gender, race/ethnicity (e.g., non-Hispanic/Non-Black, non-Hispanic Black, Hispanic, and Mixed Race), marital status (e.g., single, cohabiting, married), number of children, any criminal offending, educational attainment, any use of hard drugs (e.g., cocaine, crack, heroin), receipt of cash assistance, receipt of food stamps, and parental educational attainment (measured by the highest attainment of either mother or father).

Methods

The analytic strategy is comprised of three stages. For the first stage (R1), we use sequence analysis (SA) methods to provide descriptive information on the experiences of criminal justice contact, labor force participation, and employment over time for the NLSY97 cohort. SA methods are descriptive approaches to study the ordering of events or states as they unfold over time (Abbott 1995; Cornwell 2015). In this paper, we examine how monthly measures of criminal justice contact (arrest and incarceration), labor force participation (nonparticipation/in school, nonparticipation/not in school), and labor market outcomes (working, not working) are patterned over time using *state distribution plots*. State distribution plots describe the monthly states—e.g., working, not working, out of the labor force/in school, out of the labor force/not in school, arrested, and incarcerated—for the entire sample over time (e.g., 18 to 36 years old). Using these trajectories of experiences, we also assess instability and precariousness in the trajectories themselves using *transition rates*, or the amount of month-to-month instability in experiences of criminal justice contact, labor market participation, and employment.

We combine trajectories with cluster analysis methods to produce *typologies* of experiences over time. To carry out the cluster analyses, we restrict the sample to respondents with complete data from 18 to 31 years old (as opposed to the full sample, which has information for some people until age 36 years).[2] This restriction results in an analytic sample of 6,943 people or 79% of the full sample. We then assess sequences in terms of their dissimilarity to each other by assigning costs for actions, such as insertions, deletions, and substitutions of states, which change sequences to be more similar to each other. There are a variety of methods to assign costs to these changes, and we considered three commonly used methods. Optimal matching with constant costs (OM) is the most frequently used method with sequence analysis (Abbott and Forrest 1986; Elzinga 2007), and it assigns the same costs for insertions, deletions,

[2] Analyses that use sequences of varying lengths can lead to clusters based on sequence length as opposed to clusters based on intra-sequence changes in states, such as criminal justice contact, labor market participation, and employment.

and substitutions of states. Optimal matching with empirical costs (OMEC) defines costs based on the observed transition rates, and longest common subsequence (LCS) defines similarity based on subsequences rather than the timing of individual states. Using Ward's linkage (Ward 1963) with these three cost structures, we calculated cophenetic correlations, which assess how well the dissimilarity matrices produced by the cost structures correspond to the clusters. For this paper, we focus on the LCS cost structure, which produced the highest validity (c = .80) of the three cost structures.

To choose the number of clusters using LCS with Ward's linkage, we evaluated several cluster quality statistics, such as the Calinski-Harabasz Index and Hubert's C Index. These methods led to different conclusions, which is not uncommon (Cornwell 2015). As advised by Cornwell (2015), we based our final choice of six clusters by looking at the state distribution plots of the different clusters and determining whether the cluster solution represents meaningful and differentiated groups. For reference, we present state distribution plots of the five and seven cluster solutions for comparison (see Appendix F1 and F2). The six-cluster solution differentiates a typology of people who experience high levels of incarceration over time, which is meaningful to our research focus on criminal justice contact. The seven-cluster solution separates out a group that has higher levels of labor force nonparticipation and unemployment compared to the average group of sample respondents. Although this is an interesting distinction, it is less relevant to our main research questions. We conduct the sequence analyses in R using the TraMineR package (Gabadinho et al. 2011).

For the second stage (R2), we examine how criminal justice contact is associated with short-term labor force participation and labor market outcomes. We examine short-term changes in terms of monthly changes and yearly changes in separate fixed-effects models. Apart from adjusting for observed time-varying characteristics, fixed-effects models adjust for unobserved time-stable by differencing out time-invariant individual characteristics (Allison 2009). In a simplified model with two time periods for individual *i*:

$$Y_{i2} - Y_{i1} = (x_{i2} - x_{i1})'\beta + (z_i - z_i)'\gamma + (e_{i2} - e_{i1})$$

Here, taking the difference of characteristics that vary from time=1 to time=2 illustrates how the model controls for unobserved *z* characteristics that are stable across time=1 and time=2. By differencing across time periods, the γ term for time-invariant factors drops out of the model and cannot be estimated.

Models based on monthly information estimate very short term, month-to-month associations between changes in arrest, incarceration, labor force participation, and employment. They rely on observed measures that change on a monthly basis and adjust for time-varying covariates including marital status (e.g., single, cohabiting, married), number of children, school enrollment, and receipt of welfare. In addition to the monthly models, we also estimate models based on yearly information in order to examine associations between changes in criminal justice contact, labor force participation, and employment on a comparably longer time horizon. These models additionally include educational attainment.

For the third stage (R3), we use propensity score matching to estimate the association between criminal justice contact (arrest and incarceration) and longer-term labor force participation and employment outcomes. We examine how experiencing either an arrest or incarceration between ages 19 and 27 is associated with labor market participation and employment outcomes from age 28 to 36 years old. To help address concerns of selection bias into arrest and incarceration, we estimate the propensity for arrest and incarceration (in separate models) based on a set of observed covariates that refer to the period before the arrest or incarceration (Rosenbaum and Rubin 1983). We estimate the propensity for arrest (or incarceration, depending on the model) based on the variables listed in the Measures section above. We then use the observed propensity for arrest (or incarceration) to estimate the average treatment effect (ATE) conditional on the propensity. Because we are interested in predicting the propensity for arrest or incarceration based on covariates that occur before the arrest or incarceration, we drop sample members who report an arrest or incarceration at age 18 (arrest: N=598, incarceration: N=116). We carry out these analyses using the *teffects* command in Stata 14 with non-imputed data.[3]

Across all of these approaches, we are able to estimate associations among criminal justice contact, labor force participation, and labor market outcomes, as opposed to causal effects of criminal justice contact on participation and employment. Although we use a variety of approaches (e.g., fixed effects regression models, propensity score matching) and estimate associations at different time units (e.g., month, year, longer-term cumulative timeframes) to help address issues of selection bias, omitted variable bias, and time ordering, our reliance on observational data prohibits a causal interpretation of the estimates.

RESULTS

Sequence Analysis

First, we present descriptive results from the sequence analyses. Figure 1 describes the state distribution plot, or the amount of time spent in various states, for the entire person-month sample across ages 18 to 36 years old. At age 18, slightly more than half of the sample is working, and a small minority of the sample is unemployed. Nearly 40 percent of the sample is out of the labor force, either in school or not in school. As people grow older, more and more respondents enter the labor force, as workers and as unemployed jobseekers. By the mid-30s,

[3] The *teffects* command in Stata does not support the use of multiply imputed data.

over 70 percent of the sample reports working, slightly more than 10 percent reports being out of the labor force, and a minority of individuals report being unemployed, being out of the labor force/in school, experiencing an arrest, or experiencing an incarceration.

[Figure 1 About Here]

In the sample of person months, less than 1 percent of months are characterized by an arrest or incarceration. However, the prevalence of experiencing any arrest or incarceration across people (and not months) is much higher. For example, among the 8,818 people in the sample, approximately 26% experience at least one arrest (n=2,310) and approximately 9% experience at least one incarceration in either jail or prison (n=816).

We next assess the transition rates of the sequences for the full sample. Transition rates measure the amount of stability (or change) from an initial state in one month to another state in the following month. As Table 1 describes, there is a high amount of stability from one month to the following month among people who are working. Approximately 96% of months spent working are followed by another month of work. Among months that are spent unemployed, approximately 12% of the following months are spent working, 77% of months are spent not unemployed, 3% of months are spent out of the labor force/in school, and 8% are spent out of the labor force/not in school. In terms of arrest, approximately 45% of months following an arrest are spent working, while 12% are spent unemployed, 4% are spent out of the labor force/in school, and 26% are spent out of the labor force/not in school. In 10% of months that experience an arrest, the following month is also characterized by arrest. In other words, among months in which an arrest occurs, the modal experience for the following month is being employed. In terms of incarceration, the vast majority of months incarcerated are followed by another month incarcerated (94%), reflecting the relatively stable experience of imprisonment. In only 3% of months incarcerated, the following month is a working month. In 1% of months incarcerated, the respondent reports being unemployed in the following month, and in 2% of months incarcerated, the respondent reports being out of the labor force in the following month.

[Table 1 About Here]

The state distribution plots and transition rates described above reflect patterns for the sample overall. Next, we present the results of the cluster analyses, which distinguish among six patterns of trajectories (see Figure 2). Type 1 is comprised of 4,095 people (or 59% of the restricted sample) and reflects state distribution patterns that are similar to the sample overall but with a higher prevalence of working. For Type 1, the majority of months are spent working and the prevalence of working increases, reaching nearly full levels of employment, as respondents become older. Type 2 is comprised of 1,138 people (or 16% of the restricted sample). People in this typology have lower rates of employment, higher rates of unemployment, and higher rates of labor force nonparticipation/not in school. Type 3 is comprised of 648 people (or 9% of the restricted sample). People in Type 3 report relatively low levels of work, higher levels of unemployment, and high levels of labor force nonparticipation/not in school. Over time, the prevalence of work stays relatively low and flat, particularly compared to Types 1 and 2. Type 4 represents 685 people (or 10% of the restricted sample). People in Type 4 initially have high rates of labor force nonparticipation and are in school; over time, they move into the labor force and are employed. Type 6 is comprised of 231 people (or 3% of the restricted sample). Similar to Type 3, people in Type 6 initially have low rates of employment and high rates of labor force nonparticipation, some of which is due to school enrollment. However, unlike Type 3, people in Type 6 do not maintain a relatively steady state of work and labor market nonparticipation over time. Rather, by the mid 30s, the vast majority of people in Type 6 report that they are out of the labor force.

[Figure 2 About Here]

In terms of criminal justice contact, Type 5 is comprised of 146 people (or 2% of the restricted sample) and is characterized by high levels of incarceration relative to the other five typologies. People who are in this group start with higher rates of incarceration, high levels of labor market nonparticipation, and low levels of employment. Over time, the prevalence of both employment and labor force nonparticipation decline as more and more time is spent incarcerated. At the person-level, all of the people in Type 5 experience incarceration, and their patterns of labor force participation and employment over time reflect this high prevalence of incarceration. It is notable, however, that people in the other clusters also experience incarceration. For example, the prevalence of ever being incarcerated varies from a low of 3% (Type 4) to a high of 13% (Type 3), with most clusters experiencing rates around 10% (Type 1: 4%, Type 2: 12%, Type 6: 12%). Correspondingly, the prevalence of ever being arrested characterizes a fairly substantial proportion of respondents across all of the clusters, with rates ranging from a low of 18% (Type 4) to a high of 40% (Type 3; Type 1: 20%, Type 2: 34%, Type 6: 37%). Therefore, with the exception of a small number of people who have high rates of incarceration across ages 18 to 36 (Type 5), the experiences of arrest and incarceration characterize at least some proportion of people's experiences across different patterns of labor force and employment over time.

Monthly and yearly changes in criminal justice contact, labor force nonparticipation, and employment (R2)

First, we present descriptive statistics for the person month sample, based on non-imputed and weighted data (see Table 2). Across all person months, approximately 22% of the sample reports labor force nonparticipation. Weekly variation in labor force status is modest, as the mean of the standard deviation of weekly labor force status in any given month is .03. Across person months, the average number of weeks employed is .72 and the average wage is $10.93. Job satisfaction is relatively high at 3.92, on a scale that ranges from 1 to 5 (with higher values indicating greater satisfaction), where the value of 4 corresponds to the answer, “Like it fairly well.” In terms of shift schedules, approximately 60% of all person months include at least one week in which the respondent worked a job that involved a regular day shift (as opposed to an irregular shift, an evening shift, etc.). Looking to the arrest and incarceration variables, less than 1% of person months are characterized by an arrest and 1% of person months are characterized by an incarceration.

[Table 2 About Here]

Among person months in which an arrest occurs, there are higher rates of labor force nonparticipation (41% compared to 22% in the full sample), higher amounts of labor force status variation (.06 compared to .03), and fewer weeks of employment (.50 compared to .72). Average wage among person months in which an arrest occurs is also lower compared to the full sample ($9.37 compared to $10.93) and fewer months are characterized by a regular shift schedule (52% compared to 60% in the full sample). Among months in which an arrest occurs, job satisfaction is still relatively high and similar to the full sample (3.73 compared to 3.92 on a scale from 1 to 5). Among person months in which an incarceration occurs, there are comparably higher rates of labor force nonparticipation (76%), lower amounts of variation in labor force status (.04), fewer weeks employed (.17), and lower average wages ($6.90) relative to months in which an arrest occurs. As with arrest, job satisfaction is similar (3.64). Interestingly, among person months in which an incarceration occurs, the prevalence of regular shift jobs is higher as compared to both the full sample and the sample in which an arrest occurs (62% compared to 60% and 52%, respectively). These patterns are generally similar when aggregated to the yearly level (see Appendix Table for corresponding descriptive statistics).

To examine associations between short-term monthly changes in criminal justice contact, labor force participation, and employment status, we estimate a series of fixed-effects regression models. Fixed-effects models adjust for time-varying observed characteristics (in this case, marital status, children, school enrollment, and welfare receipt) as well as unobserved time-stable characteristics. Table 3 presents estimates for three outcomes: labor force nonparticipation (Model 1), weekly variation in labor force status in the focal month (Model 2), and average weeks employed in the focal month (Model 3). Model 1 utilizes a logit fixed-effects regression model, and Models 2 and 3 use linear fixed-effects regression models.

[Table 3 About Here]

As Model 1 shows, arrest and incarceration are both independently associated with a higher likelihood of being out of the labor market. An arrest is associated with a logit coefficient of 0.30, which translates into an odds ratio of 1.35 or a 35% increase in the odds of being out of the labor market. An incarceration is associated with a logit coefficient of 1.82, or 518% increase in the odds of being out of the labor market. These results show that even an arrest, which is considered a relatively minor form of criminal justice contact, is associated with labor market nonparticipation.

Model 2 examines whether criminal justice contact is related to weekly variation in labor force status. In other words, are arrest and incarceration related to more instability in labor force participation? As the models show, arrest and incarceration are both statistically significantly associated with variation in labor force participation; however, arrest is positively related to more variation and incarceration is negatively related to variation. An arrest is associated with a .02 increase in weekly variation in labor force participation. This represents a one-fifth (or 21%) of a standard deviation increase in labor force status variation. An incarceration is associated with a .004 decrease in labor force status variation, which translates into a 4% of a standard deviation decrease in labor force status variation. In other words, in contrast to an arrest, an incarceration is related to a very small decrease in variation.

Model 3 examines whether arrest and incarceration are associated with the average number of weeks employed in the month. Both are negatively associated with the number of weeks employed. An arrest is associated with a .04 unit decrease in the average weeks employed, which corresponds to a .10 of a standard deviation decrease in number of weeks employed. An incarceration is related to a much larger decrease in the average weeks employed; an incarceration is associated with a .31 unit decrease in the average weeks employed, or a two-thirds (70%) of a standard deviation decrease in the number of weeks employed.

Table 4 examines associations between arrest, incarceration, and various employment characteristics among person-months that are working. As Model 1 shows, both arrest and incarceration are negatively associated with average monthly wage. An arrest is associated with a modest .20 decrease in the average wage, which corresponds to approximately 5% of a standard deviation decrease in the average wage. An incarceration is related to a larger 1.23 decrease in the average wage (or a 32% of a standard deviation decrease). As Model 2 shows, neither an arrest nor an incarceration is related to the job satisfaction variable. The magnitude of the associations with arrest and incarceration are small and the standard errors are comparably large. As Model 3 shows, an arrest (but not an incarceration) is negatively associated with the likelihood that the job involves a regular day shift as opposed to a shift that is irregular, that is on weekends, or that the respondent reports as an "other" type of shift. An arrest is associated with a -.16 logit coefficient, which translates to an odds ratio of .85 or a 15% decrease in the odds of having a regular day shift job.

[Table 4 About Here]

Associations at the yearly level are generally consistent with the findings at the monthly level. Table 5 describes associations estimated from person-year fixed effects regression models. As Model 1 shows, both arrest and incarceration are associated with a higher likelihood of labor force nonparticipation. An arrest is related to a logit coefficient of .39 or an odds ratio of 1.48, which translates into a 48% increase in the odds of any nonparticipation in that year. An incarceration is related to a logit coefficient of 1.33, which corresponds to a 278% increase in the odds of any nonparticipation in that year. Arrest and incarceration are also related to more variation in labor force status in the year, at levels that are similar to those estimated in the monthly fixed effects regression models. As Model 2 describes, an arrest is associated with a .05 increase in average weekly variation in labor force participation during the year. This corresponds to nearly one-quarter (23%) of a standard deviation increase in variation in labor force status. An incarceration is related to a .02 increase in variation, or an 8% of a standard deviation increase in variation in labor force status. As Model 3 shows, arrest and incarceration are also negatively related to the average weeks employed over the year. Arrest is related to a .03 unit decrease in the average weeks employed, which corresponds to a 7% of a standard deviation decrease in the average number of weeks. Incarceration is associated with a .20 unit decrease in the average weeks employed, or approximately one-half (53%) of a standard deviation decrease in the average number of weeks.

[Table 5 About Here]

Similar to the monthly level estimates, changes in arrest and incarceration are associated with some job characteristics at the yearly level. As Table 6, Model 1 describes, an arrest is related to a small .16 decrease in the average wage in the year, or a 4% of a standard deviation decrease in the average wage. An incarceration is related to a .47 decrease in the average wage, which corresponds to .12 of a standard deviation decrease in the average wage. As with the monthly fixed effects regression models, criminal justice contact is not associated with job satisfaction at the yearly level. In a given year, people who experience an arrest or an incarceration express levels of satisfaction with their employment that are similar to people who do not experience an arrest or an incarceration. In terms of shift scheduling, unlike the monthly models, an arrest at the person-year level is not associated with changes in the likelihood of working a regular day shift.

[Table 6 About Here]

Longer-term associations between criminal justice contact, labor force participation, and employment (R3)

In this final section, we estimate the average treatment effect (ATE) for labor force nonparticipation and employment characteristics conditional on the propensity for arrest and incarceration. The labor force nonparticipation and employment measures are based on the period when respondents are ages 28 to 36. The propensity for arrest and incarceration, occurring between ages 19 and 27, are separately estimated based on time-stable covariates and time-varying covariates measured at age 18.

Table 7 presents the ATE estimates of arrest and incarceration (in separate models) for labor force nonparticipation, variation in labor force participation, and average weeks employed. Arrest and incarceration are each associated with poorer labor force participation and employment outcomes, with incarceration more negatively associated with these outcomes. As Model 1, Panel A shows, net of the propensity for arrest, an arrest is associated with an average of 26 more months of labor market nonparticipation over the time period from 28 years old to the last observed survey round (including people up to 36 years old). As Model 2, Panel A describes, an incarceration is also associated with more months (34 months, on average) of labor market nonparticipation over this time period, net of the propensity for incarceration. Arrest and incarceration are both independently associated with more variation in labor force status over this time period, net of the propensity for arrest and incarceration, respectively. An arrest is associated with a .01 increase in average monthly variation in labor force status, net of the propensity for arrest. This corresponds to 31% of the standard deviation of the labor force variation measure. Similarly, an incarceration is also associated with a .01 increase in the average monthly variation, net of the propensity for incarceration. Apart from labor force status, arrest and incarceration are also associated with fewer weeks employed. Net of the propensity for arrest, an arrest is associated with more than 7 fewer average weeks employed over the time period and an incarceration is associated with nearly 9 fewer average weeks employed over the time period.

[Table 7 About Here]

Panels D, E, and F of Table 7 describe the ATE estimates of arrest and incarceration for employment characteristics, including wage, job satisfaction, and shift scheduling. For wage, an arrest is associated with a $1.04 decrease in the average hourly wage over the time period, net of the propensity for arrest. This corresponds to approximately one-third of the standard deviation decrease in the average hourly wage (.35). An incarceration is associated with a $1.39 decrease in the average hourly wage over the period, which corresponds to .46 of the standard deviation decrease in the average hourly wage. In terms of job satisfaction, arrest and incarceration are not associated with self-reports of satisfaction. This finding mirrors the findings for the monthly and yearly fixed effects regression analyses, suggesting that people report similar levels of satisfaction with the work that they do find. For shift scheduling, arrest and incarceration are both negatively associated with the number of months that people work a regular daytime shift (as opposed to shifts that are at night, are irregular, or on weekends). Net of the propensity for arrest, an arrest is associated with an average of 7 fewer months working a job with a regular daytime shift. Net of the propensity for incarceration, an incarceration is associated with an average of 9 fewer months working a job with a regular daytime shift. Overall, with the exception of job satisfaction, the experience of arrest and incarceration are each independently associated with negative outcomes in labor force participation, employment, and employment characteristics.

DISCUSSION

Using detailed, nationally representative, and contemporary survey data, we utilized several descriptive and analytic techniques to examine associations among criminal justice contact (e.g., arrest and incarceration in jails and prisons), labor force participation, and employment characteristics. Our findings suggest three primary conclusions. First, as illustrated by the sequence analyses, people have distinct trajectories of labor force participation and employment from young adulthood through their mid 30s. Although there is a small group of people who experience long periods of incarceration over this time (and these experiences are reflected in a distinct incarceration typology), the prevalence of both arrest and incarceration are observable across various patterns of labor force participation and employment experiences. Moreover, transition rates that assess the relationship between criminal justice contact in a given month and experiences in the following month suggest that people who are arrested routinely report employment in the following month (although they also report higher levels of labor force nonparticipation).

Second, as described in the monthly and yearly fixed effects regression analyses, both arrest and incarceration are associated with labor force nonparticipation, variation in labor force status, weeks employed, and wages. On a monthly level, arrest is additionally associated with a lower likelihood that employment involves a regular day shift (as opposed to some other scheduling arrangement). On a yearly level, arrest and incarceration are not associated with shift scheduling nor are they associated with job satisfaction.

Third, as found in the propensity score matching models, the negative associations between criminal justice contact, labor force participation, and employment are also observable over comparably longer time frames. Specifically, an arrest or incarceration that occurs at younger ages (from age 19 through 27) is measurably related to labor force participation, employment, and job characteristics when respondents are 28 to 36 years old. These associations are observable net of the propensity for arrest and incarceration, respectively. Notably, across the propensity score models and the monthly and yearly fixed effects regression models, the one employment outcome that is not associated with arrest or incarceration is job satisfaction.

Respondents who experience an arrest and/or incarceration report similar levels of job satisfaction compared to those that have not experienced an arrest and/or incarceration, even though their employment experiences appear to be less satisfactory in terms of wages and shift schedules.

Overall, the findings indicate that both arrest and incarceration are related to labor force nonparticipation and employment in short and longer-term time horizons. Although incarceration is generally perceived to be negatively associated with employment characteristics, the findings regarding arrest contribute new insight into the potential consequences of lower-forms of criminal justice contact for labor force experiences and labor market outcomes. The findings regarding arrest contribute to a small number of papers that have examined associations between arrest and employment outcomes, particularly wages (Apel and Powell 2019; Grogger 1995). In Apel and Powell's recent paper, which uses the same NLSY97 data but with a different modeling strategy and a longer time horizon (examining wages after an arrest occurred 13 years ago, on average), they find no associations between arrest and wages (2019). In this paper, we find a statistically significant relationship at the monthly, yearly, and longer-term time horizons; however, the magnitudes of the associations are relatively modest. In contrast, the associations between arrest and other measures, specifically labor force participation and variation in labor force status, suggest that arrest is negatively related in particularly important ways to labor force attachment and stability.

The findings highlighting the role of arrest complement other recent scholarship that documents employer aversion to hiring those with arrests (Uggen et al. 2014), that describes how arrests can entail time consuming court appearances, community service, and other court-mandated program obligations that interfere with job search and employment (Kohler-Hausmann

2018), and that indicates that arrests can have negative mental health associations that could have ramifications for labor force participation and employment (Sugie and Turney 2017). Moreover, the associations among arrest, labor force nonparticipation, and employment are particularly consequential given the prevalence of arrest in the United States. With an estimated 30 to 41% of all individuals experiencing arrest by age 23, arrest is not unique or unusual (Brame et al. 2012). It also does not imply culpability, unlike a conviction record or incarceration in prison. The likelihood of arrest reflects not only a person's suspected conduct but also a range of factors related to race/ethnicity, neighborhood, gender, age, and their intersection (e.g., Beckett 2012, Rudovsky and Harris 2018). The selection into arrest based on these socioeconomic characteristics, combined with evidence that arrest is related to lower rates of labor force participation and less beneficial employment outcomes, contributes to the more general perception of the criminal justice system as a stratifying institution that exacerbates racial/ethnic and class inequalities (Wakefield and Uggen 2010).

The findings in this paper are based on an analysis of observational survey data, which has various strengths and limitations. First, in terms of advantages, survey data include information on labor force status and other employment measures, such as job satisfaction and shift scheduling, which are not available in alternative data sources such as administrative records from unemployment insurance. Second, survey data also includes information on arrest and incarceration in jails. Given the localized administrative structure of arrest and jail incarceration data, this information would be very difficult, if not impossible, to collect for a nationally representative sample over time (Turney and Wakefield 2019). Moreover, information on arrests is not always preserved in administrative records, depending on the disposition status of the arrest (Kohler-Hausmann 2018).

Observational survey data also have a number of limitations. First, the measures are based on retrospective self reports, which might contain error due to retrospective recall bias or social desirability bias. In the former case, people might have a hard time remembering events, dates, or experiences that are not particularly salient to them. In the case of criminal justice contact and employment, people who are arrested often or those who have particularly irregular work situations might give answers that have more error. In the latter case, people might be hesitant to report experiences that are negative, stigmatized, or potentially embarrassing, such as criminal justice contact or periods of labor force nonparticipation and unemployment. In all of these situations, survey answers might be characterized by more error, leading to imprecision of estimates that are downwardly biased. A second major limitation of observational survey data is that it is not possible to estimate the causal effects of arrest or incarceration on labor force participation and employment. Despite our various modeling approaches, which adjust for different types of selection biases, there is always the possibility that selection into criminal justice contact is driving the association with poor labor market outcomes. Absent an experimental design, these threats to causal identification cannot be fully addressed. Although it is hard to imagine a scenario in which arrest is randomly assigned, it is possible that certain aspects of the experience of arrest could be randomized to isolate particular mechanisms. For example, record clearing (or expungement) could be randomly assigned to some people with arrest records in order to test whether the stigma of the arrest record, in particular, leads to labor force nonparticipation, variation in labor force status, weeks employed, wages received, and types of shifts worked.

Notwithstanding these considerations, the findings in this paper contribute to a relatively small but growing body of scholarship that documents how minor forms of criminal justice contact, such as arrest, are consequential for labor market outcomes (e.g., Grogger 1995, Kohler-Hausmann 2018, Uggen et al. 2014; but, see Apel and Powell 2019). This work additionally contributes to a broader scope of recent research that indicates that arrest is negatively associated with other facets of life, as well, including mental health (Sugie and Turney 2017) and political participation (Lerman and Weaver 2014). Given differential selection into criminal justice contact based on race/ethnicity, neighborhood, and social class, the negative consequences of arrest not only reflect socioeconomic inequalities but they also exacerbate them in ways that are durable, measurable, and consequential (Turney and Wakefield 2019).

REFERENCES

Abbott, Andrew. 1995. "Sequence Analysis: New Methods for Old Ideas." *Annual Review of Sociology* 21:93–113.

Allison, Paul D. 2009. *Fixed Effects Regression Models*. Thousand Oaks, California: Sage Publications.

Apel, Robert and Kathleen Powell. 2019. "Level of Criminal Justice Contact and Early Adult Wage Inequality." *RSF: The Russell Sage Foundation Journal of the Social Sciences* 5(1):198-222.

Apel, Robert and Gary Sweeten. 2010. "The Impact of Incarceration on Employment during the Transition to Adulthood." *Social Problems* 57(3):448–79.

Beckett, Katherine. 2012. "Race, Drugs, and Law Enforcement: Toward Equitable Policing." *Criminology and Public Policy* 11(4):641-653.

Brame, Robert, Michael G. Turner, Raymond Paternoster, and Shawn D. Bushway. 2012. "Cumulative Prevalence of Arrest from Ages 8 to 23 in a National Sample." *Pediatrics* 129(1):21–27.

Cornwell, Benjamin. 2015. *Social Sequence Analysis*. New York: Cambridge University Press.

Crutchfield, Robert D. 2014. *Get a Job: Labor Markets, Economic Opportunity, and Crime*. New York: New York University Press.

Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. "The Effects of Pretrial Detention on Conviction, Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review* 108(2):201-240.

Fagan, Jeffrey and Richard B. Freeman. 1999. "Crime and Work." *Crime and Justice* 25:225–90.

Freudenberg, Nicholas, Jessie Daniels, Martha Crum, Tiffany Perkins, and Beth Ritchie. 2005. "Coming Home from Jail: The Social and Health Consequences of Community Reentry for Women, Male Adolescents, and Their Families and Communities." *American Journal of Public Health* 95:1725-1736.

Gabadinho, Alexis, Gilbert Ritschard, Nicolas Severin Mueller, and Matthias Studer. 2011. "Analyzing and Visualizing State Sequences in R with TraMineR." *Journal of Statistical Software* 40(4):1-37.

Glaze, Lauren E., and Danielle Kaeble. 2014. *Correctional Populations in the United States, 2013*. Washington, DC: U.S. Department of Justice.

Gottlieb, Aaron, and Naomi F. Sugie. 2018. "Marriage, Cohabitation, and Crime: Differentiating Associations by Partnership Stage." *Justice Quarterly.* Online First.

Grogger, Jeffrey. 1995. "The Effect of Arrest on the Employment and Earnings of Young Men." *Quarterly Journal of Economics* 110(1):51-72.

Harding, David J., Jeffrey D. Morenoff, and Claire W. Herbert. 2013. "Home is hard to find: Neighborhoods, Institutions, and the Residential Trajectories of Returning Prisoners." *ANNALS of the American Academy of Political and Social Science* 647:214–236.

Harding, David J, Jessica JB Wyse, Cheyney Dobson and Jeffrey D Morenoff. 2014. "Making Ends Meet after Prison." *Journal of Policy Analysis and Management* 33(2):440-70.

Holzer, Harry J., Paul Offner, and Elaine Sorensen. 2005. "Declining Employment Among Young Black Less-Educated Men: The Role of Incarceration and Child Support." *Journal of Policy Analysis and Management* 24(2):329-350.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll. 2006. "Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers." *Journal of Law and Economics* 49(2):451–80.

Honaker, James, Gary King, and Matthew Blackwell. 2011. "Amelia II: A Program for Missing Data." *Journal of Statistical Software* 45(7):1-47.
Kaeble, Danielle, and Mary Cowhig. 2018. *Correctional Populations in the United States, 2016*. Washington, DC: U.S. Department of Justice.
Kalleberg, Arne L. 2011. *Good Jobs, Bad Jobs: The Rise of Polarized and Precarious Employment Systems in the United States, 1970s to 2000s*. New York: Russell Sage Foundation.
Kling, Jeffrey R. 2006. "Incarceration Length, Employment and Earnings." *American Economic Review* 96(3):863-876.
Kohler-Hausmann, Issa. 2013. "Misdemeanor Justice: Control without Conviction." *American Journal of Sociology* 119(2):351–393.
Kohler-Hausmann, Issa. 2018. *Misdemeanorland: Criminal Courts and Social Control in an Age of Broken Windows Policing.* Princeton, NJ: Princeton University Press.
Lalonde, Robert J., and Rosa M. Cho. 2008. "The Impact of Incarceration in State Prison on the Employment Prospects of Women." *Journal of Quantitative Criminology* 24:243-265.
Lerman, Amy E., and Vesla M. Weaver. 2014. *Arresting Citizenship: The Democractic Consequences of American Crime Control.* Chicago, IL: University of Chicago Press.
Leverentz, Andrea M. 2014. *The Ex-Prisoner's Dilemma: How Women Negotiate Competing Narratives of Reentry and Desistance*: Rutgers University Press.
Loeffler, Charles E. 2013. "Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment." *Criminology* 51(1):137-166.
National Research Council. (2014). *The Growth of Incarceration in the United States: Exploring Causes and Consequences.* Committee on Causes and Consequences of High Rates of Incarceration, J. Travis, B. Western, and S. Redburn, Editors. Committee on Law and Justice, Division of Behavioral and Social Sciences and Education. Washington, DC: The National Academies Press.
Pager, Devah. 2007. *Marked: Race, Crime, and Finding Work in an Era of Mass Incarceration*. Chicago: University Of Chicago Press.
Pager, Devah, Bruce Western, and Bart Bonikowski. 2009. "Discrimination in a Low-Wage Labor Market: A Field Experiment." *American Sociological Review* 74(5):777.
Petersilia, Joan. 2003. *When Prisoners Come Home: Parole and Prisoner Reentry*: Oxford University Press.
Pettit, Becky. 2012. *Invisible Men: Mass Incarceration and the Myth of Black Progress*. New York: Russell Sage Foundation.
Pettit, Becky and Christopher J. Lyons. 2007. "Status and the Stigma of Incarceration: The Labor-Market Effects of Incarceration, by Race, Class, and Criminal Involvement." In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, edited by S. Bushway, M.A. Stoll, and D.F. Weiman. New York: Russell Sage Foundation.
Piehl, Anne M. 2009. *Preparing Prisoners for Employment: The Power of Small Rewards.* Center for Civic Innovation at the Manhattan Institute.
Rosenbaum, Paul R., and Donald B. Rubin. 1984. "Reducing Bias in Observational Studies Using Subclassification on the Propensity Score." *Journal of the American Statistical Association* 79:516-24.
Rudovsky, David, and David A. Harris. 2018. "*Terry* Stops and Frisks: The Troubling Use of Common Sense in a World of Empirical Data." *Ohio State Law Journal* 79(3):501-546.

Sabol, William J. 2007. “Local Labor-Market Conditions and Post-Prison Employment: Experiences of Offenders Released from Ohio State Prisons.” In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, edited by S. Bushway, M.A. Stoll, and D.F. Weiman. New York: Russell Sage Foundation.

Sampson, Robert J., and John Laub. 1995. *Crime in the Making: Pathways and Turning Points Through Life.* Cambridge, MA: Harvard University Press.

Sampson, Robert J., and Charles Loeffler. 2010. “Punishment’s Place: The Local Concentration of Mass Incarceration.” *Daedalus* 139(3):20–31.

Skardhamar, Torbjørn, Jukka Savolainen, Kjersti N. Aase, and Torkild H. Lyngstad. 2015. “Does Marriage Reduce Crime?” *Crime and Justice* 44(1):385-446.

Stafford, Christopher. 2006. “Finding Work: How to Approach the Intersection of Prisoner Reentry, Employment, and Recidivism.” *Georgetown Journal on Poverty Law and Policy* 13:261–81.

Stoll, Michael A., and Shawn D. Bushway. 2008. “The Effect of Criminal Background Checks on Hiring Ex-Offenders.” *Criminology and Public Policy* 7(3):371–404.

Subramanian, Ram, Ruth Delaney, Stephen Roberts, Nancy Fishman, Peggy McGarry. 2015. *Incarceration’s Front Door: The Misuse of Jails in America*. Vera Institute of Justice, New York.

Sugie, Naomi F. 2018. “Work as Foraging: A Smartphone Study of Job Search and Employment after Prison.” *American Journal of Sociology.* 123(5):1453-1491.

Sugie, Naomi F. and Kristin Turney. 2017. “Beyond Incarceration: Criminal Justice Contact and Mental Health.” *American Sociological Review* 82(4):719-743.

Sullivan, Mercer L. 1989. *“Getting Paid”: Youth Crime and Work in the Inner City*. Ithaca, NY: Cornell University Press.

Travis, Jeremy. 2005. *But They All Come Back: Facing the Challenges of Prisoner Reentry.* Washington, DC: Urban Institute Press.

Turney, Kristin and Emma Conner. 2018. “Jail Incarceration: A Common and Consequential Form of Criminal Justice Contact.” *Annual Review of Criminology.* Online First. October 24.

Turney, Kristin and Sara Wakefield. 2019. “Criminal Justice Contact and Inequality.” *RSF: The Russell Sage Foundation Journal of the Social Sciences* 5(1):1-23.

Tyler, John H., and Jeffrey R. Kling. 2007. “Prison-Based Education and Reentry into the Mainstream Labor Market.” In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, edited by S. Bushway, M.A. Stoll, and D.F. Weiman. New York: Russell Sage Foundation.

Uggen, Christopher, Mike Vuolo, Sarah Lageson, Ebony Ruhland, and Hilary K. Whitman. 2014. “The Edge of Stigma: An Experimental Audit of the Effects of Low-Level Criminal Records on Employment.” *Criminology* 52(4):627–654.

U.S. Department of Justice. 2013. *Uniform Crime Report: Crime in the United States, 2012*. Washington, DC: Federal Bureau of Investigation.

Visher, Christy, and Vera Kachnowski. 2007. “Finding Work on the Outside: Results from the ‘Returning Home’ Project in Chicago.” In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, edited by S. Bushway, M.A. Stoll, and D.F. Weiman. New York: Russell Sage Foundation.

Wakefield, Sara, and Christopher Uggen. 2010. “Incarceration and Stratification.” *Annual Review of Sociology* 36:387-406.

Western, Bruce. 2002. “The Impact of Incarceration on Wage Mobility and Inequality.” *American Sociological Review* 67(4):526-46.
Western, Bruce. 2006. *Punishment and Inequality in America*. New York: Russell Sage Foundation.
Western, Bruce and Katherine Beckett. 1999. “How Unregulated is the U.S. Labor Market? The Penal System as a Labor Market Institution.” *American Journal of Sociology* 104(4):1030-60.
Western, Bruce, and Becky Pettit. 2005. “Black-White Wage Inequality, Employment Rates, and Incarceration.” *American Journal of Sociology* 111:553-578.
Western, Bruce, Anthony A. Braga, Davis, and Catherine Sirois. 2015. “Stress and Hardship after Prison.” *American Journal of Sociology* 120(5):1512–47.
Wilson, William Julius. 1996. *When Work Disappears: The World of the New Urban Poor.* New York: Knopf.

FIGURE 1. State Distribution Plot, N=8,818

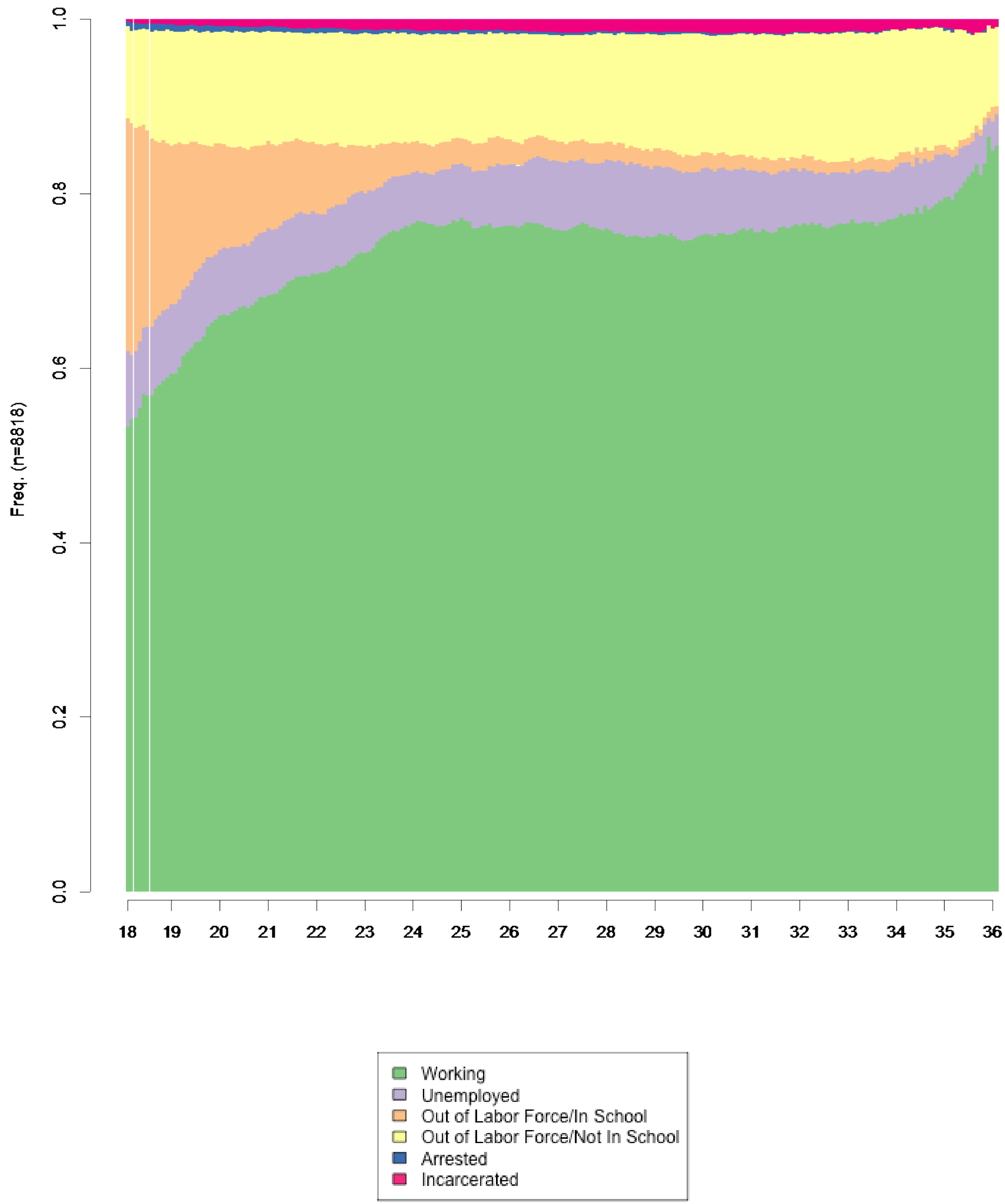


FIGURE 2. State Distribution Plot, by Typologies, N=6,943

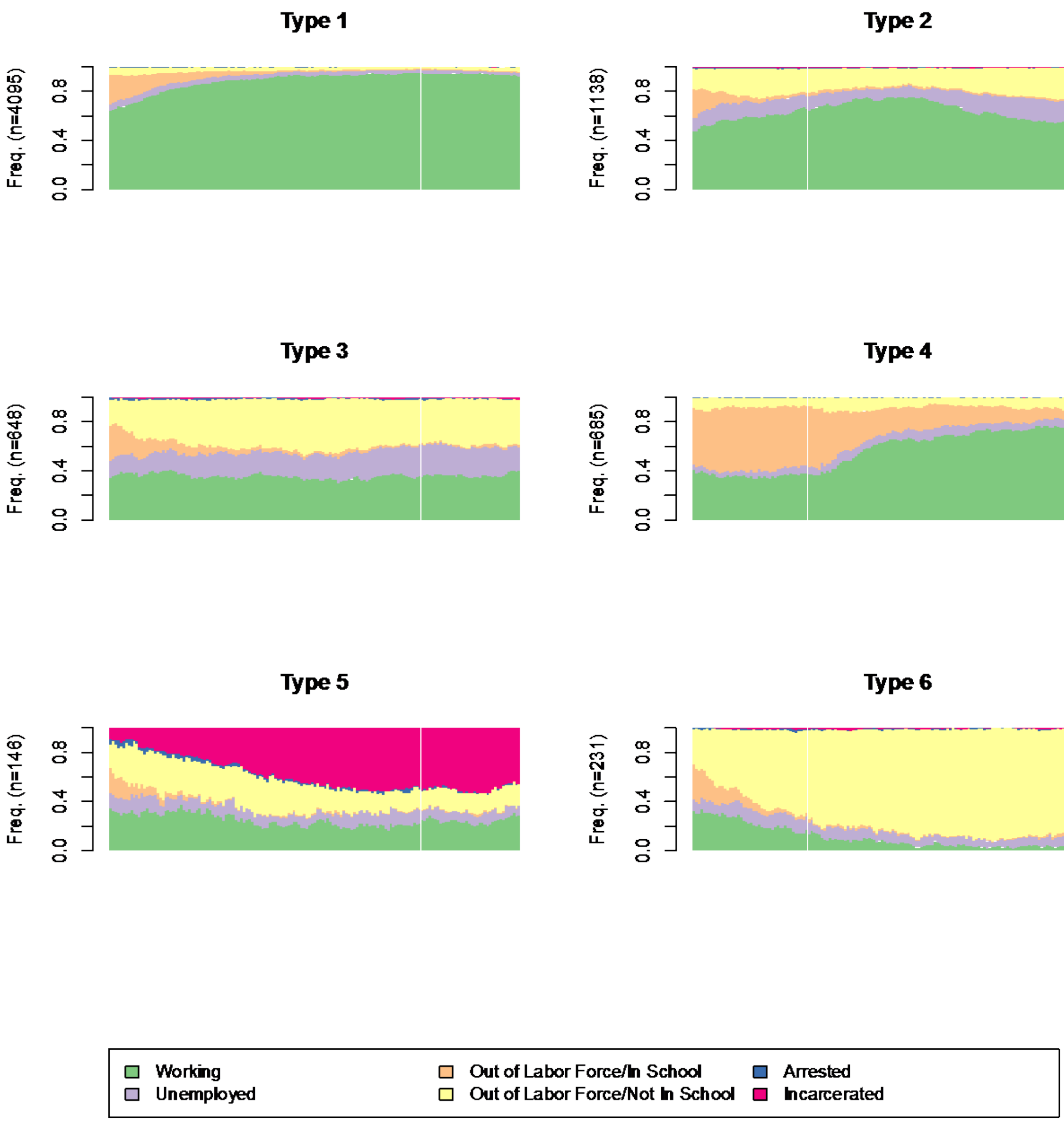


TABLE 1. Transition Rates, N=8,818

	Working	Unemployed	Out of Labor Force/In School	Out of Labor Force/Not in School	Arrest	Incarceration	Missing
Working	0.96	0.01	0.01	0.01	0.00	0.00	0.00
Unemployed	0.12	0.77	0.03	0.08	0.01	0.00	0.00
Out of Labor Force/In School	0.10	0.04	0.82	0.04	0.00	0.00	0.00
Out of Labor Force/Not in School	0.08	0.04	0.01	0.86	0.01	0.00	0.00
Arrest	0.45	0.12	0.04	0.26	0.10	0.02	0.01
Incarceration	0.03	0.01	0.00	0.02	0.00	0.94	0.00
Missing	0.31	0.03	0.08	0.04	0.01	0.00	0.53

TABLE 2. Descriptive Statistics for Person Months

	Full Sample		Arrest		Incarceration	
	Mean/%	SD	Mean/%	SD	Mean/%	SD
Labor force nonparticipation	22%		41%		76%	
Labor force status (variation)	0.03	(0.12)	0.06	(0.17)	0.04	(0.13)
Average weeks employed	0.72	(0.44)	0.50	(0.47)	0.17	(0.36)
Average wage	10.93	(3.87)	9.37	(3.65)	6.90	(5.14)
Job satisfaction (1 to 5)	3.92	(1.05)	3.73	(1.15)	3.64	(1.27)
Regular shift	60%		52%		62%	
Arrest (any)	0%				6%	
Incarceration (any)	1%		13%			
Marital Status						
Single	59%		75%		83%	
Cohabiting	15%		16%		7%	
Married	25%		9%		9%	
Children	0.62	(1.02)	0.65	(1.10)	1.02	(1.45)
School	23%		14%		3%	
Welfare Receipt	0.12	(0.42)	0.19	(0.53)	0.08	(0.34)

Notes: Estimates are based on non-imputed data with weights. Sample size for full sample ranges from 1,504,399 to 884,245 person months, given that some variables are dependent on certain statuses (e.g., job satisfaction is only asked among those working for an employer); sample size for arrest sample ranges from 6,716 to 2,509; sample size for incarceration sample ranges from 18,407 to 2,292.

TABLE 3. Monthly Fixed Effects Regression Models Predicting Labor Force Status and Employment

	Labor Force Nonparticipation Model 1			Variation in Labor Force Model 2			Avg Weeks Employed Model 3		
	Coef.	SE		Coef.	SE		Coef.	SE	
Arrest (any)	0.30	(0.03)	***	0.02	(0.00)	***	-0.04	(0.01)	***
Incarceration (any)	1.82	(0.02)	***	0.00	(0.00)	*	-0.31	(0.01)	***
Marital status (ref =single)									
Cohabiting	-0.40	(0.01)	***	-0.01	(0.00)	***	0.06	(0.00)	***
Married	-0.19	(0.01)	***	-0.01	(0.00)	***	0.03	(0.00)	***
Children	0.06	(0.00)	***	-0.01	(0.00)	***	-0.01	(0.00)	*
School	1.30	(0.01)	***	0.02	(0.00)	***	-0.14	(0.00)	***
Welfare receipt	0.37	(0.01)	***	0.01	(0.00)	***	-0.09	(0.00)	***
Constant				0.03	(0.00)	***	0.74	(0.00)	***
N (obs)	1,512,853			1,512,853			1,512,853		
N (people)	8,818			8,818			8,818		

Notes: Labor force nonparticipation is estimated with a logit regression model. Variation in labor force and average weeks employed are estimated with linear regression models. *p<.05, **p<.01, ***p<.001 (two-tailed test)

TABLE 4. Monthly Fixed Effects Regression Models Predicting Job Characteristics

	Avg Wage Model 1			Job Satisfaction Model 2			Regular Shift Model 3		
	Coef.	SE		Coef.	SE		Coef.	SE	
Arrest (any)	-0.20	(0.08)	**	-0.01	(0.03)		-0.16	(0.05)	**
Incarceration (any)	-1.23	(0.33)	***	-0.04	(0.07)		0.07	(0.07)	
Marital status (ref =single)									
Cohabiting	1.01	(0.04)	***	0.01	(0.01)		0.36	(0.01)	***
Married	1.90	(0.05)	***	0.07	(0.01)	***	0.72	(0.01)	***
Children	1.02	(0.03)	***	0.07	(0.01)	***	0.20	(0.01)	***
School	-2.23	(0.04)	***	-0.03	(0.01)	**	-1.01	(0.01)	***
Welfare receipt	-0.57	(0.03)	***	-0.03	(0.01)	**	-0.01	(0.01)	
Constant	10.10	(0.02)	***	3.85	(0.01)	***			
N (obs)	889,018			889,018			889,018		
N (people)	8,567			8,567			8,567		

Notes: Regular shift is estimated with a logit regression model. Average wage and job satisfaction are estimated with linear regression models. **p*<.05, ***p*<.01, ****p*<.001 (two-tailed test)

TABLE 5. Yearly Fixed Effects Regression Models Predicting Labor Force Status and Employment

	Labor Force Nonparticipation Model 1			Variation in Labor Force Model 2			Avg Weeks Employed Model 3		
	Coef.	SE		Coef.	SE		Coef.	SE	
Arrest (any)	0.39	(0.04)	***	0.05	(0.00)	***	-0.03	(0.01)	***
Incarceration (any)	1.33	(0.07)	***	0.02	(0.01)	*	-0.20	(0.01)	***
Marital status (ref =single)									
Cohabiting	-0.38	(0.02)	***	-0.02	(0.00)	***	0.06	(0.00)	***
Married	-0.31	(0.02)	***	-0.03	(0.00)	***	0.02	(0.00)	***
Children	-0.06	(0.01)	**	-0.01	(0.00)	***	-0.01	(0.00)	**
School	0.09	(0.00)	***	0.01	(0.00)	***	-0.01	(0.00)	***
Welfare receipt	0.05	(0.00)	***	0.00	(0.00)	***	-0.01	(0.00)	***
Education (ref = less than HS)									
HS degree or GED	-0.29	(0.03)	***	0.01	(0.00)	*	0.08	(0.01)	***
Some college	-0.71	(0.05)	***	-0.03	(0.00)	***	0.11	(0.01)	***
4-year college and above	-1.14	(0.04)	***	-0.05	(0.00)	***	0.18	(0.01)	***
Constant				0.14	(0.00)	***	0.64	(0.00)	***
N (obs)	130,821			130,821			130,821		
N (people)	8,818			8,818			8,818		

Notes: Labor force nonparticipation is estimated with a logit regression model. Variation in labor force and average weeks employed are estimated with linear regression models. *p<.05, **p<.01, ***p<.001 (two-tailed test)

TABLE 6. Yearly Fixed Effects Regression Models Predicting Job Characteristics

	Avg Wage Model 1			Job Satisfaction Model 2			Regular Shift Model 3		
	Coef.	SE		Coef.	SE		Coef.	SE	
Arrest (any)	-0.16	(0.06)	*	-0.02	(0.02)		-0.06	(0.05)	
Incarceration (any)	-0.47	(0.15)	**	-0.06	(0.04)		0.12	(0.09)	
Marital status (ref =single)									
Cohabiting	0.87	(0.04)	***	0.02	(0.01)		0.23	(0.03)	***
Married	1.49	(0.05)	***	0.09	(0.01)	***	0.32	(0.03)	***
Children	0.91	(0.03)	***	0.07	(0.01)	***	-0.02	(0.01)	
School	-0.15	(0.00)	***	0.00	(0.00)	**	-0.05	(0.00)	***
Welfare receipt	-0.04	(0.00)	***	-0.01	(0.00)	***	0.01	(0.00)	***
Education (ref = less than HS)									
HS degree or GED	0.85	(0.06)	***	0.09	(0.02)	***	0.45	(0.04)	***
Some college	2.32	(0.10)	***	0.15	(0.03)	***	0.70	(0.06)	***
4-year college and above	3.26	(0.09)	***	0.11	(0.02)	***	1.31	(0.05)	***
Constant	8.52	(0.06)	***	3.75	(0.02)	***			
N (obs)	99,984			99,984			99,984		
N (people)	8,620			8,620			8,620		

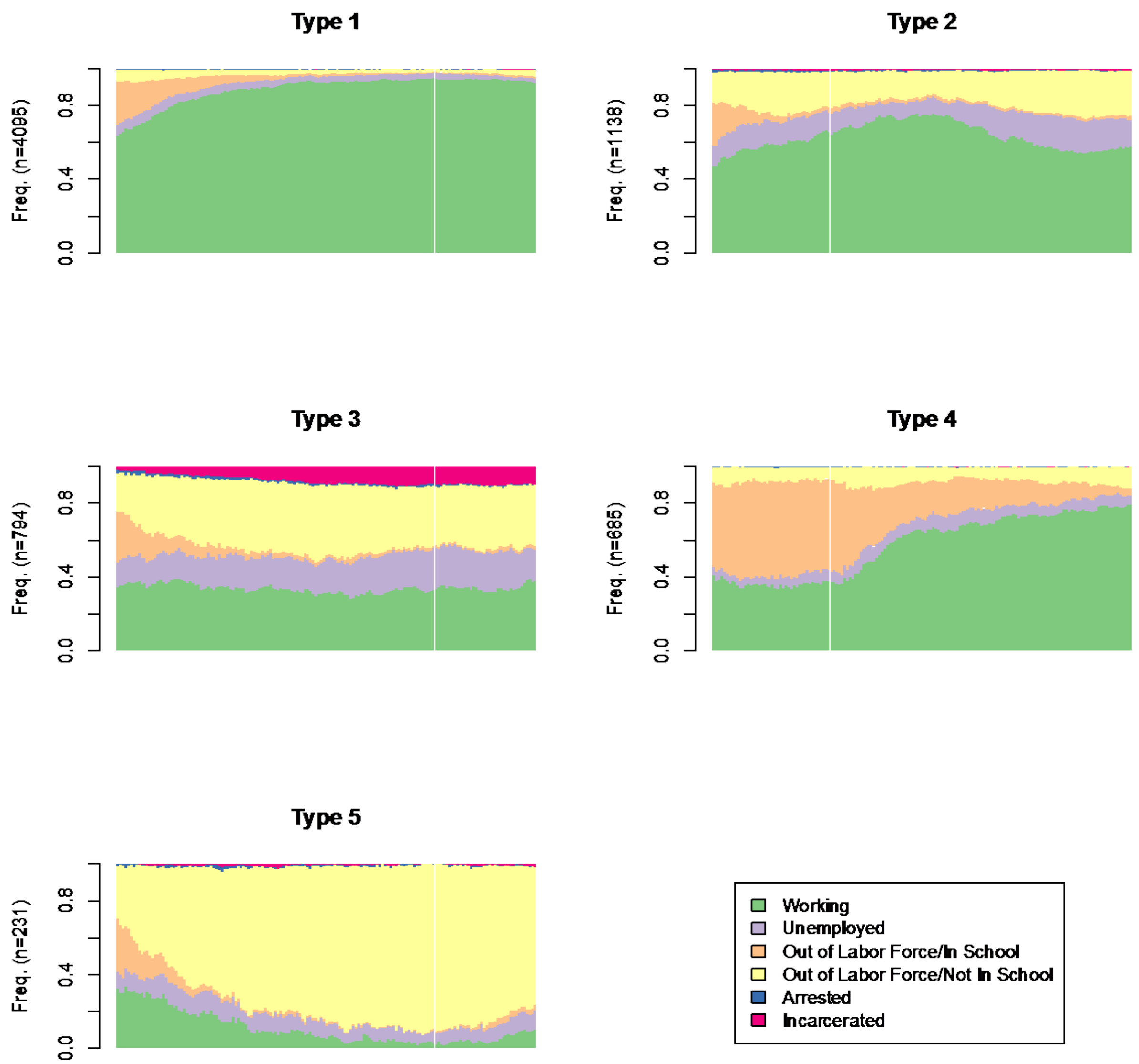
Notes: Regular shift is estimated with a logit regression model. Average wage and job satisfaction are estimated with linear regression models. **p*<.05, ***p*<.01, ****p*<.001 (two-tailed test)

TABLE 7. Average Treatment Effects of Criminal Justice Contact on Labor Force Participation, Employment, and Job Characteristics

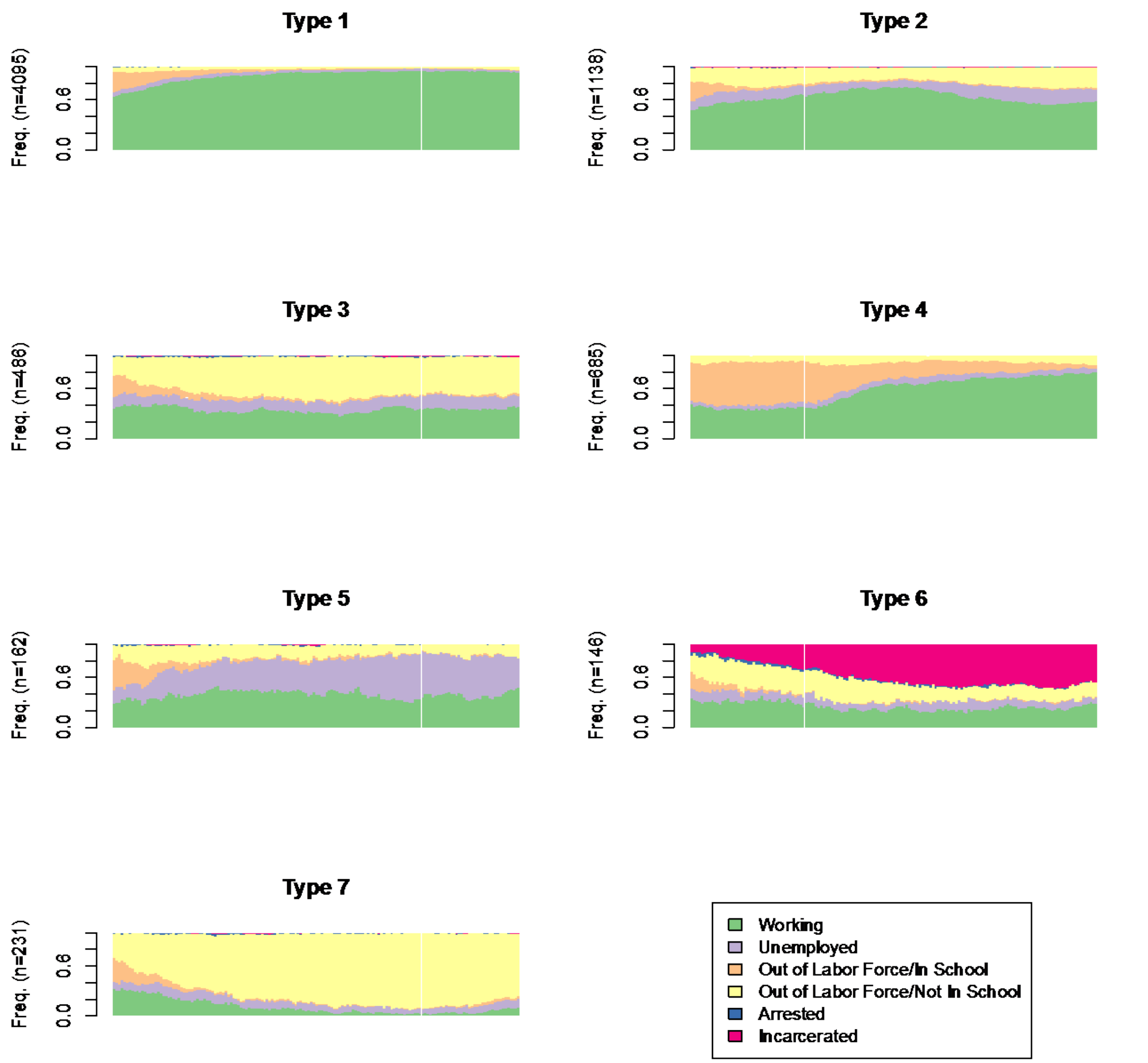
	Model 1: Arrest			Model 2: Incarceration		
	Coef	SE		Coef	SE	
A. Labor Force Nonparticipation						
Arrest	26.41	(4.37)	***			
Incarceration				34.43	(8.52)	***
B. Variation in Labor Force						
Arrest	0.01	(0.00)	***			
Incarceration				0.01	(0.00)	**
C. Weeks Employed						
Arrest	-7.33	(1.22)	***			
Incarceration				-8.95	(2.67)	**
D. Wage						
Arrest	-1.04	(0.15)	***			
Incarceration				-1.39	(0.35)	***
E. Job Satisfaction						
Arrest	-0.07	(0.04)				
Incarceration				-0.06	(0.08)	
F. Regular Shift						
Arrest	-7.01	(1.10)	***			
Incarceration				-9.38	(2.85)	**

Notes: Labor force nonparticipation measures the total number of months with any nonparticipation, variation in labor force measures average monthly variation, weeks employed measures the total number of average weeks employed per month, wage measures the average hourly wage per month over the time period, job satisfaction measures the average monthly satisfaction, and the regular shift variable measures the total months in which the respondent worked any regular day shift. Sample sizes range from 6,949 to 6,340. The following covariates were used to predict the treatment (arrest or incarceration between ages 19 and 27): male, race/ethnicity (Black, Hispanic, Mixed), educational attainment, marital status, (cohabiting, married), total children, any criminal offending, any use of hard drugs (e.g., cocaine, crack, heroin), receipt of cash assistance, receipt of food stamps, parental educational attainment. *p<.05, **p<.01, ***p<.001 (two-tailed test)

APPENDIX F1. State Distribution Plot for Five-Cluster Solution, by Typology, N=6,943



APPENDIX F2. State Distribution Plot for Seven-Cluster Solution, by Typology, N=6,943



APPENDIX TABLE. Descriptive Statistics for Person Years

	Full Sample		Arrest		Incarceration	
	Mean/%	SD	Mean/%	SD	Mean/%	SD
Labor force nonparticipation	42%		65%		84%	
Labor force status (variation)	0.14	(0.20)	0.22	(0.22)	0.22	(0.23)
Average weeks employed	0.72	(0.38)	0.55	(0.39)	0.33	(0.37)
Average wage	10.53	(3.86)	9.17	(3.53)	8.68	(4.06)
Job satisfaction (1 to 5)	3.91	(1.00)	3.74	(1.07)	3.78	(1.07)
Regular shift	62%		55%		61%	
Arrest (any)	4%				49%	
Incarceration (any)	2%		21%			
Marital Status						
Single	60%		75%		78%	
Cohabiting	14%		15%		12%	
Married	25%		10%		10%	
Children	0.66	(1.05)	0.66	(1.11)	1.01	(1.43)
School (total months)	2.70	(4.47)	1.77	(3.66)	0.52	(2.01)
Welfare Receipt	1.40	(4.54)	2.17	(5.56)	1.49	(4.63)
Education						
Less than HS	13%		32%		33%	
HS degree or GED	62%		63%		64%	
Some college	5%		2%		2%	
4-year college and above	19%		3%		1%	

Notes: Sample size for full sample ranges from 117,599 to 90,835 person years, given that some variables are dependent on certain statuses (e.g., job satisfaction is only asked among those working for an employer); sample size for arrest sample ranges from 4,356 to 2,862; sample size for incarceration sample ranges from 2,082 to 868.