

## The Impact of Prison Education Programs on Post-Release Outcomes

Gerald G. Gaes

Florida State University\*

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# The Impact of Prison Education Programs on Post-Release Outcomes

This paper reviews the evidence on the impact of correctional education programs on post-release outcomes. Such reviews have been a popular enterprise in this research domain. There have been four meta-analyses, several “vote counting” reviews where analysts list studies that support or disconfirm the benefits of correctional education and then draw conclusions about the overall impact, and many other summaries of the research literature where the researchers select a few studies, cite their results, and draw inferences. The critical analysts, those that closely evaluate and assess the study methodology are much less sanguine about the relationship between correctional education and successful reentry than the more forgiving analysts who treat all of the research results in this literature on an equal footing. I believe the takeaway message is that correctional education does promote successful prisoner reentry. However, we only have an approximation of the true impact – the actual effect size. Even small effect sizes can produce substantial net cost-benefits especially for criminal justice costs that include adult corrections.

## Overview – Theory, Measurement, and Methods

When economists theorize about the effects of education they note the difference between gains in human capital and signaling effects. Human capital gains are what educators call achievement gains and these are presumed to give the student a skills advantage. Some of these advantages are generic, such as the ability to understand and execute printed and written instructions -- skills educators often refer to as literacy. The second advantage is skill specific, such as learning welding or computer skills. By gaining some kind of certification such as a GED, this signals to potential employers that the offender is capable of completed work. This advantage may help to combat the signaling “penalty” accompanying prisoners into the labor market resulting from a spell of incarceration (Western, 2007). Some educators, notably John Dewey (1916) also argue that certain levels of education are a

prerequisite to moral thinking. Other theorists (Harer, 1995) argue that prison education promotes prosocial attitudes and instills a disposition antithetical to the anti-social norms of prison life. Harer calls this normalization, the competing process in opposition to prisonization. It is the prison process that mirrors involvement and commitment to social institutions discussed by Sampson and Laub (1993). The theory is important because it focuses our thoughts on not only the policy variables, but on the measurement of intermediate outcomes as well.

Studies of correctional education have included analyses of Adult Basic Education (ABE), General Education Development (GED) preparation and certification, college coursework, various forms of vocational training, apprenticeship training, and some combination of one or more of these programs during a prison spell. Some studies distinguish between completion of a program and whether the completion produces some form of certification. Certification confirms a special status. It demonstrates that the program participant has achieved a specific level of skill that authorizing institutions endorse, or employers and other members of the community acknowledge. The research question is whether this status confers an additional advantage to prisoners when they reenter their community, seek work, and try to re-establish their civic identity.

The primary post-release outcome analysts have examined has been recidivism, measured as arrest, conviction, but mostly recommitment. A few studies have measured legitimate labor market participation and wages, and recent studies have used earnings prior to, during, and after a spell of incarceration in well designed panel studies (Cho and Tyler, 2008; Tyler and Kling, 2007; Sabol, 2007). In only a few cases, researchers examined institutional misconduct and one study even looked at parole adjustment (Knepper, 1990). One question that has not been addressed in any depth is whether prison education spawns a greater interest in pursuing continuing education once the inmate is released. There were no studies in this literature that measured whether participation or completion of education programs increased commitment to prosocial institutions, promoted prosocial attitudes, or enhanced moral reasoning. If these processes are an important dimension of reentry success, and they

are an important side effect of education training, then we ought to innovate ways to measure and evaluate these dispositions.

Many of the studies have been plagued by potential selection artifacts. These have been noted by analysts, reviewers, and meta-analysts of this literature. The best studies in this literature have tried different approaches to handle selection artifacts including studies that directly measure intermediate levels of motivation to assess the selection process, models that simultaneously quantify the selection process and the treatment process, propensity score models that try to match treatment and comparison subjects to minimize selection artifacts, and fixed effects panel models that control for time invariant characteristics that may be associated with selection processes. These strong quasi-experimental studies have still demonstrated reductions in recidivism and effects on labor market outcomes; however, the effect sizes have been smaller than those that do not introduce selection artifact controls.

In the following sections of this paper, I review the evidence on the level of educational need for inmates, the conclusions and reasoning of analysts who have conducted meta-analyses of correctional education, and the conclusions of scholars who have conducted “vote counting” or other kinds of literature reviews. In subsequent sections, I single out what I consider to be the best studies that have been conducted in this research domain; I discuss the limitations of the majority of studies in this area, and I review the results and implications of the Washington State Institute for Public Policy cost-benefit analysis of vocational training and basic education. In the last section, I summarize all of the findings and suggest improvements for future research. All of the studies that I could find in this research domain are listed in Table 2. I restricted the studies to education training in prison, and because there are so few studies of juveniles in the published literature, I excluded those as well. A large part of this literature comes from agency studies, many of which are not published in journals. For each study in Table 2, I have indicated the study design, its potential strengths and weaknesses, the types of education programs that were included in the study, the sample sizes, and the effect sizes standardized as percent reduction in recidivism or percent increase in employment. Underlined effect

sizes indicate results where prison education was unexpectedly related to higher recidivism or lower employment outcomes. There are relatively few of these.

### The Level of Need for Education Programs – Prisoner Literacy

One of the predicates of correctional education is the level of unmet need. There have been different attempts to gauge the education and literacy levels of inmates compared to community populations. Harlow's Special Report for the Bureau of Justice Statistics (2003) tracked trends in the correctional populations from 1991 to 1997 based primarily on the inmate survey conducted by BJS. There have been two studies published by the National Center for Education Statistics, (NCES, 1994; Greenberg, Dunleavy, and Kutner, 2007) that measure the literacy levels of inmates as part of a national assessment of literacy throughout the United States. Literacy was defined for both of these surveys as "Using printed and written information to function in society, to achieve one's goals, and to develop one's knowledge and potential (Greenberg, Dunleavy, and Kutner, 2007, p. iii, Executive Summary)." Literacy was measured along three dimensions. Prose literacy is the ability to "search, comprehend, and use information from continuous texts." Examples of prose are editorials, brochures, instruction materials. Document literacy is the same set of skills applied to non-continuous texts. Examples of documents are job applications, transportation schedules, maps, tables, and food or drug labels. Quantitative literacy is the "...knowledge and skills needed to identify and perform computations using numbers that are embedded in printed materials." Balancing a checkbook and computing a tip are two examples of these tasks. These literacy dimensions are highly correlated (.78 to .87 for prisoners; Greenberg, Dunleavy, and Kutner, 2007, Table 1-1, p. 2).

The overall picture that emerges from the NALS and BJS surveys is that prisoners are an undereducated class compared to the community and have lower literacy skills to handle everyday tasks they may confront. Furthermore, both Harlow (2003) and Lynch and Sabol (2001) found that fewer inmates reported receiving educational or vocational programs in 1997 than in 1991; however,

the NALS 2007 report shows that a higher percentage of inmates in 2003 than in 1994 either had a GED or high school diploma when entering prison, or had completed the GED while in prison at the time of the interview. These data span different points in time, but it is clear that even if there has been an increase in educational attainment among inmates over time, there is still a great need for GED certification and post-secondary education.

### Meta-analyses on the Relationship between Correctional Education and Recidivism

There have been four meta-analyses of correctional education programs. I list these in Table 1 along with other reviews and summaries of the correctional education literature. The meta-analyses in Table 1 are listed with an “M” in the last column of Table 1. If the outcome of a group of studies was recidivism, an “R” appears in the fourth column of the table and the row is shaded in grey. An “E” appears for groups of studies with employment as the outcome. Table 1 lists the effect sizes as relative, as opposed to absolute, reductions in recidivism, or relative improvements in employment and wages. If 50 percent of a comparison group recidivated and 25 percent of participants in a correctional education program recidivated, then the relative reduction for the correctional education group would be 50 percent. These studies find very different average effect sizes for correctional education even when they cover essentially the same class of education training. For example, the Aos, Miller, and Drake (2006) average effect size for vocational training is a 9 percent reduction and the Wilson, Gallagher, and Mackenzie (2000) effect size is 22 percent reduction in recidivism. Effect sizes in Table 1 vary from 7 percent to almost 46 percent depending on the meta-analysis and the type of education program. Critics of this literature point to different definitions and varying risk periods when researchers measure recidivism. While this is true, it would be hard to devise some scheme that one could use to weight studies according to their definition of recidivism and length of the risk period.

One of the reasons for the discrepancies in effect sizes is that although there is overlap in the coverage of studies, there are differences as well. Unfortunately, several of these papers do not indicate

which studies they include in their research synthesis. The lowest average effect sizes are reported by Aos, Miller, and Drake (2006), but these authors screen studies for method quality and discount effect sizes if they are not randomized control trials. Chappell reports the largest average relative reductions for correctional education programs, 46 percent for post-secondary education programs. I briefly review each of these meta-analyses and cite the authors' conclusions if they state any.

Chappell (2004) used a meta-analysis to estimate the effect of post-secondary education (PSE) on recidivism. PSE training could include vocational, academic, undergraduate, graduate, certificate, and degree programs. The studies were published from 1990 to 1999. They could be quasi-experimental or correlational studies; however, there had to be a clear distinction between PSE training and other forms of education. Only 15 studies met the study selection criteria. Chappell noted that studies often lacked controls for selection bias. Effect size was measured as the correlation between PSE and recidivism. The sample weighted effect size was  $r = -.31$  across these studies. PSE participants recidivated 22 percent of the time. Non-participants recidivated 41 percent of the time. Of the three studies that used control group designs, the average effect size was lower,  $r = -.24$ . Chappell (2004) does not indicate in her bibliography which studies were included in her analysis, so it is difficult to assess the degree of coverage of her synthesis versus the other meta-analyses conducted on this subject.

Wells's (2000) dissertation was based on a meta-analysis of 124 correctional education studies that contained 329 effect sizes. He found an overall effect size of 38.4 percent relative reduction in recidivism for all types of education programs. He included studies of education involving juveniles as well as adults which partially explains why the number of studies in his analysis is much higher. Wells rated the methodological sophistication of the studies. He concluded that 23.4 percent could be considered strong, 40.4 percent were moderate, 19.5 percent were weak, and 6.4 had no scientific value. Wells did not provide an explanation for these methods categories. There was a very strong association between the method strength of a study and the effect size ( $r=.51$ ). The average effect size for strong studies was .917, moderate .623, weak .340, no scientific value .005. This is contrary to

most studies which find weaker effect sizes for studies with stronger designs. A test of homogeneity of effect sizes indicated effect size heterogeneity; however, the data were not re-analyzed with a random effects model and reweighted accordingly. Analysis of effect sizes by education program type did not indicate a great deal of variation in average effect sizes for most of the education program types, whether the program was literacy, GED, ABE, VT, higher education or multiple methods.

Aos, Miller, and Drake (2006) working for the Washington State Institute for Public Policy (WSIPP) include results of a meta-analysis of correctional education programs in their cost-benefit analyses of alternative policy options to constructing prison beds in Washington State. An annotated review of many of these same studies can be found in Phipps, Korinek, Aos, and Lieb (1999). Each study is assigned a score of scientific rigor used in “The Maryland Report” (Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway, 1997). The score ranges from 5, the highest to 1, the lowest. The lower average effect sizes found in Aos et. al., are due to the fact that Aos and his colleagues only choose studies with strong quasi-experimental and experimental designs and they discount the effect sizes of quasi-experimental studies by 25 percent for level 4 studies and 50 percent for level 3 studies. Further discounts are applied for short term studies and those designed and implemented by researchers. Aos et al., (2006) report an average effect size of 9 percent for VT training and 7 percent for basic education and post-secondary education. The cost-benefit analysis of these analysts is reviewed later in this paper.

Wilson, Gallagher, and Mackenzie (2000) conducted a meta-analysis of VT, education and work programs using as their effect size the odds ratio of the odds of a successful post-release period for “treatment” subjects relative to the odds of a successful spell of completion for comparison prisoners. An odds ratio of 1 would indicate the same odds of success for treatment and comparison subjects. They included 33 studies in their meta-analysis and these generated 53 treatment control comparisons. Seventeen of these comparisons involved VT training, 14 involved ABE/GED training, 13 were PSE training comparisons, and the remainder was based on work programs. Analyses were weighted by the inverse of the effect size variance under an assumption of random effects and included



the covariances among studies having a common comparison group in the weights matrix. Wilson et al., also used Sherman et al.,'s (1997) ranking of method quality distinguishing between a non-compromised random assignment study, a strong quasi-experimental design, a quasi-experimental design with poor controls, and a quasi-experimental design with clear lack of comparability. Of the 53 comparisons, only 6 (11.3%) were strong quasi-experimental or uncompromised experimental designs. This is less than half the percentage of strong designs reported by Wells (2000). Also contrary to Wells, Wilson et al., found that the two categories of studies that had stronger designs had lower average effect sizes than the two categories of weaker designs. Even though they calculated average effect sizes in the range of 18 percent to 34 percent, Wilson, Gallagher, and Mackenzie (2000) concluded that because most of the studies were of such poor methodological quality, they could not rule out the possibility that differences between the education and comparison groups were due to pre-existing differences in characteristics of the offenders that were related to successful post-release outcomes. Wilson et al., argue that stronger studies would show mediating effects between education programs and intermediate outcomes such as increased social bonds, increases in specific skills, and recidivism and employment.

There have been a few studies with strong quasi-experimental designs since these meta-analyses have been conducted that support the contention that correctional education can enhance post-release employment and reduce recidivism. Even Aos, Miller, and Drake's discounted effect sizes of 9 and 7 percent suggest the importance of correctional education.

### Vote Counting and Other Reviews of Correctional Education

There have been several reviews of the correctional education literature based on vote counting methods. Reviews that do not code a common effect size metric and statistically analyze those metrics are often referred to as 'vote counting.' The best of these appear in Table 1 indicated by "Review" in the last column. I distinguish between *reviews* which systematically assess the quality and findings of

each study, and *summaries*, which refer to a few studies and try to draw conclusions about the subject matter. Gerber and Fritsch, (1995) gave each study they reviewed a methodology rating from 0 to 3 with a point assigned if there was subject matching or random assignment, a point for statistical controls, and a point for significance tests. They summarized the education-post-release outcomes literature with the classic vote counting methodology. For pre-college education programs (ABE, GED) they concluded that there were many high quality studies having a methodology score of 3 that showed pre-college education programs reduced recidivism, and increased post-release employment. Their summary of in-prison VT programs also showed a number of high quality studies related to lower recidivism and gains in post-release employment. The results of the Gerber and Fritsch review are represented in Table 1 indicating the number of studies under each category of educational programming and type of outcome and the number of studies that showed a significant result in the expected direction. The Gerber and Fritsch review is one of the more systematic vote counting summaries of the literature. They conclude that there are numerous studies showing a relationship between prison educational training and post-release outcomes and that there are enough methodologically sound studies to make them "... confident that these positive findings are not statistical artifacts (p. 135)." Some of the studies which Gerber and Fritsch rate as high quality do not meet those standards when they are rated by either Wilson, Gallagher, and Mackenzie, or Aos, Miller, and Drake.

Cecil, Drapkin, Mackenzie, and Hickman (2000) used the Maryland Scale (Sherman et al., 1997) to rate ABE and life skills program. They computed effect sizes for each comparison in each of the studies and represented these and an extensive set of notes on each study. The researchers concluded that the lack of statistical tests, the sparseness of well-designed studies, and the presence of conflicting data mitigated against drawing any definitive conclusions about the effectiveness of these programs.

Jensen and Reed (2006) summarized the correctional education literature by combining an analysis of specific studies with the results of prior meta-analysis and other reviews. Their paper is

somewhere between a review and a summary. They group the studies into types of correctional education. In the category of ABE and GED programs they review a study by Boe (1998) conducted in Canada that included the ABE participants in the comparison sample and showed a relative reduction in recidivism of 8.3 percent. In addition to the Boe study, they cite the Cecil et al., review, the Wilson et al., (2000) meta-analysis, the Aos et al., (2001) meta-analysis, a study by Nuttal, Holmen and Staley (2003) of GED participants in New York State Department of Corrections adult facilities, and results from the “three states” study by Steurer and his colleagues (2001; 2006). Jensen and Reed conclude that “five of these six studies find that correctional education is effective in reducing recidivism (p. 88),” despite the fact that the Wilson et al. and the Cecil et al., reviews come to more cautious conclusions. The analysis of vocational training follows a similar pattern. They cite the review of the literature by Bouffard, MacKenzie, and Hickman, (2000), the Wilson et al., (2000) meta-analysis, the Aos et al., (2001) meta-analysis, and the Steurer et al., (2001; 2006) results. Treating the reviews and meta-analysis along with some unique studies means that the researchers were double or triple counting the same studies that appeared in all of these papers. The Bouffard et al., Wilson et al., and Aos et al., papers almost completely overlap in the studies that are reviewed. The treatment of post-secondary/ college programs is much the same.

Jancic (1998), Taylor (1992), Hrabowski and Robbi (2002), and Vacca (2004) conducted reviews of the literature, but did not synthesize their results using meta-analyses, nor did they use the vote counting method to tally those studies that showed an effect versus those that did not. These summaries are represented in Table 1 with a notation of “Summary” in the last column. If the researchers concluded that the evidence showed that education reduced recidivism, then “-R” appears in the second to last column of Table 1. If the researchers concluded that the education increased employment outcomes, then a “+E” appears in that column. Other papers such as Lewis (2006), Wade (2007), and Gehring (2000) draw attention to problems in measuring recidivism and the failure to distinguish between the types of education programs and the good measurement of those programs. Most of the reviews, especially those published in education journals, start with the Martinson (1974)

paper and the “Nothing Works” premise and then depending on the orientation of the researcher, he/she either finds diamonds in the rough or mostly rough.

## Cost-Benefit Analysis

The Taylor (1992) and Hrabowski and Robbi (2002) papers tried to demonstrate the potential cost benefit of correctional education; however, they do not use appropriate economic methods to handle marginal costs, present value considerations, and appropriate discounts. Aos, and his colleagues have developed the cost-benefit framework for economic assumptions appropriate for Washington State, and in a series of papers appearing on the Washington State Institute for Public Policy (WSIPP) web site, all of the details and assumptions including those involving the research syntheses have been published. Exhibit 4 in the Aos, Miller, and Drake (2006) report shows that vocational training and general education in prison produce some of the largest net economic benefits for adult programs. Even excluding the social benefits to crime victims accumulating from recidivism reductions, the marginal cost of VT programs is \$1,182 per prisoner and the marginal savings to the taxpayer from lower criminal justice costs is \$6,806. For general education the marginal costs were \$962 per person and the taxpayer savings were \$5,306. If you add victim savings the net benefit for VT programs was calculated as \$13,738 per inmate and for general education, \$10,669 per inmate. These are for reductions of 9 and 7 percent in recidivism for VT and general education programs respectively. These are large per person savings that depend on the quality of the underlying long term recidivism data, economic assumptions, and the veracity of the discounted effect sizes. The orientation of Aos and his colleagues is to choose conservative assumptions when they make their calculations.

Aos (2008) has argued that other state jurisdictions are likely to achieve greater net benefits from the same average effect sizes. The WSIPP cost-benefit model calculates taxpayer savings from future crimes that would be avoided because of the effectiveness of prison programs. These avoided crimes are categorized by type (violent, property, drug) and timing -- when they will be avoided in the

forecast horizon. The WSIPP analysts estimate marginal costs for each of the avoided crimes given an empirical analysis of the Washington state system. Although Washington State may or may not be a higher or lower unit marginal cost state than other states, the most important economic implication is dependent on how criminals use resources within the criminal justice system and how much of those resources they use. This most critical decision point involves sentencing decisions. For example, a convicted burglar has some probability that he/she will be sent to prison or jail, and if sent to prison or jail has some average sentence length for a crime of burglary. In the cost-benefit model, this calculation is made for every crime type that might be avoided as a result of an effective correctional education program. The key is that Washington State has lower incarceration rates than the average state (about 45 percent lower according to Aos). This means that the probability of going to prison is lower or the average length of stay is lower, or both in the State of Washington than most jurisdictions. As a result, an avoided crime in Washington (produced with some program like an education program) will generate fewer future criminal justice savings than they would in, some other state jurisdiction where the same avoided burglary would have saved more prison resources<sup>1</sup>.

There have been several new studies on correctional education that show promising results and which have not been included in prior meta-analyses. I review these along with the most methodologically sound studies that have been conducted to evaluate correctional education programs.

#### The Best Studies:

The “three state” study (Mitchell, 2002; Steurer, Smith and Tracy, 2001; Steurer and Smith, 2006) is a comprehensive assessment of prison education on post-release outcomes. The sample size is large (n=3,170). It includes measures of both post-release employment and recidivism. There is also a rich set of covariates, 500 variables overall, that were measured using a pre-release interview and administrative data. Sample selection was based on obtaining all releases during a specified time frame

from the state correctional institutions in Minnesota, Maryland, and Ohio. The release cohorts were divided into education participants and non-participants.

The Steurer, Smith, and Tracy (2001) report contrasts education participants and non-participants on a number of variables. The data on educational attainment indicates that prior to their current incarceration, 57.9 percent of the comparison group had achieved either high school or high school equivalency, had attained some level of college training, or had taken vocational training after high school. The percentage for the correctional education group was 37.8 percent. Even though the comparison group had attained higher levels of educational achievement prior to prison, their scores on the Test of Adult Basic Education (TABE) were equivalent to the study group and both had lower than 9<sup>th</sup> grade achievement levels.

The 2001 report also indicates that the education study group was a little younger, had higher proportions of whites and Hispanics, were more likely to be unemployed in the year prior to the current incarceration, less likely to hold a job for a year or more in their lifetime, more likely to have had family members in prison or jail, were more likely to be serving a sentence for violent or drug crimes, were younger at their first age of arrest, more likely to have served time in a juvenile facility, had fewer prior placements on probation or in prison, and were more likely to have a place to live upon release. Most of these comparisons suggest that the study group may have been at higher risk to recidivate than the comparison prisoners.

The 2001 report addresses selection bias by describing survey questions assessing motivation. The pre-release interview included questions assessing the motivation of participants and non-participants about preparing for a job, getting a job, receiving higher pay, improving work performance, getting better training, achieving skills to contribute to family or community, becoming less dependent on others, looking good to prison or parole officials, or achieving a better situation in prison. The only statistically significant difference between study and comparison prisoners was that non-participants were more motivated to feel better about themselves than education participants. While these questions directly address the motivation of prisoners just prior to their release, other

questions could have addressed selection issues prior to program training to sort out participant and non-participant motivation to take education courses. Given the logistical problems inherent in doing prison research, it is difficult to measure attitudes or dispositions that may change over time that may change as a result of correctional education, and that may mediate post-release outcomes. Steurer, Smith, and Tracy, (2001) and Steurer and Smith (2006) report substantial improvements in recidivism and employment outcomes as a result of correctional education in each of the three states; however, they do not use multivariate methods to control for the differences in characteristics among the education program and comparison groups. Nor do they use methods to control for potential selection artifacts. The unadjusted results are recorded in Table 2.

A follow-up analysis of the three-state study by Mitchell (2002) used a multivariate probit analysis using a binary recidivism variable and covariates that included age, race, gender, education level entering prison, longest job ever held, criminal friends, criminal family members, prior felony arrests, prior incarcerations, type current offense, motivation factor, and urban v. rural area prior to incarceration. The Maryland results were not significant in any of the multivariate analyses of arrest, conviction, or recommitment. However, the effect of education on post-release recidivism was demonstrated in Minnesota and Ohio. A propensity score was estimated with prior education level, motivation factor, prior prison terms, prior jail terms, and current offense type. Mitchell reported that there was very little common support – only 1,515 of the original 3,170 cases could be matched. Analysis of this limited matched data set produced the same results as the multivariate probit. Mitchell also conducted a bivariate probit analysis to model selection and participation simultaneously. Such analyses rely on exclusion restrictions and proper model specification. It was unclear whether either pre-condition was met in the bivariate probit analyses.

Harer (1992; 1995) analyzed a sample of 1987 prison releasees from the Federal Bureau of Prisons. In a multivariate discrete time hazard analysis, Harer found that participating in more than .5 education programs for each six months of time served reduced the hazard of re-arrest by 39% (coefficient = -.50). Harer then estimated propensity scores using a stepwise logistic regression among

all of the variables he had included in the multivariate analysis (29 variables) and used the 6 significant variables to compose the propensity scores for education participants and non-participants. The box plots of the propensity scores for the two groups seemed to indicate common support. Harer then re-estimated the multivariate discrete time hazard analysis using all of the variables in the original model except those that were used to compose the propensity score. He included the propensity score for each individual in his model. The coefficient for educational programming was still significant and the impact was a reduction of 38.5% in the hazard of re-arrest. Harer controlled for post-release employment and still found an effect unlike Batiuk, Moke and Rountree (1997) who did not find an effect of education on recidivism when controlling for employment. Harer's measure of employment was whether an offender was employed at the time of release. Harer also controlled for the number of years of schooling an inmate had achieved prior to his current incarceration, although this and other variables used in the propensity score analysis were excluded from the multivariate hazard analyses that used the propensity score. Because Harer used a cutoff of the rate at which inmates took a course (1 course per year) he may have selected the most ambitious inmates. The propensity score however, seemed to indicate balance between the group who took courses at a high rate and those that did not, or those who took no courses at all, but with so few variables to calculate the propensity score, one could argue he had omitted variables that may have been related to both educational programming and post-release outcomes.

Lattimore, Witte, and Baker (1990) conducted an experiment of an integrated set of services called the Vocational Delivery Services (VDS) that were implemented in two facilities in North Carolina for youthful property offenders. The services included vocational training, case management assessment, integration of other programs the inmate may have needed, re-entry training, and post-release employment services. The entire package of services could also include additional education training and alcohol abuse interventions depending on the specific inmate's needs. There was also a parole contract that set a specific parole release date for the offender if he completed his release plan.



This provided potential employers with precise employment availability dates for the released offenders, a large advantage to released inmates in this program.

Eligibility to participate in the study was restricted to youths (age 20) at the facility who committed income-producing offenses, who had a minimum IQ of 70, who were in good health, who were expected to be released within the state, and who had an expected stay at the facility from 8 months to three years. There were 591 subjects randomly assigned to the VT treatment, but only 247 had been released at the time of the study. If slots were available to train controls, they also received some of the training; however, community re-entry training and employment services were exclusively received by the treatment offenders. One of the strengths of this study was the tracking of services for both the control and experimental groups. This included some measures of program assessment such as grades on exams. The data showed that a large portion of experimental offenders did not receive all of the intended services and that some of the controls received services they were not intended to get. However, overall, the experimental offenders received more of the services. In general, the experimental offenders received only a subset of the services they were supposed to get and Lattimore et al., characterized this as a partially implemented treatment. On average, both the experimental and control groups were released for the same time periods. Thirty six percent of the treatment group and 46% of the control group were arrested within the follow-up period. This difference in proportions is significant at the  $p=.10$  level. Survival analysis showed that the hazard of arrest was about the same for the first 100 days after release, then was higher throughout the follow-up period for the control group, although the statistical test did not reach a conventional significance level ( $p=.12$ ). After 20 months there were very few arrests for either group.

Although this study has been included in meta-analyses of educational training, it is really a test of integrated services, and it is difficult to distinguish the impact correctional education may have had absent some of the other services youthful inmates received. Linden et al., (1984) also conducted an experiment, but the description of the follow-up periods is vague and it is not clear if they were

equivalent for the correctional education and comparison groups. Another experimental design was conducted by Ramsey (1988), but I have been unable to get the primary source for this study.

Piehl (1994) conducted a study of prisoners who completed ABE, GED, and VT program on the probability and hazard of returning to prison within 4 year of release. She was able to demonstrate a 9 percent reduction in the probability of a return based on program completion. Piehl used a two equation hazard model to test for potential selection effects. One equation predicted completion of education programs and the second equation modeled recidivism including the impact of completing education programs. Piehl also capitalized on a large set of covariates.

Tyler and Kling (2007) studied the effect of GED completion and participation on post-release legitimate wages among Florida adult inmates. One of the strengths of this study was the collection of legitimate wage data prior to, during, and after prison. They were able to use a fixed effects panel design that allows the analyst to control for potential selection artifacts associated with time stable characteristics. The advantage of such a model is that both measured and unmeasured time stable factors are controlled for in this design. However, as the researchers point out, time varying characteristics that might be related to education participation or achievement, and the outcome variables such as recidivism or wages could bias the results. The benefit of education could be overestimated if prisoners alter their attitudes over time that leads to engagement in inmate programs such as educational achievement. If inmate dispositions become more criminal during their incarceration and this is related to their education participation, then the fixed effects panel model would underestimate GED completion or participation.

Tyler and Kling argue that GED participation can affect labor market outcomes either by increasing the inmate's skills (human capital), or by a signaling effect if the prisoner earns a certificate indicating to a potential employer that he/she is more likely to be a better job candidate than those in the pool of non-credentialed applicants. They test each of these hypotheses by running two sets of regression analyses. In the first, they compare prisoners who earned a GED certificate in prison to GED-eligible inmates who did not participate in GED coursework. Average differences in these groups

indicate possible human capital returns. The certification hypothesis was tested by comparing prisoners who earned a GED certificate to GED participants who did not earn the certificate. Separate analyses were run for whites and minority offenders because of very different background characteristics in age and offense distributions that may be related to GED participation and labor market participation. The results that appear in Table 2 in this paper are broken down by these groupings. Minorities benefitted from GED participation, while whites did not. The racial differences in results found by Tyler and Kling and Saylor & Gaes (2001) suggest that future research should address this dependency. Tyler and Kling found only weak evidence of a GED signaling effect, and only for minority offenders. There was no impact of GED participation on recidivism.

The results of the Tyler and Kling analysis demonstrated that there was very little difference between the fixed effects panel model and a model that incorporated a rich set of covariates. The estimates of GED impact in Table 2 of this paper report the fixed effects panel results. Comparisons of naïve and more sophisticated models can be found in Tables 8.2 and 8.3 of the Tyler and Kling chapter.

Cho and Tyler (2008) used the same administrative data from Florida to examine the impact of adult basic education on post-release employment and wages. ABE participation had no impact on post-release wages, however, it was associated with an increase in post-release employment. The impact of ABE on employment was somewhat larger for whites and got larger over time. Since there was no wage effect, these findings suggest that while the probability of employment increases if inmates participate in ABE, the fact that there was no impact on average wages means that comparison subjects were getting higher average wages or working more hours than ABE participants. Cho and Tyler observed that there was a large variation in the number of hours of ABE participation. They examined different cutoffs of the number of hours of ABE on post-release wages and employment. Among their findings was the fact that taking at least 313 hours of ABE courses in one facility was associated with employment gain pre to post prison of 7 to 12 percent for whites and 4 to 5 percent for minorities in the three years after release from prison. Cho and Tyler also found small effects of these

uninterrupted hours of 313 hours or more of ABE instruction on recidivism. White inmates were less likely to recidivate by .5 percentage points and minorities were less likely to recidivate by 2.9 percentage points given the baseline recidivism rate of about 40 percent in three years.

Saylor and Gaes (1985; 1997; 2001) reported on the effect of vocational or apprenticeship training among a cohort of inmates released from the Federal Bureau of Prisons in the mid-1980's. To address selection artifacts, they used propensity score procedures to prospectively match each VT participant with one or more comparison subjects released in the same calendar quarter. The matching procedure followed recommendations by Rubin and Rosenbaum (Rubin, 1980; Rosenbaum and Rubin, 1984; 1985). A propensity score was developed on the 20 variables noted in Table 2 of this paper. Matches were developed by exactly matching race and sex from a large comparison pool and then using calipers of the propensity score limiting potential matches to only those people in the comparison reservoir who had a propensity score similar to the treated subjects. Calipers are based on the propensity score variance. Then the comparison subjects were chosen within these calipers based on the closest geometric distance (Mahalanobis distance) from the treated subject. If more than one comparison pool member had similar Mahalanobis distances, more than one comparison could be chosen. Saylor and Gaes had a large comparison reservoir to do their matching. For each calendar quarter a VT participant was released, every other released inmate in that quarter was available as a potential match.

For the first year of release, community supervision officers recorded arrests, employment status, and wages. These were recorded on a monthly basis. Saylor and Gaes have continued to follow this sample of about 1,700 offenders as well as offenders who participated in prison work programs beyond the one-year period by recording returns to prison for new convictions or technical violations. The impact of VT training was evident in the first 12 months and persisted for 20 years. While VT participants were more likely to work in the first 12 months after release, they earned the same amount of wages in the first 12 months as the comparison subjects who were employed. The effects of VT training were more pronounced for ethnic and racial minorities. Because the Saylor and Gaes study

used one-to-many matching from a large reservoir of potential matches, there was common support in propensity scores distributions. As in any propensity score approach, the study can be challenged based on the potential for omitted variables that could be related to the treatment selection process and the outcome. Sensitivity analysis (Rosenbaum, 2002) could bolster the conclusions of the study.

Visher and Kachnowski (2007) used data on inmates released from Illinois state prisons who returned to Chicago. The analysis reported in this paper were a subset of the data they collected primarily through interviews to develop a deeper understanding of reentry experiences. A self administered survey was given prior to release covering in-prison and pre-prison experiences. One-on-one interviews were conducted one to three and four to eight months after their release back into their Chicago communities. This study is part of a broader set of studies that compose the “Returning Home” project.

Visher and Kachnowski describe a great deal of data that was collected and used in the current study. In addition to the dependent variables indicating current employment and number of months employed, there were control variables – age, race, number of prior convictions, length of time served; pre-prison characteristics – high school graduate, married, number of minor children, worked before prison, illegal drug use, family relationship quality, lived in own house/apartment; in-prison history – property conviction, participated in job training, held work release job, satisfaction with police, spirituality, used medication for health reasons inside of prison, any visits from family last 6 months, self expressed need for job/education/financial help after release, doesn’t know where will be living after release, no close family; post release circumstances one to three months out – neighborhood disorder, any drug use/intoxication post-release, reported fair/poor health, depressed, family relationship quality, living with spouse /partner, living with anyone who is drunk often or using drugs, self-esteem, is tired of problems caused by own crimes, wants to get life straightened out, attitude towards parole officer, owes debt. Many of these measures are scales and are described in the study. The in-prison sample was 400. Of those, 264 offenders were interviewed one to three months after release and 205 were interviewed four to eight months after release. Data from the first two waves

were used to predict employment outcomes from the third wave at four to eight months. Exploratory analysis was used to examine the bivariate relationships between the inmate characteristics and the outcomes, then two multivariate analyses were conducted. In the first analysis, a logistic regression was used to model whether someone was currently employed four to eight months after release. In this analysis participation in job training was related to post-release employment. Nine percent of the sample had participated in prison job training programs; however, these programs were not described in any detail in the study. This variable was not related to the number of months worked after release. The odds ratios reported by Visher and Kachnowski for the prison job training effect ranged from 3.5 to 4.3 depending on the model specification indicating a 250 to 330 percent increase in the odds of being employed 4 to 8 months after release. The primary purpose of this research was to investigate those factors that contributed to gaining employment for offenders released back to their communities. This was not intended to be a study of prison educational training. In fact since only 9 percent of the sample participated in job training and the sample size at the time employment was assessed was only 205 individuals, at most there were only 19 inmates who had received job training, and there is no way of knowing how unique these 19 offenders may have been. Visher and Kachnowski also report in their discussion of these results that they analyzed the impact of obtaining a GED for this same sample and there was no impact on post-release employment four to eight months after release. I did not include the GED result in table 2 since no analysis was presented in Visher and Kachnowski's study.

Callan and Gardner (2007) examined the impact of VT on returns to Australian prisons. While they only used regression controls like Visher and Kachnowski, they had fewer control variables but a much larger sample (N=6,021). Callan and Gardner (2007) found a 28 percent reduction in recidivism for VT participants. The strongest regression studies use a rich set of covariates, have a large sample size, and have longitudinal data so that subjects in these studies can be compared to themselves over time. This is why the Tyler and Kling (2007) and Cho and Tyler (2008) studies are particularly insightful.

There is one well-designed study which has found results discordant with the majority of this literature. Sabol (2007a) examined the impact of the local labor market conditions on the post-prison employment experiences of 34,061 inmates released from the Ohio Department of Rehabilitation and Corrections during 1999 and 2000. Employment data were gathered from the state agency handling unemployment insurance claims. These data allowed Sabol to construct a pre- and post-prison dataset of legitimate employment measures providing him with the number of jobs and wages per quarter. Each inmate was followed for two years after their release from prison. While the primary purpose of this research was to examine county level employment conditions on post-release employment relative to pre-prison employment histories, the research includes individual offender attributes including educational training during the prison spell. As one would expect, there was a marginal impact of county level unemployment levels on post-prison employment. For each 1 percent increase in county level unemployment, there was a 2 percent decrease in the probability of an ex-offender's post-release employment; however, pre-prison employment had more of an impact on post-prison employment. Sabol also collected prison programming information including indicators of whether an inmate received a VT certificate or GED during this spell of prison. Only 3 percent of the release cohorts had earned a VT certificate and 7 percent had earned a GED (Sabol, 2007: Table 9.1, p. 276). Sabol also gathered individual measures of prior incarcerations, current offense type, the severity of the current offense, whether this release was from an admission to prison on a new crime, or a release after violation of a prior conditional release, the type of release supervision, prison length of stay, TABE education level at admission, race, and age at release. Sabol conducted two different analyses of the data. The first analysis was a discrete hazard model of quarters until first employed. While there was no impact of obtaining a GED on post-release employment, inmates who had earned a VT certificate were 1.6 percent less likely to become employed. Sabol's other fixed effects analysis of wages and employment over time also produces the perverse effect of VT training on post-release wages and employment.

Because of the perverse nature of these findings, Sabol (2007b) conducted yet another analysis. In that one, he generated propensity scores from a model predicting VT course completions using some of the prison-related covariates he had used in the prior employment analyses. He then compared the VT and matched groups on post-release employment outcomes. Using another discrete time hazard model, Sabol examined the interaction between VT certification and pre-employment history, VT certification by itself, and VT certification in interaction with the propensity score strata on the “hazard” of post-release employment. He found that if someone had been employed prior to prison, VT certification improved the likelihood of employment after release. Neither the VT certification by itself, nor the VT certification in interaction with the propensity score strata had an impact. Sabol offered several explanations of these findings arguing that there may have been a mismatch between the trades offered in prison, VT courses, and post-release job opportunities, or that trained offenders were looking for jobs related to their training and not the jobs available in the community. Sabol acknowledges that for the most part his findings run contrary to other published data on VT training, but that his GED findings are consistent with Tyler and Kling’s (2007) results notwithstanding the fact that Tyler and Kling find GED effects for minorities. Since Sabol (2007a, 2007b) did not test for an interaction between race and VT or GED training, or evaluate minorities separately as Tyler and Kling have done, there is no knowing what such an analysis would have produced. Secondly, Sabol did not have available as rich a set of covariates as some other analysts to develop the propensity scores and subsequent matching by stratification.

The experimental studies in this section would have received a rating of 5 on the Sherman et al., Maryland scale and the remaining studies would have received a 4 or 3 depending on their features. Controlling for selection artifact is difficult, and even among the strong quasi-experimental studies, some tell more convincing stories than others.

#### Methodologically Compromised Studies



Most of the studies listed in Table 2 use comparison pools of subjects who are not equivalent to program participants on many dimensions. There is no attempt to address selection artifacts, although the authors often recognize the problem. There are few if any covariates used in a regression analysis to adjust for pre-existing differences related to both program participation and the outcomes. Some studies compare inmates who complete a program to those who drop out potentially amplifying selection artifact results. Knepper (1990) compared college participants to VT and other correctional educational participants arguing that all program participants should be equally motivated and this should minimize selection artifacts. Even if this were true, it does not address the ability bias that distinguished between those capable of taking college courses and those that do not.

Some of these studies address important issues such as the Batiuk, Moke and Rountree (1997) study which was explicitly designed to test whether employment may mediate recidivism in a test of the effects of educational training. The treatment group was composed of 95 inmates who had achieved an associate degree while in prison. The control group consisted of 223 randomly selected prisoners at the same institution that had not received an associate degree. Using a limited set of covariates, the authors conducted logistic regression analyses using recidivism and employment as outcomes and a model that used recidivism as the outcome controlling for employment. These analyses demonstrated a mediating effect of employment on recidivism; however, there was no analysis to control for selection artifacts.

The details of each of these studies are listed in Table 2 along with comments on the strengths and weaknesses of the studies. If I had used the Maryland scale to rate these studies, a few would receive a rating of 3, but most would be 2 or lower.

## Summary and Discussion

Correctional education improves the chances that inmates who are released from prison will not return. Among minorities correctional education increases the wages they will earn when they return to

the community. While most studies use recommitment as their definition of recidivism, the implication of the research is that educated prisoners are less likely to commit crime once they are released to the community. While most scholars agree with this conclusion, there is much less unanimity among scholars on the size of the effect – what meta-analysts call the effect size. The most critical among these scholars argue that the majority of studies may be biased because of selection – inmates who take correctional education courses have attributes that would increase their post-release success regardless of their course participation. The best way to avoid selection bias is to conduct research using randomized controlled trials (RCT's). For example, in many medical applications, systematic reviews are limited to randomized control trials and further limited to RCT's that are based on an intent-to-treat analysis (Sherman, 2003). Unfortunately, RCT's are uncommon in this domain. As a point of comparison, Petrosino (2003) found that "...randomized studies are used in nearly 70 percent of childhood interventions in health care but probably in 6 to 15 percent of kindergarten through twelfth-grade interventions in education and juvenile justice (Petrosino, 2003: 190)." So far, there have been only a few studies using random allocation of subjects to correctional education. However, the strongest of these (Lattimore, Witte, and Baker, 1990) is really a test of integrated services that includes education as one of the services.

Of the systematic reviews I have discussed in this paper, the Aos et al., (2006) study is the most conservative in its choice of well designed studies, and in its calculation of the average effect size. Unlike other analysts who conduct systematic reviews, Aos et. al., are willing to discount the effect sizes of the studies they include in their meta-analysis based on the strength of the evaluation methods and on the extent to which the evaluation was conducted by a "neutral" independent evaluator. Aos and his colleagues work for the Washington State legislature. Their research is designed to assist legislators in making informed choices about funding criminal justice resources and making criminal justice policy decisions. More academically inclined scholars have conducted systematic assessments of correctional education where the main focus has been to critique study quality and summarize the literature. This difference in emphasis partly explains the difference in the average effect sizes reported

in these systematic reviews. The average effect sizes among different scholars also vary depending on which studies reviewers find in their literature search, and which criteria reviewers use in making their decision to include/exclude a study in their research synthesis. The Maryland scale has become a common tool for establishing the threshold of whether a study should be included in a meta-analysis; however, there is no agreement on how good the study's methods have to be to include or exclude a study.

High quality quasi-experimental studies have also shown promising results. I reviewed the best of these primarily to demonstrate that even in the absence of RCT's, one can conduct a strong quasi-experimental study. Nearly all of these studies suggest that correctional education has a modest salutary impact on post-release wages and recidivism. If there is a policy message other than the one that encourages more resources for correctional education, it is the clarion call for more strong quasi-experimental studies and RCT's in this domain. One's interpretation of cost-benefit also hinges on the "true" effect sizes. The WSIPP meta-analysis is the most conservative in its set of assumptions, yet still produces large net cost benefits. A reasonable critic could also conclude that even the discounting in the WSIPP analysis does not go far enough and argue that the net benefits are smaller than projected. Because the meta-analysis results depend on the selection of studies that further depend on whether you impose no method restrictions or very severe method restrictions, the weighted average effect size can vary considerably. If the WSIPP *economic assumptions* are valid, even small effect sizes, can produce meaningful net benefits. Economists distinguish between average and marginal costs. In this context, the marginal savings refers to the reduction in cost for each offender that does not "re-enter" the criminal justice system. Even from a taxpayer's perspective (meaning that we exclude the victim's costs and other social costs), the marginal costs of education pale in comparison to the marginal savings in criminal justice expenses associates with arrest, conviction, and confinement.

Aside from the actual impact of correctional education measured by its effect size, it is important to realize the scope and breadth of its impact<sup>2</sup>. Unlike many correctional interventions

which may only involve a small proportion of the confined population, some form of correctional education can impact almost every offender. Offenders with a grade school education or minimum literacy skills can benefit from adult basic education, GED preparation, and vocational training. Those with a GED can benefit from post-secondary training whether academic or vocational. Even college graduates may benefit from further education and specialized certification. The accumulation of small effects on a large population can have a much greater impact than the accumulation of large effects on a small population. Modest effect sizes, such as those found in the strong quasi-experimental studies and the discounted average effect sizes of the WSIPP analysis are still in the range of reducing recidivism 7 to 9 percent. Furthermore, correctional education appears to increase post-release wages for minorities in the range of 10 to 15 percent. When you put this in perspective, and couple these effect sizes with a significant number of offenders who are released each year who could benefit from correctional education, this elevates correctional education to one of the most productive and important reentry services.

As strong inference (RCT's and high quality quasi-experimental designs) studies accumulate, perhaps even the most skeptical person will be convinced of the impact of correctional education. There is always opportunity for random assignment when the pool of eligible prisoners is much larger than the training slots available. Future research in this domain could be more informative if researchers would follow the lead of the best designed studies. Short of randomized controlled designs, analysts employing quasi-experimental designs should attend to fundamental design and measurement prerequisites. Comparison reservoirs should be based on the education program being studied. If one is conducting an analysis of GED certification, the comparison pool should consist of inmates who have either participated in a GED program or require a GED but have more than just a basic education. Too many studies in this domain used comparison pools composed of prisoners who had different levels of education, certification, and training. The comparison pools in these studies are a mixture of college graduates, inmates with less than 8<sup>th</sup> grade education, and all of the intermediate levels of achievement.

Analysts must also insure equivalent skill and literacy levels. This can be part of the comparison pool selection process, or controlled through measurement and regression.

While education research in prison poses certain barriers that may not exist in the community, the nature of analyzing these effects is very much the same. We must be mindful of the fact that there is also controversy about the evidence for how much education achievement produces returns for people in the community. How much of what appears to be a return is actually an ability bias (Becker, 1993; Card, 1999; Willis and Rosen, 1979)? Furthermore, as of yet, there are not enough high quality studies to indicate which types of correctional education provide the highest post-release returns. There are no high quality studies of college coursework. Even though the average effect sizes for VT and GED training seem to be about the same within meta-analyses, they are quite different across meta-analyses. To sort this out we need a study registry that at a minimum, lists each study, the definition of the effect size, and the effect size itself. Many of the systematic reviews in this domain did not calculate effect sizes. Some did not list the studies they reviewed. Among the meta-analyses, there was inconsistency in whether the effect size was listed.

Analysts should also consider the best design for their particular study to address selection artifacts. For example, if the study lends itself to panels, then some combination of fixed effects panel models and propensity score matching might be used. Regression discontinuity designs, propensity score matching, instrumental variables, and Heckman methods are also possibilities. Each of these methods has strengths and weaknesses and raises concerns about defensible assumptions (Morgan and Winship, 2007). Studies such as Sabol (2007a; 2007b), Tyler and Kling (2007) Steurer et al., (2001; 2006), Piehl (1994), and Harer (1995) capitalized on a rich set of covariates. Regression results that control for a rich set of covariates give us greater confidence in their results. Critics of these studies who claim omitted variables are the true source of the effects on recidivism will have to make their case about what those omitted variables might be. Some designs lend themselves to sensitivity analysis. The researcher demonstrates what the magnitude of the relationship between the omitted variables, treatment, and outcomes would have to be in order to challenge the conclusion that the

treatment variable of interest was the actual cause (Rosenbaum, 2002). The larger the magnitude that the omitted variable would have to be to cause the treatment outcome, the more confident we can be that the true cause is the treatment itself.

Very few studies look at the relationship between correctional education and both employment and recidivism outcomes in the same study. Saylor and Gaes (1985; 1997; 2001), Harer (1995), Batiuk, Moke, and Roundtree (1997), and Steurer and his colleagues (2001; 2006) are exceptions; however, panel or event history approaches might solidify our understanding of the relationship among these outcomes. There is also potential for measurement improvements. However, prior to such measurement, we need sound theory to inform our understanding of variables that may moderate the effect of correctional education and variables that mediate that relationship. Intermediate outcomes might include changes in motivation before and after education courses, the level of skill acquired pre to post education, and commitment to conventional social bonds. To collect some of these measures the research may have to rely on inmate interviews or surveys, a departure from most of the studies in this domain. There is no study that measures the level of achievement either with continuous or ordinal scales. In a society where SAT scores, GRE levels, and grades are measured with such precision, these studies typically categorize educational training categorically as program participation or completion.

Separating certification from achievement gains will also require innovative designs. Students who participate in a GED programs and do not receive certification may be different from those who earn GED certificates along more than one dimension. GED certified inmates may have achieved a higher level of learning, or they may have been more committed to achievement. Either of these dimensions could produce greater post-release returns. The field needs a design that insures equivalent levels of motivation and achievement and varies certification. This logic is similar to the reasoning in Tyler and Kling when they were persuaded by Florida Department Corrections officials that a regression discontinuity design would exaggerate GED credentialing since inmates are discouraged from taking the GED exam until they had done well on the practice exams.

If there are limitations to the potential impact of correctional education on reentry success, it may be because other offender needs may also have to be addressed. Inmates who are drug dependent, who lack work skills, or who have mental health problems require additional resources and education effects may be muted by these other unmet needs. However, as Bushway, Stoll, and Weiman (2007) point out, even weak effects of educational programming for many of these offenders should foster some optimism given the weak employment histories and criminal records these inmates carry as burdens into the job market when they reenter the community. Education may be fundamental to other correctional goals. It may be a prerequisite to the success of many of the other kinds of prison rehabilitation programs. The more literate the inmate, the more he or she may benefit from all other forms of training. Thus, the link between correctional education and successful post-release outcomes may have many paths which analysts do not consider when they evaluate education programs independent of its other influences.

## Endnotes

1. My thanks to Steve Aos for raising and explaining this implication of the Washington State WSIPP cost-benefit model.
2. In a discussion with Akiva Liberman on these issues, he provided me with this insight.



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Table 1. Meta-analyses, reviews, and summaries of the effect of correctional education on post-release outcomes.

Study	Type of Education Training	N	INC/DEC Recid (R) Employment(E) Education (ED)	Review, Meta- analysis, (M) or, Summary
Aos, Miller, and Drake 2006	Vocational Training	4	-9% R	<b>M</b>
	Basic or PSE	17	-7% R	<b>M</b>
Cecil et al., 2000	ABE	12	Inconclusive	Review
	Life Skills	5	Inconclusive	Review
Chappell, 2004	PSE	15	-46.3% R	<b>M</b>
	PSE – Studies with Control Groups	3	-40% R	<b>M</b>
Gerber and Fritsch, 1995 <sup>c</sup>	Pre-college	13	9 Show - REL R	Review
	Pre-college	4	3 Show + REL E	Review
	Pre-college	2	2 Show + REL ED	Review
	College	14	10 Show – REL R	Review
	College	3	3 Show + REL E	Review
	Vocational Training	13	10 Show – REL R	Review
	Vocational Training	7	5 Show + REL E	Review
Hrabowski and Robbi, 2002	All Types of Education	5	-R, Cost effective	Summary
Jancic, 1998	All types of Education	7	-R	Summary
	All types of Education	7	+E	Summary
Jensen and Reed, 2006	ABE/GED		-R	Summary
	VT		-R	Summary
	College		-R	Summary
Taylor, 1992	PSE		-R, +E, Cost Effectiveness	Summary
Wells, 2000 <sup>a</sup>	All types of Education	329	-38.4% R	<b>M</b>
Wilson, Gallagher, and Mackenzie, 2000 <sup>b</sup>	Vocational Training	17	-22% R	<b>M</b>
	ABE/GED	14	-18% R	<b>M</b>
	PSE	13	-26% R	<b>M</b>
	Education	4	+26% E	<b>M</b>
	VT	8	+34% E	<b>M</b>
Vacca, 2004	All types of Education	7	-R	Summary

- a. This relative reduction was based on converting the average weighted effect size into a BSED (binomial effect size display) The un-weighted reduction was 29%.
- b. Wilson et al., reported percentage differences between study and comparison group (see their table 2) by assuming a 50 percent recidivism percentage in recidivism among the comparison group and then deriving the percentage reduction for the study group based on the odds ratios comparing study and comparison group participants. These percentages were as follows: VT, 39%; ABE/GED, 41%; PSE, 37%; For each the comparison group was assumed to have a 50% recidivism percentage. In table 6 they report odds ratio effect sizes on employment outcomes. I converted those to the percentage differences also assuming a 50% percentage for comparison subjects. For the education group the percentage employment was 63% and 67% respectively for the VT group of studies. Then I converted all of those percentage comparisons to percent decreases in recidivism or percent increases in employment. Wilson et al., 1999 covers essentially the same studies and reports on many of the same measures as the Wilson et al., 2000 study. The Wilson et al., 2000 study was more comprehensive and considered employment outcomes in addition to recidivism.

- c. The report by Flanagan et al., 1994 contains chapter 1 which is redundant with the Gerber and Fritsch review and chapter 3 which is redundant with the Adams et al., 1994 analysis of the effect of education training in the Texas prison system on post-release recidivism. Chapter 2 of this report demonstrates the impact capacity constraints within the Texas prison system on educational program delivery.



Table 2. Studies of Correctional Education

Study	Outcome	Type of Education Program	Effect	N
Adams, Bennett, Flanagan, Marquart, Cuvelier, Fritsch, Gerber, Longmire, and Burton, 1994 Compared inmates who participated in Academic, VT and both Academic and VT to a large cohort of non-participants -- The more hours of participation the lower the recidivism percentage – This dosage effect was confined to those inmates with the lowest initial grade levels ( No attempt to control for background characteristics or selection artifacts)	Recidivism <sup>22</sup>	Academic VT Both Academic and VT	Academic: NS VT: NS Academic and VT: NS	14,411
Allen, 1988** Two grp comparison Study Grp VT training: Cmp grp Non-VT releasees	Recidivism <sup>14</sup>	VT	67.5% Rel Dec	
Anderson, 1995 Compared the five groups indicated in the Type Education Program column to releasees having comparable TABE scores, highest grade achieved at admission, and involvement in educational programming during incarceration. Study groups with higher levels of education programming had comparison groups with higher levels of educational achievement – demographic and criminal history variables are used one at a time to contrast study groups to comparison subjects for a specific characteristic such as gender, but there was no multivariate analysis – limited matching to control for selection artifacts	Recidivism <sup>26</sup>	ABE (n=1,060) VT Certificate(n=630) GED Certificate (n=1,303) College (n=976) Any of the Above (n=3,969)	5.5% Rel Inc 3.9% Rel Dec 25.4% Rel Dec 12.4% Rel Dec 3% Rel Dec	18,068
Anderson, 1982 Compared VT/Academic participants to nonparticipants – no controls; unclear if the risk periods were the same for the TX and CMP	Recidivism <sup>27</sup>	VT/Academic	Rel Dec in Arrests (Pct. not reported)	238
Anderson, 1982 Interviewed 25 employers of releasees who had taken VT courses and estimated the relationship between number of hours of VT and amount of employment – no comparison group	Employment <sup>28</sup>	VT/Academic	No Hrs and Quality of VT Inc Employment	25
Batiuk, Lahm, McKeever, Wilcox, and Wilcox 2005 Samples of releasees who had participated in GED, High School, VT, and College prison programs and a comparison group of non-participants (Hazard rate analysis)	Recidivism <sup>21</sup>	GED, High School, VT, College	College: 62% red; GED16% red; VT: 19% red High School 2% red	972
Batiuk, Moke, and Rountree 1997 Mult. Logistic reg excluding post-release employment	Recidivism <sup>5</sup>	Assoc <sup>4</sup>	68% Dec in Prob	318
Batiuk, Moke, and Rountree 1997 Mult. Logistic reg excluding post-release employment	Employment <sup>6</sup>	Assoc <sup>4</sup>	168% Inc in Prob	318

Study	Outcome	Type of Education Program	Effect	N
Batiuk, Moke, and Rountree 1997 Mult. Logistic reg including post-release employment Indicates Post-release employment mediates recidivism	Recidivism <sup>5</sup>	Assoc <sup>4</sup>	43.3% Dec in Prob N.S.	318
Blackburn, 1979, 1981 Compared 189 participants who had completed at least 12 hours of an Associates Degree program to a matched group of 189 prisoners based on mechanical matches of age, race, month of release, Law Encounter Severity Scale score (risk assessment), Environmental Severity Scale score (combination of needs and risk factors), and the Maladaptive Behavior Record score (employment, addiction, adjustment). Also did a multivariate analysis of a transformed proportion of the amount of time in the community. The time at risk periods varied from 1 to 8 years. Because the TX and CMP subjects were matched on release date, their overall potential times at risk were comparable.	Recidivism <sup>39</sup>	Associates Degree Participation	36.4% Rel Dec	378
Burke & Vivian, 2001 Mechanical match on age, education level, sentence length, sex, ethnicity	Recidivism <sup>8</sup>	Completes College Course <sup>7</sup>	31.9% Rel Dec	64
Callan and Gardner, 2007 Compared Australian prisoners who participated in VT programs to a comparison group of non-participants. Used logistic regression to test the VT participation effect along with age, sex, indigenous status, most serious offense grouping, sentence length grouping, education grouping, risk score, literacy/numeracy prior to release, and post-release employment assistance.	Recidivism <sup>61</sup>	VT	28.1% Rel Dec	6,021
Cho and Tyler, 2008 See Tyler and Kling for a description of the data and analysis approaches.	Wages <sup>58</sup>	1 Facility and >= 313 ABE Hours	N.S.	6,871
Cho and Tyler, 2008	Employment <sup>59</sup>	1 Facility and >= 313 ABE Hours	7 to 12 % Rel Inc for Whites 4 to 5 % Rel Inc for Minorities	6,871
Cho and Tyler, 2008 See Tyler and Kling for a description of the data and analysis approaches.	Recidivism <sup>60</sup>	ABE	N.S.	13,925
Clark, 1991 Two grp comparison: Study Grp: Earned a college diploma; CMP withdrew from the degree program or were administratively removed. No covariates, no control for selection artifacts. Possible selection artifacts enhanced by the study design.	Recidivism <sup>13</sup>	College	40.2% Rel Dec	986
Davis and Chown, 1986 Compared prisoners who had completed VT training to those who never received VT training. No covariates and no controls for selection artifacts. The researchers examined survival curves through 54 months. The 3-year results are reported here.	Recidivism <sup>29</sup>	VT	<u>6.6% Rel Inc</u>	12,223
Duguid, Hawkey and Knights, 1998	Recidivism <sup>17</sup>	Post Secondary Education	40.5% Rel Dec	654

Study	Outcome	Type of Education Program	Effect	N
Compared inmates who completed 2 or more PSE programs with their risk based expected recidivism outcomes				
Gaither, 1980 Compared prisoners who completed at least 29 hours of the Texas Department of Corrections junior college program (n=26) to those that participated for less than 30 hours (n=324) and those who never participated (n=360). Prisoners in both control groups had a high school education or a GED certificate. Measured age, ethnicity, race, sex, marital status, offense category, grade point average, sentence length, and IQ but no multivariate test with the TX effect.	Recidivism <sup>38</sup>	Junior College Participants	56.8% Rel Dec	710
Harer, 1995 Multivariate discrete time and accelerated failure time models	Recidivism <sup>20</sup>	Educ Pgm <sup>1</sup>	39%	619 <sup>2</sup>
Harer, 1995 Propensity score analysis	Recidivism <sup>20</sup>	Educ Pgm <sup>1</sup>	38.5%	619 <sup>2</sup>
Holloway and Moke, 1986** Two grp comparison: Study GRP 1: Associates degree completers, Comp Grp: No High school or GED certificate	Recidivism <sup>11</sup>	AA	60% Rel Dec (NS)	317
Holloway and Moke, 1986 Two grp comparison Study Grp 2 High School Completers, Comp Grp: No High school or GED certificate	Recidivism <sup>11</sup>	GED	46.5% Rel Dec	317
Holloway and Moke, 1986 Two grp comparison Study GRP 1: Associates degree completers, Grp 2 High School Completers, Comp Grp: No High school or GED certificate	Employment <sup>12</sup>	AA	68.5% Rel Inc (NS)	317
Holloway and Moke, 1986 Two grp comparison Study GRP 1: Associates degree completers, Grp 2 High School Completers, Comp Grp: No High school or GED certificate	Employment <sup>12</sup>	GED	51.2% Rel Inc	317
Hull et al., 1995 Compared Academic completers, VT completers, VT non-completers, Academic non-completers to a sample of inmates who had never been enrolled in an education course. It is not clear how time at risk was controlled, the potential range was 1 day to 15 years. No covariates, no controls for selection artifacts. There was also an analysis of employment but only for person still on parole at the time of the study. The study indicates that persons who completed educational programming had higher employment rates, followed by non-completers followed by un-enrolled; however,	Recidivism <sup>32</sup>	Academic Completers VT Completers Academic Non-Completers VT Non-Completers	61.1% Rel Dec 56.7% Rel Dec 22.2% Rel Dec 24.1% Rel Dec	3,000
Hull et al., 1995 There was also an analysis of employment but only for person still on parole at the time of the study.	Emploment <sup>33</sup>	VT or Academic Completers VT or Academic NC	42.7% Rel Inc 12.5% Rel Inc	347

Study	Outcome	Type of Education Program	Effect	N
The risk periods of these people is even less clear and there is no control for the conditional nature of those still remaining on parole.				
Jenkins, Steurer, and Pendry, 1995 Tracked inmates completing a program; no comparison group	Employment <sup>15</sup>	ABE, GED, VT or College	The higher the level of education the higher proportion of parolees who worked	120
Kelso, 2000 reports on two studies published in The Journal of the Northwest Center for the Study of Correctional Education Study1: Haynes, 1996 (N=147 Pgm Graduates) Compared graduates of a Community College program and VT to Statewide Recidivism rates – no covariates, no controls for selection artifacts	Recidivism <sup>34</sup>	Associates Degree VT Certificate	71.6% Rel Dec 52.1% Rel Dec	7,949
Kelso, 2000 reports on two studies published in The Journal of the Northwest Center for the Study of Correctional Education Study2: Kelso, 1996 (N=93 Pgm Graduates) Compared graduates of high school, VT, and community college to Statewide Recidivism rates – no covariates, no controls for selection artifacts	Recidivism <sup>35</sup>	High School VT Community College	36.6% Rel Dec 61.5% Rel Dec 63.1% Rel Dec	8,003
Knepper, 1990 Compared participants to participants in VT, secondary and elementary programs. Knepper argues this controls for selection artifacts that may be related to motivation, since all program participants could be considered to be equally motivated. Analysis of recidivism had no other control variables.	Recidivism <sup>30</sup>	College	<u>College v. VT: 63% Rel Inc</u> <u>College v. Secondary: 32.7% Rel Inc</u> <u>College v. Elementary: 84.7% Rel Inc</u>	526
Knepper, 1990 Compared participants to participants in VT, secondary, and elementary programs. Knepper argues this controls for selection artifacts that may be related to motivation, since all program participants could be considered to be equally motivated. Analysis of adjustment controlled for age, race, gender, number prior felony convictions, any prior incarceration, and current offense level.	Adjustment <sup>31</sup>	College	College v. VT: 13.7% Rel Inc College v. Secondary: 20.3% Rel Inc College v. Elementary: 31.2% Rel Inc	526
Lattimore, Witte, and Baker, 1990 Experimental study with strong monitoring of the program elements for both experimental and control subjects	Recidivism <sup>24</sup>	VT (with a Case Management Plan, Employment, and Pre release Services)	21.7% Rel Dec	247
Lichtenberger, Eric J. (2007) Used release cohorts in years 1999, 2000, 2001, 2002. Followed releasees until the end of fiscal year 2005. Matched vocational completers to those that did not participate in vocational programming, but matches could have participated in other correctional education programming. Used a propensity score to choose a “nearest neighbor” match without replacement. The matching variables were marital status, offense type, custody code, race, gender, highest grade level completed, time served, age at	Recidivism <sup>52</sup>	VT Completers	25.7% Rel Dec	3,267

Study	Outcome	Type of Education Program	Effect	N
<p>release, major infractions, minor infractions, and release quarter. No information was provided on common support, or matching success as a result of the matching procedure. Report acknowledges that other exogenous variables could produce the differences shown in the report. No attempt to find matches based on other education program participation besides VT training. No attempt to compare VT participants who did not complete as if it were an intent-to-treat design. An individual could be a member of different release cohorts. Individuals were also included in the study even if they recidivated on the current offense and were re-released. The average three-year recidivism differences across the four cohorts are reported in this table. There is no inferential statistical analysis, but the sample sizes are clearly large enough that the reported differences are statistically significant.</p>				
<p>Lichtenberger, Eric J. (2007) Aggregate earnings are reported without adjusting for recidivism. The results could merely reflect the fact that more VT completers remained in the community rather than any difference in average wages. Employment percentages are also not adjusted for those remaining in the community – The employment calculations use all releasees as the base rather than those who have not been returned to prison.</p>	Employment/ Wages <sup>53</sup>	VT Completers	Employment 8.3% Rel Inc Wages 31.1% Rel Inc	3,267
<p>Linden et al., 1984 Experimental study: Inmates randomly assigned to participate in education programs from a list of “eligible” volunteers. The study was conducted in a maximum and a medium prison in Canada. Tested a number of covariates to see if there were differences in background characteristics of the experimental and control groups. Covariates included demographics, prior criminal activity, institutional adjustment, educational and occupational backgrounds, criminality, post-release expectations, measures of social support, and attitudes toward inmates and the institutions, Few differences among the experimental and control groups. While the recidivism percentages were lower for college course participants in both prisons, they differences did not reach statistical significance at P=.05 convention.</p>	Recidivism <sup>42</sup>	College Courses in English, History, Psychology and Sociology Maximum Security Prisons Medium Security Prison	NS	56
<p>New York State Department of Correctional Services, 1989 Compared those who completed a GED during incarceration to those who were eligible but did not enroll or did not complete a GED (Released in 1986-'87; 17 to 42 month follow-up, no covariates)</p>	Recidivism <sup>23</sup>	GED	13.1% Rel Dec	15,520
<p>Luftig, 1978 Compared VT participants to a comparison group in an adult and youth facility. Covariates were measured and there were no differences between the VT and comparison groups on age at parole, highest grade completed, previous number of incarcerations, and age at first arrest, previous occupation, previous juvenile history, race, IQ, and offense. Only the adult facility results are shown</p>	Employment <sup>43</sup>	VT	95.9% Rel Inc	146

Study	Outcome	Type of Education Program	Effect	N
here.				
Luftig, 1978 Compared VT participants to a comparison group in an adult and youth facility. Covariates were measured and there were no differences between the VT and comparison groups on age at parole, highest grade completed, previous number of incarcerations, and age at first arrest, previous occupation, previous juvenile history, race, IQ, and offense. Only the adult facility results are shown here.	Recidivism <sup>44</sup>	VT	35.2% Rel Dec	146
O'Neil, 1990 Compared prisoners participating in PSE to those who were eligible to participate, i.e. had a High School diploma a GED certificate, or had taken college courses -- (No other covariates or control for section artifacts)	Recidivism <sup>25</sup>	PSE	66.1% Rel Dec	258
Piehl, 1994 Used accelerated failure time model of time to return to prison. Used dummy variables to test program eligibility status and program completion status for education (ABE, GED) and VT program completions along with race, tested grade level, high school graduate, age at admission, age squared, prior prison term, sentence in months, serving an old sentence, security level indicators, offense type indicators, and race * completed interaction. Also used probit with the same variables to evaluate the probability of recidivism – Probit results represented here. To address selection bias, she conducted a “flexible hazards” analysis using two equations, an education completion equation and an equation testing the effect of competing education on the hazard of re-arrest. This model also allows for unobserved but random variables in each equation that can be correlated across the two equations. This secondary analysis produces the same effects as the accelerated failure time model and is interpreted by Piehl to indicate that possible selection mechanisms are accounted for by the administrative variables used in the analysis.	Recidivism <sup>37</sup>	Eligible for Education PGM Completed ABE or GED Completed ABE, GED or VT Participated (Not Completed)	NS 9% Dec 9% Dec NS	1,473
Porporino and Robinson, 1992 Compared Program completers to non-completers and withdrawals	Recidivism <sup>16</sup>	ABE	15.7% Rel Dec C v. NC 27.6 Rel Dec C v D	1,736
Ramsey, 1988** Experimental design compared GED participants and completers to a control group	Recidivism <sup>40</sup>	GED	NS	200
Robinson, 2000 Project Horizon inmates receive integrated services including education, job placement, services. The program selects the best candidates and program participants must volunteer and commit to a kind of behavioral contract. The study compares program and non-program participants on demographics. There is no multivariate analysis using covariates. The study mentions propensity score controls, but	Recidivism <sup>36</sup>	Education with Integrated Services	20.7% Rel Dec	NR

Study	Outcome	Type of Education Program	Effect	N
no information is provided in the study. Sample size is not reported.				
Sabol, 2007a Compared VT and GED certificate achievers to all other inmates released in 1999 and 2000 from the Ohio Department of Rehabilitation and Corrections. One analysis used a discrete time event history analysis to measure the impact of prison programs on the number of quarters from release to employment. A second analysis used a fixed effects panel model to assess employment is 8 quarters after release from prison. Sabol used the following variables in his analyses: pre-prison employment, county level unemployment, prior incarcerations, current offense type, the severity of the current offense, whether this release was from an admission to prison on a new crime, or a release after violation of a prior conditional release, the type of release supervision, prison length of stay, TABE education level at admission, race, and age at release.	Employment <sup>54</sup>	VT GED	1.6% Rel Dec NS	34,061
Sabol 2007b The data in Sabol (2007a) data were used to construct a propensity score of VT completions using the following variables: offense severity level and offense type, sentence type and length, the TABE score, race, prior prison experiences, and pre-prison employment experiences.	Employment <sup>55</sup>	VT by Pre-prison Employment	1.1% Rel Inc	34,061
Saylor and Gaes, (1985; 1997; 2001) Compared inmates who participated in Prison Industries or VT to a matched comparison group that was prospectively chosen using exact matches on race and sex, “calipers” of the propensity score to narrow the potential matches, then the closest comparison group member from the reservoir using the Mahalanobis distance of TX subject characteristics to the closest CMP subjects. The matching variables were: ethnicity, age at first arrest, age at completion of highest education level, age at first commitment, age at current commitment, age at current discharge, number of prior arrests, number of prior convictions, number of prior commitments, years of education, longest amount of time served on any incarceration, months of military service, history of escapes, history of violence, type of detainer, expected length of incarceration, severity of current offense, type prior commitments, security score, and security level. The top three rows of results report the one-year findings with recidivism based on arrest, employment and wages. The bottom four results report on recidivism based on return to prison over 10 years broken out by ethnic and racial minority status of TX subjects relative to the racial and minority statuses of CMP subjects. Only the exclusively VT participants and their comparisons are represented in the table.	Recidivism <sup>46</sup> Employment <sup>47</sup> Wages <sup>48</sup>  Recidivism <sup>49</sup>  Non-Hisp-W Non-Hisp-M Hisp-White Hisp-Minority	      VT	33% Rel Dec 13.6% Rel Inc NS  Race/Eth Results  3.2% Rel Dec 4.5% Rel Dec 5.6% Rel Dec 7.8% Rel Dec	1,700
Schumacker, Anderson, and Anderson, 1990 Compared those enrolled in VT only, VT/Academic, Academic Only and no coursework comparison group – no covariates, no selection artifact controls	Recidivism <sup>9</sup>	VT VT/Academic Academic	21.8% Rel Dec 28.2% Rel Dec 15.7% Rel Dec	760

Study	Outcome	Type of Education Program	Effect	N
Schumacker, Anderson, and Anderson, 1990 Compared those enrolled in VT only, VT/Academic, Academic Only and no coursework comparison group – There was no correction for employment if someone were recommitted	Employment <sup>10</sup>	VT VT/Academic Academic	25% Rel Inc 62.5% Rel Inc 12.5% Rel Dec	760
Steurer and colleagues (2001, 2006); Mitchell (2002) Compared education program participants to non-participants. A large amount of data were collected and these indicated that characteristics of the study group put them at higher risk of recidivating than the control group. Most of the results are bivariate comparisons. Those are represented in this table. A follow-up analysis by Mitchell used a multivariate probit analysis with age, race, gender, education level entering prison, longest job ever held, criminal friends, criminal family members, prior felony arrests, prior incarcerations, type current offense, motivation factor, and urban v. rural area prior to incarceration. The Maryland results were not significant in any of the multivariate analyses of arrest, conviction, or recommitment. A propensity score was estimated with prior education level, motivation factor, prior prison terms, prior jail terms, and current offense type. Mitchell reported that they were very little common support – 1,515 of the original 3,170 cases were used. Analysis of this limited matched data set produced the same results as the multivariate probit. Mitchell also conducted a bivariate probit analysis to model selection and participation simultaneously. Such analyses rely on exclusion restrictions and proper model specification. It was unclear whether either pre-condition was met in these analyses.	Recidivism <sup>50</sup>	All Education programs	Maryland: 16.2% Rel Dec (NS, Mult) Minnesota: 33.3% Rel Dec Ohio 22.6% Rel Dec	3,170
Steurer and colleagues (2001, 2006)  The employment variable indicated whether and offender was ever employed during the 3 year release period.	Employment <sup>51</sup>	All Education programs	MD & MN: NS (81% v 77%)	3,170
Steurer and colleagues (2001, 2006) Year 1 wages are reported, although two- and three-year wages were also reported in their papers. It is not clear how wages were adjusted for time at risk.	Wages <sup>51</sup>	All Education programs	MD & MN: \$7,775 v. \$5,981	3,170
Stevens & Ward, 1997 Two group comparison of Degree Completers (n=60) compared to non-student inmates in North Carolina	Recidivism <sup>3</sup>	Assoc. and/or Bacc. Degree	87.5% Rel Dec	60
Stewart, D. (2005) Compared prisoners who had achieved a basic level of literacy with those that had not achieved such a level (Large attrition in the sample from 464 to 87)	Employment <sup>18</sup>	Basic Literacy and numeracy (Similar to ABE)	NS	87
Stewart, D. (2005) Compared prisoners who had achieved a basic level of literacy with those that had not achieved such a	Recidivism <sup>19</sup>	Basic Literacy and numeracy (Similar to ABE)	NS	87



Study	Outcome	Type of Education Program	Effect	N
level (Large attrition in the sample from 464 to 87)				
<p>Tyler and Kling, 2007</p> <p>From a pool of high school dropouts who were admitted to prison, Tyler and Kling compared a group of prisoners who earned a GED during their imprisonment to inmates who did not have a high school diploma when they entered and did participate in a GED program or participated but did not earn the certificate. The comparison groups were composed so that they were admitted to prison at about the same time as the inmates earning a GED. They used panels of quarterly earnings and four different regression models to analyze the effect of GED on quarterly earnings. The simplest model used OLS estimation using only an indicator variable for GED completion. The richer models used a large set of covariates, year-quarter dummies, a variable capturing participation in the labor market post-release relative to pre-admission, and fixed effects estimates controlling for time invariant characteristics of the sample. The results in the paper are reported for each of three years post release. In all cases the effect of GED certification declines over time. The results of the fixed effects panel model are reported in this table. They show the impact of GED certification on average quarterly earnings in each of three post-release years. The benefit of GED participation for minorities is about a 20% increase in quarterly wages that disappears by the third year.</p>	Wages <sup>41</sup>	<p>GED Certificate v. No GED Education (White)</p> <p>GED Certificate v. No GED Education (Minority)</p> <p>GED Certificate v. GED Participants (White)</p> <p>GED Certificate v. GED Participants (Minority)</p>	<p>Year1=NS, Year2=NS, Year3=NS</p> <p>Year1=\$176, Year2=\$228, Year3 = NS</p> <p>Year1=NS, Year2=NS, Year3=NS</p> <p>Year1=NS, Year2=190, Year3=NS</p>	<p>5,475</p> <p>6,081</p> <p>1,849</p> <p>1,518</p>
Tyler and Kling, 2007	Recidivism <sup>57</sup>	GED	N.S.	
<p>Visher and Kachnowski, 2007</p> <p>Used regression to measure the impact of participation in job training controlling for a variety of pre-prison, orison, and post-release characteristics. These included: age, race, number of prior convictions, length of time served;, high school graduate, married, number of minor children, worked before prison, illegal drug use, family relationship quality, lived in own house/apartment, property conviction, participated in job training, held work release job, satisfaction with police, spirituality, used medication for health reasons inside of prison, any visits from family last 6 months, self expressed need for job/education/financial help after release, doesn't know where will be living after release, no close family;, neighborhood disorder, any drug use/intoxication post-release, reported fair/poor health, depressed, family relationship quality, living with spouse /partner, living with anyone who is drunk often or using drugs, self-esteem, Is tired of problems caused by own crimes, wants to get life straightened out, attitude towards parole officer, owes debt. Many of these are scales. The primary purpose of this study was an exploration of factors affecting post-release employment, and not education programming. Although this study used a lot of regression controls, there was no attempt to address possible selection issues regarding participation in job training. With only a sample of 205 and a 9% job training participation rate, there were less than 20 offenders who had participated in job training in this study.</p>	Employment <sup>56</sup>	Participated in Job Training	300% Rel Inc	205

\*\* Used a secondary source; unable to get the original source.

1. Coded 1 if an inmate completed at least ½ an education course per 6 months of incarceration.
2. A sample of federal prisoners who had served more than 1 year and were released in 1987. Recidivism was defined as a new arrest. or a parole revocation

3. The Stevens & Ward study used return to prison as the outcome; however, they excluded technical violators and this could have biased their results. (TX=5%; CMP=40%)
4. Associates Degree participation for two years versus 3 months or less or no participation
5. Recidivism was return to prison for both a new conviction and/or technical violation.
6. Employment was coded 1= yes; 0 = no if offender was employed when returned to prison or the end of parole. A time varying covariate would be more powerful.
7. Completes at least 1 3-credit college course
8. Recidivism defined as return to the Hampden County Jail within 5 years. (TX=46.8 v. CMP=68.7%)
9. Recidivism was defined as an arrest, revocation or abscond during a 12 month period post-release (VT only = 25%; VT/Academic = 23%; Academic Only = 27%; CMP = 32%)
10. Employment was defined as employment at the end of the 12 month period (VT only = 30%; VT/Academic = 39%; Academic Only = 21%; CMP = 24%)
11. Employed at the end of the first year, No indication of censoring due to recommitment, (TX/AA = 67.4%; TX/HS=60.5%; CMP=40%)
12. Recidivism defined as return to prison in one year, (TX/AA = 11.6%; TX/HS=15.5%; CMP=29%)
13. Recidivism defined as return to prison within 5 years, (TX=26.4%; CMP=44.6%)
14. Recidivism defined as return to prison, (TX=25.0%; CMP=77.0%)
15. Employment was defined as whether the offender was working or had worked during his release
16. Recidivism defined as return to prison (Completers = 30.1%; Non-completers = 35.7%; Dropouts 41.6%)
17. Recidivism defined as return to prison (TX = 25%; Expected Pct = 42%)
18. Employment defined as having found work during the release period (TX Achieved Level 1+ Numeracy = 57%, v. CMP: 52%; Spelling 58% v. 49%; Punctuation 60% v. 49%; Reading 64% v. 49%)
19. Recidivism defined as either recommitted, reconvicted, or self reported offending (TX Achieved Level 1+ Numeracy = 21%, v. CMP: 44%; Spelling 35% v. 44%; Punctuation 25% v. 46%; Reading 43% v. 41%)
20. Recidivism was defined as an arrest or revocation of parole
21. Recidivism was defined as a return to prison for either a revocation or new conviction
22. Recidivism was defined as a return to prison for either a revocation or new conviction
23. Recidivism was defined as a return to prison for either a revocation or new conviction (TX = 34.0%; CMP = 39.1%)
24. Recidivism was defined as an arrest after release from prison (TX = 36%; CMP = 46%)
25. Recidivism was defined as a return to prison for either a revocation or new conviction – follow-up period not specified (TX = 3.9%; CMP = 11.5%)
26. Recidivism was defined as a return to prison for either a revocation or new conviction within 2 years of release– (ABE=32.3%, CMP=30.6%; VT=30.1%, CMP=31.3; GED=24.1, CMP=32.3%; College=26.4%, CMP=30.4%) Anderson, 1995 reported percentages for those who completed training and those who participated. I am reporting the participant percentages as if this were an intent-to-treat design.
27. Arrests while on parole – no adjustment for time on parole.
28. Employment information was gathered by randomly selecting 50 employers of releasees who had received training. Only 25 could be interviewed
29. Recidivism was defined as a return to prison for either a revocation or new conviction up to 54 months after release. (Three-year survival: TX=66.7%; CMP=71.1%)
30. Recidivism was defined as a new conviction, charged with a new offense, or absconded (I ignored rule violations reported by the author) (Recidivism percentages: College: 37.3%, VT: 22.9%, Secondary: 28.1%, Elementary: 20.2%).
31. Adjustment was based on a needs assessment scale that determined need for services while on parole--the lower the score, the better the adjustment. (Adjustment scores based on a multivariate analysis controlling for prior felony convictions, age, race, gender, prior incarceration and seriousness of current offense: College: 15.86; VT: 18.38, Secondary: 19.9, Elementary: 23.05)
32. Recidivism was defined as a return to prison (Academic Completers: 19.1%; VT Completers: 21.3%; Academic Non-Completers: 38.2% VT Non-Completers: 37.3; No Education Involvement: 49.1%)
33. Employment was measured by sending a survey to parole offices where offenders were still under supervision and the results are based on those employed at least 90 days (All Completers: 77.9%; All Non-Completers: 61.4; No Education Involvement: 54.6%)
34. Recidivism defined as return to prison within 5 years for a new conviction or violation. Study 1 High School: 19.6%, VT: 11.9%, Associate Degree: 10.8%, State: 30.9%
35. Recidivism defined as return to prison within 5 years for a new conviction or violation. Study 2 Associate Degree: 9.1%, VT Certificate: 14.8%, State 30.9%
36. The recidivism definition is very ambiguous. It appears to be parole violations or commitment of new crimes (TX=65%; CMP=82%)
37. Recidivism defined as return to prison within 4 years of release, or time to failure using 4 years as the censoring end period
38. Recidivism defined as return to prison within 84 months of release. (TX=13.7% (All participants); CMP=31.7%)
39. Recidivism defined as arrest for a new crime or parole violation – risk period is not clearly defined (TX=37%; CMP=58.2%)
40. Recidivism defined as return to prison within 72 months of release. (TX=33% ; CMP=36%)
41. Employment was defined as legitimate quarterly wages prior to, during, and after a spell of incarceration – earnings deflated to 2002 constant dollars. Employment was whether there were legitimate earnings during the quarter.
42. Recidivism defined as different types of failure. In this table I combined all types of returns to prison, whether for a minor crime, a revocation, or major crime as a return. The follow-up period varied for the experimental and control groups, but was on average, about 80 months. The experimental groups seemed to have slightly less follow-up periods that could affect the results. (Max. Sec: TX=64.7%, CONT = 78.6%; Med. Sec.: TX=69.2%, CONT=75%)
43. Employment status while on parole. (TX=82.3%; CMP=42%)
44. Arrest while on parole. There was no indication of the risk period or whether it was the same for TX and CMP (TX=32.39%, CMP=50%)
45. Recidivism was defined as a return to prison within two years of release. (TX=23% CMP=32%)
46. Recidivism in the 1997 study was defined as an arrest with 12 months of release.
47. Employment was recorded by community supervision officers who indicated whether the offender had been employed in each of the 12 months following release. The table indicates the percentage employed

in the 12<sup>th</sup> month. (TX=71.7%, CMP=63.1%)

48. Wages were also recorded by community supervision officers. The average monthly wages were \$9,700 but there were no statistical differences in the TX and CMP groups. The average wages were \$9,700.

In the period these data were collected (1983 - 1988) the poverty level for a two person family ranged from \$6,483 to \$7,704 and for a family of four \$10, 178 to \$12, 092.

49. Recidivism for the long term results was based on returns to federal custody for a new conviction or a technical violation. Racial and ethnic minorities are compared to non-Hispanic whites.

50. Recidivism was defined as arrest, conviction, or return to prison. The results reported in Table 2 are return to prison within 3 years (MD: TX=31%, CMP=37%; MN: TX=14%, CMP=21%; OH: TX=24%, CMP=31%)

51. Used data from state Departments of Labor to record quarterly wages and a binary variable coded 1 if the offender had worked in any one of the quarters after release, 0 otherwise.

52. Recidivism was defined return to prison. An individual could be released multiple times in the four year release period and each release was counted in the recidivism data. Depending on the release cohort, the at risk period varied. (TX=18.02%, CMP=24.24% )

53. Earnings and employment rates were calculated using data from the Virginia Employment Commission.

54. Earnings and employment rates were calculated using quarterly data from the Ohio Department of Jobs and Family Services.

55. Earnings and employment rates were calculated using quarterly data from the Ohio Department of Jobs and Family Services.

56. Self reported employment was gathered during interviews at 1 to 3 and 4 to 8 months following release from prison.

57. Recidivism was defined as a return to prison within three years.

58. Wages was defined as legitimate quarterly wages prior to, during, and after a spell of incarceration – earnings deflated to 2002 constant dollars. Employment was whether there were legitimate earnings during the quarter.

59. Wages was defined as legitimate quarterly wages prior to, during, and after a spell of incarceration – earnings deflated to 2002 constant dollars. Employment was whether there were legitimate earnings during the quarter.

60. Recidivism was defined as a return to prison within three years.

61. Recidivism was defined as a return to prison within two years.