The Impact of Intensive Job-Search Assistance on Outcomes of Former Inmates

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Abstract: Of the 650,000 inmates released from prisons and jails in the United States each year, as many as two-thirds will be arrested for a new offense within three years. This study evaluates the impact of enhanced job-readiness training and job-search assistance on reducing recidivism rates among ex-offenders. This research examines the impact of an intensive job assistance program provided to recent parolees. The program also uses a network of employers, who are open to hiring ex-offenders and with whom it has long-term relationships, to place clients. Parolees were randomly assigned to standard job training and the more intensive program. We find that the intensive program reduces the likelihood that non-violent ex-offenders will be rearrested however the program has little effect on violent offenders. Only 31.1 percent of nonviolent ex-offenders who received enhanced training were arrested during the 18 to 36 months in which they were tracked, compared with 50 percent of similar participants who received standard training. Former inmates with histories of violence were rearrested at virtually the same regardless of enhanced training or not: 44.6 percent versus 42.6 percent, respectively. Findings for criminal convictions show similar patterns for arrests. Kaplan-Meyer estimates also support the main finding. The program costs approximately $5,000 for each former inmate. Using standard results in the literature, the benefits to society from averted crimes are estimated to be about $231,000 for each nonviolent ex-offender who received extra help.

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I. Introduction

Approximately 650,000 people are released from federal and state jails and prisons in the U.S. annually.[2] Ex-offenders face daunting challenges in returning to society. Upon release, they are likely to struggle with substance abuse, lack of adequate education and job skills, limited housing options, and mental health issues.[3]

A great deal of taxpayer money has been spent on programs devoted to foster job training and employment for this group. The U.S. has a long history of providing federal funding for community employment programs for ex-offenders, generally involving some combination of job-readiness (résumé writing, interview techniques, and the like), job-training (teaching skills related to specific jobs), and job-placement services (Visher et al. 2005). Although the direct benefits that come from such programs accrue to ex-offenders and are therefore private in nature, such programs also create social returns by lowering an individual's likelihood of recidivism (Drake et al. 2009; Bushway and Apel 2012). Having a legitimate job reduces the likelihood of recidivism for ex-offenders (Sampson and Laub 1997; Harer 1994).

Recidivism rates are extremely high; roughly two-thirds of ex-offenders are arrested for a new offense within three years of their release (Beck and Shipley 1989; Langan and Levin 2002).[4] If job-assistance programs reduce subsequent criminal activity as well as the chance that ex-offenders will be rearrested, social returns will be large; in the U.S., more than 23 million criminal offenses were committed in 2007, resulting in approximately $15 billion in economic losses to victims and $179 billion in government expenditures on police, judicial, and legal activities, as well as corrections (U.S. Department of Justice 2004, 2007, 2008). As McCollister et al. (2010) show, even relatively small crimes—like vandalism and larceny/theft—entail social costs of several thousand dollars, while major crimes—rape/sexual assault and murder—impose extremely high costs on society.

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[2] See http://www.justice.gov/archive/fbci/progmenu_reentry.html. Holzer et al. (2003) note that more than 600,000 offenders are released, while Raphael (2010) notes that 725,000 inmates were released from either state or federal facilities. In 2011, more than 688,000 were released (Carson and Sabol, 2012).

[3] See http://www.nij.gov/topics/corrections/reentry/Pages/welcome.aspx.

[4] The recidivism rates are quite dated. The latest published Bureau of Justice Statistics (BJS) study on recidivism comes from prisoners released in 1994 from 15 states. A different research report found that the three-year recidivism rate was 45.4 percent for inmates released in 1999 and 43.3 percent for those freed in 2004 (Pew Center on the States 2011). The BJS notes that a new study on the recidivism rates of state prisoners released in 2005 was due in 2013. See http://www.bjs.gov/index.cfm?ty=datool&surl=/recidivism/index.cfm.

With respect to finding employment, ex-offenders face many challenges because of supply-side factors as well as demand-side factors.[5] One important supply-side factor is the low level of education, training, and job experience possessed by many ex-offenders. Researchers have found that 40–70 percent of ex-offenders are high school dropouts (Harlow 2003; Travis et al. 2001; Freeman 1992). Harlow (2003) also found that 21–38 percent were unemployed when initially incarcerated.

Ex-offenders also face important demand-side barriers; most employers are very reluctant to hire individuals with criminal records (Holzer et al. 2003). Some jobs or occupations are legally closed to those with felony convictions (Hahn 1991), while other jobs require significant levels of trustworthiness that ex-offenders are unlikely to have (Holzer et al. 2003). Many companies are also averse to employing ex-offenders because of the legal risk from negligent hiring (Glynn 1988; Bushway 1996; Connerley et al. 2001).

Widespread use of criminal background checks increases the difficulty for ex-offenders to find employment. The National Task Force on the Commercial Sale of Criminal Justice Information notes an "explosion" in criminal background checks since September 11, 2001, with millions of additional criminal record checks routinely conducted.[6] Approximately two-thirds of employers conduct criminal background checks on all job candidates (Society for Human Resource Management 2012). Roughly half conduct such checks to reduce liability for negligent hiring and to ensure a safe work environment. Nonviolent felonies, in addition to violent crimes, are very influential in decisions not to extend job offers.

To the extent that job-assistance programs can overcome inherent barriers that ex-offenders face obtaining employment, such programs could play a role in reducing criminal recidivism. This study provides results from an experimental evaluation of the America Works enhanced training program. The experiment involved 259 ex-offenders. Randomization enrolled approximately half to the intensive program (130) while the remainder (129) were enrolled in a typical program offering less intensive job-readiness skills teaching and help with self-directed job searches, as opposed to formal placement. Training sessions were administered between June

[5] Holzer et al. (2003) note that supply-side factors include limited education, cognitive skills. and work experience as well as substance abuse and other physical/mental health problems. Many ex-offenders also face racial discrimination.

[6] See http://www.search.org/files/pdf/ReportofNTFCBA.pdf and http://www.search.org/files/pdf/rntfcscjri.pdf.

2009 and December 2010. Participants were then tracked for 18 to 36 months for differences in criminal recidivism.

The intensive program was administered by America Works, a New York–based private employment company with operations in seven states and the District of Columbia. The firm has 30 years of experience providing job training programs to groups that typically face significant barriers in the labor market. The program consists of intensive, short-term job-readiness training, job placement, re-placement in cases where the initial placement does not last, and regular follow-up and support for six months to ensure successful employment.[7]

Several features distinguish the America Works program from other employment programs. As with many of the chronically unemployed, overwhelming numbers of ex-offenders lack work experience, have little education (only a handful have a high school diploma or GED), and do not know how to look for a job. Ex-offenders appreciate the short-term nature of the America Works program (one to two weeks) and respond well to its tough-love approach. Above all, the program stresses interpersonal communication: listening to coworkers and supervisors, following instructions, and being honest and responsive. Other "soft skills," such as time management and anger management, are also developed. For the ex-offender population, this training may have particular resonance, as it reinforces coping and communications skills learned in prison. It is important to note that America Works operates exclusively through performance-based contracts: the firm does not receive payment for services until clients are placed and retained for a stipulated period in a job. The company's contracts with New York's Human Resources Administration (HRA) and other agencies ensure that the jobs that America Works finds for its clients are good matches and that its clients are paid fairly and have opportunities to advance. The company actively engages in finding further placements if initial placements are not successful. America Works provides a guarantee to employers that it can successfully fill positions and ensures that if problems arise, employers can discuss their concerns. Given employers' reluctance to consider ex-offenders, such a guarantee may be an important impetus to hiring high-risk applicants.

Based upon the observed characteristics, the study finds that the randomization appears to be largely successful in controlling for observable differences. Using standard approaches, we

[7] The description of America Works programs closely follows internal memos produced by Public/Private Ventures, "Moving Men into the Mainstream: Study Brief," April 2006.

find that overall while the American Works intensive program results in slightly lower recidivism than the standard program, the difference is not statistically significant. However, this simple finding masks an important difference between violent and non-violent offenders. The non-violent offenders appear to respond more favorably to the intensive program. Violent offenders in the treatment group have a re-arrest rate of 44.6 percent, while violent offenders in the control group have a re-arrest rate of 42.6 percent. This difference is not statistically significant. In contrast, only 31.1 percent of nonviolent offenders in the treatment group were rearrested during the observation period, compared with 50 percent in the control group. The baseline finding is then examined using survival analysis, regression analysis comparing finer treatment groups, and finally regression analysis comparing estimated societal costs of recidivism. In all cases we find supporting evidence that the treatment program is effective for non-violent offenders, but not statistically or economically effective for violent offenders.

Section II of this paper summarizes existing evidence on reintegrating ex-offenders into society. Section III describes the aforementioned randomized controlled trial. Section IV discusses various limitations of the analysis and explains the focus on criminal recidivism. Section V provides data description and empirical results. Section VI illustrates the costs and benefits of enhanced job placement. Section VII offers concluding thoughts.

II. Existing Evidence on Reintegrating Ex-Offenders into Society

As Visher et al. (2005) note, community-based employment interventions for ex-offenders date as far back as the 1960s, with a series of well-known federal job-training programs following in the 1970s and 1980s, including the 1973 Comprehensive Employment and Training Act (CETA), the 1983 Job Training and Partnership Act (JTPA), and the 1998 Workforce Investment Act (WIN). However, virtually all evaluations of prisoner reentry and crime-abatement programs use nonexperimental techniques. Drake et al. (2009) identify 545 program evaluations, of which fewer than 5 percent used randomized controls. As a consequence, relatively few studies are comparable with this paper.

Visher et al. (2005) conduct a meta-analysis of experimental evaluations of noncustodial employment programs for adult ex-offenders, where the program had to include, at a minimum, job training or placement. They note that only eight studies using random assignment could be identified in English-language publications; they characterize the knowledge about the effects of

such programs as "hampered by inadequate contemporary research." The eight studies, implemented between 1971 and 1994, involved the Baltimore Living Insurance for Ex-Prisoners (LIFE); Transitional Aid Research Project (TARP); National Supported Work Demonstration (NSW), a job-training program for probationers; Job Training Partnership Act (JTPA); JOBSTART; Job Corps; and Opportunity to Succeed (OPTS). In these studies, recidivism measures included arrests, based on official records or self-reported behavior, for periods of up to 36 months after participation in the employment program. Based on their meta-analysis, Visher et al. conclude that the "eight interventions had no significant effect on the likelihood that participants would be rearrested."

Raphael (2010) discusses a number of more recent experimental studies of prisoner reentry efforts, including the Center for Employment Opportunities (CEO), based in New York City. The one-year evaluations of this program show little impact on recidivism (Bloom et al. 2007), but the second-year results showed that the treatment group was 7.7 percentage points less likely to be convicted of a crime and 7 percent less likely to have experienced a post-release incarceration in prison or jail (Redcross et al. 2010). Raphael concludes that there is some evidence that income support, transitional employment, and human capital investments in ex-offenders may reduce criminal behavior and recidivism.

Several key points should be kept in mind about existing literature. First, almost all the studies are quite dated; the most recent study in Visher et al.'s meta-analysis was from 1999. They note that the lack of federal funding for ex-offender programs in the 1980s created a gap in the development and implementation of such programs. Second, the types of offenses and number of arrests may matter for the efficacy of employment services. The OPTS program, initiated in 1994, targeted ex-offenders with histories of alcohol and drug offenses. The LIFE program targeted those with high likelihoods of future arrest for property crimes and no history of drug or alcohol dependence. The NSW evaluation distinguished drug addicts from ex-offenders.

This paper contributes to existing knowledge in several ways. The America Works experiment is contemporary; the evaluation occurred in 2009 and 2010, with recidivism measured through 2012. It explicitly separates results by offenders' arrest histories: violent ex-offenders are separated from nonviolent ex-offenders. Certain results also explore the importance

of the number of charges associated with arrests. Perhaps as a consequence, this paper's results on recidivism differ markedly from those of some previous studies.

III. Description of the Randomized Controlled Trial

The randomized controlled trial (RCT) was overseen by Public/Private Ventures (P/PV), a nonprofit, nonpartisan, social research and policy organization whose mission was to improve the effectiveness of policies, programs, and community initiatives, especially as they affect vulnerable communities, at the America Works offices in 2009 and 2010.[8]

Both the intensive program and the standard program were administered by America Works. The intensive program included job-readiness training, job placement, and job retention. Obtaining and keeping a job require a set of skills. The program typically lasted two weeks with nearly daily training. The training typically focused on developing self-presentation skills through interview rehearsals and résumé preparation. The training also included work on following directions and communications with supervisors and co-workers. Following training, America Works arranges job interviews with employers and, when the placement is made, stays in contact with new hires and their employers for six months.

The intensive program is unique, but well established. The combination of both intensive training and the intensive placement and follow up services is likely extremely important. While this study is unique in examining the role of this program, we are limited in that we cannot differentiate the importance of the individual aspects of the program. Surprisingly, this program has not been evaluated rigorously before.

Recruitment of this study's 254 participants (and an additional five hardship cases) took place at the New York offices of America Works from June 15, 2009, to December 17, 2010. Participants were all men who had been released from a prison, jail, or youth correctional facility within six months prior to their acceptance in the program. When a potential participant was identified, America Works described its program and completed typical intake procedures. America Works explained that a study of the program was being conducted and that participants had a 50/50 chance of receiving enhanced services, while other participants would receive typical employment services. America Works then distributed written informed consent forms to potential participants.

[8] The discussion in this section follows directly from P/PV's document "AW Study Rationale Brief," August 2006.

The key difference between the treatment and control group is the scope and focus of services offered. P/PV documentation described enhanced services as: (1) intensive job-readiness training, (2) rapid-attachment job-placement services, and (3) retention services. Typical services involved: (1) job-readiness training and (2) self-directed job-search assistance.[9]

This RCT therefore aims to increase knowledge about the effectiveness of rapid attachment to the labor market; given data constraints discussed later, the analysis here examines the causal effect of enhanced services on criminal recidivism. Although the underlying causal mechanism is that enhanced services lead to better labor-market outcomes and less dependence on government programs—both of which, in turn, lead to reductions in recidivism— it is more difficult to convincingly examine intermediate steps.

IV. Research Questions: Opportunities and Limitations

Although P/PV successfully carried out the randomized intervention of enhanced job placement at the offices of America Works, collecting demographic and socioeconomic data at the time of the trial as well, P/PV was unable to gather data on certain outcomes that might have resulted from the intervention—outcomes pertaining to the labor market, use of government welfare programs, and criminal activity. Such data are necessary to determine if enhanced services have beneficial effects in those areas. Although gathering data on welfare use and labor-market outcomes was deemed infeasible, it was possible to obtain comprehensive data on criminal histories, both before and after interventions. Criminal history record searches were conducted through the New York State Unified Court System in early August 2012. The court system website describes the record search:

The New York State Office of Court Administration (OCA) provides a New York Statewide criminal history record search (CHRS) for a fee of $65.00.[10] One can submit a CHRS request via the on-line Direct Access program or by mailing in a CHRS application form. The search criteria are based on an exact match of Name and date of birth. The search results are

[9] On its website, America Works describes four steps that it takes to get program participants back to full-time work rapidly. One step is a job-readiness program focusing on the "hard" and "soft" skills that employers are looking for. A second is sending participants to interviews and matching them with specific jobs. A third is continuing support for the participant after he finds a job (i.e., have a case manager follow up to ensure that the client is getting to work on time each day). A final step is working with participants to ensure that they are taking advantage of opportunities to increase their human capital (work-training programs, GED classes, etc.). See http://www.americaworks.com/partners/how-we-work.

[10] See https://www.nycourts.gov/apps/chrs/ .

public records relating to open/pending and convictions in criminal cases originating from courts of all 62 counties.

Therefore, this paper's authors obtained criminal histories for felony and misdemeanor cases that occurred in New York for the RCT's 259 participants; theanalysis uses 219 participants with successful links between the data sources. Although criminal history records are available from all counties, initial data collection started at different points in time (from 1978 to 1993).

V. Data Description and Recidivism Results

1. Data Description

Data provided on the ex-offenders derive from two main sources. The primary source, which identifies ex-offenders in the experiment, is the baseline survey given to them at the initial intake interview, as well as information on whether individuals were assigned to the enhanced program (treatment group) or the standard program (control group). While data collected by P/PV concerning the treatment and control groups are complete, the baseline survey data were often incomplete, with many missing observations on specific questions. The primary data were then matched to public records on arrests and convictions (primarily from New York State) to form a criminal history of each participant. That history starts prior to the experiment and ends in July 2012.

As noted, 259 ex-offenders were enlisted for the study, including five "hardship case assignments." They joined the study on a rolling basis from June 15, 2009, to December 17, 2010, with 130 in the treatment group and 129 in the control group.

From this initial group of 259 ex-offenders, we were able to obtain redacted arrest records for 226, using public records from OCA and national search records. Overall, 1,027 pages of arrest records were collected for the 226 individuals. Because arrest records for the remaining 33 ex-offenders could not be found, those individuals were excluded from the analysis below. Almost all the arrest records were obtained from New York State; the arrest histories for seven individuals, obtained from national search records, were sufficiently different that they, too, were excluded from the analysis.

Using these detailed arrest records, the 219 remaining participants in the study were organized into four categories based on criminal acts prior to enrollment in the America Works experiment. Categories, listed in order of severity, are: Violent Offenders; Property Offenders; Drug Offenders; and Other Minor Offenders. The last three categories comprise nonviolent offenders. When classifying study participants in these four groups, we assumed a hierarchical structure under which an individual was included in only one group. In other words, if the individual had been arrested for a violent crime and a property crime, he would be classified in the violent bin, not the property bin. Violent offenders were defined as those who had committed any violent crime, as defined by the FBI Uniform Crime Reports, prior to participating in the America Works experiment. Under that definition, violent crime includes murder, rape/sexual assault, assault, and robbery. Property offenders are those who committed crimes against another person's property (burglary, grand larceny, trespassing, etc.). Drug offenders had been incarcerated for selling or possessing controlled substances but not for any other major crime included in the violent group or property-crimes group. The remaining group of other minor offenders committed only petty crimes (petit larceny, traffic/motor vehicle violations, criminal contempt, harassment, and minor drug offenses, etc.).

2. Summary Statistics

Table 1 presents a breakdown of the sample used in the analysis below. Ultimately, 219 ex-offenders were included in the empirical work on recidivism. Of the 219, 110 (50.2 percent) were assigned to the treatment group and 109 (49.8 percent) to the control group. A simple test of whether this proportion is significantly different from the ideal 50 percent finds no such evidence.

This table presents sample sizes by criminal history type (i.e., violent histories versus nonviolent), broken down by treatment and control group. Randomization appears good across multiple measures. For example, of the total sample, 126 of the participants, or 57.5 percent, had violent crimes associated with their most recent arrest. These 126 violent offenders were split nearly equally, with 65 (51.6 percent) in the treatment group and 61 (48.4 percent) in the control group. A formal test of whether this proportion is significantly different from the overall proportion assigned accepts the null hypothesis that there is no statistically meaningful difference in assignment ratios. Likewise, similar tests for assignment within the nonviolent category and

subsets of the nonviolent offenders accept the null hypothesis that the assignment remained statistically indistinguishable from the ideal of 50 percent each in the treatment and control groups.

Table 2 presents information on the length of time that we were able to observe participants after they entered treatment programs. Earliest participants (enrolled in June 2009) were observed for 18 months more than latest participants (enrolled in December 2010). Criminal histories were obtained in early August 2012; in the analysis below, the cutoff for being observed is July 31, 2012.

Thus, we observe all 219 ex-offenders for at least 18 months after they entered the America Works program. Since the intensity—and perhaps the length of time—of the job-placement services varied by treatment and control group, we simply used the time of entry into the program as the start time of treatment. We were able to follow 188 ex-offenders for at least 24 months, including 98 for 30 months. Of the 110 ex-offenders in the treatment group, 92 were followed for at least 24 months, including 51 for 30 months. Of the control group's 109 members, 96 were tracked for at least 24 months, including 47 for 30 months. Again, we were unable to reject the null hypothesis that the subgroups by length of time were not randomly assigned. Table 2 also presents the observation window for ex-offenders, categorized by initial offense.

As mentioned, a baseline survey was administered to all participants, asking standard demographic and socioeconomic questions: age, race, ethnicity, gender, marital status, primary language, income, welfare benefits, citizenship, military service, schooling, work history, children, health status, and housing/transportation situation. The survey also asked more sensitive questions about criminal histories—arrests, convictions, types of convictions, training while incarcerated, and current legal status—and substance abuse and mental health.

Table 3 presents averages derived from the baseline survey. Descriptive statistics are provided for the 219 individuals included in the full analysis. As Table 3 reveals, there were many nonresponses, with one ex-offender refusing to answer any question. (Discussion below focuses only on those who responded to all questions.)

The average age of ex-offenders was 39, with little variation across treatment and control groups. Only 7.4 percent of participants were married: those in the control group were slightly more likely to be married (8.3 percent) than those in the treatment group (6.4 percent). Education

level was an important question that many refused to answer: only 129 (58 percent) of ex-offenders responded. Of those who did, over 72 percent reported having a high school or high school–equivalent degree, with the treatment group having a slightly higher rate, nearly 74 percent. (This suggests that non-responders were predominantly *not* high school or equivalent graduates—as such, the educational level of the entire sample was likely far lower than indicated here.) More than half of ex-offenders had children and more than 90 percent reported themselves as being in good, very good, or excellent health. More than 25 percent of respondents reported being homeless and fewer than 5 percent said that they owned an automobile. These last two factors alone likely significantly inhibited ex-offenders from obtaining employment.

Race was another factor for which the nonresponse rate was high, with only 136 ex-offenders answering. Of that group, the vast majority (over 73 percent) reported African-American/black and over 23 percent reported Hispanic. Nearly 73 percent of ex-offenders possessed some kind of vocational training, and more than 60 percent had participated in job-training programs (of which nearly 42 percent participated while in prison). Fully 62 percent of the ex-offenders participated in a prerelease program. Nearly 73 percent reported receiving drug or alcohol treatment. It is quite clear from these statistics that this is, overall, a group that would struggle to obtain work.

Table 4 presents details on the criminal charges associated with the sample. (Note that a single arrest will often involve multiple charges.) As explained earlier, criminal histories were obtained from public records in New York State and merged with data collected by P/PV. Since criminal histories were limited to arrests and charges in New York, they represent an understatement if arrests and charges occurred in other states or were associated with aliases not linked to the individual. Criminal charges included *all* charges discovered for the individual at the time of data collection (August 2012).

Column 1 presents descriptive statistics on the number of charges prior to entry into the program. Overall, the average individual had 26.9 prior charges, ranging from misdemeanors to felonies and violent crimes. The treatment group had an average of 22.0 prior offenses, while the control group had an average of 31.9. In testing the hypothesis that these samples were drawn from populations with the same overall averages, no evidence of a statistical difference was found. However, the control group's slightly higher count and much higher spread (not reported) suggest some differences between that group and the treatment group. Such differences are not

statistically significant; but they suggest that, in comparing outcomes, factoring in the pretreatment arrest record may be important.

Table 4's column 2 presents posttreatment charges filed. The typical ex-offender was charged with 4.4 posttreatment charges. However, more than half in the sample (57.5 percent) were never charged after entry into the program (column 3). Considering columns 2 and 3, only 39.1 percent of the treatment group was rearrested, with an average of 2.9 charges. Still, 45.9 percent of the control group were rearrested after entering treatment, with an average of 5.9 charges. While the overall arrest rate is not statistically different, the total number of posttreatment arrests *is* statistically lower for the treatment group, evidence that the treatment group has a lower posttreatment charge count than the control group.

Results in Table 4 are most interesting when broken down by type of pretreatment charges. Violent offenders in the treatment group have a re-arrest rate of 44.6 percent, while violent offenders in the control group have a re-arrest rate of 42.6 percent. These rates are not statistically different and are clearly not different in interpretation: overall, more than 40 percent of violent offenders in the sample are rearrested during the observation window, and treatment does not appear to have any significant impact on re-arrest.

Yet for nonviolent offenders, the difference is much larger: only 31.1 percent of nonviolent offenders in the treatment group were rearrested during the observation period, compared with 50 percent in the control group. The difference is economically important and statistically significant. Similarly, we see that the average nonviolent offender in the treatment group had 1.6 posttreatment charges, while nonviolent offenders in the control group had a 5.1 average. Here again, the difference is both statistically significant and economically important.

Put another way, these differences provide evidence that the enhanced services program is effective for nonviolent offenders. No such evidence exists for violent offenders, who, as a group, register no response to treatment.

Columns 4–8 of Table 4 examine differences in arrests at different times after treatment began. Focusing on column 6—which tracks the 18-month window, the longest period for which all subjects were observed—one observes the same pattern noted previously for overall arrests and charges. Only 27.3 percent of the treatment group was rearrested in this period, while 34.9 percent of the control group had been rearrested. Violent offenders displayed the opposite result: 35.4 percent of participants in the treatment group were arrested, compared with only 29.5

percent of their control group peers. The overall difference is statistically significant at the 10 percent level, while the difference for violent offenders is not statistically significant.[11] The difference for nonviolent offenders is nonetheless stark: only 15.6 percent of nonviolent offenders in the treatment group had been rearrested within 18 months of the start of treatment, compared with 41.7 percent in the control group.

In short, the overall pattern of arrests is consistent at any time window, but differences for those followed after 18 months become less statistically significant because of much smaller samples (see Table 2 for sample sizes over time).

3. Survival Analysis on Arrests

While Table 4 begins to provide a picture of recidivism among ex-offenders, the need to break posttreatment time into large bins for presentation in tables and the difficulty of controlling for individuals with shorter windows of observation (see Table 2) suggest that a clearer picture can be obtained by using survival analysis.

Survival analysis is a statistical estimation procedure that models the time until an event occurs. Its history is rooted in medical studies, often referring to actual survival after a medical procedure or diagnosis. This paper refers to "survival" as time after beginning the treatment program until the individual is arrested on a new charge (we also examine convictions). Although many statistical approaches can be used, the Kaplan-Meier estimation procedure is often preferred when researchers are interested in comparing two or more well-defined groups. This paper seeks to compare the treatment and control group and to compare treatment and control for two different criminal histories: violent and nonviolent.

Figure 1 presents the estimated Kaplan-Meier survival function for all participants in this study. The horizontal axis measures the number of months since ex-offenders entered the treatment program; the vertical axis measures the proportion of ex-offenders who had not been arrested at that point in time. At time zero (entry into the program), 100 percent of the ex-offenders had not been rearrested. At approximately six months, the graph crosses the 90 percent line. Given the July 2012 data cutoff, tracking beyond 36 months is not possible. This paper observes that at 36 months, about 40 percent of ex-offenders had not been rearrested.

[11] Statistical significance is the probability that an effect is not likely due to chance alone. Statistical significance at the 10 percent level means that there is a 10 percent probability that the results are due to chance.

Figure 2 presents the estimated survival function for the treatment and control groups. The treatment group is slightly higher than the control group, suggesting slower recidivism. Initially, the two groups differ little, but as the months proceed, the slower recidivism of the treatment group seems to dominate. By 30 months, 57 percent of the treatment group still had not been rearrested, compared with only 50 percent of the control group. (That spread, however, does not amount to a statistical difference.)

A very different picture emerges in Figure 3, which provides survival analysis for violent offenders. Although the difference is also not statistically significant, the treatment group has higher rates of re-arrest than the control group. While at the end, near 36 months, the two graphs come together, there are periods just prior to two years where violent offenders in the treatment group appear to hit a period of high recidivism.

Figure 4 demonstrates that the overall conclusion is determined by nonviolent offenders: the treatment group clearly has significantly lower arrest rates than the control group. Indeed, at 30 months, approximately 50 percent of the control group has been rearrested while nearly 70 percent of the treatment group remains arrest-free. The difference between these two survivor functions is highly significant statistically and supports the basic findings from the descriptive statistics in Table 4.

Overall, the survival analysis provides a clear picture: enhanced employment services are effective for nonviolent offenders but do not have an impact on violent offenders.

4. Regression Analysis on Arrests

Table 5 contains initial regression results, which present formal tests for the simple differences described in previous figures. The dependent variable indicates whether the ex-offender was rearrested after entering treatment (similar to Table 4's third column). As previously noted, the nonviolent sample shows modest statistical differences but large economic ones between treatment and control groups. In general, the coefficient on receiving enhanced job services (the treatment group, "TREATMENT" in tables) is negative, meaning that enhanced services reduce arrests. Results are strongest for nonviolent offenders and the subset of nonviolent offenders with prior arrests for drugs or other minor offenses (excluding those with property crimes).

Table 6 presents initial regression results corresponding to Table 4's column 2—total arrests after entering treatment group. Again, coefficients on treatment are all negative, indicating that those in the treatment group have slightly lower counts of arrests, posttreatment. Results are statistically significant for nonviolent offenders.

As noted earlier, observable differences exist between control and treatment groups—most notably, the number of charges prior to entry into the America Works program. In Table 7A, we modify the empirical specification used in Table 6. In particular, we allow criminal history prior to entering America Works (the "Total Pretreat Charges" variable) to influence arrests after the program is completed. We also allow participation in the enhanced America Works program ("Treat*Total Pre-Charges") to influence subsequent arrests differently, depending on the individuals' histories. Finally, we include the main effect of the enhanced program ("Treatment"). The dependent variable indicates whether the ex-offender was rearrested (comparable with Table 4's column 3 and Table 5's regressions).

The results that emerge are extremely useful. Consider the first column, which uses the full sample. The coefficient on treatment is negative, indicating that, on average, holding constant pretreatment charges at a level of zero, members receiving enhanced services (treatment group) were 24.8 percentage points *less* likely to be rearrested after treatment than those in the control group. Overall, this means that the treatment group experienced lower recidivism. The second coefficient in this column, for total pretreatment charges, is small and statistically insignificant (this coefficient will be discussed in detail for later columns).

The third coefficient is the interaction between participating in enhanced services and criminal history prior to America Works. At the 99 percent level, this coefficient is positive and statistically significant: it implies that for every additional pretreatment charge, the difference between treatment and control group falls by 0.78 percentage points. Meanwhile, the average ex-offender had 26.9 pretreatment charges, implying that for the average ex-offender, treatment reduced the probability of posttreatment arrest by only 3.9 percentage points. We arrive at this number by multiplying the coefficient on the interaction term (0.00775) by the average number of pretreatment charges (26.9), and then subtracting the increase in arrests (20.9 percentage points) from the reduction from the main effect of treatment (24.8 percentage points).

Nevertheless, the distribution of pretreatment charges is highly skewed. One individual had more than 900 pretreatment charges, driving the average quite high. The median ex-offender

had only 15 pretreatment charges; 25 percent experienced eight or fewer. For the median ex-offender, treatment reduces the probability of re-arrest by 13.2 percentage points. For the 25 percent of ex-offenders with eight or fewer pretreatment charges, treatment makes it 18 percentage points less likely that they would be rearrested. The main implication: enhanced services were most effective at reducing arrests for ex-offenders with fewer charges prior to entering the program.

The second column, examining violent offenders, displays a nearly opposite story—albeit one statistically insignificant. The coefficient on enhanced job training is actually positive, while the coefficient on the interaction term is negative. This indicates that treatment is generally associated with higher rates of re-arrest (for violent offenders with the most prior charges, such effect is admittedly smallest). Yet the lack of statistical significance indicates that this result is weak and essentially should not be relied upon. However, total pretreatment charges (second row) are positive and statistically significant: violent offenders with more pretreatment charges are more likely to be rearrested. This result (combined with others) suggests that the program is generally not effective for violent offenders.

The third column is similar to the first but with higher magnitudes for the two treatment coefficients. Enhanced job services, it reveals, largely reduces the probability of being rearrested. Notably, for nonviolent offenders, having more *pretreatment* arrests (second row) is associated with a slightly lower probability of *re-arrest*. As with the first column, more pretreatment charges dilute the effect of treatment. The average nonviolent ex-offender had 25.3 charges prior to enrollment in America Works. For this average ex-offender, treatment reduces the probability of re-arrest by 5.3 percent. Again, though, distribution is highly skewed, with half of ex-offenders experiencing ten or fewer pretreatment charges. For this median ex-offender, treatment reduces the probability of rearrest by an impressive 24.3 percent. For the 25 percent of nonviolent offenders with six or fewer prior charges, treatment reduces rearrest probabilities by 29.3 percent.

Indeed, Table 7A's third column highlights this paper's main finding: enhanced services are most effective on nonviolent offenders with fewer pretreatment charges. Focusing future efforts upon this group is thus the most cost-effective strategy. Enhanced job placement is far less effective for those with violent criminal histories and numerous prior charges.

Table 7A's four remaining columns offer a number of interesting patterns. While statistical significance is difficult to establish because of the small samples (resulting from dividing nonviolent offenders into three subsets), we find that ex-offenders guilty of property crimes and minor offenses mostly drive the positive results for nonviolent offenders. Drug offenders collectively appear to have no baseline impact, though the treatment is most effective for drug offenders with the most pretreatment charges. (Given the small samples, caution is advised when interpreting these results.)

Up to this point, we have excluded P/PV's baseline survey information from statistical analysis. The nonresponse rate, as discussed, was exceedingly high for the majority of questions, rendering much of the survey unusable. Another potential problem, as with all survey data, involves measurement error—specifically, the veracity of individual responses. Given the sensitivity of the survey's questions, this risk is particularly relevant.

To explore such issues, we make use of the availability of two data sources on criminal history. Participants were initially categorized as violent or nonviolent based on actual arrest histories obtained from administrative records. But the baseline survey from P/PV also asked participants to discuss past convictions. Together, the two data streams allow we to separate the sample into four groups: (1) violent offenders based on actual arrest records; (2) nonviolent offenders based on actual arrest records; (3) self-reported violent offenders; and (4) self-reported nonviolent offenders.

When comparing these four groups, it becomes clear that relying on self-reporting is problematic. Using arrest records, we classify 126 of 219 ex-offenders as violent; using P/PV's baseline, on the other hand, we classify 76 as violent (of 213 ex-offenders who responded).[12] This discrepancy suggests that many program participants were not forthcoming about their most egregious crimes. By relying on self-reported criminal histories, therefore, many participants with official records of violent offenses would (erroneously) be classified as nonviolent.

From previous estimates, one would expect the treatment effect on the self-reported nonviolent group to be less than the treatment effect on the true group of nonviolent inmates (based on administrative records). Table 7B confirms this intuition. For convenience, columns 1 and 2 reproduce Table 7A's previous findings, using administrative arrest records for violent and

[12] Question 26 of the baseline survey asks about convictions for different crime types, including violent crimes. A respondent is supposed to check "yes" or "no" for each of 15 crime types. See Appendix 3.

nonviolent ex-offenders, respectively. Columns 3 and 4 estimate the treatment effect using self-reported criminal histories: although one observes a continued significant, negative impact of treatment on the probability of being rearrested, the estimate is lower than before. Further, the results suggest that violent ex-offenders are less likely to be rearrested when using self-reported criminal histories. Both results, together, imply that many violent offenders self-report into the nonviolent group. One should, accordingly, be cautious of relying solely on self-reported survey data.

Table 8—which presents regression results for the dependent variable counting total post-program arrests—is comparable with column 2 of Table 4 and Table 6. Table 8, like Table 7, controls for pretreatment arrests. Results, likewise, are qualitatively similar to those in Table 7. Overall, the treatment program is effective, with effectiveness decreasing as pretreatment charges rise. For violent offenders, the program has no statistical effect. Nonviolent offenders, in other words, entirely drive the main patterns in the full sample column.

Using the three levels examined above, this paper finds that for the average ex-offender, treatment reduces the number of arrests by 3.1. For the median ex-offender with only ten prior arrests, treatment lowers arrests by 3.9. For the lowest quartile, with only six prior arrests, treatment cuts arrests by 4.1. (Much of this reduction, admittedly, comes from the lack of arrests previously documented.)

5. Criminal Convictions

This paper has thus far explored effects on arrests, an approach consistent with a number of studies discussed in the literature review (Section II), such as Visher et al.'s eight surveyed RCTs. One important motivation for using arrests is that criminal activity that leads to an arrest—even if insufficient to lead to a conviction due to, say, lack of evidence or a skilled legal team—may still create important societal costs. At the same time, many arrests may be baseless—in which case, criminal convictions might better measure societal costs.

Accordingly, we duly replicated their analysis on convictions, in particular, conducting regression analyses where the outcome of interest was convictions, not arrests. Formal results are presented in the appendix, along with tables analogous to those on arrests. Findings, it turns out, are remarkably similar to those on arrests: of 104 ex-offenders arrested after enrolling in the RCT, 82 were convicted, while 22 were observed as neither convicted nor acquitted. Stated

differently, no ex-offender who, in the period studied, was later arrested was afterward acquitted. The link between arrests and convictions is plainly very high; the results are largely the same.

VI. Costs and Benefits of Enhanced Job Placement

This paper's main findings are best captured in measures of recidivism. However, given that it costs approximately $5,000 to place someone in a job through an America Works program, it is important to obtain some estimate of the social benefit of the reduction in arrests.[13]

Establishing social costs of crime is extremely difficult. This project, moreover, is not designed to provide new estimates. Instead, we use existing economic literature and other basic information to assign a dollar value for each crime committed by an ex-offender, both pre- and posttreatment. Table 9 summarizes the estimated social costs for various crime categories from eight different studies. Unsurprisingly, social costs for violent crimes— especially murder—are extremely high, while many nonviolent crimes impose relatively modest social costs. In the following analysis, we rely on estimates in comprehensive studies by Cohen and Piquero (2009); and McCollister et al. (2010).

Table 10 presents average social costs of crimes. The average ex-offender has committed crimes imposing social costs of more than $1.3 million. As with total arrests, total social costs are highly skewed, with a few prisoners generating very high social costs. Median pretreatment social cost is $381,500. For violent offenders, costs are significantly higher, with average pretreatment costs exceeding $2.1 million. Again, the distribution is skewed, with a median of $834,500 (more than double the overall median). Nonviolent crimes impose markedly lower social costs: an average of $201,530 for pretreatment costs, with a median of $109,250.

Table 10's second column displays posttreatment costs. Since more than half of ex-offenders are not rearrested in the sample period, many of these ex-offenders display a social cost of $0. While the overall sample average is $103,040, the treatment group generated only $65,068 and the control group generated $141,360. For violent offenders, the posttreatment average of $104,573 was similar to the overall average. The treatment group generated $81,684; the control group, $128,963. Meanwhile, the overall average for nonviolent offenders was $100,962. Here, the difference between treatment and control was far higher. At $157,114, the

[13] See Peter Cove, "Let's Trade Prison Beds for Work," May 16, 2013, http://www.realclearpolicy.com/articles/2013/05/16/lets_trade_prison_beds_for_work_513.html.

control group displayed the highest cost of recidivism; the low cost for the treatment group was $41,066.

As with total arrests, total social costs posttreatment are dominated by the large proportion of individuals who do not commit crimes. Simple linear regression models (not discussed here) are typically statistically insignificant but do show a reduction in social costs similar to the pattern seen in Table 10. An alternative approach when data have a preponderance of zero values is to use censored regression. This approach models the zeros, estimating the intensive margin (i.e., the effect of treatment on those who do commit crimes posttreatment). This latter technique provides a clearer measure of the marginal effect of treatment.

Table 11 presents such estimates for this paper's three main models of interest: overall sample, violent crimes sample, and nonviolent crimes sample. We find patterns similar to those discussed previously. Overall, the treatment group displays lower post-program social costs of $289,993 in reduced criminal activity. Such savings shrink as the number of charges faced by the ex-offender prior to participation rises: each additional charge reduces savings by $5,565. For an average ex-offender with 26 pretreatment charges, a net savings of $145,303 is realized.

As before, social costs of violent offenders are statistically unrelated to treatment. Our best estimate is that treatment may raise social costs. However, for nonviolent offenders, the strong results demonstrate that the typical ex-offender would see a reduction of $231,661 in social costs after treatment. We arrive at this number in a similar fashion to the way they computed Table 7A's 3.9 percentage-point reduction in recidivism: multiply Table 11's interaction term ($14,226.41 for nonviolent offenders) by the average number of pretreatment charges, and then add that to the main effect (-$601,537.10).

These results help establish the cost-benefit analysis of the America Works program. Treatment cost for one ex-offender is approximately $5,000. Reducing recidivism yields expected average savings in social costs well in excess of this amount. While some caution should be taken in using these estimates, the overall result is striking: providing intensive job-training and job-search services to nonviolent offenders more than pays for itself by reducing the social costs of crime.

VII. Conclusion

This paper examines the impact of intensive job-readiness training and job-search assistance on criminal recidivism and labor-market outcomes among ex-offenders, using recently

gathered data from a randomized controlled trial conducted at the America Works job-placement agency. Overall, such training and assistance had no effect on recidivism. This result nonetheless masks substantial heterogeneity of outcomes.

For the roughly half of program participants with nonviolent arrest histories, intensive job-search assistance significantly decreased the likelihood of recidivism. Only 35.6 percent of nonviolent offenders receiving intensive job training were subsequently rearrested; among participants receiving standard training, on the other hand, 52.1 percent were subsequently rearrested. Such results suggest that enhanced job-search assistance is most effective for the easiest of the hard-to-serve population (i.e., those without histories of violence and few charges) and far less effective for clients with more difficult histories of arrests and charges.

Although these results on criminal recidivism are noteworthy, we were unable to convincingly answer a number of other important questions originally posed when P/PV set up the experiment, including: (1) Did participation in America Works enhanced program increase ex-offenders' likelihood of finding and maintaining employment over those who did not receive intensive services? (2) Did the enhanced program help ex-offenders find jobs of a higher quality than they would otherwise have found on their own? (3) Did participation in the program reduce reliance on cash assistance from the government? (4) Did participation increase formal participation in the child-support system?

Data constraints preclude us from answering these questions. To address them, we would require high-quality administrative data or the opportunity to re-interview ex-offenders many years after initial contact with America Works. Such approaches, while conceptually possible, are difficult, given budgetary and privacy constraints.

Nonetheless, this paper's findings on recidivism suggest that the obvious path to improvement in the lives of ex-offenders—as well as the welfare of society at large—runs through the labor market.

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Table 1. General Statistics on Inmates by Group and Treatment Status						
	Inmates Arrested Posttreatment	Total, Violent Offenders	Total, Nonviolent Offenders	Total, Property Offenders	Total, Drug Offenders	Total, Other Minor Offenders
Full Sample (N=219)	104	126	93	26	51	16
Treatment Group (N=110)	48	65	45	13	24	8
Control Group (N=109)	56	61	48	13	27	8

Table 2. Sample Sizes by Treatment and Initial Offense Status				
	Sample observed for at least 18 months (full sample)	Sample observed for full 24 months	Sample observed for full 30 months	Sample observed for 36 months or more
Full Sample	219	188	98	16
Violent offenders	126	107	58	9
Nonviolent offenders	93	81	40	7
Drug offenders	51	48	22	4
Other minor offenders	16	33	18	3
Treatment Group	110	92	51	11
Violent offenders	65	55	28	6
Nonviolent offenders	45	37	23	5
Drug offenders	24	22	11	3
Other minor offenders	8	15	12	2
Control Group	109	96	47	5
Violent offenders	61	52	30	3
Nonviolent offenders	48	44	17	2
Drug offenders	27	26	11	1
Other minor offenders	8	18	6	1

Table 3. Summary Statistics from Baseline Survey

	Full Sample		Treatment Group		Control Group	
Variable	N	Mean	N	Mean	N	Mean
Treatment status	219	0.502	110	1	109	0
Total days observed	219	879.15	110	882.96	109	875.29
Age	218	39.193	109	39.505	109	38.881
Currently married	217	0.074	109	0.064	108	0.083
High school diploma or GED	129	0.721	65	0.738	64	0.703
Have children	219	0.557	110	0.545	109	0.569
Are you in excellent, very good, or good health?	199	0.94	101	0.931	98	0.949
Covered by any health insurance	197	0.721	100	0.71	97	0.732
Any physical, mental, or emotional condition	198	0.091	101	0.089	97	0.093
Are you currently homeless?	212	0.278	109	0.248	103	0.311
Do you have a current driver's license?	217	0.281	109	0.284	108	0.278
Do you own or lease a vehicle?	209	0.048	103	0.049	106	0.047
White	136	0.022	69	0.029	67	0.015
African-American/Black	136	0.735	69	0.739	67	0.731
Hispanic	136	0.235	69	0.232	67	0.239
Asian	136	0.007	69	0	67	0.015
American Indian	136	0.022	69	0.029	67	0.015
Pacific Islander	136	0.007	69	0.014	67	0
Any vocational training	215	0.726	108	0.731	107	0.72
Educational programs	213	0.61	107	0.654	106	0.566
Job-training programs	216	0.611	110	0.664	106	0.557
Classes in life Skills	211	0.445	107	0.495	104	0.394
Prerelease program	216	0.62	110	0.682	106	0.557
Ever paid to work in prison	213	0.404	108	0.38	105	0.429
Ever received drug or alcohol treatment	125	0.728	65	0.738	60	0.717
Ever received job training	211	0.417	106	0.472	105	0.362
Gained employment within six months of last release	166	0.651	86	0.593	80	0.713

Table 4. Means, by Treatment Status and Criminal History								
	(1) Average Pretreatment Charges	(2) Average Posttreatment Charges	(3) Percent Rearrested, Ever	(4) Percent Rearrested, Within 6 Months	(5) Percent Rearrested, Within 12 Months	(6) Percent Rearrested, Within 18 Months	(7) Percent Rearrested, Within 24 Months	(8) Percent Rearrested, Within 30 Months
Full Sample (N=219)	26.9178	4.3927	0.4247	0.1553	0.2146	0.3105	0.3937	0.449
	(4.4133)	(0.7418)	(0.0335)	(0.0245)	(0.0278)	(0.0313)	(0.0357)	(0.0505)
Violent Offenders (N=126)	28.0952	5.119	0.4365	0.1825	0.2381	0.3254	0.4393	0.4828
	(2.0319)	(1.0472)	(0.0444)	(0.0346)	(0.0381)	(0.0419)	(0.0482)	(0.0662)
Nonviolent Offenders (N=93)	25.3226	3.4086	0.4086	0.1183	0.1828	0.2903	0.3333	0.4
	(10.0514)	(1.0166)	(0.0513)	(0.0337)	(0.0403)	(0.0473)	(0.0527)	(0.0784)
Drug Offenders (N=51)	12.902	1.9412	0.3922	0.098	0.1373	0.2549	0.2917	0.3636
	(1.5651)	(0.4502)	(0.069)	(0.0421)	(0.0487)	(0.0616)	(0.0663)	(0.105)
Other Minor Offenders (N=16)	7.125	5.6875	0.375	0.125	0.1875	0.25	0.3077	0.1429
	(1.375)	(4.0153)	(0.125)	(0.0854)	(0.1008)	(0.1118)	(0.1332)	(0.1429)
Treatment Group (N=110)	22	2.9364	0.3909	0.1182	0.1818	0.2727	0.3587	0.451
	(2.0176)	(0.5178)	(0.0467)	(0.0309)	(0.0369)	(0.0427)	(0.0503)	(0.0704)
Violent Offenders (N=65)	26.4154	3.8615	0.4462	0.1692	0.2308	0.3538	0.4727	0.5
	(2.7435)	(0.8017)	(0.0621)	(0.0469)	(0.0527)	(0.056)	(0.0679)	(0.0962)
Nonviolent Offenders (N=45)	15.6222	1.6	0.3111	0.0444	0.1111	0.1556	0.1892	0.3913
	(2.6965)	(0.4522)	(0.0698)	(0.0311)	(0.0474)	(0.0546)	(0.0653)	(0.1041)
Drug Offenders (N=24)	11.5417	1.333	0.2917	0	0	0.0833	0.1363	0.3636
	(2.5014)	(0.5473)	(0.0948)			(0.0576)	(0.0749)	(0.1521)
Other Minor Offenders (N=8)	5.875	1	0.125	0	0.125	0.125	0.1667	0.25
	(1.3016)	(1)	(0.125)		(0.125)	(0.125)	(0.1667)	(0.25)
Control Group (N=109)	31.8807	5.8624	0.4587	0.1927	0.2477	0.3486	0.4271	0.4468
	(8.6249)	(1.3852)	(0.0479)	(0.038)	(0.0415)	(0.0459)	(0.0508)	(0.0733)
Violent Offenders (N=61)	29.8853	6.459	0.4262	0.1967	0.2459	0.2951	0.4038	0.4667
	(3.018)	(1.9827)	(0.0638)	(0.0513)	(0.0556)	(0.0589)	(0.0687)	(0.0926)
Nonviolent Offenders (N=48)	34.4167	5.1042	0.5	0.1875	0.25	0.4167	0.4545	0.4118
	(19.317)	(1.901)	(0.0729)	(0.0569)	(0.0632)	(0.0719)	(0.0759)	(0.123)
Drug Offenders (N=27)	14.1111	2.4815	0.4815	0.1852	0.2593	0.4074	0.4231	0.3636
	(1.9643)	(0.6905)	(0.098)	(0.0762)	(0.0859)	(0.0964)	(0.0988)	(0.1521)
Other Minor Offenders (N=8)	8.375	10.375	0.625	0.25	0.25	0.375	0.4286	0
	(2.4417)	(7.8625)	(0.183)	(0.1637)	(0.1637)	(0.183)	(0.202)	
Note: Standard errors in parentheses. For last column, only participants observed for three full years included.								

Table 5. Arrested Posttreatment

	All offenders	Violent offenders	Nonviolent offenders	Property offenders	Drug offenders	Drug and other minor offenders
Treatment	-0.0774	-0.0159	-0.165†	-0.0000	-0.148	-0.233*
	(0.0676)	(0.0898)	(0.103)	(0.204)	(0.139)	(0.118)
Constant	0.514***	0.508***	0.521***	0.538***	0.481***	0.514***
	(0.0481)	(0.0645)	(0.0729)	(0.144)	(0.0981)	(0.0858)
Observations	219	126	93	26	51	67
R-squared	0.006	0.000	0.028	0.000	0.023	0.056

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The † symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

Table 6. Total Number of Arrests Posttreatment

	All offenders	Violent offenders	Nonviolent offenders	Property offenders	Drug offenders	Drug and other minor offenders	Drug and property offenders
Treatment	-2.9269**	-2.5974	-3.5042*	-4.8462	-1.1481	-3.0357	-2.3202
	(1.4788)	(2.1382)	(1.9546)	(5.0977)	(0.8812)	(1.9343)	(1.7600)
Constant	5.8624***	6.4590***	5.1042***	7.3077	2.4814***	4.2857**	4.0500**
	(1.3852)	(1.9821)	(1.9016)	(4.9902)	(0.6913)	(1.8757)	(1.6848)
Observations	219	126	93	26	51	67	77
R-squared	0.018	0.012	0.032	0.036	0.032	0.034	0.021

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The † symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

Table 7A. Arrested Posttreatment, Controlling for Number of Pretreatment Charges

	All offenders	Violent offenders	Nonviolent offenders	Property offenders	Drug offenders	Drug and other minor-offense offenders	Drug and property offenders
Treatment	-0.248***	0.160	-0.367***	-0.353	0.0631	-0.0950	-0.309**
	(0.0824)	(0.129)	(0.114)	(0.257)	(0.181)	(0.157)	(0.131)
Total Pretreat Charges	-0.0000472	0.0106***	-0.000470***	-0.000622***	0.0253***	0.0218***	-0.000451***
	(0.000510)	(0.00177)	(0.000144)	(0.000176)	(0.00527)	(0.00550)	(0.000147)
Treat*Total Pre-Charges	0.00775***	-0.00528	0.0124***	0.0107**	-0.0127*	-0.00789	0.0115***
	(0.00207)	(0.00322)	(0.00289)	(0.00391)	(0.00747)	(0.00719)	(0.00298)
Observations	219	126	93	26	51	67	77
R-squared	0.059	0.146	0.124	0.162	0.208	0.196	0.104

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The † symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

Table 7B. Arrested Posttreatment, Controlling for Pretreatment Number of Charges Comparison of Administrative Data and Baseline Survey Data, Violent vs. Nonviolent

	(1)	(2)	(3)	(4)
	Violent Offenders: Actual Arrest Records	Nonviolent Offenders: Actual Arrest Records	Violent Offenders: Self-Reported Status	Nonviolent Offenders: Self-Reported Status
VARIABLES	Arrested Posttreatment?	Arrested Posttreatment?	Arrested Posttreatment?	Arrested Posttreatment?
Treatment	0.160	-0.367***	0.0705	-0.286***
	(0.129)	(0.114)	(0.156)	(0.104)
Total Pretreat Charges	0.0106***	-0.000470***	0.0141***	-0.000267
	(0.00177)	(0.000144)	(0.00461)	(0.000372)
Treat*Total Pre-Charges	-0.00528	0.0124***	-0.00722	0.00815***
	(0.00322)	(0.00289)	(0.00572)	(0.00269)
Observations	126	93	76	137
R-squared	0.146	0.124	0.115	0.070

Note: Columns (1) and (2) are identical to specifications in Table 7A for columns (2) and (3). Self-reported status comes from P/PV's baseline interview.

Table 8. Total Arrests Posttreatment, Controlling for Pretreatment Charges

	All Offenders	Violent Offenders	Nonviolent Offenders	Property Offenders	Drug Offenders	Drug and Other Minor-Offense Offenders	Drug and Property Offenders
Treatment	-4.9343***	2.9664	-4.4824**	-6.8460	-0.04420	-4.2086	-3.3458*
	(1.4816)	(2.8354)	(2.0784)	(5.9345)	(1.1649)	(3.6709)	(1.8852)
Total Pretreat Charges	0.0045	0.2828**	-0.0063**	-0.0095	0.0947*	-0.0474	-0.0046*
	(0.0120)	(0.1417)	(0.0031)	(0.0074)	(0.0497)	(0.1407)	(0.0026)
Treat*Total Pre-Charges	0.0933***	-0.1735	0.0550**	0.0478*	-0.0401	0.1033	0.0521*
	(0.0363)	(0.1488)	(0.0262)	(0.0284)	(0.0697)	(0.1480)	(0.0264)
Observations	219	126	93	26	51	67	77
R-squared	0.036	0.188	0.039	0.056	0.099	0.038	0.031

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The † symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

Table 9. Summary of Unit Crime Cost Estimates Reported in Literature (2008 dollars)

Type of Crime	(1) Aos et al. (2001)	(2) Cohen (1988)	(3) Cohen et al. (2004)	(4) Cohen & Piquero (2009)	(5) Miller et al. (1993)	(6) Miller et al. (1996)	(7) Rajkumar & French (1997)	(8) McCollister et al. (2010)
Murder	4,423,614		11,350,687	4.6–5 million	4,144,677	4,380,559		8,982,907
Rape/Sexual assault	369,739	97,962	286,277	290,000	80,403	124,419		240,776
Aggravated assault	105,545	23,025	84,555	85,000	24,987.00	21,451	76,829	107,020
Armed robbery				280,000				
Robbery	219,286	24,168	280,237	39,000	33,036	18,591	33,143	42,310
Arson				115,000	41,900	53,629		21,103
Larceny/Theft		344		4,000		529	1,104	3,532
Motor vehicle theft		6,006		17,000		5,720	1,723	10,772
Household		2,575	30,197			2,145	1,974	6,462
Drunk-driving crash				60,000				
Burglary			25,000	35,000				
Embezzlement								5,480
Fraud				5,500				5,032
Stolen property	22,739						151	7,974
Forgery and counterfeiting							833	5,265
Vandalism				2,000				4,860
Prostitution, false statements, etc.				500				

Note: Unit cost values inflated using Bureau of Labor Statistics inflation calculator based on consumer price index (CPI). U.S. Department of Labor 2008; see http://www.bls.gov/data/inflation_calculator.htm.

(1) Estimates combine Washington State and local governmental operating costs paid by taxpayers (originally reported in 2000 dollars) and costs incurred by crime victims from Miller et al. 1996 (reported in 1995 dollars). Values reflect present value cost of each offense used to calculate the benefits of adult community-based substance-abuse treatment. Cost per assault is for aggravated assault. (2) Original estimates in 1985 dollars. Jury compensation approach to estimate monetary value for pain, suffering, and fear in personal injury cases. (3) Original crime cost estimates in 2000 dollars. Estimated using contingent valuation method (willingness to pay). (4) Additional estimates to (2) by including (3). (5) Original estimates in 1989 dollars. Victim costs of violent crime and resulting injuries. (6) Original estimates in 1993 dollars. Estimates reflect victim losses including medical and mental health–care spending, tangible losses, and reduced quality of life. Excludes adjudication and sanctioning. (7) Original crime cost estimates reported in 1992 dollars. Estimated using combination of cost of illness and jury compensation approaches. Cost of assault is for aggravated assault. (8) Unit cost estimates. Cost of assault is for aggravated assault.

Table 10. Average and Median Social Costs by Time Since Enrollment (2012 dollars)				
Group	Average Total Social Cost, Pretreatment	Average Total Social Cost, Posttreatment	Social Cost of Arrests Within 6 Months of Treatment	Social Cost of Arrests Within 12 Months of Treatment
All Inmates				
Full Sample (N=219)	1,337,170 (226,397.20)	103,040 (26,829.73)	28,966.89 (12,367.77)	37,734.02 (13,033.46)
Treatment Group (N=110)	1,450,064 (391,408.40)	65,068.18 (18,792.90)	24,209.09 (14,757.03)	35,159.09 (16,145.60)
Control Group (N=109)	1,223,241 (227,141.90)	141,360.10 (50,324.41)	33,768.35 (19,952.88)	40,332.57 (20,573.98)
Violent Offenders				
Full Sample (N=126)	2,174,381 (376,072.4)	104,573.40 (25,280.85)	48,978.17 (21,336.03)	61,835.32 (22,313.08)
Treatment Group (N=65)	2,327,138 (641,513.70)	81,684.62 (29,283.20)	39,292.31 (24,823.97)	53,346.15 (26,768.20)
Control Group (N=61)	2,013,672 (374,177.40)	128,963.10 (41,910)	59,299.18 (35,427.54)	70,881.15 (36,406.32)
Nonviolent Offenders				
Full Sample (N=93)	201,530.10 (35,941.51)	100,962.40 (53,292.13)	1,854.84 (1,282.42)	5,080.65 (3,351.79)
Treatment Group (N=45)	183,177.80 (36,531.55)	41,066.67 (17,744.66)	2,422.22 (2,399.60)	8,888.89 (6,824.27)
Control Group (N=48)	218,735.50 (60,949.89)	157,114.60 (101,764.7)	1,322.92 (1,082.22)	1,510.42 (1,083.35)
Drug Offenders				
Full Sample (N=51)	166,988.70 (24,949.55)	58,068.63 (31,791)	1,088.24 (1,018.91)	1225.49 (1,020.41)
Treatment Group (N=24)	146,630.20 (39,625.02)	28,937.50 (25,133.31)	0 (0)	0 (0)
Control Group (N=27)	185,085.10 (31,618.65)	83,962.96 (55,854.44)	2,055.56 (1,922.16)	2,314.82 (1,919.71)
Note: Standard errors in parentheses				

Table 11. Social Costs of Arrests Posttreatment, Tobit Model Estimation

	All Offenders	Violent Offenders	Nonviolent Offenders
Treatment	-289,993.4***	142,920.0	-601,537.1**
	(129032.8)	(147754.8)	(264138.7)
Total Pretreat Charges	-264.159	7438.44**	-1835.15
	(944.68)	(2715.48)	(3249.42)
Treat*Total Pre-Charges	5565.242*	-6098.05	14,226.41*
	(3294.48)	(3799.59)	(8303.02)
Observations	219	126	93
R-squared	0.002	0.005	0.005

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The † symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

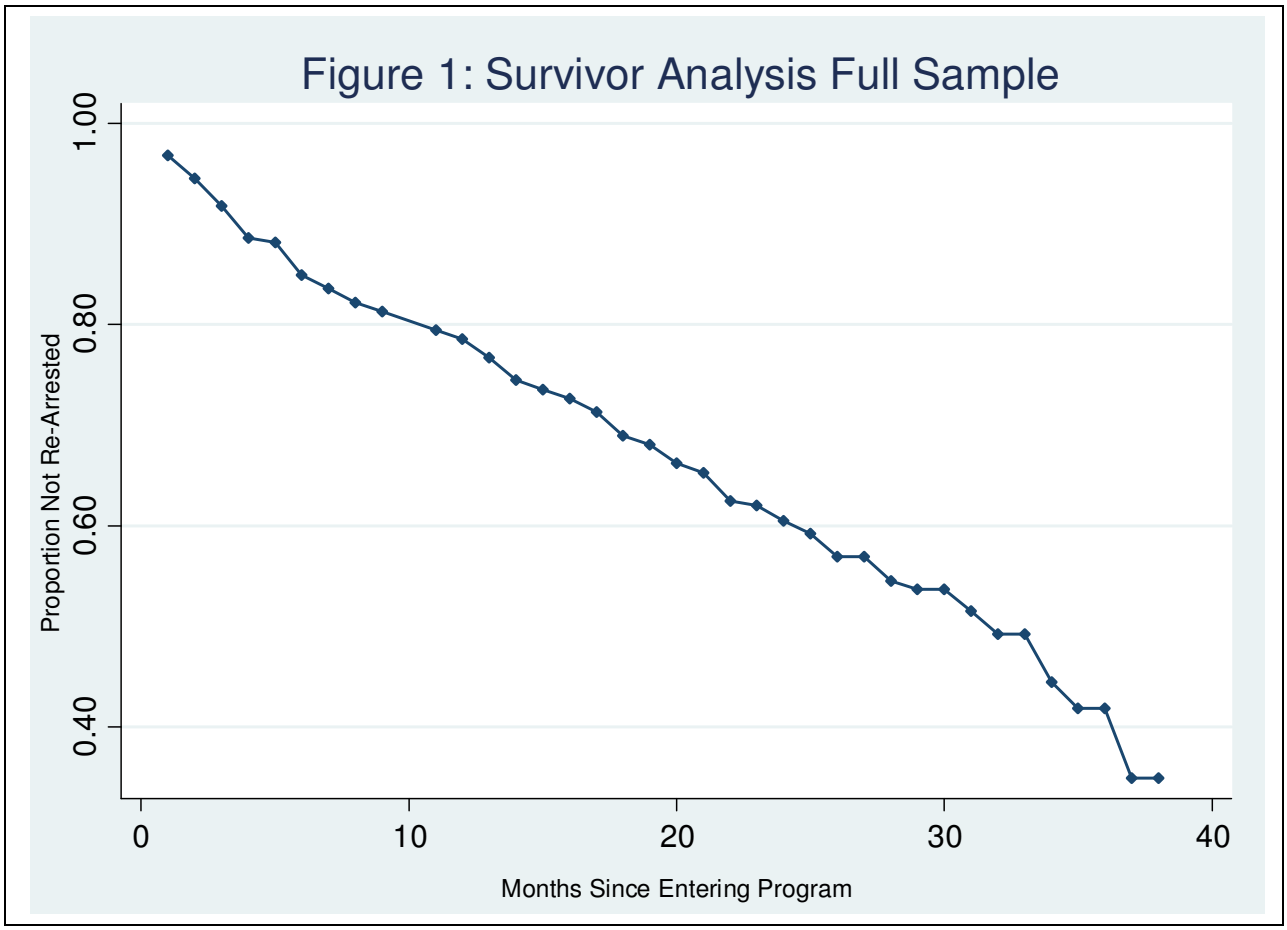
Figure 1: Survivor Analysis Full Sample
Proportion Not Re-Arrested
1.00
0.80
0.60
0.40
0
10
20
30
40
Months Since Entering Program

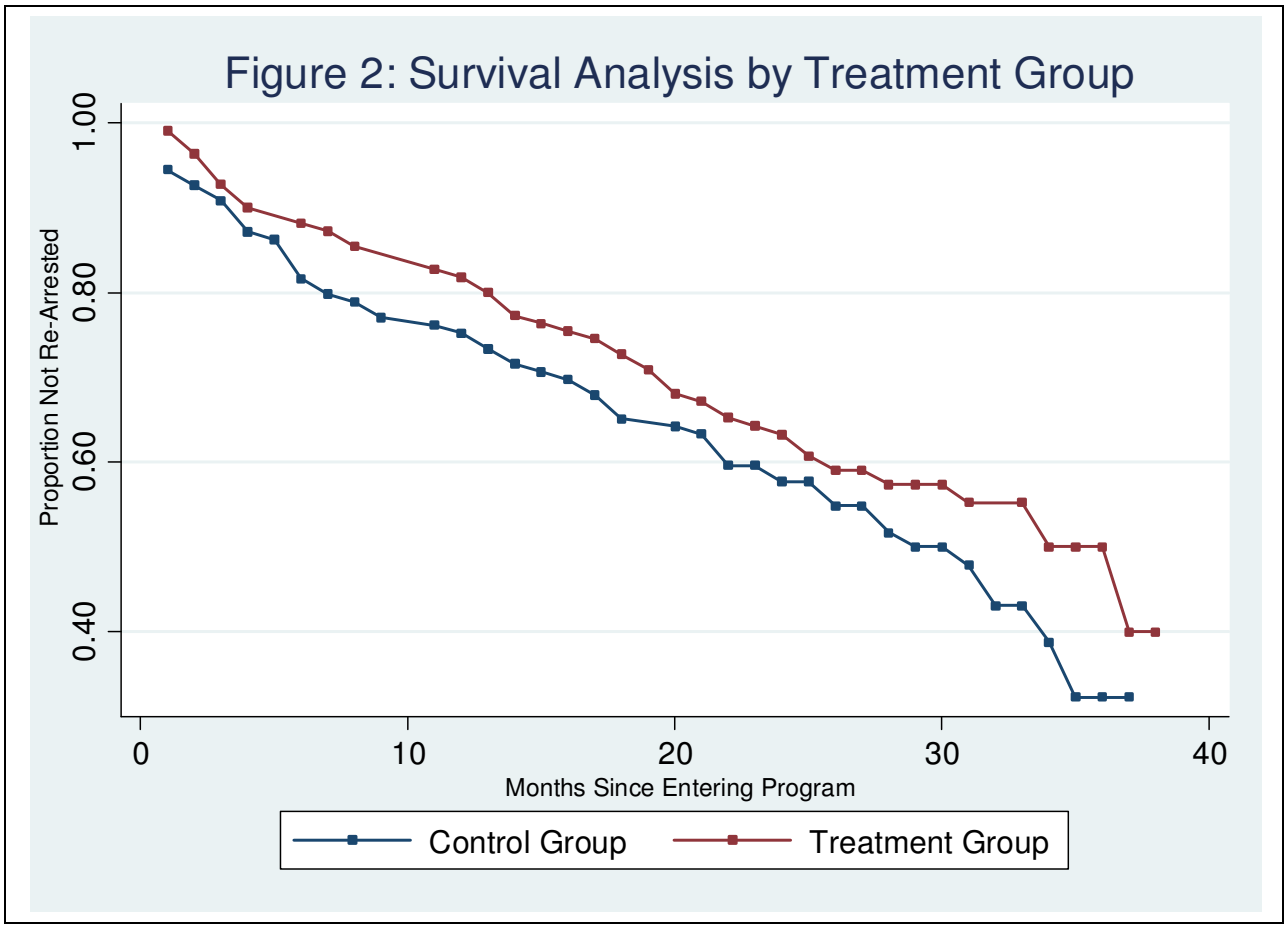
Figure 2: Survival Analysis by Treatment Group
Proportion Not Re-Arrested
0.40
0.60
0.80
1.00
0
10
20
30
40
Months Since Entering Program
Control Group
Treatment Group

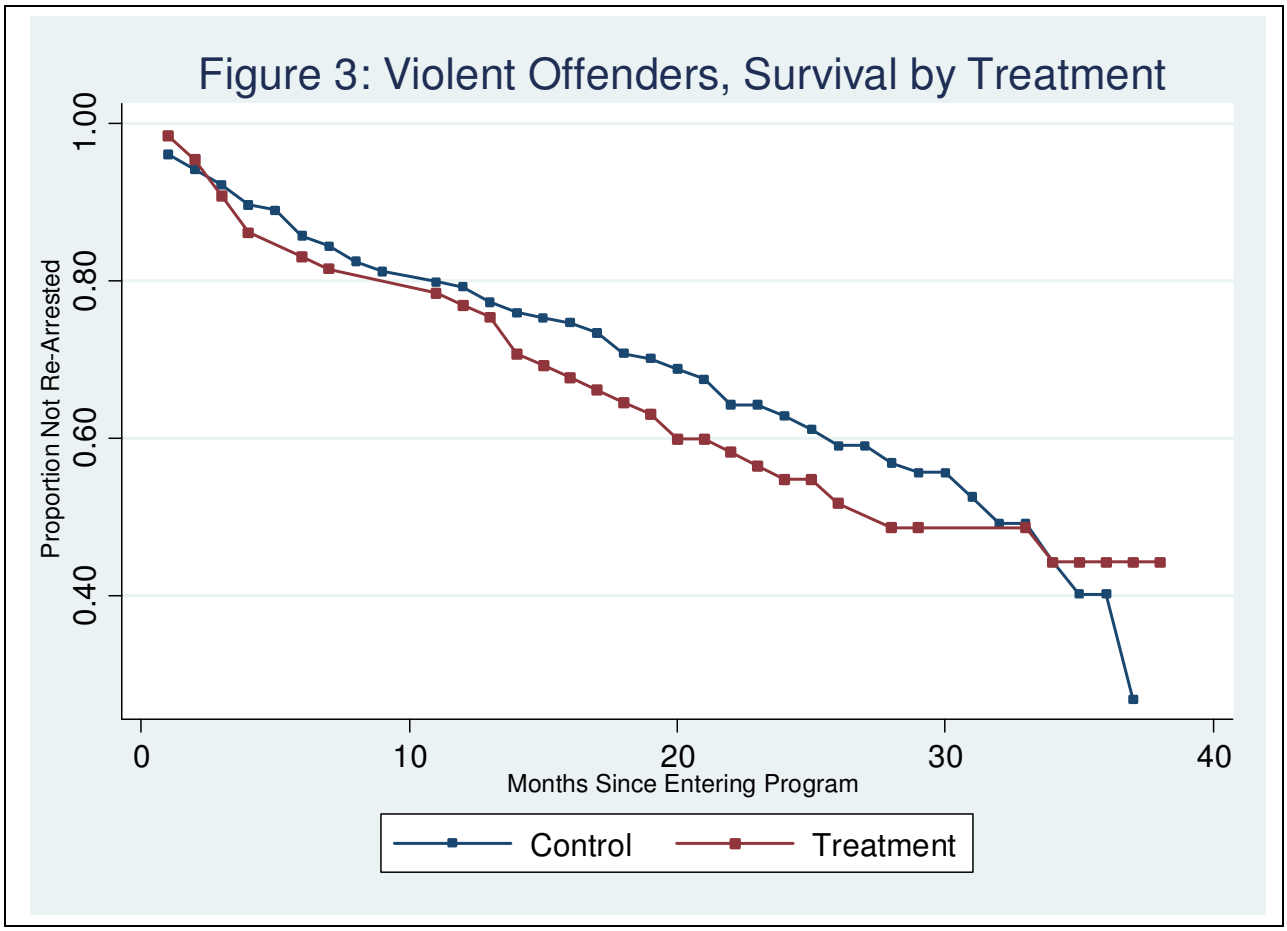
Figure 3: Violent Offenders, Survival by Treatment
Proportion Not Re-Arrested
0.40
0.60
0.80
1.00
0
10
20
30
40
Months Since Entering Program
Control
Treatment

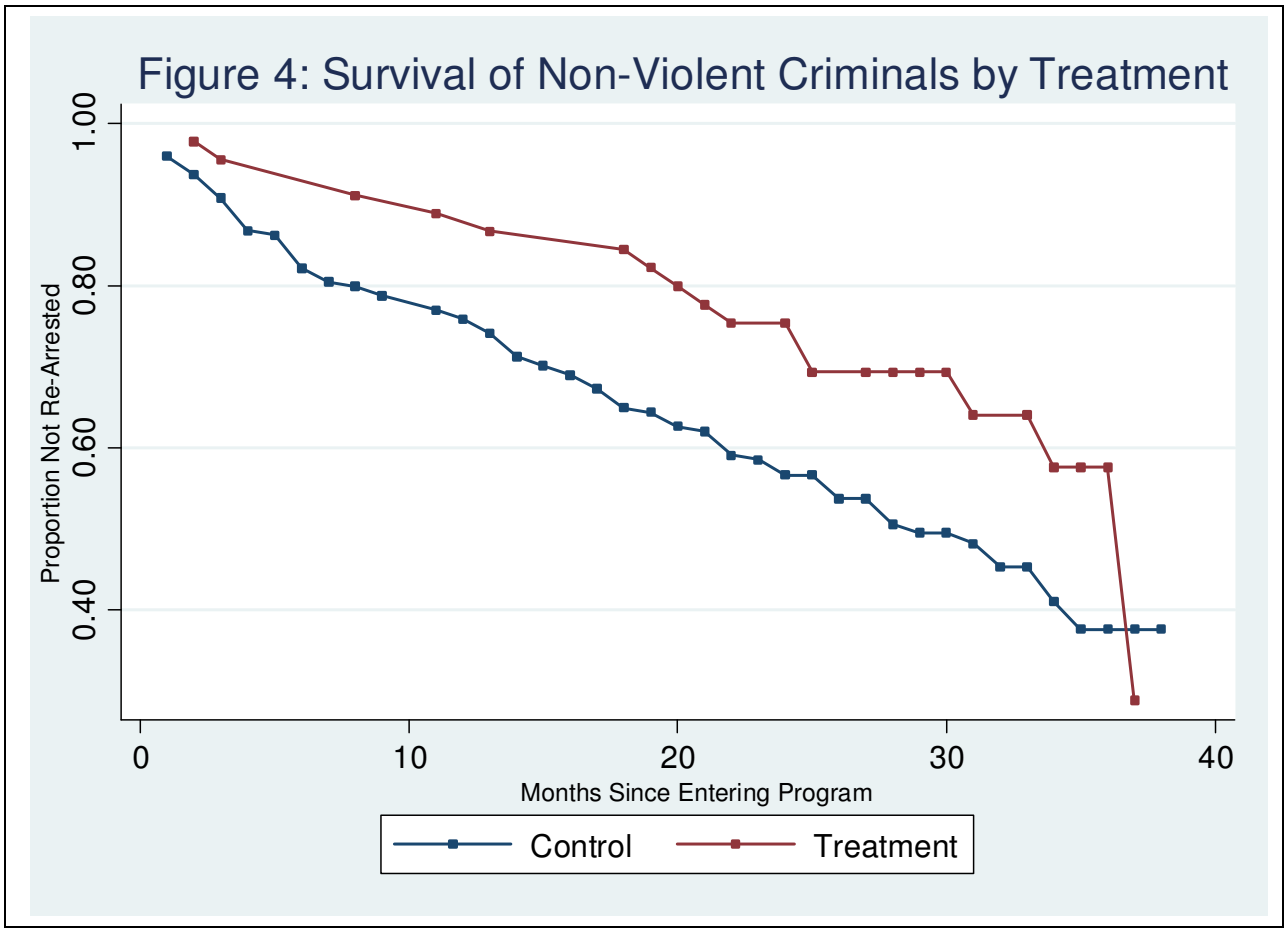
Figure 4: Survival of Non-Violent Criminals by Treatment
Proportion Not Re-Arrested
0.40
0.60
0.80
1.00
0
10
20
30
40
Months Since Entering Program
Control
Treatment

Appendix: Analysis Using Criminal Convictions

One concern that policymakers face is that arrests are, of course, different from convictions. Inmates, for instance, could be arrested on trivial charges (perhaps because of New York City's "stop and frisk" policy during this period), with such arrests not producing convictions. If one's primary concern is recidivism rates, it therefore makes sense to focus on convictions of ex-offenders enrolled in the America Works enhanced services program.

To do this, we use administrative data identifying results of all reported arrests resolved by July 2012. Accordingly, they observe three general results: found/pled guilty; dismissed/acquitted; and unknown. Of 104 ex-offenders arrested after enrolling in America Works, 82 were convicted at some point posttreatment, and the remaining 22 were neither convicted nor acquitted. Among the latter, 12 are in the control group, and ten are in the treatment group.

To replicate this paper's baseline analysis, we treat the conviction status of these 22 ex-offenders as unobserved. This modeling assumption only threatens the validity of the paper's results if the unresolved conviction data are correlated with treatment status. To test this, we use the sample of ex-offenders arrested post-enrollment and regress an indicator variable equal to one if the inmate is missing conviction data (zero otherwise) on treatment status (among other variables). We find no evidence (Appendix Table 1) that missing conviction data are correlated with treatment status.

Next, we proceed to regression results using convictions as the posttreatment outcome (Appendix Table 2). The dependent variable indicates whether the ex-offender was convicted after entering treatment (similar to Table 5, main text). As in Table 5, the nonviolent ex-offender sample shows modest statistical differences, but large economic ones, between treatment and control groups. In general, the coefficient on receiving enhanced job services is negative, meaning that enhanced services also reduce convictions.

Next, we modify the empirical specification in Appendix Table 3, allowing criminal arrest histories prior to entering America Works to influence convictions. Results are directly comparable with Table 7A (main text). The dependent variable indicates whether the ex-offender was convicted of a crime after entering America Works. Again, the coefficient on treatment is negative, suggesting that the treatment group had lower recidivism (in terms of convictions). Holding constant pretreatment charges at zero, ex-offenders receiving enhanced services (treatment group) were, on average, 25.6 percentage points less likely to be convicted after treatment than those in the control group.

Without considering past criminal history, this result suggests that, compared with arrests, enhanced services have a larger negative effect on convictions. However, once we account for past criminal charges, treatment from America Works has a slightly lower impact on the average ex-offender's probability of being convicted. Specifically, results suggest that enhanced treatment lowers the probability of being convicted by 1.4 percent for the average ex-offender and 12.1 percent for the median ex-offender. The key implication: treatment was most effective at reducing convictions for ex-offenders with fewer charges prior to entering the program.

The second column, examining violent offenders, reveals a nearly opposite story, though a statistically insignificant one. Again, the coefficient on enhanced job training is positive, while the coefficient on the interaction term is negative. This indicates that treatment is generally

associated with higher rates of posttreatment conviction (for violent offenders with the most prior charges, the effect is smallest). The lack of statistical significance nevertheless indicates that this result is weak and should not be relied upon. We do note, however, that total pretreatment charges (second row) are positive and statistically significant: violent ex-offenders with more pretreatment charges are more likely to be convicted posttreatment. This result further indicates that enhanced services are ineffective for violent ex-offenders.

The third column, examining nonviolent ex-offenders, presents larger magnitudes than those for the full sample. Enhanced services reduce the probability of posttreatment conviction. Interestingly, for nonviolent ex-offenders, more pretreatment arrests (second row) are associated with a slightly lower probability of rearrest. Prior to enrollment in America Works, the average nonviolent ex-offender had 25.3 charges. For the latter, treatment reduces the probability of posttreatment conviction by 3.3 percent; and for the median ex-offender, 23 percent. The third column confirms this paper's main finding, too: treatment is most effective for nonviolent offenders with fewer pretreatment charges. Focusing future efforts on this group is thus the most cost-effective strategy.

While many of Appendix Table 3's results closely track those in Table 7A, subtle differences emerge in the remaining columns. In Table 7A, ex-offenders classified as "property offenders" and "minor offenses" were important in driving the treatment effect. For convictions, however, minor offenders represent the lone group with no discernible baseline impact of enhanced services. For the three other groups, an economically significant baseline exists for impact of treatment on conviction rates. These results (which should be interpreted cautiously because of small sample sizes) seem intuitive: one might reasonably expect minor offenders, because of the less grave nature of their crimes, to form the group with fewer convictions.

In summary, results for criminal convictions mirror those for arrests. Enhanced services reduce both arrests and convictions—especially for nonviolent ex-offenders with few pretreatment charges.

Appendix Table 1. Test for Randomization of Unobserved Conviction Data

VARIABLES	(1) Arrest Outcome Missing?	(2) Arrest Outcome Missing?
Treatment status	0.0188	-0.0228
	(0.0816)	(0.0855)
Currently married		-0.123
		(0.157)
Currently married (missing)		-0.147
		(0.298)
Birth year		0.00881*
		(0.00478)
Birth year (missing)		17.24*
		(9.415)
Race, African-American		0.674
		(0.548)
Race, Hispanic		0.638
		(0.514)
Education, less than high school		-0.650
		(0.432)
Education, high school grad		-0.730
		(0.456)
Education, some college		-0.596
		(0.443)
Observations	104	104
R-squared	0.001	0.119

Appendix Table 2. Convicted Posttreatment

VARIABLES	(1) All Inmates Convicted Posttreatment?	(2) Violent Convicted Posttreatment?	(3) Nonviolent Convicted Posttreatment?	(4) Property Convicted Posttreatment?	(5) Drug Convicted Posttreatment?	(6) All Minor Convicted Posttreatment?	(7) Minor, Nondrug Convicted Posttreatment?
Treatment	-0.0582	0.00656	-0.150†	0.000	-0.194	-0.210*	-0.250
	(0.0656)	(0.0879)	(0.0981)	(0.199)	(0.133)	(0.113)	(0.222)
Constant	0.404***	0.393***	0.417***	0.385**	0.444***	0.429***	0.375*
	(0.0472)	(0.0631)	(0.0719)	(0.140)	(0.0976)	(0.0849)	(0.183)
Observations	219	126	93	26	51	67	16
R-squared	0.004	0.000	0.025	0.000	0.041	0.050	0.083

Note: Standard errors computed using robust standard-error formulas. Three stars indicate significance at 99% level. Two stars indicate significance at 95% level. One star indicates significance at 90% level. The "†" symbol indicates significance at 90% level for one-tailed test of sign. One-tailed test used because expected effect of enhanced treatment on recidivism is negative or zero, not positive. The null hypothesis holds that the intervention had no effect on recidivism; the alternative hypothesis holds that the intervention reduced recidivism.

Appendix Table 3. Convicted Posttreatment—Controlling for Pretreatment Charges

	(1) All Inmates	(2) Violent	(3) Nonviolent	(4) Property	(5) Drug	(6) All Minor	(7) Minor, Nondrug
VARIABLES	Convicted Posttreatment?	Convicted Posttreatment?	Convicted Posttreatment?	Convicted Posttreatment?	Convicted Posttreatment?	Convicted Posttreatment?	Convicted Posttreatment?
Treatment	-0.256***	0.134	-0.359***	-0.385*	-0.167	-0.142	0.0785
	(0.0767)	(0.118)	(0.102)	(0.218)	(0.184)	(0.148)	(0.202)
Total Pretreat Charges	3.91e-05	0.0105***	-0.000379***	-0.000447**	0.0161	0.0191**	0.0475**
	(0.000478)	(0.00214)	(0.000124)	(0.000174)	(0.00977)	(0.00904)	(0.0167)
Treat*Pre-Charges	0.00900***	-0.00343	0.0129***	0.0122***	0.00118	-0.00170	-0.0357
	(0.00201)	(0.00342)	(0.00273)	(0.00296)	(0.0104)	(0.00964)	(0.0265)
Observations	219	126	93	26	51	67	16
R-squared	0.081	0.175	0.139	0.191	0.190	0.207	0.339

All standard errors are computed using robust standard-error formulas. Three stars indicate significance at the 99% level, two stars indicate significance at the 95% level, one star indicates significance at the 90% level, the † symbol indicates significance at the 90% level for a one-tailed test of sign. A one-tailed test is used because the expected effect of enhanced treatment on recidivism is negative or zero, but not positive. The null hypothesis is that the intervention had no effect on recidivism, while the alternative hypothesis is that the intervention reduced recidivism.