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Ex-offender employment programs and recidivism: A meta-analysis

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Abstract. One consequence of the tremendous growth in the number of persons under supervision of the criminal justice system, whether incarcerated, on parole, or on probation, is the effect of this criminal history on finding and keeping a job. Ex-offenders, especially those recently released from prison, face substantial barriers to many types of legal employment; nonetheless, stable employment is one of the best predictors of post-release success. Thus, policy-makers concerned about high recidivism rates face an obvious need to improve the employment prospects of ex-offenders. Over the last 25 years, many programs that were designed to increase employment (and, by so doing, reduce recidivism) among ex-offenders have been implemented and evaluated. [Wilson, D. B., Gallagher, C. A., Coggeshall, M. B. & MacKenzie, D. L. (1999). *Corrections Management Quarterly* 3(4), 8–18; Wilson, D. B., Gallagher, C. A. & MacKenzie, D. L. (2000). *Journal of Research in Crime and Delinquency* 37(4), 347–368] conducted a quantitative synthesis and meta-analysis of 33 evaluations of educational, vocational, and work programs for persons in correctional facilities. To date, however, the evaluation literature on employment programs for those with a criminal record who are not in custody has not been systematically reviewed. This paper presents the results of a quantitative meta-analysis of eight random assignment studies of such programs, using the Campbell Collaboration methodology. The results indicate that this group of community employment programs for ex-offenders did not reduce recidivism; however, the experimental design research on this question is small and does not include some of the promising community employment programs that have emerged in the last decade.

Key words: employment programs for offenders, experimental studies, meta-analysis, offenders, prisoners, randomized controlled trials, recidivism, systematic review

Introduction

As is well known, the rapid growth of prison populations that occurred in the late 1980s and 1990s has translated into a large flow of men and women being released from prison. A key policy concern that has emerged is identifying strategies that would help former prisoners successfully reintegrate into their communities and reduce the likelihood that they would commit new crimes.

Research has indicated that having a legitimate job lessens the chances of re-offending following release from prison and that recidivism is less likely among those with higher wages and higher quality jobs (Sampson and Laub 1997; Harer 1994). A good job not only provides the means for basic survival, but also is a key element in rebuilding self-esteem, attachment to a conventional lifestyle, and a sense of belonging in the community. Work organizes daily behavior and patterns of interaction, and becomes an important source of informal social control for ex-offenders (Sampson and Laub 1977; Uggen 1999; Uggen and Staff 2001; Wilson 1997).

According to data from a national study, three-quarters of state inmates reported that they held a job just prior to their incarceration (Lynch and Sabol 2001). Nonetheless, having a criminal record represents a substantial barrier to many types of legal employment, and these barriers are compounded after a term of prolonged incarceration. Long periods of incarceration may weaken social contacts that lead to legal employment opportunities upon release (Western et al. 2001; Hagan and Dinovitzer 1999). Research also suggests that having a criminal record, whether an arrest, conviction, or prison term, adversely affects subsequent employment wages and job stability, even after controlling for duration or severity of prior criminal involvement (Bushway 1998; Western et al. 2001; Sampson and Laub 1997). Other barriers that ex-offenders face in finding and keeping a job include the lack of recent job experiences, a lessening of job-related skills, and transportation difficulties.

These barriers to gainful employment coupled with the likely public safety consequences of high levels of unemployment among ex-offenders create an immediate need to identify effective interventions that might increase employment for this population. Employment interventions can be either in-prison programs or post-release employment services, or rarely, both. Although the period of incarceration could be viewed as an opportunity to build skills and prepare for placement at a future job, the evaluation literature has provided mixed to negative support for the effectiveness of in-prison job training programs, including a meta-analysis of 33 programs (Bushway and Reuter 1997; Gaes et al. 1999; Wilson et al. 1999, 2000).

A flurry of community-based employment interventions, generally involving some combination of job readiness, job training, and job placement services, were implemented in the 1970s and 1980s, mostly with government support. There is a long history in the United States of federal funding of community employment programs for former prisoners, and for disadvantaged youth and adults who may have arrest or conviction records. The U.S. Department of Labor funded programs targeted to former prisoners under the authority of the Manpower Development and Training Act of 1962. The result of that early effort was the well-known studies of Living Insurance for Ex-Prisoners (LIFE) and the Transitional Aid Research Project (TARP), which were the first major experimental evaluations of community employment programs for ex-prisoners. However, the results did not clearly support the value of such programs in reducing recidivism (Mallar and Thornton 1978; Rossi et al. 1980). A series of federal job training programs followed, including the 1973 Comprehensive Employment and Training Act (CETA), the 1983 Job Training Partnership Act (JTPA), and the 1998 Workforce Investment Act (WIN). After the end of CETA in 1982, government funding of employment programs for adult ex-prisoners largely disappeared, although JTPA and WIN continued to target disadvantaged older youth (including those with arrest records). In 2002, with the recent resurgence of attention to prisoner reentry and the obstacles former prisoners face in finding and keeping jobs, the Department of Labor developed Ready4Work, a business, faith and community pilot initiative for increasing employment of ex-offenders in 18 cities.

Several large-scale, experimental evaluations of the older DOL programs have been conducted, but results for ex-offender subgroups have been reported only rarely. Unfortunately, the conclusions from these evaluations have generally been disappointing (Bushway and Reuter 2002; Uggen et al. 2002). In addition, a small group of other evaluations of community employment programs for ex-offenders exists that has not been integrated with the older studies. Thus, a systematic review of community employment programs for ex-offenders and their effects on recidivism will provide new information on the effectiveness of these interventions.

The primary research question for this meta-analysis is: *What is the effect of non-custodial employment services interventions on the subsequent criminal behavior of ex-offenders?* This review will survey the existing empirical evidence that examines the effectiveness of community employment programs on recidivism among persons who have been previously arrested, convicted, or incarcerated. It is limited to those studies using random assignment because of the specific interest in isolating effects from experimental designs. The remainder of this paper presents the studies reviewed and the findings from our meta-analysis; implications for policy and practice are then discussed.

Subjects and methods

Our review has focused on studies using random assignment experimental designs. Eligible studies had to have included one or more treatment groups and one or more comparison groups. Some measure of criminal behavior subsequent to the beginning of the intervention must have been reported for the ex-offenders in both the treatment and comparison groups. The outcome measure of criminal behavior may have been either official (i.e., arrest, conviction, technical violation) or self-reported and may have been reported either dichotomously or on a continuous scale.

Both the treatment and comparison groups must have been composed, at least in part, of ex-offenders: persons who had been arrested, convicted, or incarcerated in connection with a criminal charge before becoming a study subject. If either the treatment or comparison group included subjects who were not ex-offenders, the results must have been reported so that effect sizes could be coded for the ex-offenders alone. Only studies of adults (as defined by the jurisdiction within which a given study is conducted) or studies that combine older youth (age 16–17) and adults are eligible for this review.[1] Studies were excluded if the comparison group included persons who did not meet the eligibility criteria for the treatment. The comparison group could have received either 'treatment as usual' or no treatment. Comparison subjects may have been drawn from waiting lists or 'treatment as usual' pools; if the treatment group was drawn from subjects who volunteered to receive the intervention, the comparison group also had to be composed of volunteers.

In order to be included, some of the treatment must have been delivered in a non-custodial setting (i.e., not in a prison or jail); studies of treatment delivered in a halfway house, group home, or similar facility were eligible. The program may have been either residential or non-residential so long as equivalent residential and custodial requirements were placed on both the treatment and comparison subjects. The treatment program must have included a job-placement component or a job-training component, although other components, such as life-skills training, remedial education, or social-service assistance, may have been included. In the case of multiple-service delivery, all components (i.e., employment and non-employment components) were coded.

This review only includes study reports written in English. It is not known how many studies in other languages may be eligible. Further, we limited our search to those studies where at least some of the study subjects received treatment after 1964, and the study was completed during or since 1970 in order to ameliorate the potential effects that changes in the economic environment might have on programmatic effectiveness. This was necessary because most of the programs available for analysis occurred during the 1970s and 1980s.

Study identification and selection

So that we did not rely on only published studies in highly visible academic journals, where the tendency is to report on studies that demonstrate effectiveness, we used the following search modes:

- Contacts with leading researchers;
- Searches of the bibliographies of published reviews of related literature in the U.S. and Western Europe (Uggen et al. 2002; Bushway and Reuter 2002; Buck 2000; McGuire 1995; Webster et al. 2001);
- Scrutiny of annotated bibliographies of related literature (e.g., Clem 1999); and
- Searches of computerized databases.

The specific databases that were searched were:

- Catalog of U.S. Government Publications (CGP), U.S. Government Printing Office;
- Criminal Justice Abstracts;
- Digital Dissertations;
- Economic Literature Index;
- National Criminal Justice Reference Service (NCJRS) Abstracts;
- ProQuest Social Sciences Index;
- Sociological Abstracts;
- Social Science Citations Index;
- Wilson Humanities Index; and
- The Campbell Collaboration Social, Psychological, Educational and Criminological Trials Register.

The specific search terms that we used were Boolean combinations of: (1) employment, job train, job counsel, job placement, job-seekers allowance, jobless benefit, employable, after-care, case manage, job service; and (2) offender, ex-offender, criminal, arrest, convict, incarcerat, parole, probation, diversion, inmate. Each word was following by a question mark to denote any number of unspecified characters (e.g., incarcerat? could be incarcerate or incarceration).

The literature on employment and crime, broadly defined, is voluminous and our search methods generated hundreds of titles, most of which had abstracts. If the abstract did not mention an evaluation report, no further review was initiated. For the 30–35 reports thought to be evaluations using random assignment designs, full studies were requested and reviewed by one of the senior authors (Visher or Winterfield). Studies were divided into four categories: experimental studies with random assignment, quasi-experimental studies, non-experimental studies, and other (process evaluations, review articles, etc.).

The total number of independent studies using random assignment designs that satisfied our eligibility criteria was eight, including two studies in which two separate samples were coded. Upon review, two studies thought to be eligible ended up being excluded. First, a British evaluation of a program that provided employment assistance to ex-offenders was excluded because the amount of assistance provided was not standardized for the treatment group (Soothill 1999). Second, an evaluation of a work release program was excluded because the requirement that both treatment and control groups be in a similar residential status was not met and the experimental design was compromised by the addition of a matched comparison group to increase the sample size (Turner and Petersilia 1996).

Data management

A Microsoft Access database was constructed for the meta-analysis, and information from the eligible studies was entered into the database. The database included details on study eligibility, program description, sample description, treatment-group circumstances, methodological rigor, outcome information, and effect-size information.

When an eligible study report did not provide the necessary information to calculate effect size (for example, outcomes could have been reported for subgroups of treatment and control groups, differentiated by age, but the subgroup Ns may not have been available), we contacted the original authors by email; there were two instances for which this was necessary. Of these, one author could not retrieve the necessary information, and one author was able to do so. The first study was, however, included in this review after we learned from one of the authors that a reasonable approximation of the size of the treatment and comparison groups could be estimated based on the sampling criteria (ratio of treatment to comparison sample was 2 : 1).

Description of studies

The eight studies identified for this review were conducted over more than 20 years, with the first study being implemented in 1971 (Mallar and Thornton 1978) and the most recent study being implemented in 1994 (Rossman et al. 1999). (Publication dates are not a good indicator of when the study was conducted because several studies we include are based on reanalyses of previous studies.) Four studies were published in academic journal or book publications. Three studies were nonpublished reports to government agencies, including one that has not been widely cited in the recent literature on employment and recidivism (Rossman et al. 1999). Four studies included women (Rossman et al. 1999; Rossi et al. 1980; Cave et al. 1993; Schochet et al. 2001). Combining across studies, more than 6,000 older youth (aged 16–17) and adults with prior contact with the criminal justice system participated in the eight studies in this review.[2]

Six of the experiments were simple two-group designs (the exceptions being Rossi et al. 1980 and Mallar and Thornton 1978), and all reports explicitly stated that study participants were randomly assigned to either the treatment or control group. However, the specific procedures for conducting random assignment were either only vaguely described or not described at all. Recidivism measures primarily included arrests, based on either official record sources or self-reported information. The follow-up periods ranged from 6 to 36 months. Taken together, these eight experimental studies with random assignment designs that examined the impact of job training and employment programs, albeit broadly defined, among ex-offenders report modest or no effects of such services on criminal activity. We summarize each study below in chronological order by date of program initiation.

The Baltimore Living Insurance for Ex-prisoners (LIFE)

The Baltimore Living Insurance for Ex-Prisoners (LIFE) experiment was the initiation of several studies sponsored by the U.S. Department of Labor in the 1970s (Mallar and Thornton 1978; see also Rossi et al. 1980: Ch. 2). The Department of Labor was acting on a mandate from the Manpower Development and Training Act of 1962, which provided for programs that would aid released prisoners in obtaining employment. A series of demonstration projects tested the hypothesis that income support to released prisoners would facilitate post-release adjustment and reduce the likelihood of property crimes. Beginning in 1971, 432 prisoners released from Maryland state prisons and returning to Baltimore were randomly assigned to one of four groups: those who received 13 weeks of payments of $60 per week and intensive job counseling and placement services, those who received payments only, those who received counseling and placement only, and a control group who received neither payments nor counseling. However, eligibility for the program (before random assignment) was limited to prisoners who were considered at high risk for returning to prison because of their previous criminal history (see Mallar and Thornton 1978: 210–211). Recidivism was measured as any new arrest at 1 year.

The LIFE experiment found that those receiving weekly cash payments of $60 (about $225/week in 2002 dollars, based on CPI) had fewer arrests in the first year than those in the control group. Surprisingly, when examining just arrests for theft, the largest effects were for those study participants who did not receive job placement services along with the financial assistance. Uggen et al (2002) point out that this early experiment found an age interaction in that those who were at least 26 years old were much less likely to be arrested than younger participants.

Transitional Aid Research Project (TARP)

Following the results of the LIFE experiment, the Department of Labor decided to repeat the study with slightly different benefits and no limits on eligibility in two additional experiments, commonly referred to as the Transitional Aid Research Project (TARP). Initiated in Texas and Georgia in 1976, approximately 4,000 ex-prisoners participated in two studies (one in each state) with random assignment into four experimental and two control groups in each study (Rossi et al. 1980; Berk et al. 1980).[3] The experimental treatments included either unemployment insurance benefits or job placement. For those who received the unemployment insurance benefits, either 13 or 26 weeks of eligibility for unemployment insurance benefits could be received; for those who received the 13 weeks of benefits, either 100% or 25% tax rate on earnings could be received. Computerized arrest records in each state were examined 1 year after participants had been released from prison.

TARP, which was intended to be a replication and extension of LIFE, added a program detail that was not communicated effectively to participants (termination of or reduction in payments when employment was secured), which may have led to a work disincentive effect (Rossi et al. 1980: 7).[4] The evaluators claim that the resulting unemployment of program participants had the effect of increasing arrests for the treatment group; no significant differences were found in arrest rates between the four TARP experimental groups and two control groups in either Georgia or Texas.[5] Thus, the financial assistance experiments of the 1970s (LIFE and TARP) were not consistent in their findings of an impact of such programs on criminal activity.

National Supported Work Demonstration

The National Supported Work Demonstration, also funded by the U.S. Department of Labor, enrolled in nine U.S. cities men who had been recently incarcerated, were currently unemployed, and had been employed for no more than three of the preceding 6 months between 1975 and 1977 (Piliavin and Gartner 1981; Uggen 2000). Study participants were randomly assigned to either minimum-wage jobs in crews with 6–8 other workers or a control group. In a reanalysis of the original data, Uggen (2000) examined first self-reported arrest for two theoretically important subgroups: those under age 26 and those aged 26 and older. The combined sample size was 3,105 and the follow-up period ranged from 18 to 36 months, depending on the date of enrollment into the program.

Uggen's (2000) reanalysis of the National Supported Work Demonstration showed that the effect of an employment program varied by the age of the study participants. Specifically, a program that originally was deemed a failure was found to significantly reduce recidivism among ex-offenders over the age of 26. For younger ex-offenders, at the end of 1 year, 31% of those in both the treatment and control groups reported that they had been arrested. Among older offenders, however, those in the treatment group had arrest rates about 8 percentage points lower than those in the control group. These differences increased to 11 percentage points after 3 years (Uggen 2000). [Exact percentages of those arrested by age group are not provided.] Uggen's work (1999, 2000; Uggen et al. 2002), documenting the significance of age of participant in the success of the employment program, is an important step forward in the disappointing 20-year history of job training and employment programs for ex-offenders.

Job training program for probationers

In a study conducted in a Midwestern city during the years 1979, 1980, and 1981, 216 probationers were randomly assigned to either a job training program or to standard community probation (Anderson and Schumacker 1986). Program participants were CETA-qualified and were aged 18 to 25 years. The program provided a variety of job-training skills including preparing resumes and employment applications, role-playing job interviews, and providing some skills training. Participants were compared on an overall measure of recidivism, including arrests, probation revocation, and new sentence, at 6 and 12 months. Anderson and Schumacker (1986) found no differences in 6- and 12-month outcomes in their evaluation of the job training program for probationers. At 6 months, 15% of the control group and 13.5% of the treatment group had 'difficult' outcomes, defined as probation revocation, or new conviction resulting in a jail or prison sentence. At 12 months, the adjusted means showed fewer difficult outcomes for the treatment group compared to the controls (15.5% vs. 23%), but the difference was not statistically significant. Because of the need to control for some differences between the groups, we chose to code adjusted means for the meta-analysis.

Job Training Partnership Act (JTPA)

The Job Training Partnership Act (JTPA) supported employment and training programs for economically disadvantaged Americans, including school dropouts with previous arrest records. Services provided varied across sites and were individually tailored to study participants. For the ex-offender youths, services typically included basic education and "miscellaneous services" such as job-readiness training, vocational exploration, job shadowing, and tryout employment (Bloom et al. 1994: 27, 51). JTPA is described as a less intensive approach than either JOBSTART or the youth component of the National Supported Work Demonstration. The evaluation, commissioned by the U.S. Department of Labor, required an experimental design with random assignment to treatment and control groups at 16 study sites during the period 1987 to 1989. The study reports arrest outcomes for 390 male ex-offenders at an average follow-up period of 21 months and 198 participants at 36 months (Bloom et al. 1994).

The evaluation of the Job Training Partnership Act (JTPA) program found no discernable effects on male youth (aged 17–21) with previous arrest records. During the first follow-up period (at 21 months, on average), 43% of both the treatment and control group had been arrested. At the second follow-up (at 36 months, on average), 59% of the youth in JTPA were arrested, compared to 56% of the control group (Bloom et al. 1994: Exhibit 11).

JOBSTART

The JOBSTART demonstration was created in 1985 as an alternative approach to both Job Corps (see below) and the Job Training Partnership Act (JTPA). JOBSTART provided a combination of basic skills education, occupational training, support services and job placement assistance to young, low-skilled school dropouts in 13 sites between 1985 and 1989. One subgroup in the evaluation comprised 291 male and female ex-offenders (with a prior arrest) aged 17–21 who were either randomly assigned to the experimental group or a control group (Cave et al. 1993). Arrest records were examined for participants and controls at 1 and 4 years after enrollment in the program.

JOBSTART, which provided longer-term services than JTPA to an essentially similar population of disadvantaged young adults with arrest records, also found no differences between the treatment and control groups at the end of 4 years. At the end of 1 year, 35% of both those in the program and the control group had been arrested, but at 4 years, 69% of the experimentals and 75% of the controls had been arrested. However, this difference was not statistically significant because of the small sample sizes in this subgroup (Cave et al. 1993: 194). Thus, following the mixed results of the financial assistance experiments of the 1970s, the federally-sponsored employment demonstrations targeting disadvantaged young adults with a criminal history, were found to be very disappointing.

Job Corps

Job Corps is a long-term residential program that emphasizes academic and vocational preparation with some job placement assistance for a seriously disadvantaged population, primarily school dropouts. Funded by the U.S. Department of Labor since 1964, Job Corps received $1.3 billion and enrolled 60,000 youth aged 16 to 24 in 1999. An evaluation conducted in 2000 used random assignment on all applicants who applied to Job Corps between November 1994 and February 1996. The control group was not allowed to sign up for the program for 3 years, but many did receive some type of training elsewhere, often vocational training (Schochet et al. 2000). The evaluation examined self-reported arrests over a 48-month period for a subgroup of 998 ex-offenders (defined as ever been arrested) who were enrolled in the program as compared to ex-offenders in the control group.

In the recent evaluation of Job Corps, Schochet et al (2001) found no differences in self-reported arrests between Job Corps participants with prior arrest records and controls. The difference in proportions rearrested was 1.3% for a group with prior arrests for nonserious crimes and 4.7% for a group with serious prior arrests (Schochet et al. 2001: Table F. 12). Additional data presented on follow-up convictions also do not indicate any impact of the Job Corps program for those with prior arrests (Schochet et al. 2001: Table F. 12). However, alcohol consumption and hard drug use declined among Job Corps participants with a prior nonserious arrest (Schochet et al. 2001: Table H. 4).

Opportunity to Succeed (OPTS)

The most recent study, the Opportunity to Succeed (OPTS) program, initiated in 1994, was a 3-year demonstration program designed to reduce substance abuse relapse and criminal recidivism by providing comprehensive post-release services, including job readiness classes, job training, and job placement to ex-prisoners with alcohol and drug offense histories (Rossman et al. 1999). The program operated in five communities but the evaluation was carried out in three: Kansas City, MO, St. Louis, MO, and Tampa, FL. The evaluators randomly assigned 398 participants to treatment and control groups; services were available for up to 2 years for OPTS clients. Outcomes included both self-reports and official records. Official criminal justice records of arrest and technical violations were obtained for 84% of the sample at the end of the first year of supervision or OPTS program participation.

An evaluation of OPTS found that there was little substantive or statistical difference between the participants in the program and the control group on self-reported arrests (Rossman et al. 1999). Program clients reported committing fewer robberies and engaging in less disorderly conduct than the controls, but these differences are significant only at the 0.10 level (Rossman et al. 1999: Figure 6-2). Analysis of official records found no differences in the two groups on number of arrests, but the program participants did have a greater rate of technical violations than the controls. The authors suggest that OPTS clients had greater contact with case managers which may have resulted in increased detection of violations.

Meta-analysis

Effect sizes for the eight studies were computed using inverse-variance methods and followed the meta-analytic approach recommended by Lipsey and Wilson (2001). Continuous outcome measures were preferred to dichotomized outcomes wherever both were available. All effect sizes were coded so that a positive effect size indicates the treatment group subjects experienced less recidivism than the comparison group. We applied the formula recommended by Hedges (1981) to adjust for upward bias in standardized mean difference (SMD) effect sizes due to small sample sizes. This bias adjustment was trivial for all studies as all of the effect sizes were based on samples of 200 or more subjects. An arcsine transformation was applied to the effect sizes computed from dichotomized outcome measures to make them comparable to the SMD effect sizes.

All of the studies reported arrests during the follow-up period as an outcome measure. Dichotomized arrest measures (i.e., the proportion of subjects who were arrested) were reported for six of the eight studies. We applied an arcsine transformation to these proportions and computed effect sizes as differences of proportions. The remaining two studies (Rossman et al. 1999; Rossi et al. 1980) reported a continuous recidivism measure (i.e., the mean number of arrests during follow-up), and so SMD effect sizes were computed. The follow-up periods for which we were able to code outcomes from the eight studies ranged from 6 to 48 months with a mode of 12 months.

Two of the studies (Mallar and Thornton 1978; Rossi et al. 1980) used crossed designs involving multiple treatment groups, each of which received a different intervention, being compared with a single comparison group. Separate effect sizes computed for each treatment group would not have been statistically independent because of the common comparison group. To keep the effect sizes independent, we computed a weighted mean of the outcome measures for the multiple treatment groups in each 'sub-study' using the degrees of freedom in each treatment group (i.e., $n - 1$) as a weight. A single effect size was computed for each study using the weighted mean outcome for the treatment group effect and the sum of the degrees of freedom for the k treatment groups (i.e., $n_1 + n_2 + \ldots + n_k - k$) as the treatment group sample size. In short, we used aggregation to avoid statistically dependent effect sizes at the cost of the ability to examine the effects of the different treatment modalities separately.

The TARP experiment (Rossi et al. 1980) was actually two simultaneous studies, one in Texas and another in Georgia, using the same design. Four treatment groups and one comparison group were created in each state, so we were able to compute a single effect size for the Texas study and an independent effect size for the Georgia study.

Besides the TARP experiment, the only study to contribute two effect sizes was Uggen's (2000) reanalysis of the National Supported Work Demonstration Project. He split the sample into subjects 26 years of age and younger and those 27 and older. This produced two independent treatment groups and two independent comparison groups. Consequently, we were able to compute two effect sizes, bringing the total to 10 effect sizes for the eight studies.

To summarize, we formulated and applied three rules to reduce the set of coded effect sizes to a group of 10 *statistically independent* effect sizes: (1) where studies had reported the same outcome for the same subjects at multiple time points, we used the average outcome measure across the time points to compute a single effect size; (2) where studies reported both adjusted (for detected areas of initial non-equivalence between the study groups) and unadjusted effect size information, we used the adjusted estimates; and (3) where studies reported the same outcome for *differing groups* of subjects at multiple time points (e.g., as a consequence of subject attrition during the follow-up period), we used the effect size information from the follow-up period nearest to 12 months, which was the modal follow-up period for entire sample of effect sizes. The second and third of these rules were applicable only to the handling of Anderson and Schumacker (1986), where effect size data were reported after 6 months of follow-up and again after 12 months of follow-up.[6] We computed the effect size for the Anderson and Schumacker (1996) study using the 12-month effects and the 12-month sample sizes.

Results

The first stage of the analysis is summarized in Table 1. The mean of the 10 effect sizes is 0.03, which is not statistically significant (z = 1.34; P = 0.1790). This finding indicates that, on average, the employment interventions examined did not reduce arrest among the treatment group subjects by more than the amount

Table 1. Mean effect size and heterogeneity test statistic, Q.

			95% CI	
Study	*ES*	*se*	*LL*	*UL*
Bloom	− 0.01	0.11	− 0.22	0.20
Cave	0.13	0.13	− 0.12	0.39
Schochet	0.03	0.04	− 0.05	0.11
Uggen (>27)	0.20	0.06	0.08	0.33
Uggen (<27)	− 0.03	0.04	− 0.12	0.06
Anderson	0.19	0.14	− 0.09	0.46
Mallar[a]	0.07	0.11	− 0.14	0.29
Rossi (TX)[a]	0.02	0.08	− 0.14	0.17
Rossi (GA)[a]	− 0.07	0.08	− 0.22	0.09
Rossman	− 0.05	0.11	− 0.26	0.17
MEAN	0.03	0.02	0.01	0.07
Q	12.5, df = 9, $P(> Q)$ = 0.1871			

[a]Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.

expected by chance. We also computed a Q statistic to test the null hypothesis that the variance of the sample of 10 effect sizes could be accounted for by sampling error alone. The value of Q is distributed as chi-square. Our test yielded a value of 13.45 ($P = 0.1462$; $df = 9$), which indicates that sampling error alone could explain the effect size variance in our sample.

With only 10 effect sizes in the sample, this null finding was easy to explain. Only one of the individual effect sizes, Uggen's (2001) sample of older subjects, was statistically significant (Figure 1). This effect was positive, indicating that treatment subjects had a lower incidence of arrest than comparison subjects. Four of the remaining nine effect sizes were negative and not significant, however.

To gauge the extent to which single effect sizes were driving our statistical inferences, we re-estimated the mean effect size and Q statistic excluding each study one at a time. Only when Uggen's (2001) younger sample was excluded did the remaining nine effect sizes yield a statistically significant ($P > 0.05$) mean effect size (Table 2). The value of Q never reached statistical significance. However, even this lone significant finding was tenuous. We found that it was contingent on our handling of the effect size from the Anderson and Schumacker (1986) study. In that study, the 6-month effect size was substantially smaller than the 12-month effect size. If we had elected to use the 6-month effect size or to average the two, there would have been no combination of nine effect sizes that yielded a statistically significant mean.

We were also concerned that our initial inferences based on the model in Table 1 might be sensitive to our choice of analytic approaches, so we tested an alternative. We had outcome estimates in the form of proportions for all of the studies. Using these dichotomized measures of recidivism, we computed 10 new logged odds ratio (OR) effect sizes, a new inverse-variance weighted mean effect size, and a new estimate of Q. This approach offered a greater degree of analytic consistency than the earlier sample comprised of a mix of seven arcsine transformed proportion differences and three SMD effect sizes. Two of the LOR effect sizes, Rossi (TX) and Rossman, differed in sign from their SMD

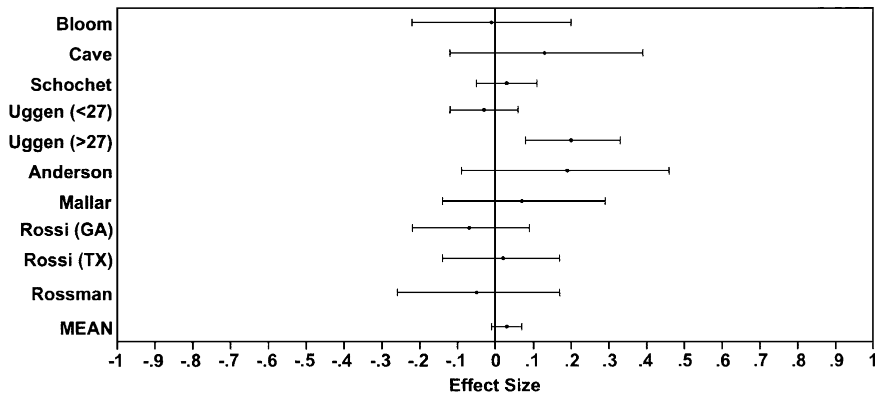

Figure 1. SMD effect sizes, 95% confidence intervals, and inverse-variance weighted mean effect size.

Table 2. Sensitivity of mean effect size and Q to the exclusion of single effect sizes.

Excluded ES	*Mean ES*	*se*	*P(> Mean ES)*[a]	*95% CI* *LL*	*UL*	*Q*	*P(> Q)*
Bloom	0.03	0.02	0.0805	− 0.01	0.08	13.20	0.1052
Cave	0.03	0.02	0.1168	− 0.02	0.07	12.76	0.1204
Schochet	0.03	0.03	0.1232	− 0.02	0.08	13.38	0.0994
Ugeen (> 27)	0.01	0.02	0.3942	− 0.04	0.05	5.07	0.7501
Uggen (< 27)	0.05	0.03	0.0244	0.00	0.10	10.83	0.2115
Anderson	0.03	0.02	0.1256	− 0.02	0.07	12.07	0.1481
Mallar[b]	0.03	0.02	0.1082	− 0.02	0.07	13.21	0.1048
Rossi (TX)[b]	0.03	0.02	0.0918	− 0.01	0.08	13.36	0.1000
Rossi (GA)[b]	0.04	0.02	0.0503	− 0.01	0.08	11.79	0.1608
Rossman	0.03	0.02	0.0723	− 0.01	0.08	12.89	0.1157

[a]P values are one-tailed.
[b]Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.

counterparts because these studies reported both the mean number arrests during follow-up as well as the proportion of subjects arrested. In these cases, the LOR and SMD effect sizes were based on different estimates.

The new mean effect size (0.06) was somewhat larger than the first (0.03), but the standard error of the mean of the logged OR effect sizes was also larger (0.05 vs. 0.02) (Table 3, Figure 2). The basic inference, however, remained the same: On average, these employment services interventions had no significant effect on the

Table 3. Mean effect size and heterogeneity test statistic, Q, computed from logged-odds ratios.

Study	*ES*	*se*	*95% CI* *LL*	*UL*
Bloom	− 0.01	0.22	− 0.44	0.41
Cave	0.30	0.29	− 0.28	0.87
Schochet	0.06	0.08	− 0.11	0.22
Uggen (> 27)	0.42	0.13	0.16	0.68
Uggen (< 27)	− 0.06	0.09	− 0.23	0.11
Anderson	0.54	0.36	− 0.17	1.26
Mallar[a]	0.13	0.22	− 0.31	0.58
Rossi (TX)[a]	− 0.07	0.16	− 0.39	0.25
Rossi (GA)[a]	− 0.04	0.16	− 0.35	0.27
Rossman	0.06	0.23	− 0.39	0.50
MEAN	0.06	0.05	− 0.03	0.15
Q	13.0, $df = 9$, $P(> Q) = 0.1631$			

[a]Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.

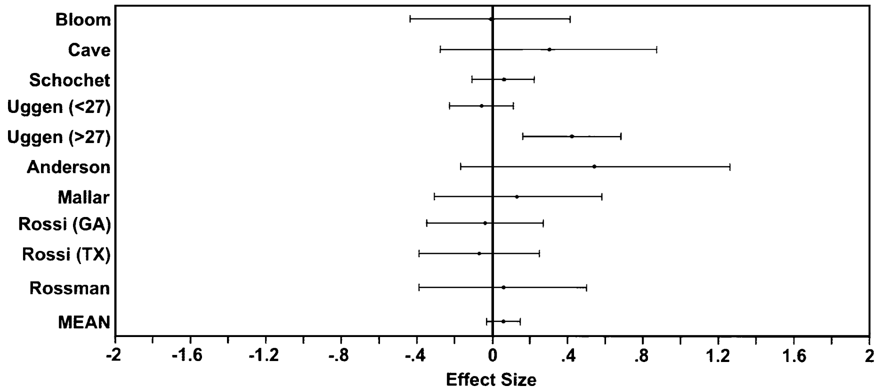

Figure 2. LOR effect sizes, 95% confidence intervals, and inverse-variance weighted mean effect size.

likelihood of arrest among ex-offenders. Furthermore, the Q statistic was not significant in this sample of logged odds ratios indicating that it is plausible to claim that all of the effect sizes were drawn from the same population. The variance in this sample of effect sizes can be plausibly attributed to sampling error alone.

We repeated our sensitivity analysis with the logged-odds ratios and found a similar pattern. The only combination of nine logged-odds ratios to yield a significant mean effect size excluded Uggen's (2000) younger sample (Table 4). The Q statistic was not significant for any combination of nine effect sizes.

We concluded that, on average, these eight employment services interventions had no significant effect on the likelihood that the treatment subjects would be

Table 4. Sensitivity of logged OR mean effect size and Q to the exclusion of single effect sizes.

				95% CI			
Excluded ES	*Mean ES*	*se*	*P(> Mean ES)*[a]	*LL*	*UL*	*Q*	*P(> Q)*
Bloom	0.07	0.05	0.0737	− 0.02	0.16	12.86	0.1168
Cave	0.06	0.05	0.1032	− 0.03	0.15	12.34	0.1367
Schochet	0.07	0.05	0.1078	− 0.04	0.17	12.98	0.1125
Ugeen (> 27)	0.02	0.05	0.3675	− 0.08	0.11	4.71	0.7881
Uggen (< 27)	0.11	0.05	0.0201	0.00	0.21	10.28	0.2459
Anderson	0.06	0.05	0.1096	− 0.03	0.14	11.23	0.1890
Mallar[b]	0.06	0.05	0.0945	− 0.03	0.15	12.90	0.1153
Rossi (TX)[b]	0.07	0.05	0.0570	− 0.02	0.17	12.28	0.1391
Rossi (GA)[b]	0.07	0.05	0.0616	− 0.02	0.16	12.52	0.1295
Rossman	0.06	0.05	0.0838	− 0.03	0.15	12.99	0.1122

[a] P values are one-tailed.
[b] Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.

Table 5. Sub-sample analysis by conviction status of the subjects.

				95% CI			
Sub-sample	*Mean ES*	*se*	*P(> Mean ES)[a]*	*LL*	*UL*	*Q*	*P(> Q)*
Convicts[b]	0.01	0.04	0.4272	− 0.08	0.09	3.14	0.5347
Non-convicts	0.04	0.03	0.0729	− 0.01	0.09	9.90	0.0421

[a]*P* values are one-tailed.
[b]Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.

rearrested. With only 10 independent effect sizes, however, our statistical power was, no doubt, modest. The possibility of Type II error cannot be discounted, especially since our sensitivity analysis showed that we might have concluded that these programs had a modest salutary effect but for the inclusion of the younger sample from Uggen's (2000) study. On the other hand, the largest mean effect size obtained for any nine of the 10 effect sizes was 0.11, a rather small effect. The evidence seemed to support a rather confident conclusion that the effect of the employment services interventions on recidivism was either null or salutary and quite small.[7]

With this null finding and a non-significant heterogeneity test, we might have concluded the analysis. However, we wanted to investigate explicitly the possibility that the effect of the intervention was related to the significance of the subjects' prior criminal records. Five of the 10 effect sizes (Uggen, Schochet, Bloom, and Cave) were contributed by studies that relied on samples of persons who did not necessarily have a prior criminal conviction. The remaining five effect sizes (Mallar, Rossi, Rossman, and Anderson) were contributed by studies of persons with one or more convictions. The Mallar, Rossi, and Rossman studies included only former prisoners; the Anderson study examined probationers. We divided the effect sizes accordingly into two sub-samples, convicts and non-convicts, and computed a new mean for each (Table 5). The results suggested that the studies involving samples of less serious offenders (no recent convictions or incarcerations) showed evidence of larger, but still not significant, effects.

Discussion

This systematic review reveals that knowledge about the effectiveness of non-custodial employment services for ex-offenders is hampered by inadequate contemporary research. Only eight studies using random assignment, dating back to the early 1970s, could be identified in English-language publications. Moreover, these studies are quite disparate in terms of primary intervention and target population. Nonetheless, we concluded that, overall, the eight interventions had no significant effect on the likelihood that participants would be rearrested. When the studies were divided into two groups, based upon whether the target population had a prior conviction or had only a prior arrest, the results did not change.

The original intent of this systematic review was to examine employment services interventions for non-custodial offenders. Unfortunately, only one random assignment study was located that was completed in the last 10 years with this target population (Rossman et al. 1999). The lack of federal funding for ex-offender employment programs in the 1980s appears to have created a gap in the development and implementation of these programs, particularly for persons leaving prison. Thus, rigorous evaluations of contemporary employment interventions for former prisoners are sorely needed. Although many such programs operate in communities, evaluations of their effectiveness are rare and random assignment designs have not been used (Finn 1998). In the course of this review, several quasi-experimental studies of more recent community employment programs were located (e.g., Finn and Willoughby 1996; Lattessa and Travis 1991; Menon et al. 1992; Turner and Petersilia 1996), but none report that the programs significantly reduced recidivism. Thus, inclusion of these studies in a broader meta-analysis appears unlikely to alter our conclusions.

In the 1990s, a new generation of community employment programs for ex-offenders emerged. Programs such as the Safer Foundation in Chicago, Center for Employment Opportunities in New York, and Project Rio in Texas are run by non-profit organizations but work closely with the criminal justice system. These employment programs are more intensive and are prepared to help clients with basic life skills, job readiness, social support, job-placement assistance, and continued support after a job is secured (Buck 2000; Finn 1998; Solomon et al. 2004). They also appear more focused on matching the needs of clients with appropriate services than the government-funded community employment programs of the past. For many of these programs, rigorous, random assignment evaluations are underway.

Stable, satisfying employment is a critical predictor of post-release success for individuals released from prison. However, former prisoners typically have poor work histories and a limited range of skills. These deficits, coupled with a recent felony conviction and period of incarceration, often lead to difficulty finding and keeping a job that will allow these individuals to provide financial support for themselves, and for many of them, their families. Employment interventions can include a range of services such as job-readiness classes, vocational education, GED certification, job training, job placement, and job monitoring by a case manager for some time period. Not all returning prisoners need all these services. Many held legitimate jobs before incarceration and only need assistance in locating an employer who would hire them, given their recent conviction and incarceration. Others may never have held a full-time job with regular hours and need a full set of services before entering the labor market. Community-employment programs may be more effective with this population if the needs of individuals can be identified and linked to specific services. Ideally, a new generation of evaluations would provide some direction to policy-makers as to the most effective combination of services for specific types of former prisoners.

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Notes

1 We permitted one exception to this criterion. The Job Corps study (Schochet et al. 2001) used a study sample that included some 16-year-olds.
2 This discussion benefited greatly from the overview of many of these studies provided in Uggen et al. (2002).
3 Only one of the two control groups in each study was interviewed in the same manner as the treatment group. The second treatment group was followed through review of official records only. We ignored the non-interviewed comparison groups when coding the TARP studies because of this dissimilarity in data sources.
4 Participants in the LIFE program were told that they were entitled to partial benefits if they worked. In fact, almost all participants received the full $780 in the first 13 weeks; hence, in practice, participants did not encounter the 'employment tax' that the TARP participants faced (Mallar and Thornton 1978: fn. 3).
5 In Georgia, the arrest rates of the four experimental groups ranged from 48.4% to 49.9%, compared to 48.4% or 48.7% in the two control groups (Rossi et al. 1980: Table 5.1). In Texas, the arrest rates ranged from 34% to 42.5% for the experimentals, and 35.5% to 36.5% for the controls (Rossi et al. 1980: Table 5.2).
6 One of the 101 treatment group subjects dropped out of the sample between the 6- and 12-month observations and none of the 103 comparison subjects were lost.
7 We also examined our results using a 0.10 alpha level and none of the inferences from our analysis changed.

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