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Systematic Review of Non-Custodial Employment Programs: Impact on Recidivism Rates of Ex-Offenders

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Systematic Review of Non-Custodial Employment Programs: Impact on Recidivism Rates of Ex-Offenders

Campbell Collaboration Research Review

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ABSTRACT

Background

Over the last 25 years, many programs that were designed to increase employment for persons with a criminal history have been implemented and evaluated. The implicit, and often explicit, intent of these programs has been to reduce recidivism. Wilson et al. (1999, 2000) conducted a quantitative synthesis of 33 evaluations of educational, vocational, and work programs for persons in correctional facilities. To date, however, the evaluation literature on employment services programs for those with a recent criminal record who are not in custody has not been systematically reviewed.

Objectives

To assess the effects of programs designed to increase employment through job training and/or job placement among formerly incarcerated persons (i.e., those recently released), aimed at improving employment and reducing recidivism.

Search Strategy

Searches of literature reviews by the first author were augmented by structured searches of nine electronic data bases, including the Campbell SPECTR database of trials to identify random assignment studies conducted after 1970. Experts in the field were consulted and relevant citations were followed up.

Selection Criteria

Selecting studies based on the original objective – to examine employment services interventions for recently released prisoners – did not produce a sufficient number of studies for analysis. Thus, the selection criteria were expanded to include studies that tested the effects of job training or job placement programs or both for persons who had been arrested, convicted or incarcerated in connection with a criminal charge. Only random assignment studies of adults or studies that combined older youth (ages 16-17) and adults were included. If the treatment or comparison groups included subjects who were not ex-offenders, the results must have been reported separately for the ex-offenders.

Data Collection and Analysis

We report narratively on the eight eligible studies. More than 6,000 older youth (aged 16-17) and adults with prior contact with the criminal justice system participated in these studies. Two studies contributed two independent effect sizes for a total of ten effect sizes for the eight studies. We used arrests during the follow-up period (typically, 12 months) as the outcome measure. We ran three analyses: one with a mixture of dichotomized and continuous arrest

measures, a second with logged odds ratio effect sizes, and a third splitting the sample into those with a conviction and those without a conviction.

Main Results

The analyses show that employment-focused interventions for ex-offenders in these studies did not reduce recidivism, although this group of random assignment studies is highly heterogeneous both in the type of employment program delivered and the individuals enrolled in the program. Thus, the results should not be generalized to former prisoners who are enrolled in employment programs after release. The studies are also mostly out of date and the average subject was not typical of persons released from prison in the U.S. in the early 2000s.

Reviewers' Conclusions

We conclude that employment-focused interventions for former prisoners have not been adequately evaluated for their effectiveness using random assignment designs. After broadening the selection criteria to include individuals with criminal records, only eight studies, the majority of which are more than 10 years old, could be identified. Nonetheless, overall, the eight interventions had no significant effect on the likelihood that participants would be rearrested. Many employment-focused interventions for ex-offenders are being implemented. A new generation of rigorous evaluations is needed to provide direction to policymakers as to the most effective combination of employment-related services for specific types of ex-offenders.

SYSTEMATIC REVIEW OF NON-CUSTODIAL EMPLOYMENT PROGRAMS: IMPACT ON RECIDIVISM RATES OF EX-OFFENDERS

BACKGROUND

Many offenders, including serious ones, have had prior employment experiences. Three-quarters of state inmates reportedly held a job just prior to their incarceration, and, of those, just over half were employed full-time (Lynch and Sabol 2001). 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 wage (or higher quality) jobs (Sampson and Laub 1997; Harer 1994; Uggen 1999). Research evidence also suggests that being labeled by the criminal justice system (e.g., by being arrested) may adversely affect subsequent employment stability, even after controlling for duration or severity of prior criminal involvement (Bushway 1998).

While the period of incarceration might be viewed as an opportunity to build skills and prepare for placement at a future job, the evaluation literature provides mixed to negative support for the effectiveness of in-prison job training programs (Bushway and Reuter 1997; Gaes et al. 1999; Wilson et al. 1999; 2000). In addition, long periods of incarceration may weaken social contacts that lead to legal employment opportunities upon release (Western et al. 2001; Hagan and Dinovitzer 1999). Finally, many barriers exist for persons released from prison who are looking for work, including the stigma attached to incarceration, the lack of recent job experiences, and a lessening of skills needed to find and hold jobs (Western et al. 2001; Sampson and Laub 1997).

Not surprisingly, 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. Having a criminal record represents a substantial barrier to many types of legal employment, and these barriers are compounded for those seeking work after a term of prolonged incarceration. Because there are now a substantial number of released prisoners, policymakers face an overwhelming need to improve the employment prospects of former prisoners.

Many programs designed to increase the likelihood of future employment and, by so doing, reduce recidivism among persons who are incarcerated have been implemented and evaluated in the two decades. Although Wilson and his colleagues (1999, 2000) conducted a quantitative synthesis of 33 evaluations of educational, vocational, and work programs for individuals held in correctional facilities; to date, the evaluation literature on employment programs for individuals with a recent criminal history who are not incarcerated has not been systematically reviewed.

The primary research question is: What is the effect of non-custodial employment services interventions on the subsequent criminal behavior of ex-offenders?¹ This review will

¹ The authors acknowledge that the term “ex-offender” is not ideal and that many practitioners in the field are moving away from its use. It is used in this review to be consistent with the terminology in the studies being analyzed.

survey the existing empirical evidence that examines the effectiveness of employment programs on recidivism among persons who have been recently incarcerated. Because there is specific interest in isolating the effects from experimental designs, this review is limited to those studies using random assignment.²

OBJECTIVES OF THE REVIEW

The objectives of this systematic review were to conduct a thorough literature search for existing evaluations of the effect of employment programs for former prisoners on recidivism. Many critical literature reviews of the research on employment and recidivism have been published, but a systematic meta-analysis of program evaluations has not been conducted. Given the widespread research, policy, and practical interest in knowing whether employment-focused interventions reduce recidivism among recently incarcerated individuals, a systematic review of the available literature is warranted.

METHODOLOGY

Eligibility Criteria

Types of Studies

Our initial review included both experimental and quasi-experimental evaluative studies; only the experimental set is analyzed and presented here. Eligible studies had to have included one or more treatment groups and one or more comparison groups.

Types of Participants

Both the treatment and comparison groups must have been composed, at least in part, of ex-offenders -- for the purposes of this review, persons who have 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 17) and adults are eligible for this review.³ Initially, we set the eligibility criteria broadly to avoid overlooking relevant studies and then planned to select those studies whose participants had been recently incarcerated.

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

² Because of the extensive literature on the topic and the numerous critical, yet not systematic, reviews, this first review chose to focus solely on random assignment studies. A later analysis may include the quasi-experimental studies and compare the meta-analytic results between these two groups. Some thoughts about the newer quasi-experimental studies are included in the Discussion.

³ 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.

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.

Types of Interventions

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 as were studies of traditional work release programs. 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.

Outcome Measure

Some measure of criminal behavior subsequent to the beginning of the intervention must have been reported for the participants in both the treatment and comparison groups. The measure of criminal behavior may have been either official (i.e., arrest, conviction, technical violation) or self-reported and may be reported either dichotomously or on a continuous scale. In some studies, post-intervention employment status could be coded from the reports, but the available data on employment were too limited for meta-analysis.⁴

Language and Time Frame

This review only includes study reports written in English. It is not known how many studies in other languages may be eligible. Because we understood that much of the interest in understanding the role of work in reducing criminality had declined in the last twenty or so years, and because we were concerned about the potential effect that changes in the economic environment might have on programmatic effectiveness, 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.

Search Strategy for Study Identification

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

- Contacts with leading researchers;

⁴ Several of the studies collected data on employment-related outcomes (e.g., employment status, weeks worked, total legal earnings). We did not code effect sizes for the employment outcomes, however, because we did not find enough studies reporting **comparable** employment measures for the sub-sample of those with a prior criminal record to warrant meta-analysis.

- 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 (see list below).

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, aftercare, 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).

Selection of Studies

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 a focus on 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.).

Upon examination of the studies, it became clear that there would not be sufficient random assignment studies involving former prisoners to conduct a formal meta-analysis. Thus, we expanded the selection criteria to include subjects with a previous arrest or conviction.

The total number of independent studies using random assignment designs that satisfied our revised 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 and Extraction

A Microsoft Access database was constructed for our 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 (Bloom et al., 1994) 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 re-analyses 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, Berk, and Lenihan 1980; Cave et al. 1993; Schochet et al. 2001). Combining across studies, more than 6000 older youth (aged 16-17) and adults with prior contact with the criminal justice system participated in the eight studies in this review.

Six of the experiments were simple two-group designs (the exceptions being Rossi, Berk, and Lenihan 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 employment services programs, albeit broadly defined, for 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 initiation⁵, and then provide a brief assessment of this group of rather disparate studies before presenting the meta-analytic results.

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, Berk and Lenihan 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 short-term income support to released prisoners, coupled with job counseling and placement assistance, would speed an ex-prisoner's return to the labor force and, consequently, reduce the likelihood of criminal activity (especially, property crimes used to gain income).

Beginning in 1971, 432 prisoners released from Maryland state prisons and returning to Baltimore were randomly assigned to receive 13 weeks of payments of \$60 per week and/or intensive job counseling and placement services or placed in a control group that received no payments or 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 one year.

The LIFE experiment found that the job placement and counseling intervention had no impact on arrests in the first year, but 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, the fewest arrests (for theft) occurred for those study participants who only received financial assistance with no job placement services. Uggen and his colleagues (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 (see also Lenihan 1976).

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

⁵ This discussion benefited greatly from the overview of many of these studies provided in Uggen, Piliavin, and Matsueda (2002).

experiments, commonly referred to as the Transitional Aid Research Project (TARP). Initiated in Texas and Georgia in 1976, approximately 4000 ex-prisoners participated in two studies (one in each state) with random assignment into four experimental and two control groups in each study (Rossi, Berk, and Lenihan 1980; Berk, Lenihan, and Rossi 1980).⁶ 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, and for those who received the 13 weeks of benefits, either 100 or 25 percent tax rate on earnings could be received. Computerized arrest records in each state were examined one 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).⁷ 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.⁸ The financial payments actually reduced the number of weeks worked, although ex-offenders who received payments did get better jobs, and those who were employed had fewer arrests. However, the **employment services** component in both the LIFE and TARP experiments had no effect 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 arrested, convicted, or incarcerated, were currently unemployed and had been employed for no more than three of the preceding six 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 3105 and the follow-up period ranged from 18 to 36 months, depending on the date of enrollment into the program.

⁶ 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.

⁷ 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).

⁸ In Georgia, the arrest rates of the four experimental groups ranged from 48.4 to 49.9 percent, compared to 48.4 or 48.7 percent in the two control groups (Rossi et al. 1980:Table 5.1). In Texas, the arrest rates ranged from 34 percent to 42.5 percent for the experimentals, and 35.5 to 36.5 percent for the controls (Rossi et al. 1980:Table 5.2).

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 one year, 31 percent 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 employment services interventions, 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, 108 probationers enrolled in a job training program during the years 1979, 1980, and 1981 were compared to a random sample of 108 community probationers (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 six and twelve months.

Anderson and Schumacker (1986) found no differences in six and twelve month outcomes in their evaluation of the job training program for probationers. At six months, 15 percent of the control group and 13.5 percent of the treatment group had "difficult" outcomes, defined as probation revocation, or new conviction resulting in a jail or prison sentence. At twelve 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 percent of both the treatment and control group had been

arrested. At the second follow-up (at 36 months, on average), 59 percent of the youth in JTPA were arrested, compared to 56 percent 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 one and four 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 four years. At the end of one year, 35 percent of both those in the program and the control group had been arrested, but at four years, 69 percent of the experimentals and 75 percent 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, 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 three 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 and his colleagues (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 percent for a group with prior arrests for nonserious crimes and 4.7 percent 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 three-year demonstration program designed to reduce substance use 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 two 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 percent 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 .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.

Summary

These eight studies find little or only modest effects of employment services programs for reducing the recidivism of ex-offenders. In several of the studies, the experimental subjects did have better outcomes than the control subjects but the differences were not statistically significant. Unfortunately, these random assignment studies of employment services programs for ex-offenders are dissimilar in many respects. First, these eight studies span almost 25 years. Second, the employment services interventions tested in these studies vary widely, from job readiness programs (including basic education) and intensive vocational training to job placement assistance to work crew assignments. Third, the participants in these studies range in age from older youth (16-17 years) to those in their 40s. Fourth, the prior criminal histories of the participants vary from an arrest for a nonserious crime to a recent incarceration for a serious offense. Special analyses of two of the studies by Uggen did find that older participants (at least 26 years) were much less likely to be arrested than younger participants. However, the interventions in these two studies were quite different – financial incentives and work crew assignment.

Such a heterogenous group of studies is not ideal for a meta-analysis, although with a larger number of studies, characteristics of the studies themselves (i.e., type of program, age of participant) could be taken into account in a multivariate analysis. However, an N of eight studies precludes such an analysis. Nonetheless, we proceeded with the meta-analysis because over 6,000 subjects with a history of criminal justice involvement had participated in these employment services interventions, the studies had not been previously subjected to a quantitative meta-analysis, and we wanted to establish a baseline for future systematic reviews of this literature when additional studies become available.

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 so as to preserve the greater detail of the continuous measure and avoid being potentially misled by the selection of cut-points used to dichotomize the measures. 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 formulae 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 months to 48 months with a mode of 12 months.

Two of the studies (Mallar et al. 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., $n_1 + n_2 + \dots + n_k - k$) 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) re-analysis 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.⁹ We computed the effect size for the Anderson and Schumacker (1986) study using the 12-month effects and the 12-month sample sizes.

Attrition can be a serious source of bias in random assignment studies, potentially affecting the validity of the results and hence, producing misleading meta-analytic results. In the process of coding the eight studies from which we computed the 10 effect sizes, we recorded two dichotomous items related to subject attrition: (1) whether there was a noteworthy number of subjects lost to attrition overall (from all study groups combined), and (2) whether the treatment and comparison groups experienced different degrees of attrition. We noted that overall attrition might be an issue for two studies (Anderson and Schumacker, 1986); Schocet et al., 2001) and noted differential attrition as a potential problem for one study (Bloom et al., 1994). We observed that the effect sizes were not outliers and that our sensitivity analysis, in which each effect size was dropped in turn from the sample (see below), showed that our inferences were insensitive to whether these studies were retained in the analysis.

Results

The first stage of the analysis is summarized in Table 1. We 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 ($P = .1462$; $df = 9$), which indicates that subject-level sampling error alone could explain the effect size variance in our sample. We proceeded with the analysis based on fixed-effects assumptions, most notably that all 10 effect sizes were drawn from the same population. We based this decision on the fact that our eligibility criteria imposed a high degree of methodological and subject-area uniformity on the studies. In addition, the P -value of the Q statistic was greater than .10 and was based on 10 effect sizes based on relatively large samples ranging in size from 203 to 2,125 subjects.

⁹ 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. Anderson and Schumacker reported both adjusted and unadjusted proportions of the treatment and comparison subjects who were not arrested during follow-up. The adjusted proportions corrected for observed non-equivalence between the groups following random assignment; these adjusted proportions were used to compute the effect size for this study. Had we used the unadjusted proportions, we would have reported a smaller, positive effect size that would have slightly attenuated the reported mean effect size. None of our inferences would have changed.

The mean of the 10 effect sizes is 0.03, which is not statistically significant ($z = 1.34$; $P = .1790$). This finding indicates that, on average, the employment interventions examined did not reduce arrest among the treatment group subjects by more than the amount expected by chance.

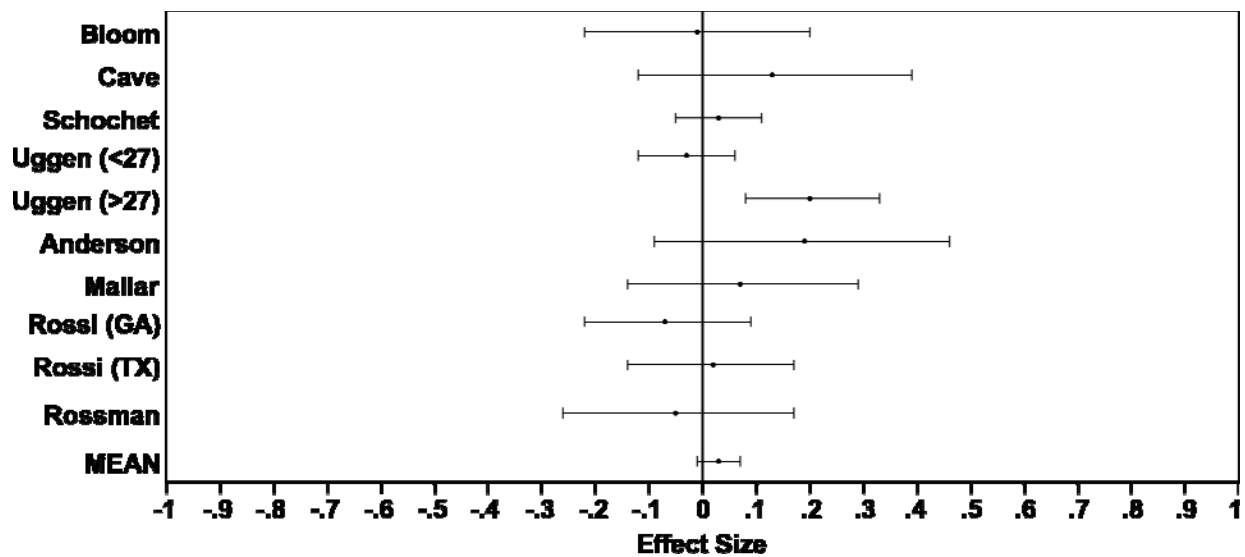
To provide context for the mean effect size, we computed mean proportion of comparison subjects who were not arrested during the follow-up period for the seven comparison groups where that proportion was available (i.e., those to which we applied arcsine transformation in computing the effect size). We found that, on average, 54.3% of the comparison subjects were not arrested during the follow-up period, and the standard deviation of this proportion was 49.1%. If we expect 54.3% of the comparison subjects to not be arrested and the mean effect size of the employment interventions on arrest is 0.03, then we would expect an average of $[54.3\% + (0.03 * 49.1\%)]$ 55.8% of treatment subjects to not be arrested.

Table 1. Mean Effect Size and Heterogeneity Test Statistic, Q

Study	ES	se	95% CI	
			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
Rossmann	-0.05	0.11	-0.26	0.17
MEAN	0.03	0.02	-0.01	0.07
Q	13.45, df = 9, $P(>Q) = .1462$			
^a	Effect sizes computed from the weighted mean outcome in multiple treatment groups contrasted with a single comparison group.			

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.

Figure 1. SMD Effect Sizes, 95% Confidence Intervals, and Inverse-Variance Weighted Mean Effect Size



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 ($P > .05$) 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 (1996) 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.

Table 2. Sensitivity of Mean Effect Size and Q to the Exclusion of Single Effect Sizes

Excluded ES	Mean ES	se	$P(>\text{Mean ES})^a$	95% CI		Q	$P(>Q)$
				LL	UL		
Bloom	0.03	0.02	.0805	-0.01	0.08	13.20	.1052
Cave	0.03	0.02	.1168	-0.02	0.07	12.76	.1204
Schochet	0.03	0.03	.1232	-0.02	0.08	13.38	.0994
Uggen (>27)	0.01	0.02	.3942	-0.04	0.05	5.07	.7501
Uggen (<27)	0.05	0.03	.0244	0.00	0.10	10.83	.2115
Anderson	0.03	0.02	.1256	-0.02	0.07	12.07	.1481
Mallar ^b	0.03	0.02	.1082	-0.02	0.07	13.21	.1048
Rossi (TX) ^b	0.03	0.02	.0918	-0.01	0.08	13.36	.1000
Rossi (GA) ^b	0.04	0.02	.0503	-0.01	0.08	11.79	.1608
Rossman	0.03	0.02	.0723	-0.01	0.08	12.89	.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.						

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 counterparts because these studies reported both the mean number of 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.

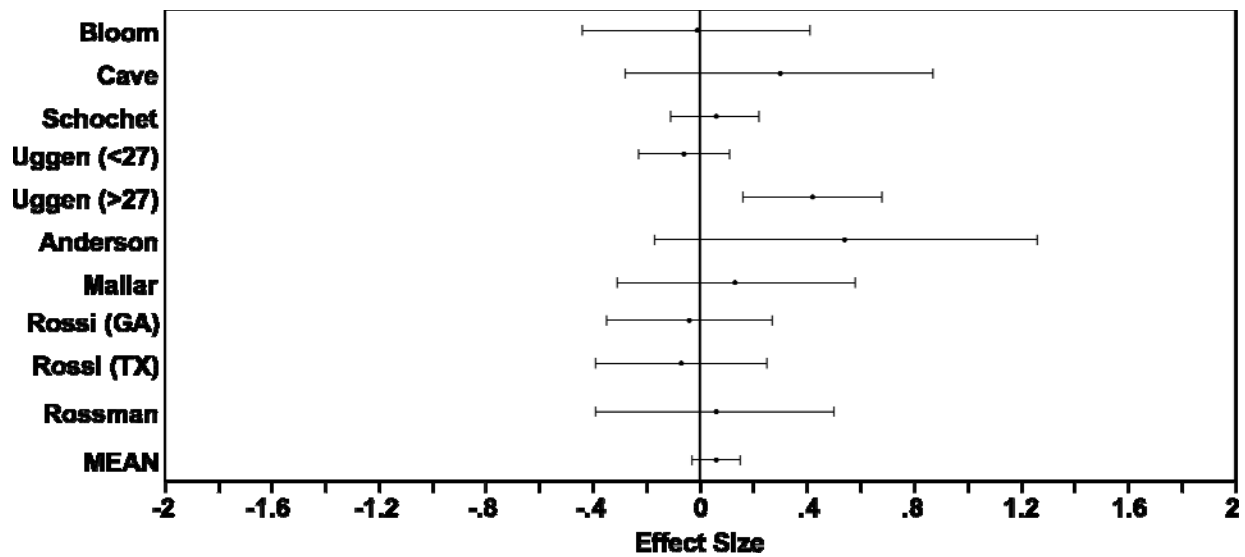
The Q statistic was not significant in this sample of logged odds ratios indicating that it is plausible to claim that all of the effect sizes were drawn from the same population. Once again, we proceeded under fixed-effects assumptions.

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 (.05 vs. .02) (Table 3, Figure 2). The basic inference, however, remained the same: On average, these employment services interventions had no significant effect on the likelihood of arrest among ex-offenders.

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, $df = 9$, $P(>Q) = .1631$			
^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



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 (2001) younger sample (Table 4). The Q statistic was not significant for any combination of nine effect sizes.

Table 4. Sensitivity of Logged OR Mean Effect Size and Q to the Exclusion of Single Effect Sizes

Excluded ES	Mean ES	se	$P(>\text{Mean ES})^a$	95% CI		Q	$P(>Q)$
				LL	UL		
Bloom	0.07	0.05	.0737	-0.02	0.16	12.86	.1168
Cave	0.06	0.05	.1032	-0.03	0.15	12.34	.1367
Schochet	0.07	0.05	.1078	-0.04	0.17	12.98	.1125
Uggen (>27)	0.02	0.05	.3675	-0.08	0.11	4.71	.7881
Uggen (<27)	0.11	0.05	.0201	0.00	0.21	10.28	.2459
Anderson	0.06	0.05	.1096	-0.03	0.14	11.23	.1890
Mallar ^b	0.06	0.05	.0945	-0.03	0.15	12.90	.1153
Rossi (TX) ^b	0.07	0.05	.0570	-0.02	0.17	12.28	.1391
Rossi (GA) ^b	0.07	0.05	.0616	-0.02	0.16	12.52	.1295
Rossman	0.06	0.05	.0838	-0.03	0.15	12.99	.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.						

We concluded that, on average, these eight employment services interventions had no significant effect on the likelihood that the treatment subjects would be 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 (2001) study. On the other hand, the largest mean effect size obtained for any nine of the 10 effect sizes was .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.

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 and the timing of the intervention (upon release from prison or shortly after sanction). 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).

Table 5. Sub-Sample Analysis by Conviction Status of the Subjects

Sub-Sample	Mean ES	se	$P(> \text{Mean ES})^a$	95% CI		Q	$P(>Q)$
				LL	UL		
Convicts	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.						

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

Implications for Research

This systematic review reveals that our ability to make definitive conclusions about the effectiveness of non-custodial employment services for ex-offenders is hampered by inadequate research. Only eight studies that use random assignment could be identified in English-language publications, and these date back to the early 1970s. 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.

We found that, on average, 54.3 percent of the comparison subjects were not arrested during the one-year follow-up period. Given a mean effect size of the employment interventions on arrest of 0.03 and taking into account the variation in the individual studies, we would expect an average of 55.8 percent of treatment subjects to not be arrested.

The original intent of this systematic review was to examine employment services interventions for formerly incarcerated individuals who had been recently released to the community. Unfortunately, only one study was completed in the last ten years with that specific target population (Rossman et al. 1999). Thus, contemporary studies of employment services 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.

As part of our search for appropriate studies for this meta-analysis, we did discover a number of studies that used quasi-experimental designs (i.e., see Buck 2000; Finn 1998a; Finn 1998b; Finn 1999; Menon, et al 1992; Martin, et al. 1999; Soothill 1974; Soothill, Francis, and Ackerly 1997; Turner and Petersilia 1996; Virginia Department of Corrections 1985, as discussed in Buck 2000). Some of the employment services interventions tested in these studies appear promising, although most of the treatment groups in these studies suffer from the problem of offender self-selection into the intervention, which likely renders the comparison group at higher risk for failure.

The quasi-experimental set of studies is representative of a new generation of employment services programs for ex-offenders. In an effort to move beyond the generic ‘employment program’ and consider aspects of such programs that may be specifically efficacious for former prisoners (as opposed to disadvantaged young adults), it is useful to examine these ‘new’ programs and consider the differences in organizational and programmatic structure.

Most of the quasi-experimental studies focus on programs that have been specifically designed for recently released prisoners. Elements of these programs include traditional work release, transitional employment in the community, pre-employment services (basic education, life skills, substance use and mental health counseling), job readiness training (resume preparation, interviewing, job search skills), job placement assistance, and post-employment support. In addition, some programs begin providing services before release from prison and continue services after release. Programs also differ on their role as a community intermediary (see Solomon et al., 2004) between the former prisoner and the business community.

The more comprehensive programs appear to have incorporated knowledge about the multiple challenges former prisoners face as they are released (see Petersilia, 2003; Travis et al., 2001) and have been designed to be programs that are not purely “employment services.” It is hard to argue with a conceptual philosophy that focuses initially on basic needs and skills (i.e., housing, sobriety, education) before offering employment services. On the other hand, transitional employment programs that immediately assign former prisoners to day labor and work crews were one of the interventions found effective in the 1970s, at least for older offenders. The cornerstone of the Center for Employment Opportunities in New York, which has been in operation for over twenty years, is immediate transitional employment, although the program also provides some pre-employment services and job readiness training.

Thus, the new generation of employment services programs for ex-offenders relies on several different models and types of service delivery. Moreover, many programs bundle several of these different types of interventions – for example, combining pre-employment services with job placement assistance. Future evaluations are likely to find it difficult to isolate the independent effect of these intervention components. Among the highest priority for rigorous research and evaluation are examinations of the effectiveness of transitional employment,

continuity of services from in prison to after release, continued support/services after job placement, and traditional work release programs.

This systematic review also points to the need for an examination of age of the subject as an important correlate of the effectiveness of employment services for ex-offenders. Two studies implemented in the 1970s show stronger effects for older ex-offenders, those at least 26 years old. However, these findings have not been replicated for more contemporary programs. Moreover, age may simply be a proxy for motivation. By virtue of their frequent experiences with the criminal justice system, older offenders may be more motivated to take full advantage of employment services programs in an effort to desist altogether from criminal activity.

Thus, our knowledge about the effectiveness of contemporary employment services programs for former prisoners, or even ex-offenders in general, is extremely limited. A number of random assignment evaluations of various programs supported by the Department of Labor in the U.S. are underway, but results are not expected for several years. In the meantime, a useful next step would be to canvass research in other countries to determine whether rigorous evaluations of employment services programs for ex-offenders have been conducted.

Implications for Practice

Stable employment is a critical predictor of post-release success for individuals released from prison. However, employment programs may only be effective for motivated individuals and standard employment programs are unlikely to change motivation. Steady, satisfying employment can provide a way to new social networks and a conventional lifestyle and thus be a critical component in the desistance process (Bushway, 2003; Laub and Sampson 2003; Sampson and Laub 1993).

Nonetheless, former prisoners and other ex-offenders typically have poor work histories and a limited range of skills. These deficits, coupled with a recent felony conviction and possible 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. Moreover, ex-offenders may have other needs that preclude immediate employment, including serious educational deficits, substance abuse, mental illness, and a lack of affordable or stable housing. In most cases, these issues will need to be addressed before an individual is deemed job-ready.

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 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 pre-employment and job readiness services before entering the labor market. Employment programs may be more effective with this population if the needs of individuals can be identified and linked to specific types of services. Ideally, a new generation of evaluations would provide some direction to policymakers as to the most effective combination of services for specific types of former prisoners.

The challenges for practitioners are many. Buck (2000) interviewed service providers and policymakers for her review of employment programs for ex-offenders and identified critical

components of their work that need strengthening, including improving the continuity of services between program activities inside and outside of prison, moving ex-offenders beyond entry-level jobs by offering occupational skills training and GED classes, and better communication among service providers to provide opportunities to learn from one another. In addition, limited public resources for these programs hampers the number of ex-offenders who can benefit. Waiting lists are common for programs in prison and in the community. Finally, practitioners agree that the dearth of knowledge about what makes an employment services program effective inhibits their ability to substantially improve the long-term employment rate of ex-offenders.

It is hoped that the results of the random assignment evaluations currently underway will provide better information to policymakers and practitioners who are working with ex-offenders.

PLANS FOR UPDATING THE REVIEW

In 2005, a number of random assignment and quasi-experimental studies of employment services programs for ex-offenders are underway. Depending on resources, the authors of this review intend to update this review in three to five years.

STATEMENT CONCERNING CONFLICT OF INTEREST

The authors of this review have no financial or personal conflicts that would influence judgments made in this review.

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