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# What is the Value of a "Prison GED?"

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## What Is the Value of a "Prison GED"?

Using data on criminal justice offenders who were incarcerated in Florida state prisons between 1994 and 2002, we estimate the effects of obtaining a General Educational Development (GED) credential while incarcerated on post-release quarterly earnings, probability of employment, and probability of recidivating. We find that non-white offenders who obtain a GED while in prison have higher earnings and a higher probability of being employed after release than non-white offenders who do not obtain a GED while in prison. We find no evidence that a GED improves the labor market outcomes of white offenders lacking a high school diploma, and we do not find that a GED improves the recidivism rates of any group. Tempering any causal inferences is evidence that non-white offenders who obtain a "prison GED" had somewhat better labor market outcomes before they entered prison than non-white offenders who do not obtain a GED. In general, we can match results from earlier research on the effects of prisonbased education programs. Our differential findings by racial/ethnic group are new to the literature as are our findings that earlier estimates tended to overstate the value of a "prison GED" because they lacked sufficient controls for important observable differences between offenders who do and do not leave prison with a GED.

#### What Is the Value of a "Prison GED"?

## I. Introduction

It is now widely known that after seventy years of stasis, the U.S. penal population has grown enormously over the past twenty years. Between 1980 and 1999 the number of incarcerated individuals rose from about 502,000 to almost 1.9 million (Bureau of Justice Statistics 2001). An additional component of the recent run up in the prison population is that it has been composed disproportionately of low skilled minority men. By the end of 2000, there were 3,457 sentenced black male inmates per 100,000 black males in the United States, compared to 1,220 sentenced Hispanic male inmates per 100,000 Hispanic males and 449 white male inmates per 100,000 white males (Bureau of Justice Statistics 2001). Over a third of black male high school dropouts ages 20 to 35 were in prison or jail on an average day in 1996—a higher fraction than those found in paid employment (Western and Pettit 2000).

Growth in incarceration appears to have been fueled by tougher sentences for repeat offenders and drug offenders, and a more punitive approach to post-release supervision, rather than any underlying behavioral shifts in the patterns of criminal offending (Blumstein and Beck 1999; Mauer 1999). These shifts in criminal justice policy have had a disproportionate effect on disadvantaged and minority men, individuals who have traditionally maintained marginal positions in the mainstream labor market. An important question therefore is to what extent education and training programs generally available in correctional facilities help criminal offenders successfully reintegrate into the mainstream labor market.

One of the most ubiquitous education opportunities available to inmates who lack a high school diploma is the ability to study for and obtain a General Educational Development (GED) credential. For example, about 10,000 of the approximately 144,000 dropouts who were entered a Florida prison at some time between 1992 and 2000 obtained a GED while they were incarcerated. The widespread availability of the GED credentialing program for incarcerated individuals raises the question of whether there are, in fact, any economic benefits associated with a GED that is obtained in a correctional setting.<sup>2</sup> To address that question this paper draws upon a unique data set containing information on individuals who were in a Florida state prison at any time between 1994 and 2002. Quarterly earnings and employment comparisons between offenders who obtained a prison GED and three different comparison groups incarcerated dropouts who attempted but failed the GEDs, incarcerated dropouts who never attempted the GED exams, and incarcerated offenders who entered prison with exactly twelve years of completed schooling<sup>3</sup>—indicate that minority group members who obtain a GED in prison have better post-release outcomes than minority offenders who enter prison without a high school diploma. We show, however, that minority offenders who obtain a GED while in prison had somewhat better labor market outcomes in the pre-prison years than minorities who do not obtain a prison GED. We find no evidence of a "GED effect" for white offenders, and we also show that offenders who

<sup>&</sup>lt;sup>1</sup> These 144,000 dropouts include individuals for whom information on education level is missing. When the statistics are limited to individuals for whom education level is known, there are about 108,000 dropouts who entered Florida prisons between 1992 and 2000.

<sup>&</sup>lt;sup>2</sup> In Florida, for example, offenders housed in virtually all state prisons have the opportunity to test for the GED provided they meet certain guidelines (Gilbert 2001).

<sup>&</sup>lt;sup>3</sup> We infer that the latter group are individuals with a high school diploma and no additional years of education, though there is no information on whether or not the individual was actually awarded a diploma.

<sup>&</sup>lt;sup>4</sup> "Minority group members" is everyone who is *not* coded as white, non-Hispanic in the data. For expositional simplicity minority group members will be referred to as "minorities" in the rest of the paper.

leave prison with a GED fare substantially worse in the labor market than offenders with a high school diploma.

## II. The GED, Incarceration, and Past Research

To obtain a GED individuals have to pass exams that cover math, science, social studies, reading, and writing. All of the test items are multiple choice except for a section in the writing exam that requires GED candidates to write an essay. The total test time if all tests are taken at the same time is about seven and three-quarters hours. While there has been little research examining the potential effects of passing the exams and obtaining a GED in prison or jail, there has been substantial work in the past ten years on the general labor market effects of a GED. Cameron and Heckman (1993) effectively opened this line of research by showing that GED holders were not the labor market equivalents of regular high school graduates. Their results were more ambivalent on the question of whether GED holders fared better than dropouts who lacked the credential. Recent work has tended to indicate that dropouts who leave school with very low skills benefit from obtaining a GED, while there are no payoffs to the credential for dropouts who leave school with higher skills (Murnane, Willett, and Boudett 1999; Murnane, Willett, and Tyler 2000; Tyler, Murnane, and Willett 2000). Estimates from these studies generally show that after about five years, the earnings of low skilled dropouts who obtain a GED are 15-20 percent higher than those of low skilled uncredentialed dropouts.5

<sup>&</sup>lt;sup>5</sup> Heckman, Hsse, and Rubinstein (2000) (HHR) do not find the same pattern as the aforementioned studies. An important difference in the two sets of findings is that HHR do not allow time for the effects of the GED to accrue, a result that the other authors found to be important.

These results raise the question of what are the mechanisms through which the GED could increase wages or employment. For incarcerated individuals there are at least three. First, to the extent that individuals have to study and learn new skills to pass the GED exams, they may increase their human capital, which in turn may lead to increased wages (Becker 1993). This may be an especially important avenue for incarcerated GED candidates, since their pre-GED skill levels are likely lower than those of dropouts in the "free world." Second, the GED may serve as a "labor market signal" allowing employers to identify individuals they suspect of having productive attributes such as higher cognitive skills or motivation levels within the pool of dropout job applicants (Spence 1973). Third, the GED could positively impact labor market outcomes if inmates use their GED to obtain post-incarceration higher education or training. This is not likely to be an important mechanism since the data indicate that GED holders obtain very little post-secondary education or company-provided training (Boudett 2000; Murnane, Willett, and Tyler 2000; Tyler 2001).

Even if any or all of these mechanisms actually come into play, however, it could still be that the net effect of a "prison GED" on mainstream labor market outcomes is zero as a result of a potentially overriding negative effect of incarceration on outcomes. Western, Kling, and Weiman (2001) identify four mechanisms through which incarceration could negatively impact mainstream labor market outcomes. First, experimental studies have suggested that convicted criminals carry a "conviction" stigma

<sup>&</sup>lt;sup>6</sup> The GED could lead to more post-secondary education and training by either satisfying admittance requirements or by providing access to student higher education loans. Work by Murnane, Willett, and Boudett (1997) has shown that male dropouts do not benefit from private, for-profit vocational training. Meanwhile, a study by Boudett (2000) indicates that on average women do benefit from these proprietary programs. These same authors show that the returns to academic post-secondary education are as high for GED holders as for regular high school graduates.

with them into the labor market that negatively impacts their outcomes (Boshier and Johnson 1974; Buikhisen and Dijksterhuis 1971). Also, recent survey data reinforces the idea that employers would be more likely to hire welfare recipients or applicants with little work experience than ex-convicts (Holzer 1996). Second, in most jurisdictions a felony record can disqualify employment in licensed or professional occupations, as well as in health care and skilled trades. Also, in many states felony status bars public sector employment. The third mechanism is that incarceration may erode the job skills of inmates, that time in prison may exacerbate mental or physical illnesses, and that adaptive prison behaviors may be inconsistent with work routines in the free world. Finally, incarceration may affect social as well as human capital. To the extent that jobs are found through personal connections that link workers to employers, time spent in prison may erode these networks.

If these potential "prison effects" are sufficiently dominant in the labor market, then the effect of possessing a criminal record could outweigh any positive effect of possessing a GED. In this case we would see no net labor market gains associated with a GED obtained in prison.

Prior research on the effects of "prison GEDs" on post-release outcomes is relatively limited in spite of the fact that a 1995 survey showed that 75 percent of all state and federal prisons in the U.S. offered some form of adult education, including GED programs. Furthermore, most of the previous research has examined the relationship between obtaining a GED and the probability of recidivating, giving little attention to whether or not prison GEDs are related to post-release labor market outcomes.

<sup>&</sup>lt;sup>7</sup> Source: Bureau of Justice Statistics.

A 1995 review of the research at that time on adult academic correctional programs (Gerber and Fritsch 1995) cited two studies of prison GEDs. The first study, an unreviewed report, claimed that "those who completed a GED/High School or higher, upon release, had a higher employment rate, lower unemployment rate, and lower criminal activity at twelve months than those releases who had less than a GED" (Anderson, Anderson, and Schumacker 1988). The second study, a report from the New York State Department of Correctional Services, found that inmates in New York who earned a GED had a 5.1 percent reduction in recidivism relative to those who attended GED classes but did not earn the diploma (New York State Department of Correctional Services 1989). Noting the generally weak research designs employed by these and other studies reviewed, the authors of the 1995 research review state in their conclusion that "[future research] must employ more precise controls for extraneous variables that may have an independent effect on various outcomes...[otherwise] it is difficult to speak definitively about the impact of correctional education programs" (Gerber and Fritsch 1995, pg. 137).

A 1999 survey of the literature by Wilson et al. (1999) cited eight studies that included an evaluation of the relationship between the GED or the GED plus some additional Adult Basic Education (ABE) and the likelihood of returning to prison. Five of the eight studies found that offenders who obtained a GED were less likely to recidivate than those who did not. However, the authors of the research review point out that "...all of these studies had weak research methodologies, simply comparing either participants with nonparticipants or program noncompleters, with little to no control or adjustment for selection bias" (Wilson et al. 1999, pg. 14).

The one research review that directly discussed the models used to study the impact of correctional education programs on outcomes states that "[t]he control variables were generally restricted to gender, race, and age...[and only one study] controlled for important sources of selection bias between participants and nonparticipants, such as prior criminal history, in the analysis of recidivism" (Wilson, Gallagher, and MacKenzie 2000, pg. 355). In their meta-analysis of 53 corrections-based education, vocation, and work programs, Wilson et al. found that nearly two-thirds had very weak research designs "...evidencing clear nonequivalence between the program and comparison participants" (Wilson, Gallagher, and MacKenzie 2000, pg. 355).

In their 2000 review Wilson et al. point to several potential selection mechanisms that could lend an upward bias to the estimated impact of correctional education programs on post-release outcomes. In the prison setting selection mechanisms could work through both the individual and through administrative procedures, since enrollment in correctional education programs is predicated on variables such as good behavior and time to release. Fixed characteristics of the individual such as self-control or motivation that might affect post-release outcomes could also affect placement in a GED program through both self- and administrative selection processes. More transitory characteristics such as motivation toward positive life changes and attitudes toward work could work in the same ways.

Wilson et al. also point to studies (Sampson and Laub 1993; Thornberry and Christenson 1984) indicating that program participants may have a higher level of social bond to conventional, non-criminal society than do program nonparticipants. These authors posit that program participants may be more likely to be married, to have children

with whom they are in contact, to have had a job before incarceration, and so. Assuming such factors lead to more positive post-release outcomes, failure to control for such attributes will lead to overestimates of the effect of education program participation on outcomes.

Another possibility cited in the Wilson et al. work is that a sentence to prison may act as a critical life event for some offenders resulting in a change in motivation to both participate in correctional programs and conduct one's life in a more positive manner post-release (Zamble and Porporino 1988). In this model any estimated program effects could simply be measures of the commitment of program participants to a life away from crime rather than effects of the program itself on outcomes.

The consistent message from the prior research on correctional-based education programs, including the GED, is that the positive effects that abound in the literature are all seriously compromised by research designs that fail to account, even in the most rudimentary ways, for unobserved heterogeneity between program participants and nonparticipants. This study addresses this shortcoming by bringing an extremely rich data set to bear on the question.

## III. Data

To determine whether ex-convicts who acquire a GED while in prison fare better in the mainstream labor market than former offenders who do not possess the credential, we use a unique data set constructed for this project by the Florida Department of Corrections (DOC), the Florida Department of Law Enforcement (FDLE), and the Florida Education and Training Placement Information Program (FETPIP). These data contain basic demographic, criminal justice, and test score information, along with the

quarterly earnings for males who were incarcerated in Florida state prisons any time between 1994 and 2002. Quarterly earnings for the sample come from the Florida Unemployment Insurance (UI) system and cover the third quarter of 1993 through the first quarter of 2002.<sup>8</sup>

For reasons we explain below, we only include in our analysis individuals who had a new commitment to a Florida prison after October 1994. The master data set from which we form our analytic sample contains information on 116,639 males who fit this criterion. Among these individuals are 11,498 offenders who obtained a GED while incarcerated in a Florida prison at some time after October 1994. In a process described in detail below, we were able to match 5,396 of these GED holders to individuals who entered prison at approximately the same time and had the same initial sentence length, but who never obtained a GED. This matched data set contains 43,958 individuals.

Since our analysis is concerned with GEDs obtained while in prison, and since there is a non-trivial amount of mobility between the "free world" and prison, we must identify the times when individuals in our data were and were not in prison. Using information on all movements of individuals in the Florida Department of Corrections system we are able to construct distinct prison spells for each individual based on prison entry resulting from a new commitment and exit dates resulting from prison release. In our definition a spell must last more than one day and a spell is assumed to continue until a permanent release that lasts for more than one day is observed. If there is a subsequent entry into prison for that individual, a second spell begins and ends according to the same

<sup>&</sup>lt;sup>8</sup> All earnings are deflated to 2002 constant dollars using the CPI-U deflator.

<sup>&</sup>lt;sup>9</sup> For example, the Florida Department of Corrections estimate that about one-third of released offenders return to prison within two years of being released and almost one half return within six years (Department of Research and Data Analysis 2001).

guidelines. For example, 63 percent of the individuals in the matched data set had only one spell, 24 percent had two spells, 7 percent had three spells, and 3 percent had four spells. The median spell length is about 16 months and the mean is 20 months.

Individuals admitted to a Florida prison prior to October 1994 were eligible for "control release." Control release is an administrative function that was used to manage the state prison population within lawful capacity. In the era of control release many inmates were not in prison long enough to participate in academic programs, and those who were likely differed in important ways from prisoners who were being granted control release. As a result, we have limited out analysis to individuals who were admitted to Florida prisons on or after October 1, 1994.

In analyzing the impact of the GED on the post-release outcomes of offenders, there is a defining spell for GED holders that we call the "target spell," defined as the prison spell in which the GED holder obtained his credential. The year and quarter in which a target spell begins form the basis for constructing another tool that is critical to our analysis—cohorts based on the target spell entrance and the initial sentence length.

It is important that we compare individuals as who are as similar as possible on all dimensions when evaluating the impact of the GED on outcomes. One way in which we do this is to group individuals into cohorts defined by the target spell entry date (measured by year and quarter) and initial sentence length (measured in half-years). In constructing cohorts each GED holder is grouped with all other individuals who have the same target spell entry date and initial sentence length.

Based on these definitions for creating cohorts, the 43,958 individuals in the matched data set fall into 423 cohorts. The smallest cohorts have two individuals who were

matched on prison entry date and expected sentence length, and the largest cohort has 583 matched individuals. The median cohort size is 224. The number of GED holders in a given cohort ranges from one to 62, with the median being 20. Also, each cohort contains at least one person who is not a GED holder with the number ranging from one to 570 with a median of 201.

In both the earnings and employment analysis and the recidivism analysis GED holders will be compared to three different comparison groups. The first group is composed of offenders who entered prison without a high school diploma and who attempted but failed the GED exams. Comparisons between GED passers and GED failers control for unobserved factors that are correlated with both GED program participation and later outcomes since both groups attempted to obtain a GED while incarcerated. A second comparison group is composed of offenders who entered prison without a high school diploma and who never attempted the GED exams. A third comparison group is composed of offenders who entered prison with a high school diploma. These individuals are not eligible to participate in the GED program. Comparisons between GED holders and high school graduates captures the extent to which a GED is indeed a high school equivalency certificate for criminal justice offenders.

A potential problem in examining the earnings and employment outcomes of offenders is the non-trivial number of invalid Social Security Numbers (SSNs) in the administrative records of offenders. Since SSNs are required to link the Florida DOC data to the UI earnings data, verification of valid SSNs in this population is critical. All SSNs in our data were verified by programmers at the federal Social Security Administration

based on standard verification algorithms used by the Employment Verification Service that matched DOC data on the digits of the SSN, date of birth, sex, and name to SSA records with some tolerance for clerical error. As might be expected in data on criminal offenders, about 20 percent of the sample lack a valid SSN. In analysis not shown here we find that individuals with non-valid SSNs are more likely to be minority, a non-Florida resident, and unemployed at the time of arrest. About 20 percent of the potential non-attempting dropouts have non-valid SSNs, while only 15 to 17 percent of the other three groups (GED passers, GED failers, and high school graduates) have non-valid SSNs. While individuals lacking a valid SSN can be used in the recidivism analysis, they must dropped from the earnings and employment analysis.

Table 1 compare basic descriptive statistics across GED holders and the three comparison groups in the earnings/employment sample. GED holders are substantially more likely to be white than are GED failers, non-attempting dropouts, and high school graduates. About 58 percent of the GED holders are white compared to about 30 percent of the GED failers and 44 percent of the non-attempting dropouts and high school graduates who are white. GED passers and failers are younger than the other two groups and are less likely to be married. Mean years of completed schooling are very similar across the three dropout groups and in both samples.

In terms of criminal justice related factors, GED holders look most like regular high school graduates in terms of the probability of having a prior prison term and the average length of time spent in prison prior to the current prison spell. GED passers and failers are more likely to have had a disciplinary report at some point before the present prison spell, and their average initial sentence length in the target prison spell is somewhat

longer than is the case for non-attempting dropouts and high school graduates. A higher percentage of GED holders are in prison in the target spell for violent types of crimes than are the other groups, and substantially fewer GED holders are in prison for drug related crimes.

The bottom rows of Table 1 present preliminary information on the dependent variables used in the empirical work. Roughly three in ten individuals return to prison within two years after most in their cohort have been released. Unconditional mean quarterly earnings and mean employment rates suggest two potential stories. First, GED holders have larger gains in both unconditional earnings and unconditional probability of employment between the quarter prior to prison release and the 10<sup>th</sup> quarter after prison release than do the comparison groups. Thus, obtaining a GED may be effective in improving the post-release outcomes of offenders. The fact that these gains are almost as large for offenders who attempted, but failed, the GED tempers this initial inference, however, by suggesting that the GED program may simply be selecting out those offenders who have the highest future labor market potential. The goal of the empirical analysis is to sort out these stories.

## IV. Analytic Methods

Our analyses will primarily be concerned with measuring post-release labor market outcomes associated with Unemployment Insurance (UI) quarterly earnings and post-release employment as inferred by non-zero UI quarterly earnings. Since UI earnings are recorded quarterly, we will want to account for absolute calendar time in quarters. In addition, however, we will use two definitions of relative time in our analysis.

The first definition is the time in quarters relative to the start of a cohort's incarceration spells, which we index by "s" and refer to as "s-time". For example, s=-1 refers to the calendar quarter before the entry of a cohort to prison. The second definition is the time relative to the release of a cohort from their incarceration spells, which we index by "r" and refer to as "r-time." Since members of an entry cohort with the same initial sentence length are released at different dates (due to gaintime accrued and lost for behavior while incarcerated, as well as to credited jail time that occurred before sentencing to state prison), zero in r-time for a cohort is set to the calendar quarter of the median release date for that cohort. For example, r=1 refers to the calendar quarter after the median release quarter of a cohort from prison.

The motivation for using two definitions of relative time stems from our analytical interest in both periods before a spell (which we analyze in s-time) and after a spell (which we analyze in r-time). Relative time is particularly important in analyzing the periods after a spell. Past analyses have tended to look at offenders in "real" time after they leave prison. That is, outcomes are measured at, say, 1 year after the offender was actually released from prison. This approach may over or under estimate the total effect of program participation on earnings that can result from the correlation between program participation and release date. We address this problem by measuring release time relative to the median cohort release time, rather than time relative to when the offender was actually released. The latter we call "real-time." A polar example can illustrate the issue.

<sup>&</sup>lt;sup>10</sup> Another way to frame the problem is that because of the ability of prisoners to affect their release date through "good behavior" and program participation, real-time release time is endogenous.

In the simplest example, consider two offenders, A and B, who are in the same cohort. Assume that the median release quarter for this cohort is 12 months after prison entry. In this example we assume that offender A obtains a GED in prison and that he is also released from prison two months earlier than most members of the cohort because of the gaintime he accrued when awarded the GED. Offender B does not obtain a GED and exits prison in the median cohort release quarter. Finally, for this simple example assume that there is a common linear growth in quarterly earnings for all released offenders and assume that the GED has no independent effect on earnings. This scenario is presented graphically in Figure 1, where quarterly earnings upon release are assumed to be \$1,000, and each quarter earnings are assumed to grow by \$100.

# <Figure 1 about here>

In this simple example, if outcomes were compared in real-time after release from prison, there would be no differences in the earnings of offender A and B. Each would have earnings of \$1,000 in the quarter after release, \$1,100 two quarters after release, etc. This comparison accurately estimates the "pure" labor market effect of the GED on earnings that is assumed to be zero in this example. However, this comparison underestimates the total effect of earning a GED in prison on earnings since it completely misses the affect of participating in the GED program on prison release date. Comparing results in r-time allows for receipt of a GED to affect earnings via its effect upon release date. At r = 1 and each subsequent r-time quarter, offender A has earnings that are \$200 higher than offender B (\$1,200 versus \$1,000 at r = 1) due to the fact that he has been in the labor market for an extra two quarters. This stylized, polar example highlights the fact that we want to capture in our analysis the total effect of the obtaining a prison GED on

outcomes, including the potential effect on length of time served relative to expected sentence length. Note that while this issue is important in principle, we find in practice that there is little difference in average time served within cohorts between GED and non-GED recipients—although there is substantial variance within cohorts having the same sentence length.

A second, but related issue has to do with the rationale for having separate definitions of time before and after spells, rather than only using s-time in all analyses. We do this because we believe it is more meaningful to pool results around the time of median release for cohorts that have different sentence lengths. To be concrete, consider cohorts X and Y with sentence lengths of 8 quarters and 16 quarters, respectively. Pooling results on post-spell outcomes at time s = 10 is not very meaningful because most members of cohort Y will still be in prison. At r = 2, however, most members of both cohorts will have recently re-entered mainstream society, so we use r-time for post-spell analyses pooling different cohorts.

Figures 2, 3, and 4 display mean quarterly earnings around the time of release based on real-time (Figure 2), r-time (Figure 3), and s-time (Figure 4). In Figures 2 and 3 the zero quarter represents the release quarter. In Figure 2 this represents the quarter in which individuals were actually released from prison, while in Figure 3 this represents the median cohort release quarter. The patterns in these two graphs are roughly similar, not surprising since roughly the same number of individuals are released before and after the median cohort release quarter.

<Figures 2, 3, and 4 about here>

Figures 2 and 3 indicate that even during quarters when most individuals are in prison (real-time and r-time just before zero), there are non-zero quarterly earnings. Non-zero earnings during prison result primarily from employment in prison work release centers. Inmates in work release centers work in the surrounding community for market wages, returning to secure facilities at night. For example, about 5 percent of the offenders in our data were in work release centers at r = -4, working an average of 61 days during that quarter.

Figure 4 shows that the pre-target spell earnings of all dropout groups are similar and are substantially lower than the pre-spell earnings of high school graduates. On the other hand, Figures 2 and 3 show that dropouts who earn a GED while in prison have post-prison spell earnings that lie between the earnings of high school graduates and dropouts who do not leave prison with a GED. Taken together these observations are consistent with the hypothesis that obtaining a "prison GED" increases the post-release earnings of dropouts. Figures 2 and 3 do not, however, account for the role played by observable factors that may affect earnings and the other outcomes of interest. This analysis accounts for observable factors in a more complete manner than previous work.

The empirical analyses that will address this issue examines three outcomes: post-release quarterly earnings, post-release employment as measured by the presence of positive earnings in a quarter, and the probability of recidivating. In each case we will compare the post-release outcomes of offenders who obtain a GED while in prison relative to the post-release outcomes of the three comparison groups who do not have a GED: GED failers, non-attempting dropouts and high school graduates. While we have no clear source of exogenous variation in GED status in these data, we do have a

substantially richer set of available covariates to bring to the analysis than did previous studies. To fix ideas consider the estimating equation for examining the effect of acquiring a prison GED on post-release earnings.

Let  $Y_i$  denote quarterly UI earnings for individual i.<sup>11</sup> Let  $GED_i$  be a (0,1) indicator for whether individual i passed the GED exams and obtained the credential while incarcerated. Let  $X_i$  denote a set of covariates that includes:

- race/ethnicity,
- a set of dummies that control for the years of completed schooling among dropouts,
- age at prison spell entry and age squared,
- marital status and number of dependents upon prison spell entry,
- years in Florida prior to prison spell,
- whether or not a Florida resident at prison entry,
- state or region of birth,
- whether or not employed prior to arrest and conviction,
- industry and occupation of employment prior to arrest and conviction,
- whether or not an English speaker,
- whether or not a confirmed U.S. citizen or alien,
- cumulative years in prison prior to the target spell,

<sup>11</sup> UI earnings are not available for out-of-state earnings or for jobs that are not usually covered by the UI system, such as self-employment, work which may be "off the books" such as domestic service or informal child care, or for employers who do not report earnings. Thus, UI earnings may understate "true" earnings. A comparison of data from UI and data with more complete coverage from the Social Security Administration (SSA) found that average earnings from SSA data were about 25% higher. Self-reported

Administration (SSA) found that average earnings from SSA data were about 25% higher. Self-reported earnings for adult men were 30% higher than UI reports, with the additional difference apparently due mainly to uncovered jobs rather than out-of-state jobs (Kornfeld and Bloom 1999).

- number of disciplinary reports ever accumulated in prison prior to the target spell,
- type of offense for target spell in prison, and
- measure of cognitive skills at beginning of the target spell.

The measure of cognitive skills that we employ is the Test of Adult Basic Education (TABE). The first stop for offenders entering the Florida prison system is the "reception center" where they are processed. While at the reception center, all offenders are administered the Survey test battery of the TABE. This test is particularly appropriate for our use since it is administered before any prison interventions. Since different versions of the TABE tests were administered during the period we cover, and since TABE scores are not necessarily equitable across versions, we created a vector of 58 TABE score by version indicators. We control for TABE scores with this vector of indicators.

As discussed earlier, we also placed individuals in cohorts defined by target spell entry and initial sentence length. The primary regression adjusted estimating equation we employ is:

$$Y_{ij} = \alpha_j + GED_{ij} \gamma_1 + X_{ij} \beta + \varepsilon_{ij}$$
 (1)

where i indexes person and j indexes the prison entry and sentence length cohort. Using Ordinary Least Squares (OLS), we fit equation (1) separately for the three different comparison groups and present separate estimates for each of the available quarters of data around the prison release quarter.<sup>12</sup>

<sup>&</sup>lt;sup>12</sup> With earnings available from the third quarter of 1993 through the third quarter of 2000, offenders whose relative time quarter is in the third quarter of 2000 have twenty-eight quarters of pre-release earnings data but no post-release data, while offenders whose relative time quarter is the third quarter of 1993 have twenty-eight quarters of post-release data but no pre-release data. All other offenders grouped by prison

When making earnings comparisons in the quarters prior to prison entry we focus on s-time and let  $s \in [-12,...,0]$ , since we are primarily interested in the quarters before prison entry (s < 0) rather than earnings comparisons when most individuals will be in prison (s > 0). When making post-release earnings comparisons we focus on r-time and let  $r \in [-4,...,0,...,12]$ . We examine negative r-time since offenders may be housed in work release centers in the quarters before release.

In each regression,  $\alpha_j$  captures the fixed cohort effect associated with date of prison entry and initial spell length that might affect both GED status and later earnings. When estimating the effect of the GED on the probability of employment we fit equation 1 letting Y be a (0,1) indicator for the presence of positive earnings in relative quarter.

The same basic equation is used to estimate the effect of a prison-acquired GED on the probability of recidivating. However, the dependent variable in this analysis is dichotomous and does not vary across time, so there is only a single estimation. The dependent variable in the recidivism analysis equals one if the offender was convicted of a crime that resulted in prison or probation within two years of the median cohort release date, and zero otherwise. In this analysis we limit the sample to individuals whose median cohort release date is at least two years plus 250 days prior to the end of the data collection date. <sup>13</sup>

release quarter have variations of pre- and post-attempt earnings that lie within those two bounds. We limit the analysis to 15 quarters on either side of prison entry or release because of increasingly small cell sizes at further distances from s=0 or r=0.

<sup>&</sup>lt;sup>13</sup> The cutoff period is two years plus 250 days because there is a lag between offense date and registration in the DOC system; Florida DOC personnel have informed us that records are available for three-quarters of all eventual inmates within 250 days of the offense.

## V. Results

# Earnings and Employment

To account for the potential effects of both observable characteristics and fixed cohort effects on earnings and the probability of employment, we fit a series of quarter-by-quarter regression models based on equation (1), each time comparing GED holders to non-attempting dropouts, GED failers, and high school graduates. In order to assess the importance of the different sets of controls, we present, in each instance, estimates from an additional specification that controls only for fixed cohort effects, along with race/ethnicity, years of completed schooling, age, and age squared.

Figure 5a contrasts the pre-target-spell earnings of GED holders and non-attempting dropouts in s-time based on the two different specifications. In order to reduce sampling variability, each point in Figure 5a is based on a pooled regression of three quarters of data (s-1, s, s+1). Standard errors account for the fact that an individual's error terms are correlated over the three quarters. The pattern of estimates in Figure 5a indicate that until about the fifth quarter prior to prison entry there are essentially no differences in the prespell earnings of GED holders and non-attempting dropouts. In the five quarters just prior to the prison spell, eventual GED holders have quarterly earnings that are about \$50 per quarter higher than dropout offenders who will not attempt to obtain a GED in prison.

# <Figures 5a-c about here>

Figure 5b shows the companion estimates to Figure 5a, but this time the comparison group is dropouts that attempted but failed the GED exams while in prison. There are no pre-prison differences in the mean earnings of these two groups.

Figure 5c gives companion estimates comparing GED holders to high school graduates. Figure 5c indicates that even with the full set of controls, eventual GED

holders had unambiguously lower earnings than high school graduates in the quarters before prison entry.

Figures 6a-6c present s-time estimates from linear probability models where the dependent variable is a (0,1) indicator for having positive quarterly earnings in a quarter. The specifications and comparison groups for Figures 6a-6c are the same as for 5a-5c, respectively. These results suggest that eventual GED holders had slightly higher (about one percentage point) probabilities of employment than non-attempting dropouts, though none of the results are precisely estimated. In Figure 6b there are no statistically significant differences in the pre-spell employment probabilities of GED passers and GED failers. Meanwhile, as with pre-spell earnings, eventual GED holders have a consistently lower probability of pre-spell employment than do high school graduates as shown in Figure 6c.

# <Figures 6a-c about here>

The analysis now focuses on the primary question. Do individuals who obtain a GED while in prison have more favorable post-release outcomes? Figures 7a-7c present earnings contrasts in r-time using non-attempting dropouts, GED failers, and high school graduates (respectively) as the comparison group for GED holders.

Figure 7a contrasts GED holders to non-attempting dropouts. In addition to results from the two primary specifications used in the earlier figures, Figure 7a includes results from a specification similar to what has been used in previous studies of prison-based education programs. The estimates indicated with triangles in Figure 7a are from a specification that controls for race/ethnicity, age at admission, prior time served in prison, offense severity of the target prison spell, and sentence length. In this specification, GED

holders have quarterly earnings that are \$150 to \$300 higher and the differences are statistically significant in every quarter.

# <Figures 7a-c about here>

Specifications that control only for fixed cohort effects (in addition to age, years of schooling, and race/ethnicity) reduce the post-prison GED advantage substantially relative to the results based on specifications from the literature, except in the later quarters. In the specification using the full set of controls the estimated "GED effect" is further reduced. Nevertheless, by the second quarter the mean quarterly earnings of GED holders are about \$50 higher than observationally similar dropouts who never attempted to obtain a GED while in prison. This advantage is about \$100 by the fourth quarter and remains steady for another 5 quarters. By the tenth quarter after the median cohort release the estimated GED earnings advantage apparently begins to decline, but these estimates are quite noisy.

There are two points to take away from Figure 7a. The first is that most of the "GED effect" found in earlier work can be explained by additional covariates available in our data. In fact, simply comparing GED holders to individuals with who enter prison at the same time with similar sentence lengths can explain one-half to two-thirds of the estimates based on specifications similar to those used in earlier work. Second, even after controlling for a wide array of observable characteristics, including cognitive skill level at prison entry (as measured by the TABE test scores), dropouts who obtain a GED tend to have higher earnings in the quarters after they leave prison than do uncredentialed dropouts. The earnings differences for any given quarter are, however, quite modest.

Figure 7b shows that when GED passers are compared to dropouts who attempted, but failed, the GED exams, the post-release differences are yet smaller. There are no estimated differences based on the fully controlled model that are statistically significant. One interpretation of the pattern of results in Figure 7b is that once we control for factors associated with *attempting* the GED, there are no earnings gains associated with the credential. This interpretation must be tempered, however, with the fact that all of the point estimates in Figure 7b are relatively poorly estimated.

Figure 7c compares GED holders to high school graduates. The lesson from this figure is that GED holders have consistently lower estimated post-release earnings than do high school graduates. As with a "free world" GED, offenders who leave prison with a GED are not the labor market equivalents of offenders who enter and leave prison with a regular high school diploma (Cameron and Heckman 1993).

To obtain an estimate of the cumulative effect of the GED on labor market earnings over the first three years after the median cohort release quarter, we fit a model based on equation 1 that used all of the data from one year prior to release to two years after the median cohort release quarter. We stack the data into a person-quarter data set and obtain estimates using OLS, adjusting the standard errors to account for the within-person correlation of the errors across time. The model includes interactions between r-time dummies and the GED indicator variable. <sup>14</sup> The sum of the coefficients on the GED by r-time dummies is the estimate of the cumulative differences in the earnings of GED

 $<sup>^{14}</sup>$  We also interact r-time intervals (i.e., the intervals -4 to 0, 1 to 3, 4 to 6, 7 to 9, and 10 to 12) with all of the other regressors in equation 1.

holders relative to non-attempting dropouts. This estimate for r-time quarters -4 through 12 is \$1,200 and is statistically significant (p = 0.022).

Figures 8a-8c explore the effect of acquiring a GED while in prison on post-release employment. As with Figure 7a, Figure 8a includes the estimates from a specification that includes the types of variables used in previous research. Based on this specification GED holders have about an 8 percentage point greater probability of being employed in the second quarter after release from prison that steadily declines to about a two percentage point differential after 12 quarters. All of these estimates are statistically significant at the 0.05 level. Estimates based on both of the other two specifications indicate that after the second post-release quarter, GED holders are more likely to be employed than non-attempting dropouts in the quarters after release. As is the case with earnings, employment probability estimates based on our full set of covariates are substantially smaller than those based on the more limited controls used in previous research, at least until the later quarters where the estimates are similar.

# <Figures 8a-c about here>

Figure 8b shows no statistically significant differences in the post-release employment of GED passers and failers. Meanwhile, Figure 8c demonstrates that high school graduates have a consistently slightly higher probability of being employed than do GED holders.

# Other Specifications

Since there is some evidence that the pre-prison spell labor market dynamics of eventual GED holders may be different than that of other dropouts, we fit models that

<sup>15</sup> We also fail to reject the null that the cumulative earnings of GED failers is the same as that for non-attempting dropouts (p = 0.71).

25

control for these potential differences. In order to account for observable preincarceration differences in labor market outcomes, we control for employment and log
earnings in each of up to eight quarters prior to the beginning of the target spell (and an
indicator for no observed pre-spell employment at all). Figure 9a gives the quarter by
quarter estimates of the quarterly earnings contrasts between GED holders and nonattempting dropouts. The usual two specifications are augmented in Figure 9a with the
controls for the pre-spell labor market dynamics. Adding these controls does little to alter
the basic story of Figure 7a. Figure 9b presents estimates of the post-release employment
probabilities from the same specifications. In this case the slight GED employment
advantage in Figure 8a (which is also present for offenders whose target prison spell
began in 1996 or later) is explained away by the addition of the controls for pre-prison
labor market dynamics.

We also explored the extent to which there might be differences in outcomes by (1) age, (2) Florida residency status at the time of arrest, and (3) race. We examined the possibility of differences across these dimensions by estimating models where the dependent variable is quarterly earnings in the  $8^{th}$  quarter after the median cohort release quarter (r-time = 8). The results are in Table 2. The first columns show that there are no GED differences for younger or older offenders.

#### <Table 2 about here>

According to the middle columns, there is a slightly higher "GED effect" for Florida residents than non-residents. This is likely because non-resident GED holders may leave

<sup>&</sup>lt;sup>16</sup> We set the log of earnings to zero whenever there are zero earnings in a quarter. Note that the earliest spells, such as those beginning in 1994:4, do not have a full eight quarters of earnings history since our earnings data begin in 1993:3.

Florida upon release and, hence, have zero Florida UI earnings post-release. This reasoning suggests that our estimates of GED effects for Florida residents may be slightly understated, since some Florida residents also leave the state after release. However, we also know that less than 5% of inmates indicate that their intended destination upon release is outside Florida to another state and that many have post-release probation restrictions on their geographic mobility.

The most important finding in Table 2 is in the last columns where there are no differences in the 8<sup>th</sup> quarter earnings by GED status among white offenders and large positive GED differences among black offenders.

To examine the extent to which our earlier results differ by race/ethnicity, we compared the pre- and post-release outcomes of GED holders and non-attempting dropouts separately for white and minority individuals. Figures 10a and 10b show the pre-spell earnings and employment estimates based separate, by race, regressions that use the full set of controls. There is some evidence in Figure 10a that minority individuals who will obtain a prison GED have higher earnings than minority offenders who will not in the two years before their prison spell. The eventual GED holders have earnings that are consistently larger than the non-attempting minority group offenders, but the differential is never more than about \$75 per quarter and it is never statistically significant. There are also no significant differences for white offenders by eventual GED status.

Figures 11a and 11b show the post spell outcomes for whites and minorities, controlling for pre-spell employment and earnings as in Figures 9a and 9b. Figure 11a indicates that nearly all of the GED differences in post-spell earnings seen earlier accrue

to minority group members who obtain a GED. There is no significant evidence that white offenders who obtain a GED while in prison have higher post-release earnings than white offenders who are dropouts and who do not obtain a GED. In Figure 11b GED holders in both racial/ethnic groups tend to have a higher probability of post-prison employment, but the effects are larger for minority offenders.

#### Recidivism

By far, the most studied outcome in the corrections-based education literature is the effect of programs on the probability that an offender will recidivate. Not only has this been a question of central policy interest for those who work in and design policy for our nation's correctional systems, but the data to study this question has also been much more readily available than data on the labor market outcomes of ex-offenders.

While some of the earlier recidivism studies were able to control for criminal justice history, most used very rudimentary controls when comparing program participants to non-participants. In attempt to match prior studies we first fit two linear probability models comparing GED holders to non-attempting dropouts. In all models our definition of recidivating is whether or not an individual committed an offense within two years of the median cohort release date that led to a prison or probation sentence. Given the differential "effects" of the GED by race/ethnicity that we have found thus far, we fit all of the recidivism models separately for whites and minority group members.

Estimates from Model 1 in Table 3 are from a model that only controls for race/ethnicity, age, and the year in which the offender entered the target spell in prison. The results based on white offenders are in Panel A of Table 2 and those for minority offenders are in Panel B. Panel C details the specification of each model. In Model 1

white GED holders are no less likely to recidivate within two years of the median cohort release date than are white non-attempting dropouts.<sup>17</sup> Meanwhile, minority offenders who leave prison with a GED are four percentage points less likely to recidivate than minority dropout offenders without a GED.

Estimates in the second column are from Model 2 that adds controls for prior time in prison and offense type and sentence length of the current prison spell. Adding these variables explains about one-third of the minority GED advantage regarding recidivism.<sup>18</sup>

#### <Table 3 about here>

In the third column of the table we add failers and high school graduates to the sample and compare the recidivism rate of GED holders to all three groups controlling for race/ethnicity, age, and fixed cohort effects. In this specification white GED holders are actually more likely to recidivate within two years than are non-attempting dropouts without a GED and the GED advantage for minority offenders disappears. For comparison across models 2 and (in particular) 3, Model 4 uses the same specification as Model 3, except that there is no fixed cohort effect. The lesson from comparing the estimated GED effect in Model 3 to those based on Models 2 and 4 is that controlling for the prison entry quarter and initial sentence length explains all of differences in the recidivism rates of minority offenders with a GED and minority offenders without the credential. Model 5 adds the other controls in the *X* vector and the basic story remains consistent, relative to Model 3.

<sup>&</sup>lt;sup>17</sup> From Table 1, the unconditional probability of recidivating within two years of the median cohort release date is about 28 percent.

<sup>&</sup>lt;sup>18</sup> Inferences in Table 3 are essentially unchanged if the data is fit with logistic regression instead of OLS.

In summary, we find that past research has tended to overestimate the effect of obtaining a prison GED on recidivism. Furthermore, any GED benefits that do exist seem to apply only to minority offenders. We have no good explanation regarding why white GED holders tend to recidivate at higher rates than white dropouts who leave prison without a GED. This is an area that needs further examination.

## Discussion

The convergence of two trends has made an understanding of prison-based interventions such as the GED increasingly important. At the same time that low skilled individuals are facing increasingly dim economic prospects, our nation has seen a run up in the prison population driven primarily by the incarceration of low skilled males. The result is a historically large proportion of inmates who, upon release, will face steep obstacles as they try to reintegrate into the mainstream labor market. A primary vehicle across the nation's corrections systems for trying to rehabilitate and prepare offenders for re-entrance into society and the mainstream labor market has been the corrections-based GED program. In spite of the wide spread use of this program, however, there has been scant research on how effective it actually is in meeting the desired goals. Past research conducted on corrections-based education programs and the GED in particular has often suffered from a missing data problem: the fundamental types of control variables one would want to use have often been unavailable to researchers.

We utilize a unique data set on individuals who were in Florida prisons during the 1990s to examine whether obtaining a GED while incarcerated leads to higher earnings, a greater probability of employment, or a reduced probability of returning to prison. The contribution of this paper is that our data set is large, containing information on tens of

thousands of offenders, has a uniquely rich set of control variables, and has linked criminal justice and UI earnings records.

In our data offenders who will eventually obtain a GED in prison look similar to other dropout offenders in the quarters before they enter prison. After release from prison, however, some dropouts who obtain a GED tend to have higher earnings and a greater probability of being employed. In particular, we find that among minority offenders who enter prison lacking a high school diploma, obtaining a GED seems to be beneficial in boosting post-release labor market outcomes. We find that minority offenders who obtained a prison GED tend to have mean quarterly earnings that are about \$150-\$200 higher than minority dropout offenders without the credential. We find no earnings advantage for white dropout offenders who obtain a GED.

On the one hand \$200 per quarter is a quite small increase in earnings levels. On the other hand it represents a 17-18 percent increase in the quarterly earnings of an uncredentialed dropout offender two years after release from prison. Also, we note that controlling only for race/ethnicity and age, there is only about a \$100 per quarter difference in the post-release earnings of offenders who have 11 versus 10 years of completed schooling. Thus, for minority offenders, obtaining a GED while in prison is associated with a relatively higher earnings than obtaining an extra year of schooling among offenders lacking a high school diploma.

It is, however, inaccurate to think of the GED as a high school "equivalency" credential. Consistent with what has been found in the research on GEDs earned in the "free world" (Cameron and Heckman 1993), in no comparisons can we say that GED

holders are the labor market equivalents of offenders who enter prison already possessing a high school diploma.

In a departure from previous research, we find no GED advantage among dropouts regarding the probability of re-offending within two years of prison release. This is the case for minority and white offenders. As with labor market outcomes, GED holders have worse recidivism outcomes than do offenders with an high school diploma.

It should also be noted that participation in prison-based GED programs may generate benefits that we have not yet examined. For example, security considerations are of paramount importance in correctional institutions, and it could be that participation in a GED preparation program leads to better behavior while incarcerated. While this is possible, we did not find that GED program participants in our data had fewer disciplinary reports during the target spell than did the comparison groups. Still, given the importance of security issues, this is an area that deserves a closer look. Also, it could be that obtaining a GED confers post-release benefits that are not directly connected to the labor market such as the ability to be a better parent or more effectively engage in the civic life of the free world. Were this systematically the case, however, one would expect GED holders to recidivate at lower rates, something we did not find. In short, there may be advantages to corrections-based GED programs not examined in this paper.

Table 1. Descriptive statistics for the earnings and employment and the recidivism analytic samples.

	Earnings Sample				
	GED passers	GED failers	non- attempting dropouts	high school graduates	
N	5,396	634	28,463	9,465	
% white	57.5	30.1	44.4	43.3	
% black	34.6	60.1	46.4	50.7	
% Hispanic	7.4	8.5	8.9	5.7	
% other race	0.5	0.5	0.3	0.2	
% married <sup>a</sup>	9.8	7.6	11.5	15.2	
mean years education	9.7	9.7	9.5	12	
mean age at admission	25.1	24	29.0	33.0	
% prev. in prison	24.3	27.1	15.3	18.3	
mean # days previously in prison	119	160	60	72	
% with prior disciplinary report	11.4	16.7	6.1	6.9	
% with violent crime offense this spell	42.9	43.7	37.7	39.5	
% with property crime offense this spell	39.1	32.3	34.3	30.1	
% with drug crime offense this spell	14.8	22.2	23.5	26.0	
% with other crime offense this spell	3.2	1.7	4.5	4.4	
mean sentence length in months this spell	25.6	25.3	19.1	19.2	
% who recidivate within 2 years	30.4	30.2	30.4	23.8	
% employed in the 10 <sup>th</sup> qtr before entry	27.1	24.4	29.6	39.1	

% employed in 10 <sup>th</sup> qtr after release	35.8	36.9	32.3	38.4
wage in 10 <sup>th</sup> qtr before entry	518	400	582	1083
wage in 10 <sup>th</sup> qtr after release	1228	1089	1042	1501

a. This is only the percentage who have definitive information on marital status. From 30 to 40 percent of the sample have missing information regarding marital status.

Table 2. Quarterly earnings comparisons between GED passers and non-attempting dropouts 8 quarters after prison release by subgroup (standard errors in parenthesis).<sup>a</sup>

	Age <sup>b</sup>		Florida Resident <sup>c</sup>		Race	
	Young	Old	No	Yes	White	Black
GED passers	61	68	35	84~	7	172**
	(51)	(97)	(133)	(47)	(63)	(71)

a. Estimates based on the specification of equation 1 using the full set of controls, and  $^{\sim} = p < 0.10$ ,  $^{**} = p < 0.01$ .

b. Those in the "young" group were less than 30 years of age at the time of their admission to the target prison spell, while those in the "old" group were 30 or older at admission.

c. Status is based on whether or not a resident of Florida at the time of the arrest leading to the target prison spell incarceration.

Table 3. Estimates from linear probability models of the probability of recidivating. (standard errors in parentheses).<sup>a</sup>

	Model 1 <sup>b</sup>	Model 2 <sup>b</sup>	Model 3	Model 4	Model 5
Panel A, white sample:					
GED passer	0.009	0.016	$0.026^{*}$	0.012	$0.027^{*}$
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
GED failer			0.040	0.026	0.017
			(0.045)	(0.044)	(0.044)
High school graduate			-0.032**	-0.033**	-0.015
			(0.011)	(0.011)	(0.011)
N	8,721	8,721	10,948	10,948	10.948
$R^2$	0.013	0.028	0.027	0.013	0.059
Panel B, minority sample:					
GED passer	-0.041**	$-0.027^{\sim}$	-0.006	-0.036**	-0.014
-	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
GED failer			-0.034	$-0.066^*$	-0.043
			(0.031)	(0.031)	(0.031)
High school graduate			-0.050**	$-0.050^{**}$	$-0.036^{**}$
			(0.010)	(0.010)	(0.010)
N	10,987	10,987	14,245	14,245	14,245
$R^2$	0.031	0.063	0.053	0.023	0.086
Panel C:					
Race/ethnicity	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Year entered prison	Yes	Yes			***
Prior time in prison		Yes Yes			Yes
Offense type this spell Initial sentence length this spell		Yes			
Fixed cohort effects <sup>c</sup>		1 03	Yes		Yes
Family structure			1 05		Yes
Number prior disciplinary reports					Yes
State of birth					Yes
State of residence					Yes
Number years Florida resident					Yes
Pre-arrest employment status					Yes
Pre-arrest industry and occupation Citizen/alien status					Yes Yes
TABE scores					Yes
$a_{\text{a}} = p < 0.10$ . $* = p < 0.05$ . $** = p < 0.01$					105

a.  $^{\sim}$  = p<0.10,  $^{*}$  = p<0.05,  $^{**}$  = p<0.01 b. The excluded category includes only non-attempting dropouts. GED failers and high school graduates are not included in the regression.

c. Cohorts formed on target spell entry year and quarter and initial sentence length (in half years).

Figure 1. Illustration of r-time versus real-time of release.

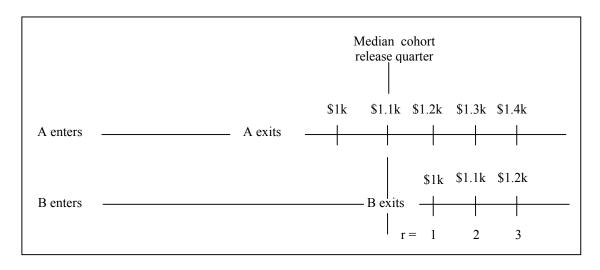


Figure 2. Raw earnings profiles in real-time around the actual prison release quarter by education group.

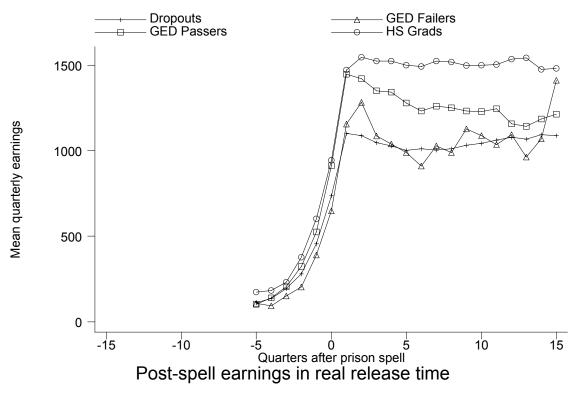


Figure 3. Raw earnings profiles in r-time around the median cohort prison release quarter by education group.

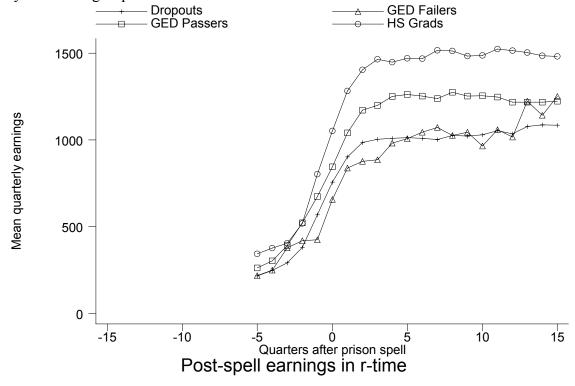


Figure 4. Raw earnings profiles in s-time around the prison entry quarter by education group.

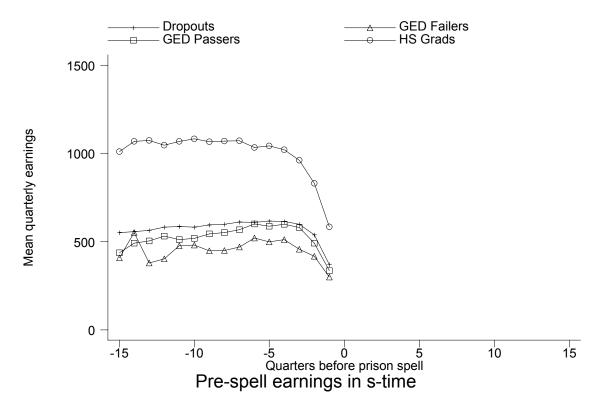


Figure 5a. Pre-spell earnings contrasts between GED passers and non-attempting dropouts (95% CIs enclose estimates).

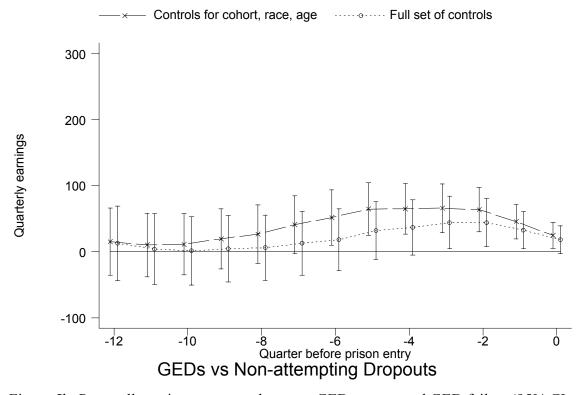


Figure 5b. Pre-spell earnings contrasts between GED passers and GED failers (95% CIs enclose estimates).

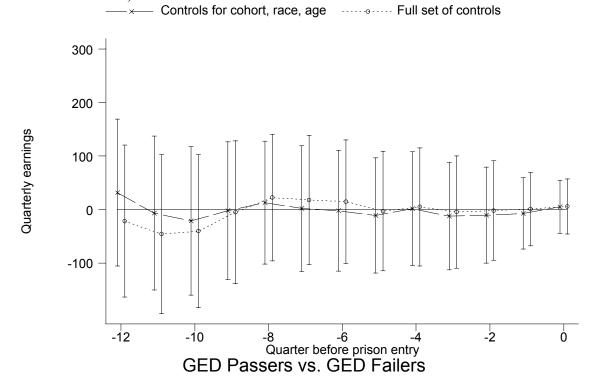


Figure 5c. Pre-spell earnings contrasts between GED passers and high school graduates (95% CIs enclose estimates).

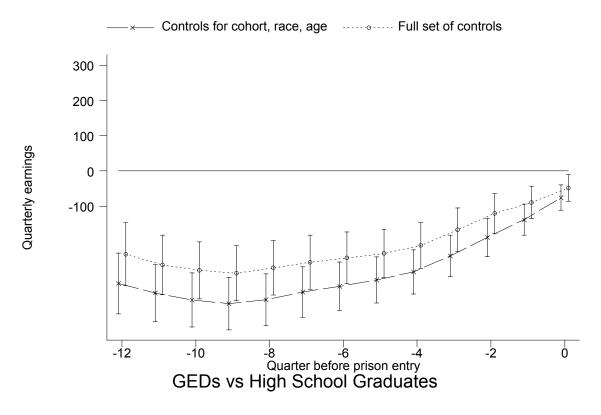


Figure 6a. Pre-spell employment probability contrasts between GED passers and non-attempting dropouts (95% CIs enclose estimates).

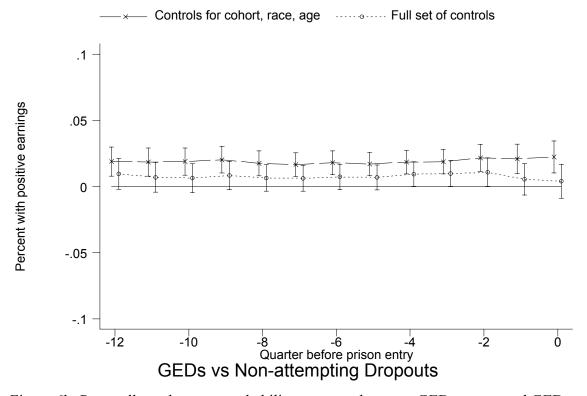


Figure 6b. Pre-spell employment probability contrasts between GED passers and GED failers (95% CIs enclose estimates).

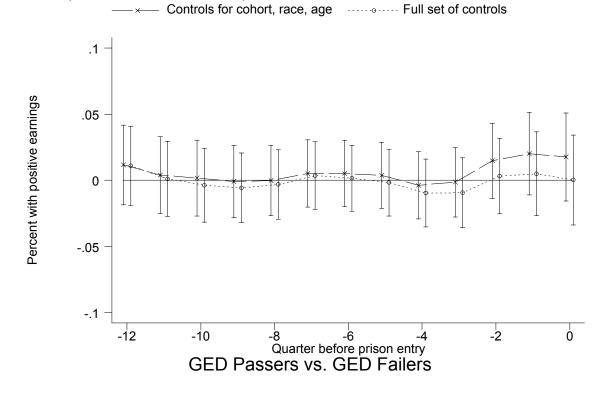


Figure 6c. Figure 6a. Pre-spell employment probability contrasts between GED passers and high school graduates (95% CIs enclose estimates).

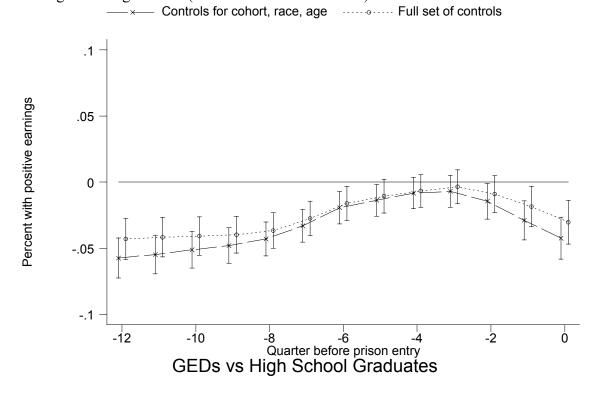


Figure 7a. Post-release earnings contrasts between GED passers and non-attempting dropouts (95% CIs enclose estimates).

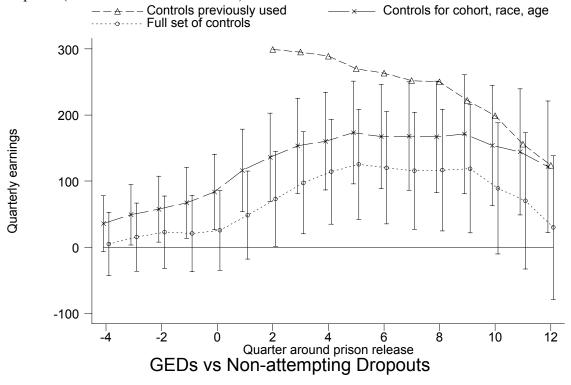


Figure 7b. Post-release earnings contrasts between GED passers and GED failers (95% CIs enclose estimates).

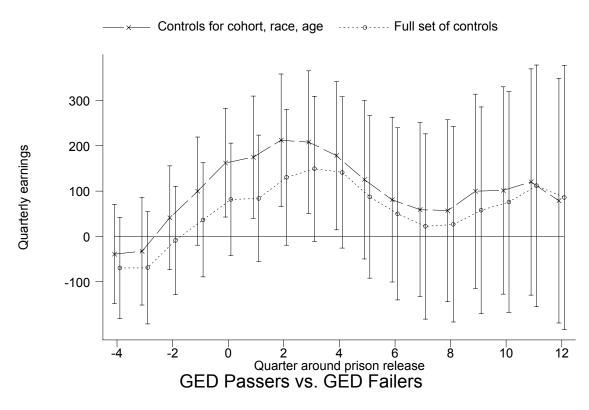


Figure 7c. Post-release earnings contrasts between GED passers and high school graduates (95% CIs enclose estimates).

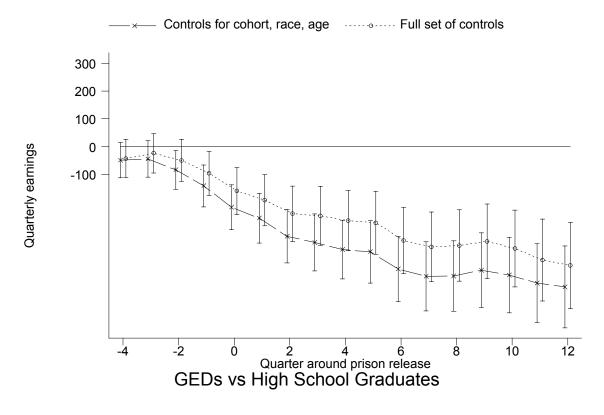


Figure 8a. Post-release employment contrasts between GED passers and non-attempting dropouts (95% CIs enclose estimates).

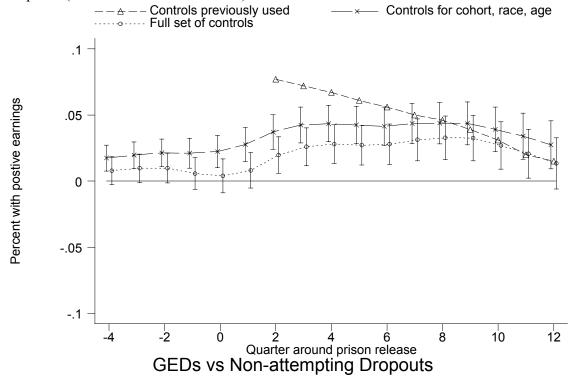


Figure 8b. Post-release employment contrasts between GED passers and GED failers (95% CIs enclose estimates).

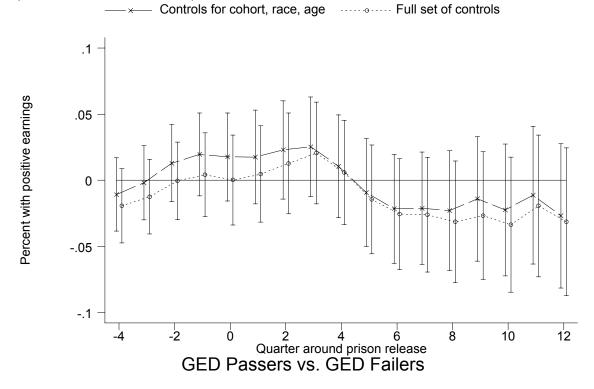


Figure 8c. Post-release employment contrasts between GED passers and high school graduates (95% CIs enclose estimates).

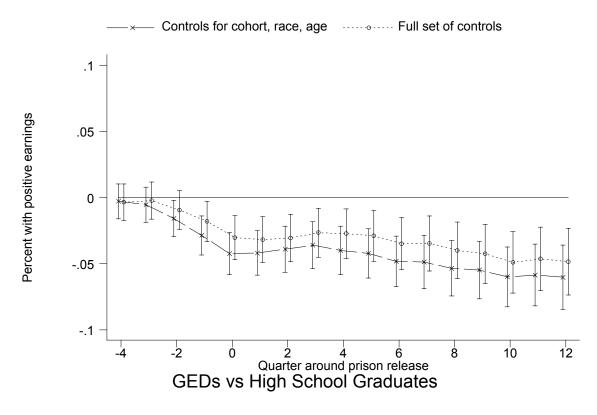
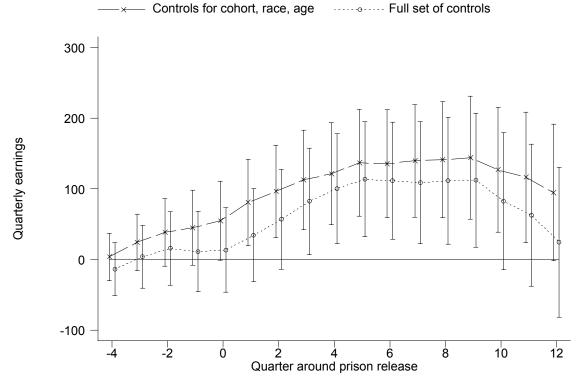


Figure 9a. Post-release earnings contrasts between GED passers and non-attempting dropouts who entered prison in 1996 or later, controlling for pre-prison earnings and employment dynamics (95% CIs enclose estimates).



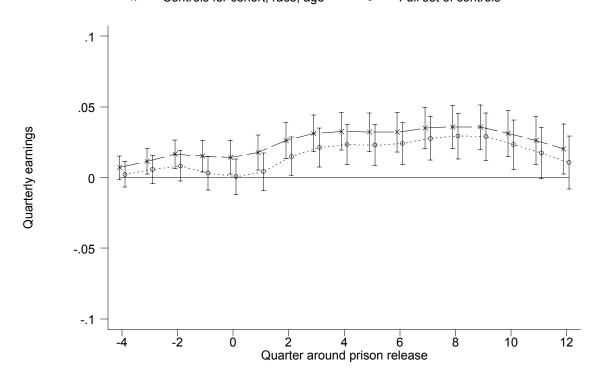


Figure 10a. Pre-spell earnings contrasts for white and minority group individuals based on the full set of controls in equation 1 (95% CIs enclose the estimates).

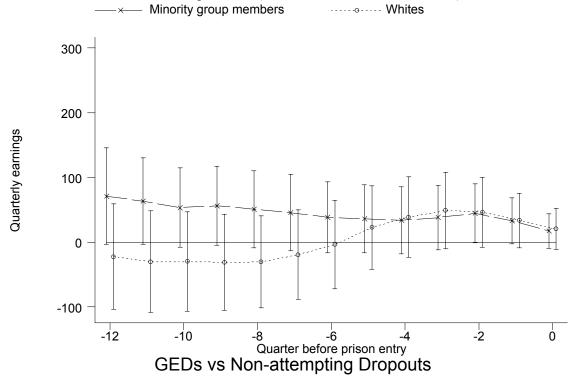


Figure 10b. Pre-spell employment contrasts for white and minority group individuals based on the full set of controls in equation 1 (95% CIs enclose the estimates).

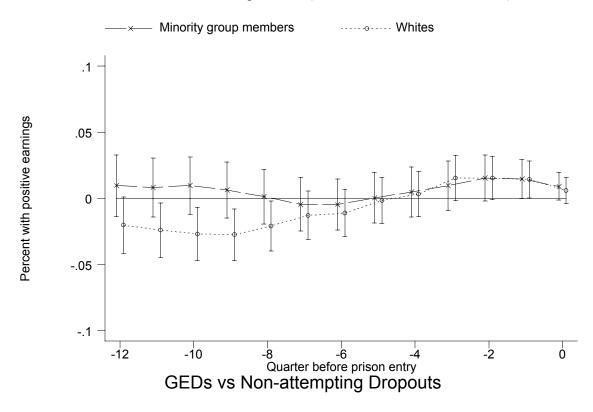


Figure 11a. Post-release earnings contrasts between GED passers and non-attempting dropouts for minority group members and whites, controlling for pre-prison earnings and employment dynamics (95% CIs enclose estimates).

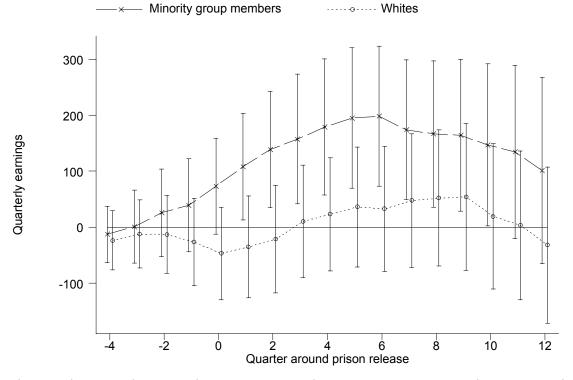
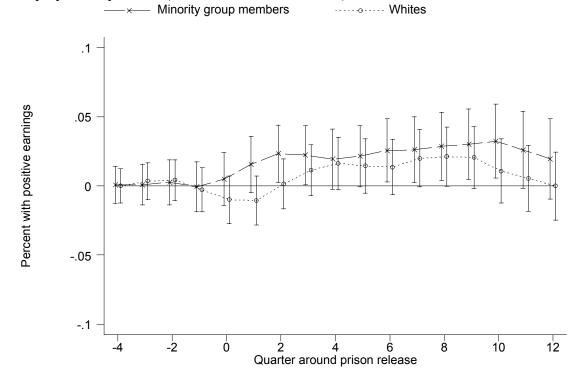


Figure 11b. Post-release employment contrasts between GED passers and non-attempting dropouts for minority group members and whites, controlling for pre-prison earnings and employment dynamics (95% CIs enclose estimates).



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