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# **Job Training Programs as Crime Deterrents? Evidence from a Low-Income Targeted Training Program RCT**

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Abstract<sup>5</sup>

The primary goal of job training programs is to improve employment and earning outcomes of participants. However, effective job training programs may have potential secondary benefits, including in the form of reduced arrests. In this paper, we evaluate the impact of a job training program in New Orleans that was implemented using a randomized controlled trial design. We find that among those who had a prior criminal record, those assigned to the treatment group were two-fifths as likely to get arrested as those assigned to the control group at any time point after randomization. We explore several potential mechanisms for why this effect occurs and find suggestive evidence that the training program's impact on wages, as well as peer effects from other trainees, can partially explain this effect.

Keywords: human capital, job training, arrests

JEL classifications: J44, J24, J40, J01

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## 1. Introduction

Hundreds of job training programs have been implemented over the past several decades with the goal of improving employment outcomes. The programs, varying highly in structure, duration, and direction, typically share a few common features: they usually target interested unemployed and underemployed individuals; they typically aim to improve skills and proficiencies of participants; and their goals are ultimately focused on increasing employment rates and earnings of the trainees. The literature evaluating the impacts of these job training programs on employment and earnings outcomes has produced somewhat mixed results, although demand-driven training programs—those for which programmatic and curricular decisions are informed by local employers based on their needs—have typically been found to have positive impacts on employment and earnings (Andersson et al., 2013; Baird et al., 2019; Card et al., 2018; Crépon & Van Den Berg, 2016; Fortson et al., 2017; Roder et al., 2008; Van Horn et al., 2015).

Despite the typical focus on these proximal outcomes, there are many other potential beneficial impacts that job training programs can have on individuals. These may include job and life satisfaction, physical and mental health improvements, and community and civic engagement. In this paper, we focus on an additional potential benefit, that of crime deterrence. Insofar as the returns to job training programs are not limited to labor outcomes but extend to reduced criminal activity, public policy may be underinvesting in these programs.

Despite positive employment outcomes having been shown to be related to lower criminal activity (Becker, 1968; Grogger, 1998; Raphael & Winter-Ebmer, 2001), very few rigorous studies have examined the impacts of job training programs on crime. Most evaluations are non-experimental, and the few that have been conducted using a randomized research design were conducted decades ago and have tended to focus on younger individuals, typically age 21 or less.

These studies have found mixed evidence (Visher et al., 2005). Evaluations of the Job Training Partnership Act (Orr et al., 1994) and JOBSTART (Cave, 1993) indicated these programs had no impact on arrest rates, while an evaluation of Job Corps showed it led to a slight decrease in arrest rates (Schochet et al., 2008). The National Supported Work Demonstration program was shown to have no impact on the arrest rates for those younger than 26, but did lead to a sizeable decrease in the arrest rate for participants older than 26 (Uggen, 2000). In more recent work, Davis and Heller (2020) evaluated the impact of a summer jobs program for youth and found it led to a reduction in violent crime arrests. Relatedly, job programs specifically targeted towards newly released prisoners have also found mixed impacts regarding their impacts on recidivism, with different effects found across studies as well as across participant types within studies (Bollinger & Yelowitz, 2021; Cook et al., 2015; Drake et al., 2009; Newton et al., 2018; Raphael, 2010).<sup>6</sup>

In this paper we evaluate the effects on arrests of a recently implemented job training program in New Orleans (*Career Pathways*), which offered vocational training as well as the opportunity to earn relevant industry-based credentials in one of three areas (advanced manufacturing, health care, and information technology) across 25 training cohorts. The program was implemented using a randomized control trial design and was funded by the U.S. Department of Labor's Workforce Innovation Fund between 2015 and 2019. Earlier evaluations of the program focused on labor market outcomes, and found the program had beneficial impacts on participants' wages and employment probabilities, especially among short-term unemployed workers (Baird et al., 2019; Baird, Engberg, & Gutierrez, 2022).

Our study adds to the job training-arrest rate research in three key ways. First, the training program we evaluate was targeted towards unemployed and underemployed adults of all ages,

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<sup>6</sup> Job programs targeted towards individuals recently released from prison tend to focus more on providing income supplements or transitional jobs rather than providing typical workforce development skills.

which is quite different than the previous set of programs which focused primarily on younger populations. Participants in the program were between ages 18 to 66, with a median participant age of 36. It is imperative to understand the impacts of job training programs for a broad set of individuals, given that a large fraction of the population that could potentially benefit from these programs are in somewhat older age brackets than the prior literature has examined. This is especially true considering the fact that criminal activity significantly declines with age (Laub & Sampson, 2001), and thus the results for youth may not be representative. Second, the majority of previous studies were conducted decades ago, when the focus and arrest patterns in the criminal justice system were quite different (Travis et al., 2014). Our study, which examines a program implemented in 2017 and follows arrest outcomes up through 2019, allows us to understand the link between arrest rates and job training programs in a more current setting.

Third, our paper contributes to the literature by seeking to evaluate potential mechanisms through which the estimated impact may or may not be operating, an area prior literature has not been able to definitively answer (Heller, 2014). One critique of RCTs is that researchers commonly examine only the outcomes of an RCT as implemented in that experiment's particular context as a black box, which limits the generalizability of the findings (Heckman, 2010; Ludwig et al., 2011). Understanding the mechanisms through which training programs impact arrests adds to our understanding of the generalizability of the findings and informs future design iterations of these programs so as to allow for the maximal effect on participant outcomes to be realized. Additionally, disentangling mechanisms can yield insights that extend beyond just job training programs. For example, employment or income effects on job arrests would be relevant for many programs aimed at improving these labor outcomes. So too would peer effects be relevant for any program aimed at populations with similar demographics.

In our study, we find that, among the full sample, those assigned to the treatment group were about half as likely to be arrested post-randomization as those assigned to the control group. The impact gets stronger when we restrict the analysis to only those who had at least one arrest prior to randomization, who presumably are the set of individuals most at risk of offending. Among this subsample, those in the treatment group were two-fifths as likely to be arrested post-randomization than those in the control group (a larger reduction than one half).

After establishing that being offered the training program reduces future arrests, we then turn to examining four potential mechanisms through which this effect could occur: (1) the treatment incapacitated<sup>7</sup> participants from offending during the training period by putting them into a classroom for several hours a week; (2) it improved their probability of employment; (3) it led to higher earnings; and/or (4) it exposed them to a less criminally inclined set of peers. While we find no evidence that an incapacitation effect or an employment effect are driving our main result, we do find suggestive evidence of both income and peer effects in the hypothesized directions. Challenges with disentangling mechanisms responsible for the effects of job training programs on criminal activity have been consistently noted in the literature, and our findings lend further support to the need for subsequent experimental research that is designed specifically with mechanisms analyses in mind (Cook et al., 2015; Heller, 2014; Ludwig et al., 2011; Redcross et al., 2010; Zweig et al., 2010).

The remainder of the paper is organized as follows: Section 2 provides a conceptual framework for how job training programs may impact arrests, while Section 3 provides details on the specific training program and context considered here and Section 4 describes the data.

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<sup>7</sup> Here, by the term “incapacitated” we mean are made busier or taken away from criminal activities during the training sessions, and do not mean incapacitation in the sense of being imprisoned.

Section 5 examines the impact the training program had on arrest rates, and Section 6 examines the mechanisms through which this impact might have occurred. Section 7 concludes.

## **2. Conceptual Framework**

There are several mechanisms through which a job training program can potentially lower subsequent participant arrest rates. Below we highlight the four mechanisms that we will explicitly examine in this paper, and then discuss other mechanisms that could be responsible, but are harder to empirically test.

**Potential Mechanism 1, Incapacitation during training:** Participation in the job training program may lower the opportunity individuals have to engage in criminal activity during the training period since they are spending roughly 20 hours a week in classes—this is referred to as an incapacitation effect.

**Potential Mechanism 2, Increased probability of employment:** Employment status itself, not accounting for earnings, may decrease the likelihood of criminal activity. This could happen through a post-training incapacitation while working, positive peer effects from new coworkers, or other harder to measure psychological factors, such as improved life-satisfaction and reduced psychological distress (Uggen, 2000; Uggen & Wakefield, 2008).

**Potential Mechanism 3, Increased earnings:** Larger earnings increase the opportunity cost of incarceration, and so may reduce criminal activity. Having larger earnings also reduces the incentive for property crimes, as well as the same hard to measure psychological factors described previously. An improvement in wage outcomes might be expected to reduce criminal activity due to the direct link prior research has identified between formal labor market earnings and increased criminal activity (Uggen & Thompson, 2003; Yang, 2017).

**Potential Mechanism 4, Peer effects:** Individuals attending the job training program may come in contact with peers who are motivated to succeed in the labor market and who provide a constructive feedback mechanism to the trainee, which could make the trainee less likely to offend. While most existing research on peer effects and crime has evaluated the impacts of exposure to high-risk peers on future crime (Bayer et al., 2009; Poulin et al., 2001; Stevenson, 2017), some research has suggested that exposing individuals to new networks or contexts can facilitate crime desistance (Kirk, 2015).

**Other Mechanisms:** In addition to these four mechanisms, there are also several other residual mechanisms which represent factors that are often more difficult to observe and measure. These include factors such as the beneficial effect of interaction with the government workers, the benefit of good trainers, and the overall inspiration and increased hopefulness from training that might all result in lowering the job training participant's arrest propensity. Additionally, the prior mechanisms implicitly assume that the impact of the program comes through attending the program. However, the program could also impact those selected for the treatment group even if they do not attend classes—this benefit would accrue just from being assigned to the program. This might happen if participants selected for treatment felt their local government/community was trying to help them, and as a result these individuals became less likely to break local laws.

### **3. Program Description and Experiment Design**

This study evaluates the locally-developed Career Pathways job training program, which was intended to assist unemployed, underemployed, and discouraged workers in New Orleans develop human capital that would help them succeed in one of three sectors: advanced manufacturing, healthcare, and information technology (IT). These sectors were chosen due to

high demand by local employers for workers who were skilled in these areas. The city of New Orleans' Office of Workforce Development (OWD) received a grant from the U.S. Department of Labor Workforce Innovation Fund (WIF) to implement the Career Pathways program in a randomized manner. There were three stages of the program: recruitment, randomization, and training. We provide the details of each below.

### *3.1. Recruitment*

OWD recruited individuals for the Career Pathways program by advertising on social media, the radio, Craigslist, and at the city's five Opportunity Centers.<sup>8</sup> OWD also held informational meetings about the training program at community centers, reached out to previous users of OWD services, and used One-Stop Centers to provide program information to individuals participating in government programs that were often required to partake in career readiness activities under the federal funding mandate. OWD's general target population was comprised of individuals who were dissatisfied with their current work situation. This included both workers who were unemployed, as well as those who were gainfully employed in other sectors but seeking a career change or increase in salary.

Individuals who saw the recruitment efforts and were interested in a given training area attended an orientation meeting. After this, OWD then screened the individuals still interested in participating using a method that was intended to objectively select the individuals with the greatest potential to succeed in the training (i.e., they would not drop out) and who would benefit the most from the training (i.e., they would get the biggest increase in employment and earnings). The screening used tests aimed at gauging literacy and numeracy readiness—such as the Test of Adult Basic Education—as well as a structured, scored interviews aimed at gauging responsibility,

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<sup>8</sup> OWD runs the five Opportunity Centers in New Orleans—these centers operate as the public place where OWD interacts with people seeking help with their employment situations.

availability, and other soft-skills. Candidates advanced past this screening process and who gave consent to participate in the randomization and the study were then placed into the randomization pool for the training cohort that they had selected. We term all those placed into the randomization pool as study participants.

### *3.2. Randomization*

Each of the three career pathways—advanced manufacturing, IT, and healthcare—offered multiple training cohorts throughout 2017 and 2018. There were distinct training areas within each pathway. For example, advanced manufacturing included trainings in electrical, welding, pipefitting, and lineman training. New candidates were recruited across several cohorts in each pathway and put through the screening process on a semi-continuous basis throughout this two-year time period. We define a cohort as all individuals that entered the randomization pool for a specific training area (e.g., welding) that was to begin on a given date in the same class. All individuals entering into a given randomization pool filled out a baseline survey which contained information on their gender, employment status, annual income, age, race, and military status. Once all the individuals in a given cohort completed the survey, they were randomized into treatment (invited to training) or control (not invited to training) using a stratified random sampling design. The stratification was based on four binary variables for each individual: gender (male or female), employment status (working or not working), annual income (more or less than \$5,000), and age (younger or older than 35 years old).<sup>9</sup> The training program typically started within one week of the date of randomization so as to minimize drop-out.

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<sup>9</sup> If any of the 16 strata comprised only one person, it was pooled with the closest matching stratum, according to a pre-programmed ordering of proximity (e.g., male, low-income, unemployed, and young individuals would be pooled with male, low-income, unemployed, and older individuals). Next, each person was assigned a random number and ordered by that number within each stratum, with half assigned to treatment and half to control. Strata with an odd number of individuals were randomly ordered and alternated between picking one more or one less person to be assigned treatment than control for each consecutive group. Note that military veterans were eliminated from the randomization scheme because they were guaranteed participation in the training by

Individuals that were assigned to the control group were not allowed to participate in the training program for the cohort in which they had been recruited, but they were allowed to use the local Opportunity Centers for other assistance which included accessing information on job opportunities, resume development, and attending job readiness seminars (business as usual). These services offered by the local Opportunity Centers were also provided to those in the treatment group, as well as all individuals unaffiliated with this study that were otherwise eligible. Control group participants were also permitted to return to future randomizations to attempt to join a future training. However, OWD did not actively recruit them into future cohorts. A minority of individuals that were assigned to the control group did return into later randomization cohorts. In order to retain the randomization and eliminate selection bias into returning, we classify individuals only based on their initial randomization assignment. For example, a person might be first randomized into the control group, and then return into a later cohort and be randomized into the treatment group. We would classify such an individual in the data as being assigned to the control group throughout. Thus, our analysis reflects an intent-to-treat approach with non-compliance both for the treatment group (not attending any training—17.8% of the individuals) and the control group (entering later cohorts and receiving training—13.5%). The intent-to-treat analysis will presumably produce smaller effect estimates than a treatment on the treated analysis.

Panel A of Table 1 presents information on the number of cohorts used in this paper that went through each of the three career pathways, as well as the average number of individuals in the treatment and control groups per cohort.<sup>10</sup> Candidates entered into randomization pools for a specific training pathway, and Panel B of Table 1 shows how the candidate characteristics at

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Department of Labor protocols. While these individuals participated in training, they were dropped from our sample as they could not be randomized.

<sup>10</sup> A few sample restrictions were made to generate Table 1. These restrictions are discussed in Section 4.

randomization differ across the three career pathways. Notably, those seeking training in advanced manufacturing were mainly male, while those seeking training in healthcare were mainly female. Around 90 percent of participants were African American, which is greater than their representation in the population of New Orleans.<sup>11</sup> Those seeking training in the IT pathway were more likely to be already employed and more likely to be earning an annual income above \$5000 prior to training (approximately the median, given many unemployed) than those choosing the advanced manufacturing or healthcare pathways.

### *3.3. Program Provision*

Panel C of Table 1 shows the specific areas in which cohorts could receive training under each career pathway. Candidates who were randomized into the treatment group were invited to participate in a first training round, which lasted roughly 6-8 weeks with courses typically taking place four hours a day, five days a week. After completing this program, candidates in most cohorts were given the option of enrolling in a second training program that was similar in length.<sup>12</sup> These training programs were offered in a classroom structure (as opposed to learning in the field); most were in-person, while some of the IT training cohorts were held almost exclusively online. The training programs all offered counseling and social supports to encourage course completion. They also offered students the option to be tested so they could earn various credentials that might help them earn a job in their selected career pathway—some of these credentials indicate the industry-specific knowledge the person has, while others were required of all individuals a firm might hire.

Panel D of Table 1 shows how far in the training programs candidates who were randomized into treatment progressed. 54% of those in the advanced manufacturing pathway

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<sup>11</sup> According to the 2010 Census, 60% of New Orleans residents are African American.

<sup>12</sup> As an example, the first training session in advanced manufacturing taught math and industry occupation safety. The program included nine math modules that a student had to pass before moving on to the second training program. For those choosing electrical training within the advanced manufacturing pathway, the second training session taught students entry-level skills related to electrical occupations and provided them with opportunities to apply the tools in their classes.

completed the first training, while 59% and 88% of those in the IT and healthcare pathways completed the first training, respectively.<sup>13</sup> An even smaller proportion of the individuals assigned to the treatment group started or completed the second training—about one-third of those in advanced manufacturing completed the training, while only 4% and 2% of those in IT and healthcare completed the second training, respectively—primarily due to so few of them entering the second round of training. About half of the individuals in each pathway acquired at least one credential through the program.

#### **4. Data and Summary Statistics**

The data used in this project were obtained from several sources. We obtained information on program participants' Social Security Numbers (SSN), names, birth dates, baseline income and employment status, and demographics directly from the baseline survey and consent forms participants filled out when they entered the randomization pool. Information on treatment status was recorded at randomization, and program attendance, completion, and credentialing data were collected from the training providers at the end of training. Data on employment outcomes and earnings came from the Louisiana Workforce Commission (LWC), which was matched to the sample by SSN. The LWC data encompasses all paid work history tracked by the state from 2014 quarter 1 (before training for all participants) through 2019 quarter 1.

Criminal activity data are most accurately kept at the local level, and we thus sought to obtain this information from the areas where the majority of the potential criminal activity was likely to have taken place. Study participants were recruited exclusively from the New Orleans

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<sup>13</sup> Individuals were encouraged not to work during the time period during which they were going through training; however, data that was subsequently collected on labor force involvement indicates that many of these individuals continued to work during this time period. Interviews with program providers revealed that some students dropped out because they needed to take on a job to provide for their families.

area, and thus it was important to obtain information on any criminal activity conducted in and around New Orleans.

We used two separate data sources to obtain information on all program participants' adult arrests in New Orleans and Jefferson Parish (the most populous of the four parishes (analogous to counties in other states) that are adjacent to New Orleans). First, the New Orleans Police Department (NOPD)—whose jurisdiction includes all of Orleans Parish—provided us the lifetime record of every arrest made by NOPD up through April 24, 2019 for each of the SSNs in our sample. Second, Jefferson Parish publishes their criminal court records online, and we thus manually looked up each program participant's name and date of birth in the system and recorded information on any arrests. To keep this consistent with the New Orleans data, in Jefferson Parish we only collected information on arrests up through April 24, 2019. Every arrest record listed the statute that was violated, a brief description of the crime, and either the date of the offense or the date the individual was arrested. We did not obtain any information on subsequent convictions for these crimes.<sup>14,15,16</sup> For every arrest, we used the Louisiana criminal codes, as well as the municipal criminal codes for Orleans and Jefferson parishes, to code the sentencing range in the statutory guidelines, crime type, and felony/misdemeanor status for the set of charges associated with that

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<sup>14</sup> Obtaining information on the resulting dispositions of cases in New Orleans would have been difficult because the case records for charges resolved in the municipal courts (which handle misdemeanor violations) are not available online.

<sup>15</sup> We dropped several minor offenses that seemed to indicate criminal activity we viewed as not too concerning. These were all crimes where the statutory guidelines did not allow for jail time and included violations such as minor traffic violations, begging, spitting on the sidewalk, burning trash improperly, and littering. We also dropped arrest charges associated with warrants, parole violations, and attachments. Warrants indicate a defendant is wanted in another parish for a given crime—we dropped these both because we could not observe what the crime was and when it took place, and we did not want to include criminal activity in a parish outside of Orleans and Jefferson in a haphazard way. We dropped parole violations because we did not want to count a parole violation as an offense (as these can be triggered by minor issues), and we theoretically should have already had the underlying offense in our arrest records that led to the parole sentence. Attachments often occur when an individual is arrested because they failed to pay a fine on a previous traffic ticket; we dropped these because we didn't view this violation as being too serious and we could not identify when the actual violation (not paying the fine) took place.

<sup>16</sup> Due to a mistake in the data collection procedure for Jefferson Parish, we did not collect information on arrests associated with reckless driving or driving on a suspended license. These violations both carry potential jail time and were recorded in the arrest file provided to us by NOPD. We thus are potentially missing a subset of relevant arrests in Jefferson Parish.

arrest. Using the recommended sentencing range, we retained only the dominant (i.e., most serious) charge for each arrest incident, defined as the arrest of a given person on a given date. Based on the recommended sentencing range, we classified arrest types as low-level misdemeanors, high-level misdemeanors, low-level felonies, and high-level felonies. We defined low-level misdemeanors as misdemeanors (or municipal violations) in which jail time was not possible, or that was a misdemeanor involving a traffic violation from either reckless driving or driving with a suspended license. All remaining misdemeanors were defined as high-level misdemeanors. Low-level felonies were defined as felonies with a statutory maximum sentence of 20 years or less, while high-level felonies correspond to felonies with a statutory maximum of more than 20 years. In practice, due to the small number of arrest occurrences, for the majority of analysis we focus on any type of arrest and do not make use of the information surrounding the dominant charge.

While the RCT included 25 cohorts, it was necessary to drop the first two and last three cohorts from the analysis. The first two cohorts were dropped because members of these cohorts were subject to a very different recruitment, screening, and training process than the subsequent 23 cohorts had, and thus represented a separate treatment (see Baird et al. 2019). The last three cohorts were dropped because we did not have post-training data for these cohorts. This left us with 20 cohorts to be used in the analysis (see Table 1). As noted in Section 3, we only include individuals once. Thus, if an individual was in the control group in one cohort but later entered into the treatment group in a subsequent cohort, we only include their first (control group) observation.

To arrive at our final sample, out of the 20 cohorts in the analysis we dropped 34 observations because we suspected that the SSN we were provided was invalid. We could not

match the SSN associated with these observations to any of the LWC quarterly earnings data between 2014-2018. While it is possible these individuals did not engage in any jobs that would be reported in this data set (which includes virtually all above-the-table jobs in Louisiana), we viewed it as more likely that these SSNs were invalid based on the fact that there were also no arrests associated with any of these SSNs in New Orleans. We tested including non-response weights in the analysis, but the results were not materially affected and we thus opted for the simpler model without weights. Our final sample, which we refer to as the full sample in our analysis, includes 400 individuals.

Table 2 shows the covariate balance between the treatment and control groups for the full sample, as well as the subsample of participants who had been arrested at least once before the time of randomization. Many of the results in this paper are estimated both for the full sample, as well as for this subsample of prior arrestees, because the latter represents the group that is most at risk of engaging in criminal activity in the future and thus potentially might benefit from the intervention more.<sup>17</sup> Due to the stratified random assignment scheme, we expected to find balance between the treatment and control groups for both participants' personal characteristics and the career pathway they chose among the full sample. This balance is shown in Table 2, as none of the differences for these characteristics were statistically significant. The remainder of Table 2 focuses on the balance between the two groups with respect to their criminal history, which is measured in terms of adult arrests up until the day individuals in their cohort were randomized. Individuals may have had multiple arrest incidents prior to randomization and thus may have been arrested for different types of crimes; the proportions in the crime type categories will thus not sum to one.

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<sup>17</sup> Put another way, individuals with a median age of 36 who so far have not engaged in criminal activity are unlikely to do so in the future. To the extent this group is not really at risk of committing criminal activity, this would essentially preclude the treatment from having an effect on future arrest likelihood.

The results indicate that the treatment and control groups are well-balanced with respect to criminal history (e.g., 53.0 percent with no prior arrest in the treatment group, 56.8 percent in the control group) even though we did not stratify participants based on this measure prior to randomization. Table 2 also indicates that there is general covariate balance among the subsample that has been arrested previously.

The majority of the analyses in the remainder of the paper will compare how the treatment group differs from the control group starting after randomization, formally defined as the date on which the treatment group within the cohort first began their training. The date of randomization will thus differ by cohorts and encompasses dates from February 2017 through September 2018. We observe an average of 1.6 years after randomization during which a sample member could be observed to have been arrested, although the follow-up time varies across cohorts.

Table 3 examines the prevalence of our key outcome variable of arrest activity after randomization. The first row shows that only 9% of participants are arrested for any kind of criminal activity post-randomization. Most of these arrests are for a high-level misdemeanor or felony, which in part reflects our procedure of only retaining the dominant (i.e., most serious) charge for a given arrest incident. The remainder of Table 3 shows the level of post-randomization criminal activity among participants with a given characteristic. There are a few key takeaways from this table. First, this is a population of individuals relatively at-risk for criminal activity. In Table 2, we saw that over half of the study participants had been arrested some time before randomization. Here in Table 3, we find nearly 10 percent of the total group being arrested in the on-average 1.6 years following randomization. Second, there are several groups among which there is almost no criminal activity post-randomization—in particular, only 2% of those with no arrest history were arrested post-randomization. Finally, the last four rows of Table 3 indicate that

those in the treatment group have a lower rate of arrests post-randomization despite having had a slightly higher rate pre-randomization (Table 2), although this does not account for differences in the amount of time participants in these groups are observed, the slight differences in prior arrest history, or when arrests happen. The next section will account for these using a hazard framework.

## **5. The Impact of the Career Pathways Program on Arrests**

Our analyses in this section focus on identifying the causal impact that being assigned to receive treatment has on the likelihood of being arrested, which is the intent-to-treat effect. From a policy perspective, identifying the intent-to-treat effect is the most relevant because it estimates the average impact the program might be expected to have in a jurisdiction, taking into account that some individuals that sign up for the training will end up not attending or completing that training. We use two approaches to estimate this effect, both of which utilize the survival time information in the data and take into account that the amount of time we observe individuals post-randomization varies by cohort. Throughout this section we focus on arrest activity post-randomization, as opposed to post-program completion, in order to allow the program to have a more immediate effect—this accounts for the possibility that just being assigned to the program might yield some benefits to participants regardless of whether they end up attending or completing the training. In the next section, we allow for the effect to during the training versus after the training.

Our first estimation approach calculates Kaplan-Meier survival curves for the treatment and control groups. These survival curves, shown in Figure 1, graph the probability that a member of a given treatment status will make it to a certain period of time post-randomization without getting arrested. Panel A shows the survival curves for the treatment and control groups in the full

sample, while panel B estimates these curves on the subsample of individuals that had an arrest history prior to randomization. In both cases, there is a clear separation between the treatment and control groups in time to first arrest post-randomization. The results are strongest in panel B. Examining the curves one-year out, there is about a 78% chance that those in the control group will not be arrested in the first year after randomization, while for the treatment group this probability is around 90%. The p-value corresponding to a log-rank test of whether these curves are statistically different from each other is 0.06. This difference in survival curves continues to persist two years out. The results are more muted for the overall population considered in panel A, and the p-value of the log-rank test of whether the treatment and control survival curves are the same is 0.34.

To increase the precision of these intent-to-treat estimates we move to a Cox hazard model with right-censored data, where failure in the hazard model is defined as being arrested for a crime after randomization. The Kaplan-Meier survival curves are essentially a visual representation of what the Cox hazard model will estimate. However, the Cox hazard model allows one to control for additional covariates beyond treatment status, which may increase precision. We thus estimate the following empirical model:

$$h_i(t) = h_0(t)\exp(\beta Treat_i + X_i\lambda_i) \tag{1}$$

where  $h_i(t)$  is an indicator for whether individual  $i$  was arrested in time period  $t$ , conditional on not having been arrested in time period  $t-1$ .  $Treat_i$  measures whether an individual was assigned to the treatment group (whether or not they ended up attending or completing the training), and  $X_i$  represents the covariates we include, which we discuss in more detail below. We use heteroskedasticity-robust standard errors given that random-assignment was at the individual

level, although we show at the end of this section that our results are robust to alternative standard error specifications.

One issue that arises in determining the relevant covariates to include in  $X$  is that being arrested post-randomization is a relatively rare event. As noted in Peduzzi et al. (1995), one should have at least 10 events-per-variable (EPV) when estimating a proportional hazard model. With an EPV of less than 10, the estimator can be biased and statistical significance tests can be invalid. When being arrested for a crime of any severity post-randomization is the outcome, our full sample has a total of 35 events, and our subsample of prior arrestees has 31 events. As a result, we tend towards parsimony and only include a very limited set of controls that should explain the most variation in the outcome, especially since our main goal of including covariates is to increase precision and not to remove bias (since theoretically our RCT design should not require us to control for covariates to eliminate bias). Based on the criminology literature, two of the most important predictors of future criminal activity are previous criminal activity and age (Gendreau et al., 1996). Thus, when using our sample of prior arrestees,  $X_i$  includes only two variables: the number of arrests the individual had pre-randomization, and whether they are age 35 or younger at the time of randomization. When we use the full sample,  $X_i$  includes an additional control of whether the individual had been arrested prior to randomization. While gender is an important correlate of arrest patterns (see Table 3), we do not control for it in our main analyses due to concerns with overfitting the data; however, we did estimate models which additionally control for gender, and the results are very similar.

The presented estimates for  $Treat_i$  in Table 4 represents the ratio between the hazard rates for those assigned to the treatment group relative to those assigned to the control group based on equation 1, where the definition of a failure differs across the columns as labeled. Columns 1 and

3 define a failure as being arrested for a crime of any severity post-randomization, while columns 2 and 4 define a failure as being arrested for a high-level misdemeanor or any felony. The results are shown separately for both the full sample and the subsample of prior arrestees, and we also show the hazard rate ratios for the other covariates included in  $X_i$ .

The results in Column 1 of Table 4 indicate that participants assigned to the treatment group have a hazard rate of being arrested for a crime of any severity post-randomization that is approximately half the hazard rate of the control group, although this result is only marginally significant. As hypothesized, having any prior arrests, having more prior arrests, and being younger are all predictive of being arrested (i.e., these characteristics correspond to a hazard ratio greater than one).

Column 3 repeats the analysis but limits the sample to those with prior arrests. Here, we find stronger results, with those in the treatment group having a hazard rate 40% as large as that of the control group: compared to a similar individual in the control group, a person with a prior arrest record that was assigned to the job training program was roughly two-fifths as likely to be arrested at any time point after randomization. This result is statistically significant at the five percent level. The results from columns 2 and 4, which examine the somewhat rarer arrests of high-level misdemeanors and felonies, are not as large (are closer to one) and are not statistically significant (see Appendix Figure A1 for Kaplan-Meier survival curves for this outcome). This result may at least in part be driven by not having a large enough sample to tease out this even-rarer event. We will hereafter limit our attention to any arrest, regardless of severity or type.

As noted earlier, we use heteroskedasticity-robust standard errors in this analysis. However, we also test the two main results in Table 4 using permutation tests that replicate the within-cluster stratified randomization across 1,000 alternative treatment assignments. Using this

we calculate the two-tailed p-value from the empirical distribution of estimated treatment effects. Appendix Figure A2 presents the permutation distribution from these two tests. When we do so, we find nearly the same p-values as when we use the robust standard errors. Specifically, the analytic p-value in Table 4 column 1 for the treatment effect (0.525) is 0.089 while the permutation-based p-value is 0.09. The analytic p-value in column 3 for the treatment effect (0.405) is 0.025, while the permutation-based p-value is 0.024. This lends credibility to the handling of the standard errors, and for the remainder of the paper we present the results using the analytic robust standard errors and corresponding p-values.

We also explore potential heterogeneity in treatment effects along observable demographics (see Appendix Table A1). We find suggestive evidence that the treatment effects are larger for younger trainees and those who enter randomization without a job, although none of the differences between groups was statistically significant.

## **6. Mechanisms Through Which the Training Program Impacts Arrests**

Given the beneficial effects the training program has on rearrest rates shown in Section 5, we next attempt to disentangle some of the potential mechanisms driving these results. In this section we examine, in turn, the potential role that incapacitation, employment effects, wage effects, and peer effects may play in explaining why the training program causes rearrest rates to decline.

### *6.1. Potential Mechanism 1: Incapacitation*

We first investigate the possibility that any found treatment effects are the result of incapacitation. Here, the hypothesis is that individuals in training are kept busier while in class and thus have less opportunity to commit crime, leading to fewer arrests. If the result were driven by

incapacitation, then we would expect to see differences in arrests arising during the training period, but that once the program ended—approximately two, or at most four months later—the treatment group would begin reoffending at similar rates to the control group. The Kaplan-Meier survival curves indicate this is not the case, as during the first few months the two lines between treatment and control are very similar, and only appear to diverge after the end of training. This implies that the program seems to be changing the behavior of participants rather than just incapacitating them from offending while they are taking classes. Further, this behavior change seems to be relatively long-lived, as it is present two years after randomization, which is the farthest out we can observe.

To more formally examine whether the treatment effect is present during both the training period and after the training period we use a Cox hazard model with time-varying covariates. Equation 2 presents the specification used, where time period (i.e., during or after training) is included as a regressor as well as interacted with treatment status:

$$h_{ic}(t) = h_0(t) \exp \left( \delta 1(\text{Training Period} = 1) + \sum_{j=0,1} \beta_j \text{Treat}_i \times 1(\text{Training Period} = j) + X_i \lambda_i \right) \quad (2)$$

Table 5 presents the results from estimating equation 2. In the full sample, we find no discernable difference in arrest patterns between treatment and control groups during training, but do find evidence of an effect in the post-training period. The prior arrestee sample in column 2 shows similar results; although the magnitude of the treatment effect is large for the treatment period, the standard error is also much larger, and the result is not statistically significant. Thus, we do not find evidence that incapacitation was the reason for the treatment effects we estimated.

## 6.2. Potential Mechanisms 2 and 3: Increased Employment Probability and Earnings

The second and third mechanisms we consider are whether the program’s beneficial impacts on arrest rates are driven by the program’s potential effects on labor market outcomes (employment probability and earnings, respectively). For these mechanisms to be an important factor, it first requires that those assigned to the program saw improved employment and earning outcomes. We thus begin by examining the effect of treatment assignment on employment and wage earnings through the following specification:

$$Y_{it} = \alpha^Y + \beta^Y Treat_i + \theta^Y Y_{iB} + X_i \gamma^Y + \phi_t^Y + \varepsilon_{it}^Y \quad (3)$$

$Y_{it}$  represents the outcome of interest: a binary variable for whether individual  $i$  is employed in quarter  $t$  or the dollar amount of their earnings from all reported jobs in quarter  $t$ . We control for the baseline outcome  $Y_{iB}$  (employment history prior to randomization or the individual’s average wages prior to randomization, for 0.5 to 2.5 years pre-randomization). We also control for an indicator for whether an individual was age 35 or less at the time of randomization, the total number of prior arrests an individual had, year-quarter fixed effects, and for the full sample an indicator for any arrest prior to randomization. We estimate the outcomes using seemingly unrelated estimation to allow for correlation in the error terms across outcomes within individual/quarter.

Table 6 presents the results from estimating equation 3 across both the full sample as well as the prior arrest sample. Note that the labor market effects of the program might be expected to depend on the prior criminal history of participants, as there is substantial evidence of employer discrimination against individuals with criminal records (Agan & Starr, 2018; Doleac & Hansen, 2020; Pager, 2003). We find being assigned to the treatment causes an increase in employment and earnings, and that both of these treatment effects are approximately twice as large for the prior

arrest sample than the full sample.<sup>18</sup> The treatment effects for these labor outcomes are also relatively large in magnitude relative to the means, representing increases of between 6 and 9 percent for the overall sample for employment and earnings respectively, and 9 and 22 percent for the prior arrest sample for employment and earnings respectively.

The results in Table 6 establish that being assigned to the treatment improved both employment and earnings outcomes. However, to establish that the beneficial impact the program has on arrest rates operates at least partially through its impact on employment and earnings outcomes, it must be the case that improving employment and earning outcomes leads to lower arrest rates. In contrast, if we found that these improved labor market outcomes either did not impact arrest rates (or impacted them negatively), then our results pattern would indicate that the program might be having a more direct effect on lowering arrest rates, which might then have subsequently led to improved employment outcomes. To examine whether employment outcomes drive arrest outcomes (or vice versa) we leverage differences in the *timing* of events (arrests, employment, earnings increases) to explore how these happening in a prior quarter predict each of the outcomes in the subsequent quarter, as shown in equation 4. The identification approach in equation 4 does not use the program randomization, but instead relies on differences in timing to infer causal directions. In other words, do arrests precede changes in employment and earnings, or is it the other way around? We can explore the potential directions by exploiting the longitudinal nature of our data to examine the temporal sequencing of arrests and labor outcomes.

$$Y_{it} = \alpha^Y + \lambda_A^Y \text{Arrests}_{it-1} + \lambda_E^Y \text{Emp}_{it-1} + \lambda_W^Y \text{Wage}_{it-1} + \theta^Y Y_{iB} + X_i \gamma^Y + \phi_t^Y + \varepsilon_{it}^Y \quad (4)$$

$Y_{it}$  represents either a binary variable for whether individual  $i$  is employed in quarter  $t$ , or the dollar amount of their earnings from all reported jobs in quarter  $t$ , or for whether they were arrested in

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<sup>18</sup> Note that these results include the training period. Omitting the training period in the regression sample slightly increases the treatment effects.

quarter  $t$ . For each individual, we use all quarters after randomization through the final quarter available,<sup>19</sup> leading to multiple records across time for individuals. The key variables of interest are the lags of arrests, employment status, and quarterly wages. The covariate  $Y_{iB}$  represents the average outcomes *before* the training periods—it includes controls for whether the individual was arrested and the number of arrests pre-randomization, as well as the average quarterly employment rate and average quarterly earnings for the period between 2.5 years and 0.5 years prior to the randomization. We also include time (year-by-quarter) fixed effects ( $\phi_t$ ). We again estimate all three outcome models jointly using seemingly unrelated estimation to allow for correlation in the error terms across outcomes within individual/quarter.

Table 7 presents the results from estimating equation 4. The coefficient on lagged earnings in column 6 indicates that, for employed individuals, an additional \$1,000 in earnings in the previous quarter lowers the arrest probability in the current quarter by 0.48 percentage points for the prior arrest sample. The coefficient on lagged employment is more complicated to interpret as there is not a *ceteris paribus* interpretation of this coefficient since earnings are conditional on being employed (i.e. you cannot change employment status without also changing your earning status). To estimate the joint impact of employment and wages, we estimate the treatment effect at different levels of the wage. These results are presented in Appendix Table A2. For low levels of the wage, the effect of lagged employment on arrests is negative and statistically significant. For both the full sample and the prior arrest sample, the impact of lagged employment on arrests is not significant when wages are above around the 66<sup>th</sup> percentile (\$5,900 per quarter for the full sample, and \$5,500 for the prior arrest). While the effect of employment is positive at higher earnings, it is not statistically significant, and is only significant at near the maximum observed

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<sup>19</sup> In our sample, the minimum quarters post-randomization observed is one and the maximum is eight, with an average of 4.4 quarters post-randomization.

quarterly earnings. Overall, we find that treated persons that end up in low-paying jobs are actually more likely to be arrested in the following quarter. However, higher paying jobs mitigate this negative effect. This result, combined with the results showing that higher earnings lead to lower arrests, indicates that the beneficial impact the job training program has on arrests might be partially occurring through the program's effect on earnings, but that its effects on employment are not decreasing arrest rates. Taken alongside prior evidence that a large proportion of individuals released from incarceration who obtain employment earn far below the poverty line (Cook et al., 2015), our differential findings by earnings percentile suggest that relatively high-paying labor market opportunities may be necessary to encourage desistance from illegal activities.

### *6.3. Potential Mechanism 4: Peer Effects*

The final mechanism we consider is whether the impact the job training program has on arrests operates by improving the network of peers with whom the participant interacts. If randomization to the job training program resulted in exposure to peers less prone to criminal activity than the individual would have had in the community (e.g., because those that self-select into the job training program are on average less criminal-prone than their peers), then arrests may be reduced even in the absence of training completion or labor market improvements.

To estimate the effect of cohort peers on arrest outcomes, we used the pre-treatment arrest history data for all individuals enrolled in a given cohort. For each person randomized to receive the training, we constructed their training cohort's average arrest history profile, leaving out the individuals' own arrest history prior to randomization—this is defined as the individual's peer score. We generated analogous peer scores for individuals in the same cohort who were assigned

to the control group. Peer effects were then estimated by identifying  $\beta_i$  in the following specification, which is an extension of the hazard model shown in equation 1:

$$h_{ip}(t) = h_0(t)\exp(\beta_1 Treat_i * PeerScore_i + \beta_2 PeerScore_i + \beta_3 Treat_i + \lambda_p + X_i\gamma) \quad (5)$$

$$PeerScore_i = \frac{\sum_{k \neq i} D_{i,k} * I(Treat_i = Treat_k) * PriorArrest_k}{\sum_{k \neq i} D_{i,k} * Treat_k} \quad (6)$$

where  $D_{i,k}$  is an indicator equal to one if individual  $i$  and  $k$  were randomized to the same cohort. Baird, Engberg, & Opper (2022) use a similar strategy evaluating this program looking at peer effects based on labor outcomes, and find evidence consistent with random assignment of peers and of large peer effects on labor outcomes. Here, we consider two ways of measuring peers' prior arrest history ( $PriorArrest_k$ ): (1) whether the participant had any arrest prior to randomization, and thus the peer score measures the proportion of peers with a prior arrest history, and (2) the number of arrests a participant had prior to randomization, and thus the peer score measures the average number of prior arrests among peers. Equation 5 also controls for pathway fixed effects ( $\lambda_p$ ) given strong differences in the average peer prior arrest rate (see Table 3); however, in sensitivity tests, the results are qualitatively similar when pathway fixed effects are excluded.

The peer effects specification estimated in equation 5 is akin to a difference-in-difference strategy—it essentially estimates the difference in arrest outcomes for those in the treatment group that are exposed to a higher percentage of low-arrest peers, and then subtracts the difference in arrest outcomes for those in the control group with the same peer compositions.<sup>20</sup> Subtracting off the difference in the control group counterparts reflects that certain cohorts may draw better

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<sup>20</sup> This strategy assumes that those assigned to the control group were “uncontaminated” by peer effects. If service access through the local Opportunity Centers led to increased contacts between control group individuals and peers in their cohort, this estimation strategy may bias effects downward.

participants overall and thus directly comparing treatment groups with different peer scores may pick up this selection, resulting in an overstatement of the importance of peers.

Before presenting the estimation results it is useful to first examine the distribution of the peer measures to ensure we have enough variation in peer scores to estimate heterogeneous treatment effects across the different peer score levels. Figure 2 indicates that, for the proportion of peers with an arrest pre-randomization (panel A), the peer measure ranges from individuals in cohorts with no prior arrestees (zero) to those in cohorts with all having had a prior arrest (one), with density throughout. The measure of average number of prior arrests of peers ranges from zero to just above seven. Thus, we have substantial variation in the level of peer's criminal history across trainees which we can use to estimate heterogeneity in the treatment effect.

Figure 3 presents the results from estimating the peer effects specifications, plotting the estimated effects of treatment on the hazard of post-randomization arrest at different levels of the peer group arrest history (Appendix Table A3 presents the underlying estimates). The results are shown for both the full and prior arrest sample, as well as by the two measures of peer scores we developed. For the prior arrest sample, regardless of the peer score measure used, the treatment effects are largest for treated persons in cohorts contained of peers with very low arrest probability. For example, consider the prior arrest sample, with peer effects measured by the fraction of peers with prior arrests. An individual in a cohort with all peers having zero prior arrests has a coefficient estimate around -0.9, which corresponds with a hazard ratio of around 0.4, or 2/5ths as likely to be arrested due to treatment. However, the results shift closer to zero and become statistically insignificant for treated persons with peers having more moderate or high levels of prior arrest history. The slope of that trend (having peers with lower arrest histories being related to larger treatment effects), related to the coefficient on the interaction term, is only marginally significant

for the second measure (average number of prior arrests among peers). Thus, there is suggestive evidence that peer effects may be a small contributory factor in the impact of training on arrests.

## **7. Conclusions**

This paper presents evidence that being selected to receive job training through the New Orleans Career Pathways program significantly reduced the likelihood of future arrests, especially for those with a prior arrest history. This is an important finding for a program that includes older trainees and is not specifically targeting individuals at risk for criminal activity. These results add to the relatively nascent literature examining the link between job training programs and criminal activity by focusing on a set of workers that is more representative of the disadvantaged work force than previous studies have examined, which often tended to focus on relatively young workers.

In addition to documenting this primary effect, we also explored potential mechanisms that could explain this effect, which prior research has had difficulty establishing (Davis & Heller, 2020; Zweig et al., 2010). We find that the effects were not the result of simple incapacitation, which means that an effective job training program design is likely important to achieving these results. We also find evidence in favor of an income effect on arrests, as higher lagged incomes are associated with lower probabilities of arrest and higher earnings arise from treatment, although we do not find evidence of an employment effect. We also find suggestive evidence in support of peer effects being a contributing factor, as only individuals in cohorts with peers having very low prior arrest histories had statistically significant treatment effects on later arrests. The main additional contenders that could explain the large arrest effect from job training include interaction with public programs and the training resulting in built unobserved factors, such as higher expectations for future opportunity, resilience, and self-efficacy.

The results found here indicate that when evaluating the overall effectiveness of job training programs, it is important to also consider their impacts on criminal activity, as opposed to solely focusing on their employment impacts. Further, it is crucial that job training programs do not screen out individuals with a prior criminal history, as our study indicated this group accrued benefits both with respect to earnings and lower arrest rates.

This study also highlights the fact that it is important that future randomized experiments be developed such that they would have the ability to determine the underlying mechanisms through which these job training programs lower arrest rates. The importance of designing experiments that can identify the mechanisms through which the main effect occurs is well documented in (Ludwig et al., 2011). Prior studies that have found evidence that these job training programs lower arrest rates have not been able to definitively determine why this effect occurs. This is often because these initial studies were focused on first finding an effect (regardless of mechanism), and often the arrest outcome was not the primary outcome considered. Our study provides initial evidence of potential mechanisms which can guide future study design and analysis. This would allow one to tailor future job training programs further to meet not only the primary goals of improved labor outcomes, but of this secondary goal that we demonstrate of reduced arrest probability.

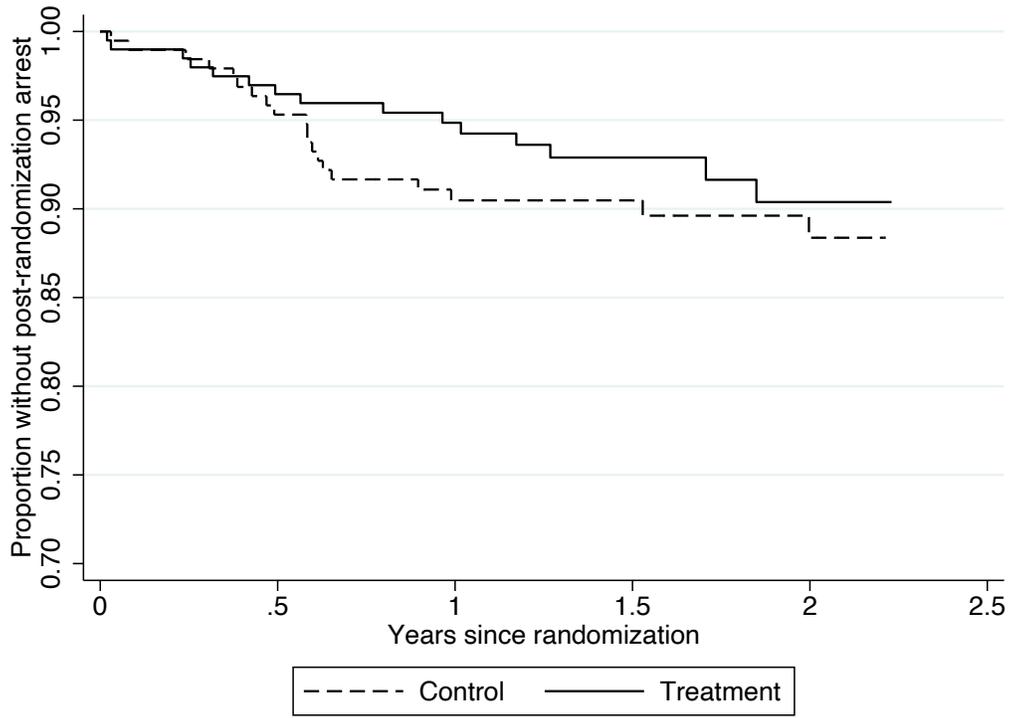
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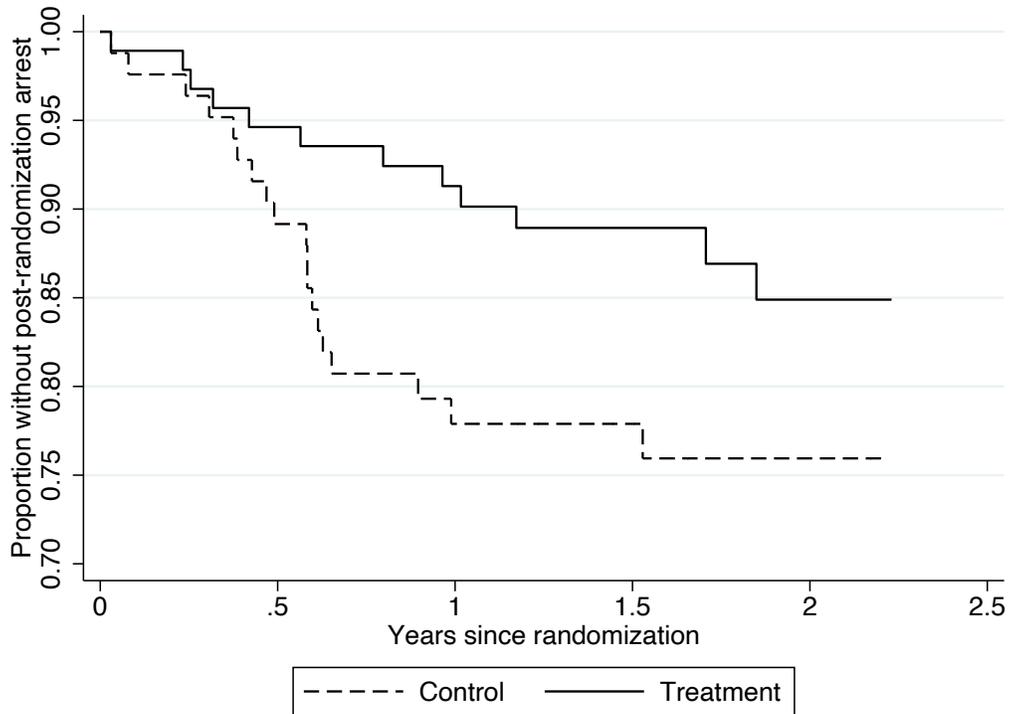
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**Figure 1.** Kaplan Meier Survival Curves for No Arrests Post-Randomization  
(a) Full sample

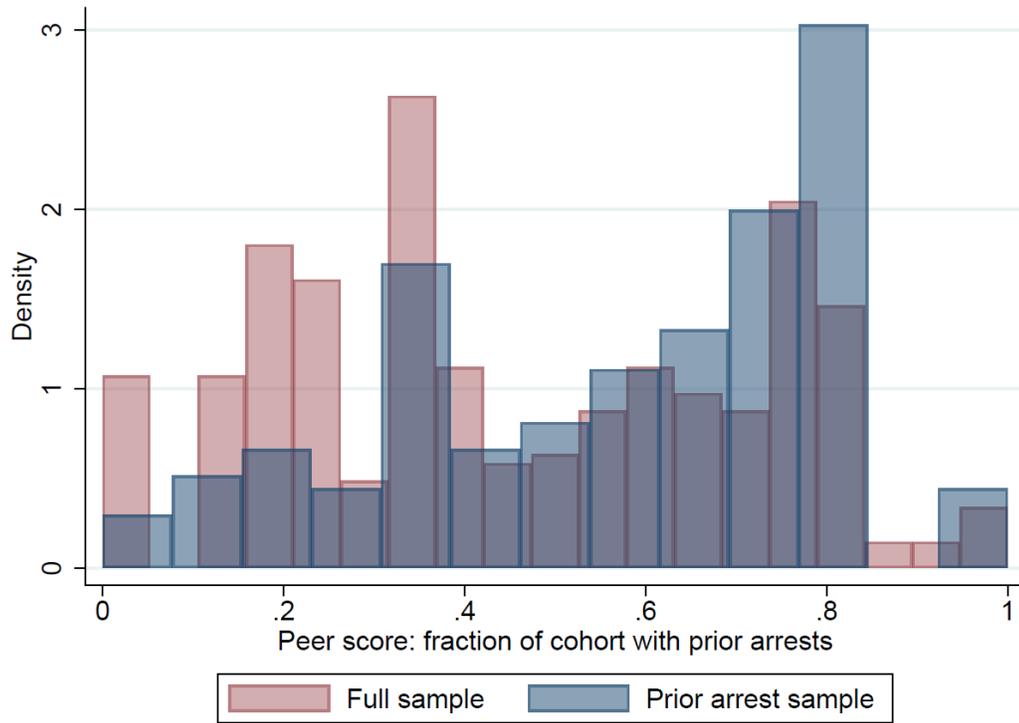


(b) Prior arrest sample

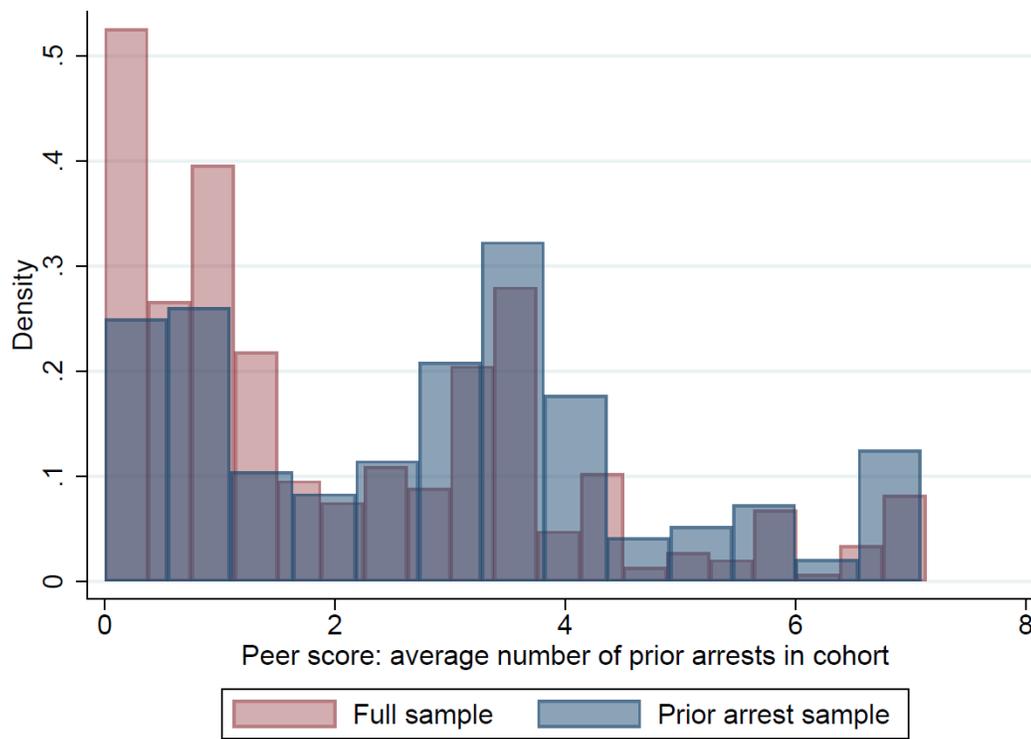


**Figure 2.** Histograms of Peer Measures

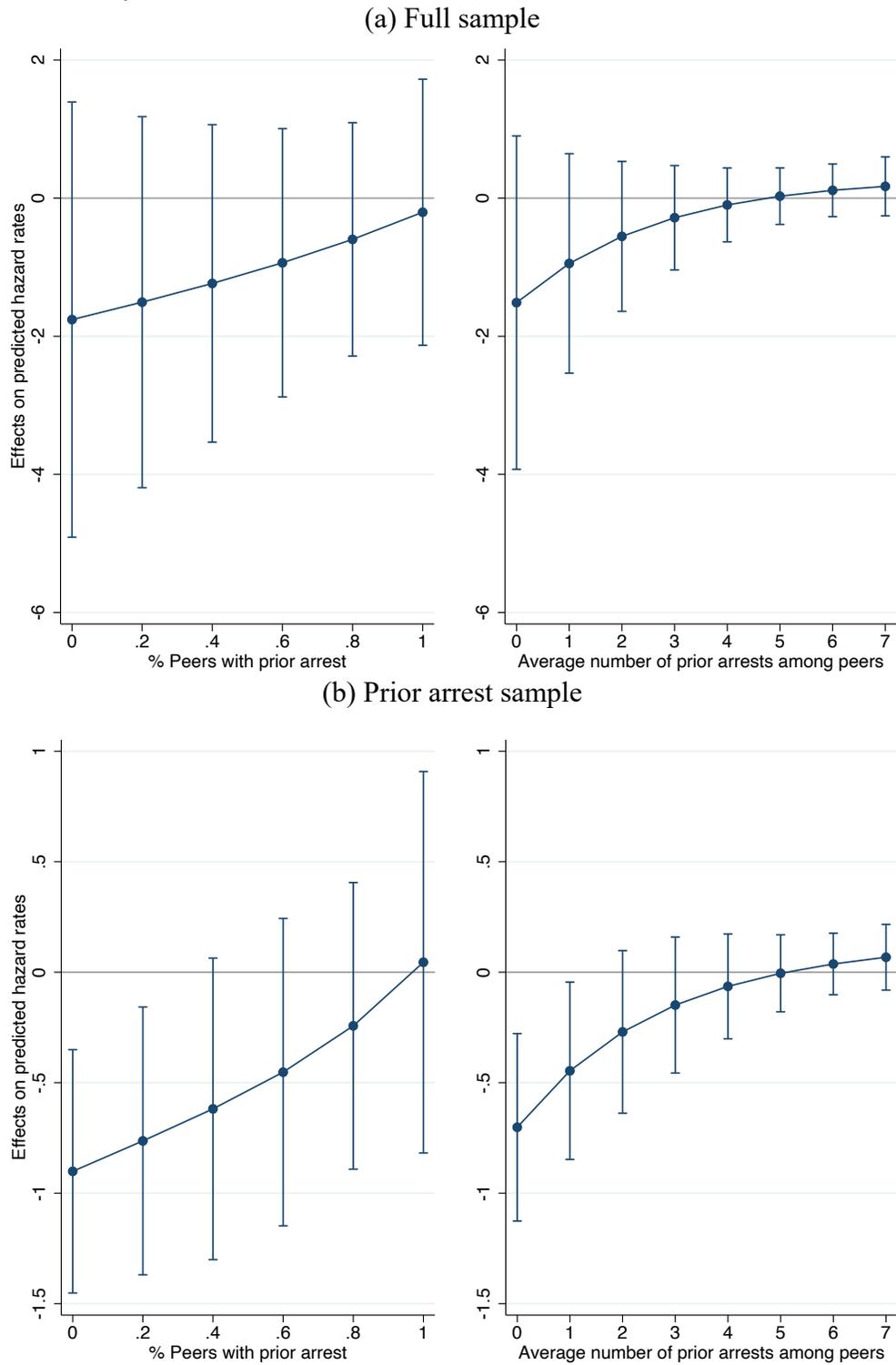
(a) Percent of peers with prior arrest



(b) Average number of prior arrests among peers



**Figure 3.** Marginal Effect of Treatment on Arrest Outcome at Different Levels of Average Peers' Criminal History



Note: Figures present average marginal effects of treatment at varying proportions of peers with a prior arrest history from Cox proportional hazard models, with bars representing 95% confidence intervals. All regressions control for number of prior arrests, an indicator for being age 35 or lower, and indicators for pathway.

**Table 1.** Characteristics of the Career Pathway Program and Participants

	Advanced manufacturing	Information technology	Healthcare
<i>Panel A: Cohort numbers</i>			
Number of cohorts	7	8	5
Average number in treatment group per cohort	13.3	6.8	10.2
Average number in control group per cohort	11.6	7.4	10.4
<i>Panel B: Candidate characteristics at randomization</i>			
Proportion age 35 or older	0.546	0.531	0.544
Proportion male	0.856	0.469	0.029
Proportion white	0.046	0.062	0.019
Proportion Black	0.885	0.858	0.971
Proportion Hispanic	0.017	0.044	0.000
Proportion other race	0.052	0.035	0.010
Proportion unemployed	0.529	0.372	0.544
Proportion with annual income below \$5,000	0.414	0.265	0.388
<i>Panel C: Training provided</i>			
Areas of training	Electrical (5 cohorts), Welding (1 cohort), Pipefitting (1 cohort)	IT (8 cohorts)	Medical billing and coding (4 cohorts), patient access representative (1 cohort)
<i>Panel D: Percent assigned to treatment that...</i>			
Attended at least one session of the first training	0.720	0.833	1.000
Completed the first training	0.538	0.593	0.882
Attended at least one session of the second training	0.376	0.148	0.020
Completed the second training	0.344	0.037	0.020
Acquired a credential	0.538	0.444	0.549

**Table 2.** Comparison of Treatment and Control Groups

	<i>Full sample</i>		<i>Prior arrest sample</i>	
	Treatment	Control	Treatment	Control
<i>Personal characteristics</i>				
Age	38.2	37.6	38.4	37.3
Proportion male	0.545	0.505	0.753	0.699
Proportion white	0.045	0.042	0.022	0.048
Proportion Black	0.909	0.891	0.925	0.916
Proportion Hispanic	0.015	0.026	0.011	0.000
Proportion other race	0.030	0.042	0.043	0.036
Proportion unemployed at randomization	0.490	0.484	0.484	0.530
Proportion Annual Income below \$5K at randomization	0.348	0.380	0.376	0.434
<i>Career Pathway</i>				
Advanced manufacturing	0.470	0.422	0.699	0.663
Information technology	0.273	0.307	0.194	0.157
Healthcare	0.258	0.271	0.108	0.181
<i>Arrest History Before Randomization</i>				
No prior criminal activity	0.530	0.568	-	-
<i>Highest charge arrest for:</i>				
Low-level Misdemeanor	0.056	0.042	0.118	0.096
High-level misdemeanor	0.136	0.151	0.290	0.349
Low-level felony	0.126	0.094	0.269	0.217
High-level felony	0.152	0.146	0.323	0.337
<i>Type of criminal activity arrested for:</i>				
Violent crime	0.146	0.167	0.312	0.386
Property crime	0.177	0.125	0.376	0.289
Drug crime	0.237	0.229	0.505	0.530
Weapon crime	0.056	0.052	0.118	0.120
Traffic crime	0.227	0.193	0.484	0.446
Other crime	0.242	0.188	0.516	0.434
<i>Summary</i>				
Number of prior arrests	2.419	1.760	5.151	4.072
Number of misdemeanor arrests	1.697	1.281	3.613	2.964
Number of felony arrests	0.722	0.479	1.538	1.108
Number of years since last arrest	-	-	6.111	6.141
Range of time observed post-randomization	0.60-2.23	0.64-2.21	0.66-2.23	0.66-2.21
Years observed post-treatment	1.646	1.666	1.802	1.706
Sample size	198	192	93	83

Note: Most IT cohorts were at least partly asynchronous and online, such that trainees could start on different days. We assigned the mean training start date within cohort to the control

group members in these cases. No differences between treatment and control means are statistically significant at  $p < 0.05$ .

**Table 3.** Proportion of Subsample with given Post-Randomization Arrest Outcome

	Any arrest	High misdemeanor or felony
Overall	0.090	0.079
<b>Subsample</b>		
<i>Personal Characteristics</i>		
Age 35 or Older	0.052	0.052
Younger than 35	0.134	0.112
Male	0.137	0.117
Female	0.038	0.038
African American	0.085	0.077
Not African American	0.128	0.103
Employed	0.085	0.070
Unemployed	0.095	0.089
Annual Income Above 5k	0.073	0.060
Annual Income Below 5k	0.120	0.113
<i>Training Program</i>		
Manufacturing	0.167	0.155
Information Technology	0.044	0.027
Healthcare	0.010	0.010
<i>Prior Criminal Activity</i>		
Arrested at Least Once Before	0.176	0.153
No Arrest History	0.019	0.019
Arrested w/in last 5 years	0.253	0.231
Not Arrested w/in last 5 years	0.040	0.033
<i>Treatment vs. Control</i>		
Treatment	0.076	0.076
Control	0.104	0.083
Treatment, prior arrest	0.129	0.129
Control, prior arrest	0.229	0.181

**Table 4.** Intent-to-Treat Effect of Program on Hazard Rate of Arrests

	(1)	(2)	(3)	(4)
	<i>Full sample</i>		<i>Prior arrest sample</i>	
	Any arrest	High misdemeanor or felony	Any arrest	High misdemeanor or felony
Treat	0.525* (0.199)	0.759 (0.293)	0.405** (0.163)	0.605 (0.247)
Arrested before randomization	6.902*** (3.829)	6.138*** (3.439)		
Number of arrests before randomization	1.087*** (0.0302)	1.067** (0.0313)	1.094*** (0.0298)	1.073** (0.0312)
Age $\leq$ 35	2.940*** (1.087)	2.372** (0.916)	2.827*** (1.105)	2.189* (0.896)
Observations	390	390	176	176

Note: \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Robust standard errors in parentheses.

**Table 5.** Intent-to-Treat Effect of Program on Hazard Rate of Arrests, Differential Effects for Training Period

	(1) <i>Full sample</i>	(2) <i>Prior arrest sample</i>
Training period X treat	0.893 (0.884)	0.422 (0.509)
Post-training period X treat	0.487* (0.196)	0.401** (0.171)
Training period	1.080 (0.971)	1.565 (1.384)
Observations	776	349
p-value on difference between training period X treat and post-training period X treat	0.561	0.968

Note: \*p<0.1, \*\*p<0.05, \*\*\*p<0.01. Cox proportional hazard model, where failure is measured by whether the participant had an arrest post-randomization. Each regression additionally controls for number of arrests prior to randomization and an indicator for whether they were age 35 or less at the time of randomization. Model 1 additionally controls for being arrested prior to randomization. Robust standard errors are in parentheses.

**Table 6.** Intent-to-Treat Effect of Program on Quarterly Employment, Wages, And Arrests

	(1)	(2)	(4)	(5)
	<i>Full sample</i>		<i>Prior arrest sample</i>	
	Employed	Total earnings	Employed	Total earnings
Treatment	0.0390** (0.0183)	296.4** (146.9)	0.0610** (0.0265)	685.4*** (200.4)
Employed at baseline	0.263*** (0.0240)		0.236*** (0.0356)	
Average quarterly earnings at baseline		0.334*** (0.0262)		0.288*** (0.0334)
Arrested before randomization	0.0784*** (0.0224)	251.1 (169.5)		
Number of arrests before randomization	-0.0108*** (0.00284)	-77.62*** (17.63)	-0.0116*** (0.00284)	-87.52*** (17.81)
Age≤35	-0.0488*** (0.0182)	-306.7** (147.2)	-0.0260 (0.0263)	-52.16 (200.3)
Number of observations	2,604	2,604	1,252	1,252
Number of individuals	390	390	176	176
Average outcome	0.655	3,377.09	0.666	3,174.73

Note: \*p<0.1, \*\*p<0.05, \*\*\*p<0.01. Linear regressions additionally control for year-quarter fixed effects. Robust standard errors estimated using seemingly unrelated estimation within samples.

**Table 7. Regressions Exploiting Timing of Arrests and Earnings**

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Full sample</i>			<i>Prior arrest sample</i>		
	Employed	Total earnings	Arrested this quarter	Employed	Total earnings	Arrested this quarter
Lagged employment	0.470*** (0.0227)	0.351** (0.143)	0.0230*** (0.00704)	0.426*** (0.0329)	0.609*** (0.201)	0.0450*** (0.0139)
Lagged earnings (\$1000s)	0.0288*** (0.00236)	0.768*** (0.0259)	-0.00234*** (0.000764)	0.0297*** (0.00359)	0.690*** (0.0408)	-0.00481*** (0.00165)
Lagged arrest	-0.117 (0.0732)	-0.242 (0.360)	0.0576 (0.0467)	-0.136* (0.0743)	-0.391 (0.365)	0.0534 (0.0494)
Arrested before randomization	0.0380** (0.0176)	0.0265 (0.112)	0.0149** (0.00620)			
Number of arrests before randomization	-0.00328 (0.00223)	-0.0240** (0.0116)	0.00279** (0.00123)	-0.00352 (0.00225)	-0.0276** (0.0117)	0.00291** (0.00125)
Employed at baseline	0.126*** (0.0273)	-0.0971 (0.188)	-0.00764 (0.00930)	0.0934** (0.0415)	-0.121 (0.282)	-0.00151 (0.0187)
Average quarterly earnings at baseline	-0.0089*** (0.00278)	0.0717*** (0.0223)	0.000311 (0.000594)	-0.00423 (0.00447)	0.0811** (0.0356)	-0.000709 (0.00133)
Age>35	-0.0154 (0.0146)	-0.0440 (0.0988)	-0.0209*** (0.00524)	-0.0111 (0.0219)	0.0821 (0.140)	-0.0385*** (0.0103)
Number of observations	2,604	2,604	2,604	1,252	1,252	1,252
Number of individuals	390	390	390	176	176	176
Average outcome	0.655	3,377.09	0.017	0.666	3,174.73	0.033

Note: \*p<0.1, \*\*p<0.05, \*\*\*p<0.01. Linear regressions additionally control for year-quarter fixed effects. Robust standard errors estimated using seemingly unrelated estimation within samples.

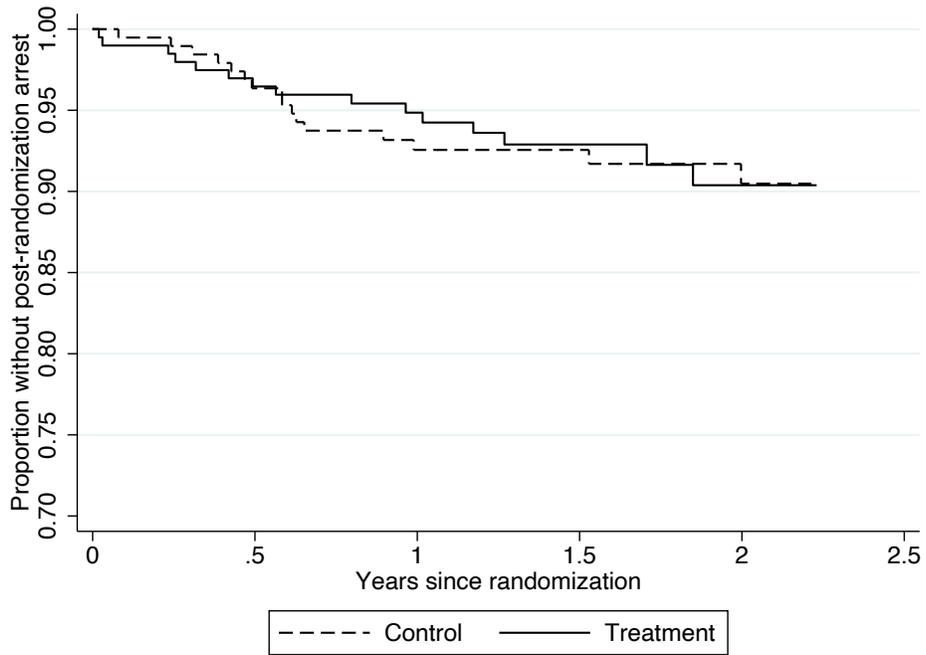
**SUPPLEMENTARY APPENDIX**

Figures and Tables

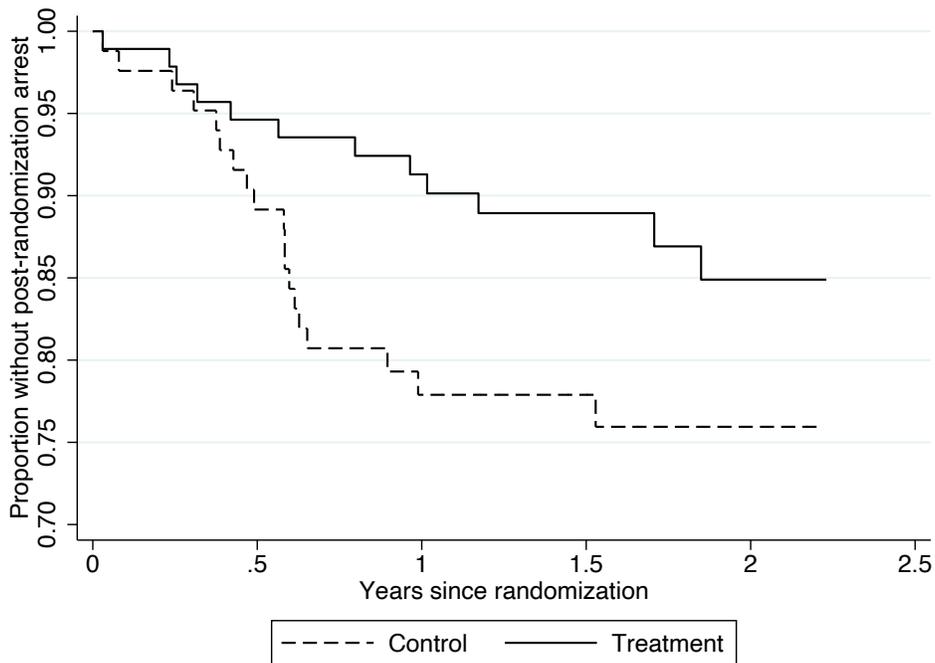
**Figure A1.** Kaplan Meier Survival Curves for No High-Level Misdemeanor or Felony Arrests

Post-Randomization

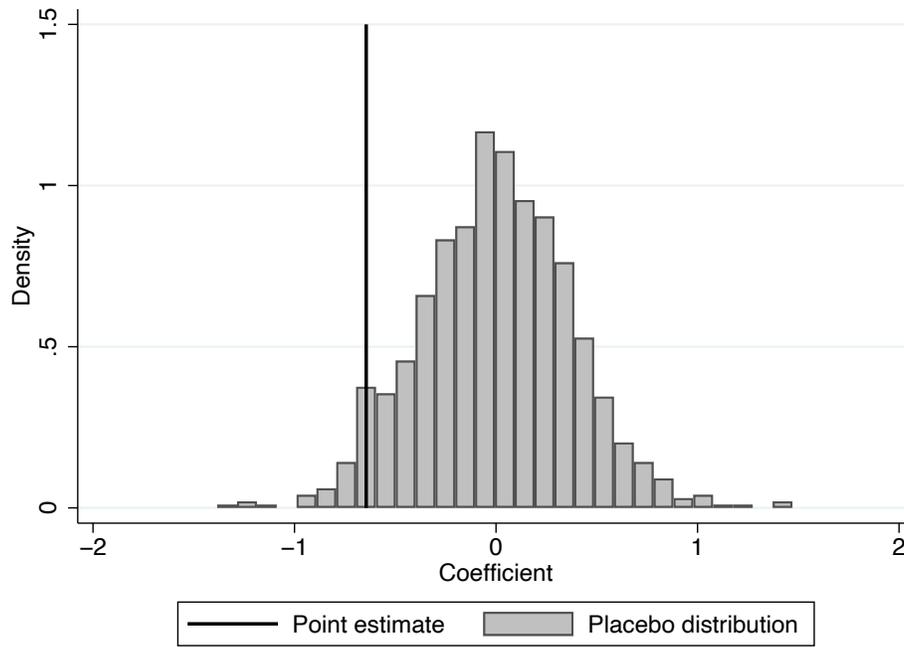
(a) Full sample



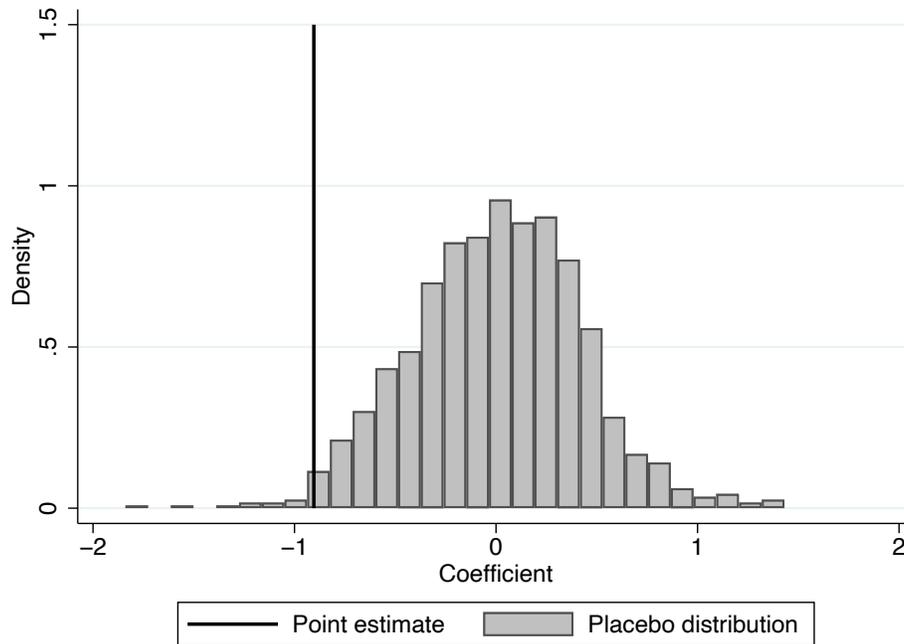
(b) Prior arrest sample



**Figure A2. Permutation Distributions of Coefficients**  
(a) Full sample



(b) Prior arrest sample



Note: Panel (a) permutation-based two-tailed p-value=0.09, compared to analytic p-value of 0.089. Panel (b) permutation-based two-tailed p-value=0.024, compared to analytic p-value of 0.025.

**Table A1.** Treatment Effects of Program on Hazard Rate of Arrests Among Various Sub-Samples

	Hazard Ratio	Standard error of coefficient
<i>Panel A: Treatment Impacts Across Gender Groups</i>		
Treat*Male	0.410*	0.191
Treat*Female	0.388	0.262
p-value for equality of interaction hazard ratios		0.945
<i>Panel B: Treatment Impacts Across Age Groups</i>		
Treat*Age≤35	0.320**	0.168
Treat*Age>35	0.614	0.392
p-value for equality of interaction hazard ratios		0.416
<i>Panel C: Treatment Impacts by Employment Status Prior to Randomization</i>		
Treat*Unemployed	0.323*	0.205
Treat*Employed	0.490	0.248
p-value for equality of interaction hazard ratios		0.605
<i>Panel D: Treatment Impacts by Annual Income Prior to Randomization</i>		
Treat*Income<\$5000	0.410	0.237
Treat*Income>\$5000	0.406	0.222
p-value for equality of interaction hazard ratios		0.988
<i>Panel E: Treatment Impacts by Severity of Prior Criminal Activity</i>		
Treat*Most Serious Prior Arrest is a Felony	0.446*	0.209
Treat*Most Serious Prior Arrest is a Misdemeanor	0.339	0.282
p-value for equality of interaction hazard ratios		0.777

Note: \*p<0.1, \*\*p<0.05, \*\*\*p<0.01. Each panel corresponds to a separate Cox proportional hazard model, where failure is measured by whether the participant had an arrest post-randomization. All specifications are run on the subsample of participants with prior arrests, and include controls for the covariate being interacted in the model, as well as the total number of prior arrests the individual had and an indicator for whether they were age 35 or less at the time of randomization. Robust standard errors are in parentheses. N=176.

**Table A2.** Predicted Treatment Effect of Lagged Employment and Earnings on Arrests at Different Earnings Levels

Earnings percentile	Full sample			Prior arrest sample		
	Earnings	Total impact of arrest on probability	p-value	Earnings	Total impact of arrest on probability	p-value
Minimum	0.001	0.023	0.001	0.002	0.045	0.001
10th	0.464	0.022	0.001	0.472	0.043	0.001
25th	1.722	0.019	0.002	1.660	0.037	0.002
50th	4.415	0.013	0.012	4.184	0.025	0.013
75th	6.930	0.007	0.143	6.316	0.015	0.122
90th	9.583	0.001	0.909	8.882	0.002	0.827
Maximum	29.924	-0.047	0.010	19.309	-0.048	0.043

**Table A3.** Effect of Average Peers' Criminal History on Arrest Outcome

	(1)	(2)	(3)	(4)
	<i>Full sample</i>		<i>Prior arrest sample</i>	
Treat	-1.788 (1.125)	-1.591** (0.696)	-3.011** (1.287)	-2.164*** (0.764)
% Peers with prior arrest before	-0.370 (0.990)		-0.602 (0.981)	
Treat x % Peers with prior arrest before	1.637 (1.662)		3.094 (1.921)	
Average # prior arrests among peers		-0.362* (0.205)		-0.382* (0.217)
Treat x Average # prior arrests among peers		0.336 (0.226)		0.424* (0.244)
Prior arrest before	1.692** (0.735)	1.719** (0.724)		
Average # prior arrests	0.0633** (0.0292)	0.0595** (0.0284)	0.0701** (0.0294)	0.0658** (0.0291)
Number of observations	390	390	176	176

Note: Coefficients reported instead of hazard ratios. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . All specifications include controls for age over 35 and training pathway fixed effects. Robust standard errors are in parentheses.