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Abstract
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Cover Page Footnote
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DOES THE G.E.D. IMPROVE EARNINGS? ESTIMATES FROM A SAMPLE OF BOTH SUCCESSFUL AND UNSUCCESSFUL G.E.D. CANDIDATES

JOHN H. TYLER*

The author uses a unique data set constructed from Florida General Education Development certificate (GED) and Unemployment Insurance (UI) administrative records to estimate the relative earnings of two groups of male high school dropouts who took the GED exams in 1995: those who passed the exams, obtaining the credential, and those who failed the exams. Estimates from several different specifications all show greater mean quarterly earnings growth among those who obtained a GED than among unsuccessful candidates. The advantage was small in dollar terms—only about $1,400 annually, six years after the exams—but, given the very low initial earnings of these school dropouts, it was large in relative terms, growing from zero immediately after the 1995 exams to 13–20% six years later. Most of these gains appear to reflect not differential wage growth between the two groups, but a differential rate of movement from non-employment to employment.

The General Education Development certificate (GED) is the primary academic certificate for school dropouts in this nation; each year about 500,000 obtain the credential. One reason many dropouts pursue the GED is a belief that it will lead to greater labor market success. Several studies over the past decade have attempted to determine whether that belief is realistic, with most focusing on the value of this credential to the random dropout in the population.

The purpose of this paper is to augment what we know about the GED by estimating the post-exam quarterly earnings of male dropouts who seek to obtain the credential—both those who successfully earn the credential and those who try but fail to earn it. While this parameter does not inform us as to the value of the GED for the random dropout, it is nevertheless of considerable

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To honor promises of confidentiality, the author cannot share the data used in this paper, which are from a specially constructed data set put together for the project by several state agencies in Florida.

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policy interest. Since substantial public monies fund GED preparation programs, we should know whether there is a payoff to the credential among those dropouts who try to obtain it. To answer this question, I employ a specially constructed panel data set that contains GED test scores, basic demographic variables, and administrative earnings records in both the pre- and post-GED-attempt periods for a sample of dropouts who all took the GED exams. The sample represents all dropouts who last took the GED exams in Florida in 1995. With these data I am able to employ four different specifications that are all focused on the same question: what is the value of the GED for dropouts who pursue this credential?

The GED and the Earnings of Dropouts

Cameron and Heckman (1993) first drew attention to the fact that male GED-holders are not the labor market equivalents of regular high school graduates. Results in that paper also indicated that while the estimated wages of GED holders fell somewhere between those of regular high school graduates and uncredentialed dropouts, they were closer to the latter. Studies that followed took up the question of how GED holders compare to dropouts in the labor market who lack the credential. That is, once the dropout decision has been made, does the GED buy you anything in the labor market? While the answer has been somewhat mixed, two recent studies—differing from each other both in the data set used and in empirical strategy—offered evidence that low-skilled GED holders have higher earnings than comparably low-skilled dropouts who lack a GED (Murnane, Willett, and Tyler 2000; Tyler, Murnane, and Willett 2000). In addition, Murnane, Willett, and Tyler (2000) discussed how these more recent results can be reconciled with the earlier Cameron and Heckman findings. Heckman, Hsse, and Rubinstein (2000), on the other hand, found that failing to control for time-invariant heterogeneity between low-skilled credentialed and uncredentialed dropouts may lend an upward bias to estimates of a GED treatment effect. Thus, the current literature offers suggestive evidence on how GED holders compare to uncredentialed dropouts, but no definitive answer.

Included among the data used in this study are the quarterly earnings of dropouts who attempt, with or without success, to acquire a GED credential. For those dropouts who pass the GED exams, there are several mechanisms through which acquisition of a GED could affect their quarterly earnings. First, employers may use the GED as a signal of productive attributes in a pool of dropouts, choosing to hire GED holders over uncredentialed dropouts (Spence 1973). If this is the case, then we can expect to see higher employment rates among the GED holders. Also, for those dropouts who become employed, employers may use the GED as a positive signal in assigning wages, hours of work, or both. In all cases we would expect the earnings of GED holders to be higher than the earnings of uncredentialed dropouts. There could, however, be a delay in GED-related earnings advantages in this signaling story if the jobs employers assign GED holders have lower initial wages and a steeper age-wage profile than the jobs they assign uncredentialed dropouts.

Second, there could be a human capital impact on wages. Dropouts obtain a GED by gaining a passing score on a five-test examination battery that takes about seven and one-half hours to complete.[1] If school dropouts have to work to increase their cognitive skills in order to achieve a passing score on the GED exams, then human capital theory (Becker 1993) would lead us to expect wages for these dropouts that are higher than they otherwise would have been.[2] Assuming there is no wages-for-

[1]The GED exams 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.

[2]The only data on GED preparation time indicated that the average study time was only about 30 hours (Baldwin 1990), probably too little for substantial

hours tradeoff, higher wages mean that the quarterly earnings of GED holders could increase immediately after receipt of a GED under a human capital model if employers have good information about the skill levels of the dropouts they hire. As with the signaling model, however, one would expect a delay before the quarterly earnings of GED holders are appreciably higher than those of uncredentialed dropouts if fully informed employers tend to assign the more highly skilled dropouts to jobs with lower initial wages but steeper age-wage profiles than less skilled dropouts. There would also be a delay in the appearance of any wage differences if employers lack perfect initial information but learn about workers' skills over time, or if new GED holders, in the quarters after they obtain their credential, engage in more intensive job search than do uncredentialed dropouts.

Third, GED holders may use the credential to gain access to and funding for post-secondary education.[3] To the extent that GED holders are in post-secondary education programs in the years after they acquire the credential, we would expect their employment rates, their hours of work, and, possibly, their wages to be lower than in later years when their additional education affects their labor market outcomes.

There is also a non-causal explanation for a positive GED-earnings correlation. Assume that weak labor market connection, motivation, and productivity mark the immediate post-dropout years. Now assume that at some point a subset of "reformed" dropouts begin to look for better jobs, exhibit more motivation, display more commitment to work, and also decide to obtain a GED. In this case one would expect a positive GED-earnings correlation even in the absence of a GED treatment effect.

To eliminate this type of selection bias, the ideal would be to use data from an experimental design or data that include the variables necessary to accurately model the GED selection process. Unfortunately, neither type of data currently exists. Potential selection bias in this paper is, however, mitigated in several ways. First, estimates in this paper are based on data covering dropouts who have all tried to earn a GED. As a result, any unobservable differences between GED holders and uncredentialed dropouts that are related to the decision to *attempt* to obtain a GED are eliminated by the nature of the data.[4]

A second advantage the data provide is the ability to observe several years of pre-GED employment and earnings. Studies of job training recipients (Ashenfelter and Card 1985; Heckman 1998) and of displaced workers (Jacobson, LaLonde, and Sullivan 1993) have found that accounting for pre-treatment earnings and employment histories tends to reduce bias from transitory shocks that may be related to treatment status. As I explain in the next section, one of the four empirical specifications in this study uses the pre-GED attempt data to reduce or eliminate bias related to pre-treatment earnings or employment shocks.

In the end, however, selection bias is still a concern if any unobserved heterogeneity

human capital accumulation. However, the study did not distinguish between successful and unsuccessful candidates, nor is it likely that time spent in Adult Basic Education, English as Second Language, or pre-GED courses was included in the respondents' calculations of their time spent "studying for the GED." As a result, 30 hours could be an underestimate of the total time spent in preparing to take and pass the GED exams.

[3]Most degree-granting post-secondary education programs require applicants to possess a high school diploma or a GED. Also, Pell grants and guaranteed federal student loans for post-secondary education require applicants to demonstrate an "ability to benefit" from the funding. Dropout applicants for these federal monies can satisfy this requirement if they possess a GED.

[4]The discussion in the literature usually characterizes the problem as one of selection on positive unobservables. However, Heckman, Hsse, and Rubinstein (2000) raised the possibility that GED holders have negative unobservables relative to uncredentialed dropouts. The data used in this study control for both types of heterogeneity.

(1) is time-varying and (2) is not captured by pre-GED employment and earnings histories or by GED test scores. In a later section of the paper I return to a discussion of the biases that may result from this type of heterogeneity.

Methods

In estimating the impact of obtaining a GED on the earnings of dropouts who seek the credential, I present estimates from four different specifications. The data are discussed at length in the next section. Briefly, I use a balanced panel data set containing quarterly earnings from the second quarter of 1993 through the first quarter of 2002 of male dropouts in Florida who last took the GED exam in 1995.

Estimates from the first model compare the quarter-by-quarter earnings of successful and unsuccessful GED candidates, regression-adjusting for differences in observable characteristics, including GED test scores. This method relies primarily on the presence of GED test scores to account for any differences between successful and unsuccessful candidates. A second approach also looks at quarter-by-quarter earnings differences and contains all of the control variables of the first model. However, this model adjusts for differences in earnings and employment dynamics prior to the GED attempt by including as regressors log earnings and employment indicators by quarter for quarters –7 through –1 before the GED attempt.[5] The third empirical strategy controls for time-invariant heterogeneity between successful and unsuccessful GED candidates by estimating a fixed effects model over the panel of data. Finally, a regression discontinuity approach takes advantage of the fact that a sharp cutoff in GED test scores determines GED status.

The empirical analysis is based on a pool of dropouts who have all taken the GED exams. Let Y_{it} denote the total quarterly earnings for individual i at time t. Let $EverGED_i = 1$ if individual i ever passes the GED exams and obtains the credential, and zero otherwise. Since the data yield information on Y_i only every quarter, let T_t be a set of year-quarter dummy variables that capture the effect of calendar time on the outcome. Finally, let X_i denote a set of other covariates, including age when earnings are measured, race/ethnicity, and years of completed education, and let S_i include all GED test scores and their squares.[6] The basic regression-adjusted estimating equation is

$$(1) \quad Y_{it} = EverGED_i\gamma + X_{it}\beta + S_i\theta + T_t\lambda + \varepsilon_{it},$$

where i indexes person and t indexes the quarter relative to the GED attempt in which Y is measured, and $t \in [-6, -5, \ldots, 0, \ldots, 24]$.

Equation (1) is fit separately for each of the 31 available quarters of data (–6 through 24) around the GED attempt. While there are actually 33 quarters of earnings in the data, each regression is based on a pooled three quarters of data (t–1, t, t+1) in order to reduce sampling variability.[7] Standard errors in the pooled regression are adjusted to account for the fact that an individual's error terms are correlated over the three quarters. When GED status is uncorrelated with ε, estimates of γ in (1) represent the effect of ever obtaining a GED on the outcome as measured in the tth quarter. The primary assumption in this model is that the inclusion of GED test scores (along with the covariates in X) accounts for all differences between successful and unsuc-

[5]Estimates that do not use earnings and employment histories in the four quarters just prior to the GED are very similar to the estimates presented in this paper that do include the relevant variables from these quarters in the regressions. The alternative estimates are available from the author upon request.

[6]Individuals who failed the GED exam on a first attempt and made a later attempt will have two sets of GED test scores, initial and final scores. Both sets of test scores and their squares are entered for these individuals. Final scores are set equal to initial scores for individuals who only tested once.

[7]The results are not sensitive to using a specification that pools data across three quarters versus simply using the data from a single quarter. For an example in empirical work of the use of pooled data on three quarters of UI earnings, see Kling (2002).

cessful candidates that affect outcomes.

Even after we control for the effect of GED test scores and the observable characteristics in X, however, there remains the possibility that individuals who pass the GED exams have permanent unobservable characteristics that distinguish them from unsuccessful GED exam-takers and thus lead to different labor market outcomes. In this case, eventual GED status and ε are positively correlated, and γ is biased.

The next two models offer different strategies to control for this potential source of bias. In the first case I use pre–GED-attempt earnings and employment outcomes as an indication of permanent differences between eventually successful and eventually unsuccessful candidates that would have affected later outcomes even in the absence of the GED. In this model pre-attempt data from quarters –7 through –1 are pooled with data from the post-attempt quarter of interest. This model is

$$(2) \qquad Y_{it} = EverGED_i\gamma^{\sim} + X_{it}\beta^{\sim} + S_i\theta^{\sim} + T_t\lambda^{\sim} + E_{it^{\sim}}\wp + \varepsilon^{\sim}_{it},$$

where $t \in [2,\ldots,24]$, $t^{\sim} \in [-7,\ldots,-1]$, and E is a vector capturing pre-GED-attempt employment and earnings dynamics for the ith person in quarters –7 through –1. As with equation (1), I fit separate pooled three-quarter regressions to reduce sampling variability, this time focusing on quarters 2–24 after the GED attempt. Correct inferences are obtained with (2) if pre-attempt earnings and employment histories as represented by E effectively capture differences among GED candidates that are related both to eventual GED status and to future labor market outcomes. The E vector in equation (2) contains, for quarters –7 to –1 prior to the GED attempt, indicators for positive earnings in each quarter, log quarterly earnings for each quarter (zero when total earnings are zero in the quarter), and an indicator for no positive earnings in any quarter prior to the GED attempt.[8]

[8]Results based on equation (2) are essentially the same if earnings and employment histories in the year just prior to the GED attempt (quarters –4 through –1) are omitted in the construction of E.

The next estimation strategy controls for all time-invariant heterogeneity by estimating the fixed effects model specified in equation (3).

$$(3) \qquad Y_{it} = \alpha_i + Quarter_{tt}\phi_1 + Quarter^2_{tt}\phi_2 + Age_{it}\beta^*_1 + Age^2_{it}\beta^*_2 + T_t\lambda^* + HaveGED_{it} \cdot [\gamma^*_1 + Quarter_{tt}\gamma^*_2] + \varepsilon_{it},$$

where $t \in [-6, -5\ldots, 24]$. To estimate this equation, I stack the data in a person-quarter data set and fit the model across all available quarters. The time variable *Quarter* can range from quarter –6 through 24, and *Age* is the age in years of individual i in quarter t. The variable *HaveGED* equals zero in all quarters before the GED attempt for everyone and one in the quarters after the GED attempt for those who pass. The ϕ's capture the relationship between time (in quarters) and earnings, while the γ^*'s capture increments to the level and slope of the earnings-time profile for successful GED candidates once they obtain a GED relative to the level and slope of this profile for uncredentialed dropouts. α_i captures individual fixed effects in this model.[9] Correct inferences are obtained if the model is specified correctly and if all differences between successful and unsuccessful candidates are time-invariant and, hence, contained in α_i.

A last strategy exploits the fact that obtaining a GED is a function of a sharp cutoff in the mean GED test score, conditional upon a minimum score of at least 40 on each of the five individual tests. Equation (4) models this regression discontinuity approach, and it is fit over individuals who have a score of at least 40 on each of the five tests in the GED battery. In this equation the variable *Mean* captures the linear effect

[9]More complex models in which the earnings-time profile for unsuccessful dropouts was allowed to be different on either side of the GED attempt, as well as models in which GED acquisition was allowed to affect both the slope and the curvature of the earnings-time relationship, were fit. All inferences regarding earnings differences between successful and unsuccessful dropouts were the same as in the more parsimonious model represented in equation (3).

of the mean GED test score on earnings, while *EverGED* allows for a discontinuity in the earnings-mean score conditional regression line at the GED passing threshold.

$$Y_{it} = EverGED_i\gamma' + X_{it}\beta' + Mean_i\theta + T_t\lambda' + \varepsilon'_{it}, \quad (4)$$

with $t \in [2,\ldots,24]$. As with equation (2), I fit separate regressions for each quarter from 2 through 24 after the GED attempt, using pooled three-quarter regressions to reduce sampling variability. Since the conditional earnings–mean score relationship may not be linear over the entire range of GED mean scores, equation (4) is fit using individuals whose mean score is five points on either side of the 45 mean score passing threshold.

Data

The Florida Education and Training Placement Information Program (FETPIP) constructed the data set used in this study by linking GED test information with quarterly earnings records collected by Florida's Unemployment Insurance (UI) system. Florida was chosen because of the opportunity to produce such a data file. Centralized electronic file storage of GED records is a relatively recent occurrence in many states. Florida is the only state (other than Texas) where the records of GED candidates who last tested as early as 1995 can be matched to UI earnings.[10]

While the results from this study will not be based on a nationally representative sample of GED candidates, Florida does offer some attractive research design features in addition to the linked GED-UI data. First, a relatively large number of GEDs nationally are awarded in Florida. In 1995 the 37,000 GED credentials issued in Florida ranked only behind the number issued in Texas (57,800), California (39,300), and New York (39,300). About 6% of all GED credentials issued in the United States that year went to Florida residents (GED Testing Service 1995). Second, while caution should be exercised in generalizing the results from one state to other areas of the United States, there is no strong evidence suggesting that a study of the labor market outcomes of Florida GED candidates would be uninformative for what might happen elsewhere. For example, unemployment rates and high school graduation rates in Florida were very close to mean and median national rates during the relevant time period of this study.

For this study the GED records of all male GED candidates who last tested in 1995 were matched with the UI earnings records of these individuals from the second quarter of 1993 through the first quarter of 2002. The resulting data set contains basic demographics, quarterly earnings, and both initial and final scores on the five tests that comprise the GED test battery. Individuals who had no quarterly earnings in a given quarter were assigned earnings equal to zero.[11] All the men covered by the data were between the ages of 16 and 40 when they attempted the GED. Individuals who attempted the GED exams while incarcerated were excluded, for two reasons. First, many of these men may still have been in prison instead of the regular labor market months or years after they attempted to pass the GED exams. Second, dropouts who enter the labor market with both a GED and a criminal record may differ markedly from other GED-holding dropouts in the relationship between the GED and la-

[10]All earnings are deflated to 1999 constant dollars using the CPI-U deflator.

[11]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 that may be "off the books" such as domestic service or informal child care, or jobs provided by 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) showed 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).

bor market outcomes. This sample restriction reduced the number of white candidates by about 7%, the number of Hispanic candidates by 6%, and the number of black candidates by 24%.

In these data, time (T) is measured in calendar quarters, since this is how the earnings data were collected. The dependent variable in all regressions is total quarterly UI earnings from any and all employers in the quarter. An inherent weakness in using quarterly earnings as a dependent variable is the inability to disentangle hourly or weekly wages from hours of work during the quarter, since earnings are a function of both. However, UI earnings data have at least three advantages: the dependent variable comes from administrative records, rather than self-report; UI data are collected more often (quarterly) than are survey data (usually annually at best); and sample attrition is less a problem in administrative data than in survey data. These advantages and the increasing availability of state UI data have resulted in the increasing use of UI quarterly earnings for research purposes. One notable example is Jacobson, LaLonde, and Sullivan's (1993) study of the earnings losses of displaced workers.

In addition to quarterly UI earnings and GED test scores, the data used in this study contain age at the time the outcome is measured and the year and quarter in which the GED exam was last taken. Inclusion of these variables implicitly controls for the dropout's age at the time he took the GED exam. The data also contain years of completed schooling and a set of indicator variables for race/ethnicity and for whether the GED exam was taken more than once.[12]

Descriptive statistics for the data are provided in Table 1. The complete data set contains 16,304 GED candidates. White GED candidates make up about two-thirds of the sample. Successful candidates are slightly younger than unsuccessful candidates, but both groups completed about ten years of schooling before dropping out of school. The mean test scores of successful candidates are substantially higher than those of unsuccessful candidates.

About four out of every five male Florida candidates successfully obtained their GED. White candidates made up about 70% of the successful candidates, but only 49% of the unsuccessful candidate pool. In contrast, black dropouts comprised a much larger proportion of the unsuccessful than of the successful candidates.

In the first quarter after the GED attempt the mean quarterly earnings of successful and unsuccessful candidates differed by only about $40 in favor of dropouts who failed the GED exams. The unadjusted mean quarterly earnings of successful candidates were, however, $360 higher than those of unsuccessful candidates by the 24th quarter after the GED attempt. While this is a relatively small difference in earnings levels (about $1,400 annually), it represents a 15% difference in earnings. The very low quarterly earnings levels of all GED candidates are explained by the last rows in the table. The percentage of GED candidates with positive earnings rose from about 40% in the quarter after the attempt to around 53% in the 24th quarter after the attempt. Clearly, nonemployment is a major reason for the depressingly low earnings of dropouts who attempt to earn a GED.[13]

Results

As a starting point, the raw earnings dynamics of GED candidates in Florida before and after the GED attempt are summarized in Figure 1.[14] Several facts emerge

[12]Individuals who fail the GED exams can, with certain restrictions, retake them. In these data about 20% of the individuals took the GED exams more than once.

[13]I infer nonemployment from zero quarterly earnings. I cannot determine which individuals were in the labor market but on leave or unemployed, and which individuals were out of the labor market.

[14]Figure 1 looks very similar when plotted separately for white non-Hispanic, black non-Hispanic, and Hispanic candidates. The only exception is that the graphs for black non-Hispanic candidates lie below those for white and Hispanic candidates.

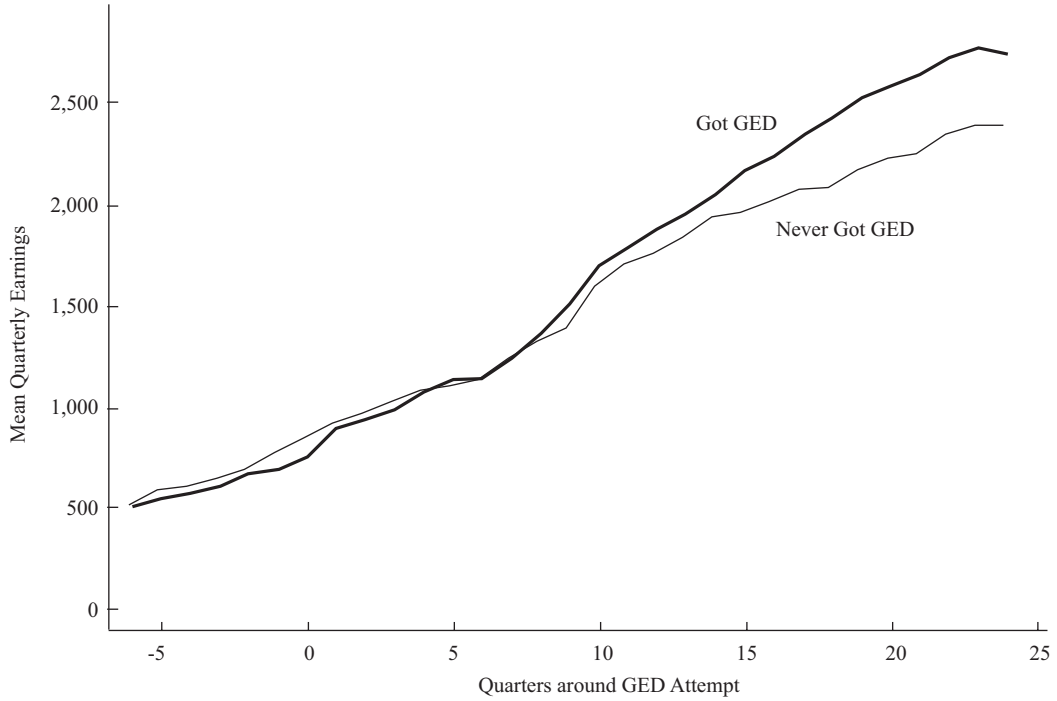
Table 1. Descriptive Statistics of the Florida Universe of GED Candidates Who Last Tested in 1995. (Standard Deviations in Parentheses)

Description of Statistic	*Statistic*
Sample Size and Race/Ethnicity at GED Attempt	
Total Number of Observations, N	16,304
Percent of N Who Are…	
White, Non-Hispanic	66.3
Black, Non-Hispanic	15.1
Hispanic	18.6
Demographic and GED Test Score Information	
Median Age at the GED Test Attempt:	
Successful Candidates	19.5
Unsuccessful Candidates	20.5
Years of Completed Schooling:	
Successful Candidates	10.2 (1.1)
Unsuccessful Candidates	10.1 (1.2)
Mean GED Test Score:	
Successful Candidates	52.0 (5.3)
Unsuccessful Candidates	39.8 (5.4)
GED Attainment	
Percent of N Who Successfully Obtain a GED	81.3
Percent of Successful Candidates Who Are…	
White, Non-Hispanic	70.4
Black, Non-Hispanic	11.5
Hispanic	18.2
Percent of Unsuccessful Candidates Who Are…	
White, Non-Hispanic	48.5
Black, Non-Hispanic	31.2
Hispanic	20.3
Quarterly Earnings	
Mean Quarterly Earnings in 1st Quarter after GED Attempt:	
Successful Candidates	881 (1,745)
Unsuccessful Candidates	917 (1,730)
Mean Quarterly Earnings in 24th Quarter after GED Attempt:	
Successful Candidates	2,710 (3,584)
Unsuccessful Candidates	2,350 (3,171)
Percent with Positive Earnings in 1st Quarter after GED Attempt:	
Successful Candidates	39.5
Unsuccessful Candidates	38.2
Percent with Positive Earnings in 24th Quarter after GED Attempt:	
Successful Candidates	53.4
Unsuccessful Candidates	50.6

Source: Florida GED and Unemployment Insurance (UI) administrative records.

from this figure. The first is that the unadjusted pre-attempt earnings of dropouts who eventually passed the GED exams and of those who never passed are essentially the same. Second, no immediate jump in unconditional mean earnings is evident for those who passed the tests. Third, the quarterly earnings of successful GED candidates—those who obtained the credential—begin to diverge from the earnings of unsuccessful candidates around the 9th post-attempt quarter.

Figure 1. Mean Quarterly Earnings in the Quarters before and after the GED Attempt, by Eventual GED Status. (Raw Earnings Comparisons)

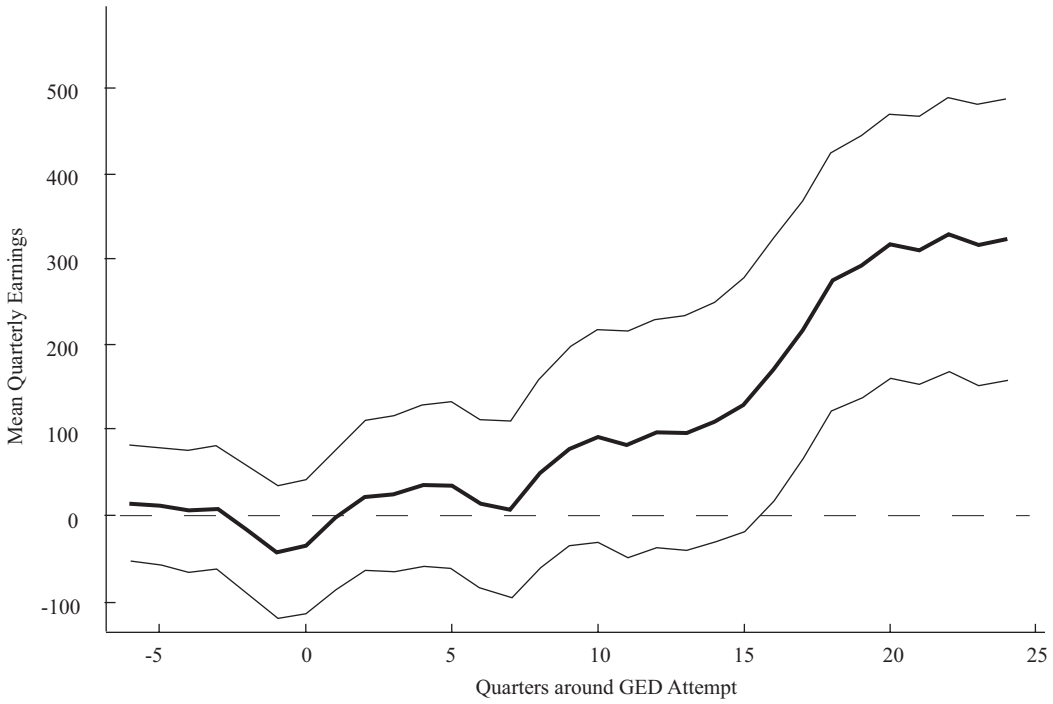


While Figure 1 is informative, the plotted estimates do not control for the effects that observable factors might have on earnings. To account for this, a series of regression models based on equation (1) are fit for each of $t = -6, -5, \ldots, 24$. Estimates of γ by quarter on either side of the GED attempt, along with 95% confidence bands, are plotted in Figure 2.

Figure 2 indicates that there were essentially no differences in regression-adjusted quarterly earnings between successful and unsuccessful GED candidates in the quarters before the GED attempt. There is some evidence that the quarterly earnings of eventually successful GED candidates declined relative to the earnings of eventually unsuccessful candidates in the quarters just prior to the GED, though none of the pre-attempt differences are statistically significant.[15] Around the 8th quarter after the GED attempt, the estimated mean quarterly earnings of successful candidates began to grow relative to those of unsuccessful candidates. By the 24th quarter after the GED attempt the difference was a statistically significant $313, indicating that only about 13% of the $360 raw earnings differ-

[15]While the pre-treatment decline in relative earnings seems to reflect a classic "Ashenfelter dip" (Ashenfelter and Card 1985) in earnings, it is important to remember that *both* successful and unsuccessful candidates select into the GED program, complicating an "Ashenfelter dip" story for Figure 2. An alternative explanation for the observed dip is that GED candidates who will eventually pass the exams trade hours of work for GED preparation time, lowering their pre-treatment relative earnings. Unfortunately, there are no data that would allow an examination of this hypothesis.

Figure 2. Estimates by Quarter around the GED Attempt of γ, Quarterly Earnings Contrasts, from Equation (1), along with 95% Confidence Bands. (Regression-Adjusted Earnings Comparisons)



Note: γ comes from a model that controls for race/ethnicity, age when the GED was last attempted, year and quarter when earnings were measured, and GED test scores and their square.

ence in the 24th quarter in Table 1 can be explained by the observable factors, including GED test scores, that are included in equation (1). Estimates and standard errors by quarter after the GED attempt based on (1) are displayed in the first column of Table 2.

Estimates based on equation (1) will be biased if the variables in X and S do not capture all differences between successful and unsuccessful GED candidates that are related to post-attempt labor market outcomes. To the extent that any such differences are reflected in *pre-attempt* earnings and employment histories, estimates based on equation (2) should diverge from those based on equation (1). Figure 3 illustrates quarter-by-quarter estimates of γ from equation (2) along with 95% confidence bands for quarters 2 through 24 after the GED attempt, while the second column of Table 2 provides the point estimates and standard errors by quarter. The estimates based on equations (1) and (2) are very similar, indicating either that there is no omitted variable bias in equation (1) or that adding pre-attempt earnings and employment dynamics does a poor job of controlling for unobserved differences between successful and unsuccessful dropouts.[16]

[16]A third explanation could be that the lagged earnings variables are correlated with both *EverGED*

Table 2. OLS Estimates of Quarterly Earnings Contrasts Based on Equations (1), (2), and (4). (Standard Errors in Parentheses)

	Model[a]				*Model*[a]		
Quarters after the GED Attempt	*Equation (1) Estimates of* γ	*Equation (2) Estimates of* $\tilde{\gamma}$	*Equation (4) Estimates of* γ'	*Quarters after the GED Attempt*	*Equation (1) Estimates of* γ	*Equation (2) Estimates of* $\tilde{\gamma}$	*Equation (4) Estimates of* γ'
2	20	52	112	14	103	118*	219
	(44)	(36)	(96)		(70)	(67)	(154)
3	23	53	145	15	123	137	284*
	(46)	(39)	(98)		(74)	(71)	(159)
4	32	59	153	16	163**	177**	370**
	(48)	(42)	(101)		(77)	(74)	(164)
5	31	56	143	17	210***	224***	412**
	(49)	(44)	(104)		(77)	(74)	(167)
6	10	31	94	18	266***	280***	467***
	(50)	(45)	(108)		(77)	(74)	(166)
7	3	22	84	19	283***	296***	457***
	(52)	(48)	(113)		(78)	(75)	(170)
8	44	61	107	20	308***	321***	483***
	(55)	(51)	(117)		(79)	(76)	(174)
9	73	90	153	21	302***	315***	532***
	(58)	(55)	(124)		(80)	(77)	(177)
10	88	104*	174	22	320***	331***	530***
	(62)	(59)	(131)		(82)	(79)	(184)
11	78	94	184	23	307***	331***	521***
	(66)	(63)	(138)		(84)	(79)	(185)
12	90	106*	176	24	313***	318***	461**
	(67)	(64)	(145)		(84)	(80)	(190)
13	91	107	183				
	(69)	(66)	(150)	N	16,304	16,304	5,231

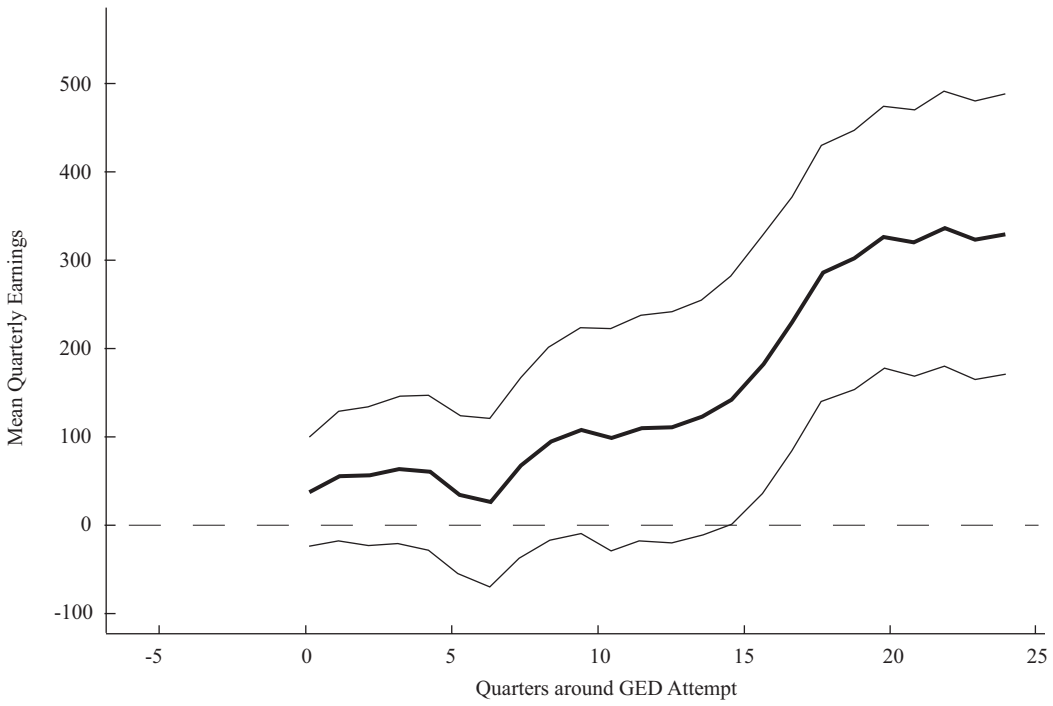
[a]γ comes from a model that controls for race/ethnicity, age when the GED exam was last attempted, year and quarter when earnings were measured, and GED test scores and their square. $\tilde{\gamma}$ comes from a model that adds pre–GED-attempt earnings and employment histories. γ' comes from a regression discontinuity model that controls for race/ethnicity, age when the GED exam was last taken, and year and quarter when earnings were measured, while restricting the sample to those who have a minimum GED score of at least 40 and whose mean GED score is within 5 points on either side of the GED passing standard.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

If there are permanent differences between successful and unsuccessful candidates that affect earnings and are not captured by either *X, S,* or *E,* then estimates based on equation (1) or (2) will provide incorrect inferences. In particular, Heckman, Hsse, and Rubinstein (2000) (hereafter HHR) found that controlling for AFQT scores in NLSY data did not eliminate differences between GED holders and uncredentialed dropouts. Their fixed effects estimates in models fitted over dropouts with low AFQT scores were approximately 50% smaller than regression-ad-

and the error term of equation (2), resulting in a biased estimate of $\tilde{\gamma}$. This explanation is somewhat tenuous, however, since the similarity of the estimates between (1) and (2) suggests that this induced bias would have to be offset by the reduction in heterogeneity bias between (1) and (2) due to the inclusion of the lagged variables.

Figure 3. Estimates by Quarter around the GED Attempt of $\tilde{\gamma}$, Quarterly Earnings Contrasts, from Equation (2), along with 95% Confidence Bands. (Earnings Comparisons Controlling for Pre-Attempt Histories)



Note: $\tilde{\gamma}$ comes from a model that controls for race/ethnicity, age when the GED was last attempted, year and quarter when earnings were measured, GED test scores and their square, and pre-GED attempt earnings and employment histories.

justed estimates that controlled for AFQT score. This suggests that time-invariant heterogeneity could also be a problem for the estimates from either equation (1) or equation (2).

When fit over the entire panel, the fixed effects model of equation (3) controls for time-invariant heterogeneity. Table 3 shows estimates from two versions of equation (3). The first column of Table 3 gives the results from a model similar to that used by HHR, in which acquisition of a GED is constrained so that it only affects the level of post-attempt quarterly earnings. In this model, acquisition of a GED is associated with a $136 mean increase in quarterly earnings, controlling for individual fixed effects.

Past research and the analysis thus far in this paper suggest, however, that acquisition of a GED may affect earnings over time rather than immediately (Murnane, Willett, and Boudett 1997; Tyler, Murnane, and Willett 2000), in which case the model used to generate the estimates in the first column may be mis-specified. The estimates in the second column of the table are from the less constrained version depicted in equation (3) that allows acquisition of a GED to affect the level and slope of the earnings-time profile. Estimates from this model indicate that acquisition of a GED

Table 3. Fixed Effects Estimates Using Equation (3). (Standard Errors in Parentheses)

	Model[a]	
Parameter or Statistic	*Constrained Version of Equation (3)*	*Equation (3)*
Earnings-Time (Quarter) Slope (ϕ_1)	51.8*** (4.0)	60.1*** (4.0)
Earnings-Time (Quarter) Curvature (ϕ_2)	0.42*** (0.15)	–0.21 (0.16)
Indicator for Passing the GED Exams (γ_1^*)	136.4*** (14.7)	–15.5 (16.6)
Increment to the Post-Attempt Earnings-Time Slope for GED Holders (γ_2^*)		19.1*** (1.0)
N	16,304	16,304

[a]The individual fixed effects model of equation (3) contains controls for a general time trend in earnings, year and quarter effects, along with age and age squared.

***Statistically significant at the .01 level.

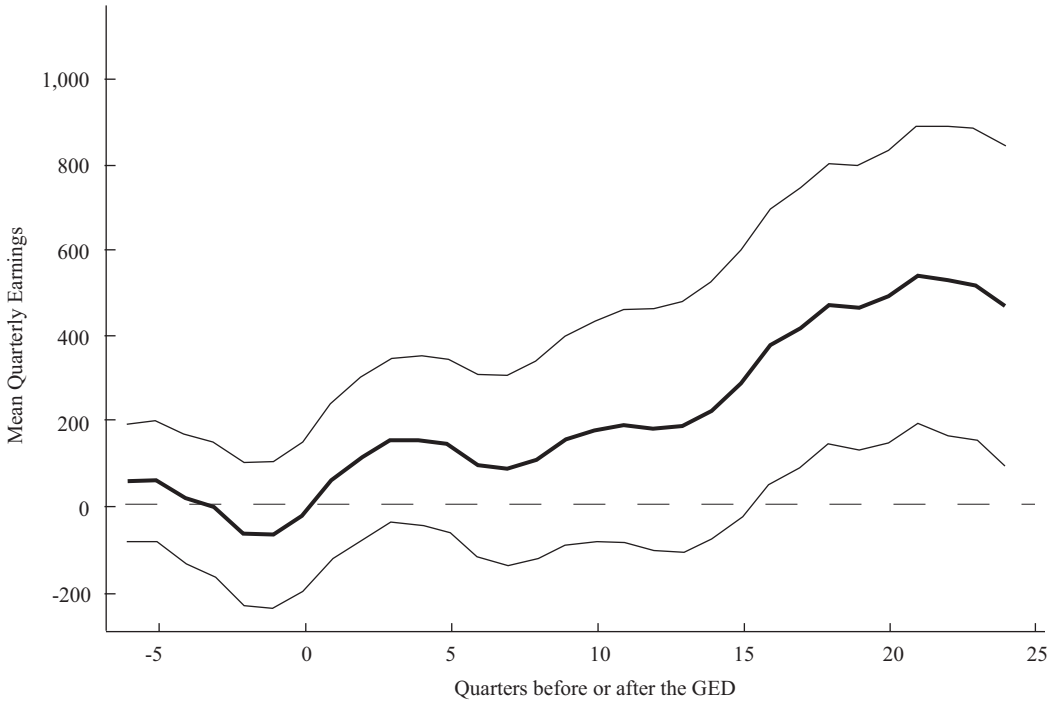
had no immediate effect on quarterly earnings levels (($15 and statistically insignificant). The estimates do indicate, however, that successful GED candidates experienced greater earnings growth after the GED attempt than did candidates who failed to acquire the credential, as evidenced by the estimated increment to the post-attempt slope (γ_2^*) of the earnings-time profile of successful candidates. Based on this model, the predicted earnings advantage for GED holders in the 24th quarter after the GED attempt is about $450.

The analysis to this point has treated GED test scores as simply a measure of cognitive skills similar to the AFQT in the NLSY. However, GED test scores are the determinants of GED status, a fact that can be exploited in a regression discontinuity design. In Florida, GED candidates must have a mean score over the five tests in the battery of at least 45 in combination with a minimum score of 40 on each of the five tests. If acquisition of a GED has an independent effect on earnings, this sharp cutoff suggests that conditional on having all five scores above 40, those with mean scores of 45 or greater (the "passers") should have higher earnings than GED candidates with mean scores below 45, even after controlling for the linear effect of test score on earnings. That is, there should be a vertical discontinuity in the conditional earnings-mean score regression line at the passing threshold of 45. Equation (4) tests this hypothesis.

This model is fit using only individuals who have no scores below 40, and it is restricted to individuals whose mean scores lie within five points on either side of the passing threshold of 45. The model is fit over this relatively tight interval because the linear earnings–mean score relationship assumption is more tenable over a narrower mean score interval. Figure 4 presents quarter-by-quarter estimates of γ' based on three-quarter pooled regressions similar to those used for equations (1) and (2). While the point estimates based on (4) are somewhat larger than estimates from the other models, the pattern of results is the same: earnings differences favoring successful GED candidates grow over time, becoming statistically significant by the 15th quarter after the GED attempt. The point estimates and standard errors used to construct Figure 4 are presented in the third column of Table 2.[17] The key assumption in

[17]The estimated effects are similar, though somewhat larger, when mean score intervals of four points

Figure 4. Estimates by Quarter around the GED Attempt of γ', Quarterly Earnings Contrasts, from Equation (4), along with 95% Confidence Bands. (Regression Discontinuity Comparisons)



Note: γ' comes from a regression discontinuity model that controls for race/ethnicity, age when the GED was last attempted, and year and quarter when earnings were measured, while restricting the sample to those who have a minimum GED score of at least 40 and whose mean GED score is within 5 points on either side of the GED passing standard.

the regression discontinuity model is that, conditional on being in the immediate area of the GED passing cutoff, the slope of the earnings–GED-score regression line is the same on either side of the cutoff.

Estimates from the four models all indicate that successful GED candidates have statistically significant earnings advantages by about the 16th quarter after the GED attempt, and that this advantage continues to grow over the remaining two years for which we have data. An important question from a policy standpoint is how much of the estimated GED-related earnings increase is the result of increasing the earnings of those who would be working anyway, and how much of the effect is due to moving individuals from non-employment to employment. To answer this question I first fit a series of Tobit models for quarters 2 through 24 after the GED attempt based on the specification of equation (1). The resulting estimates are then decomposed into their constituent earnings and employ-

on either side of the passing threshold (41–48) and three points on either side (42–47) are used to construct samples. These results are available from the author upon request.

ment "effects."[18] The results of this analysis are presented in Table 4.

For ease of comparison, the first column of Table 4 redisplays the OLS estimates of γ from Table 2. The second column of Table 4 gives the Tobit estimates of γ, the coefficient on *EverGED* in equation (1). The third column gives the percentage of individuals in the sample who have positive earnings in each quarter. The fourth column gives the portion of the γ estimate resulting from a change in positive earnings weighted by the probability of having positive earnings. One minus the estimate in column (4) gives the portion of the estimate of γ due to a change in the probability of moving from zero to positive earnings, weighted by the expected value of positive earnings. The estimates in column (4) can be interpreted as the portion of the total GED effect that is due to the credential increasing the earnings of those who would have positive earnings, and one minus this estimate is the portion of the total effect due to the GED moving people from non-employment to employment. The lesson from Table 4 is that the primary mechanism through which the GED increases quarterly earnings is by moving dropouts from non-employment into employment. Results in the fourth column of the table indicate that by the 24th quarter two-thirds of the total GED effect on quarterly earnings is due to the increased employment of GED holders.

Assessing the Role of Unobserved Heterogeneity Bias

The results so far indicate that acquisition of a GED leads to greater earnings and a higher probability of employment. The estimated effects indicate that 24 quarters after the GED attempt, dropouts who acquired the credential had a 13–20% quarterly earnings advantage over dropouts who took but failed the GED exams. While these results can be explained by either human capital or signaling theory, they are also potentially due to unobserved heterogeneity. I would argue, however, that heterogeneity explanations must eventually settle on a very particular type of unobserved heterogeneity.

First, it may certainly be the case that dropouts who desire a GED have permanent, unobserved, and productivity-related characteristics differing from those of dropouts who never pursue a GED. However, since the data used in this study only include dropouts who took the GED exams, this type of time-invariant heterogeneity is eliminated.

It may also be the case that dropouts have time-varying, unobserved traits that are associated with the desire to obtain a GED and with labor market potential. For example, a dropout may at some point acquire more maturity, get married, or simply decide it is time to make more money. If these actions were accompanied by a decision to obtain a GED, then acquisition of a GED would be positively correlated with better labor market outcomes even if there were no GED treatment effect. The nature of the data used in this study also controls for this type of time-varying heterogeneity between dropouts who attempt a GED and those who never do so by limiting the comparison to people who have all attempted to earn a GED.[19]

The concern in these data, then, is unobserved heterogeneity—either invariant or time varying—*among dropouts who all take the GED exams.* As discussed earlier, time-invariant heterogeneity is eliminated in the fixed effects model of equation (3). Thus, the primary concern with the estimates has

[18]This decomposition is based on McDonald and Moffitt (1980). I thank an anonymous reviewer for suggesting this improvement to an earlier draft.

[19]HHR found that GED holders often have less desirable observable characteristics than uncredentialed dropouts. In this case GED holders might be expected to have "negative" unobservable characteristics. The nature of these data controls for all unobservable traits associated with the *decision* to obtain a GED, including negative unobservables.

Table 4. Tobit Estimates of Quarterly Earnings Contrasts and the Proportion of the Total Effect That Is Due to Earnings Increases Conditional on Having Positive Earnings. (Standard Errors in Parentheses)

	Column 1	*Column 2*	*Column 3*	*Column 4*
Quarters after the GED Attempt	*Equation (1) Estimates of* γ^a	*Tobit Estimates of* γ^a	*Fraction with Non-Zero Earnings*	*Proportion of Column (2) Estimate Explained by Increased Earnings Conditional on Having Positive Earnings*
2	20	57	0.61	0.49
	(44)	(43)		
3	23	37	0.61	0.48
	(46)	(45)		
4	32	53	0.60	0.48
	(48)	(48)		
5	31	50	0.60	0.47
	(49)	(50)		
6	10	8	0.60	0.45
	(50)	(51)		
7	3	12	0.59	0.49
	(52)	(54)		
8	44	47	0.57	0.44
	(55)	(57)		
9	73	126**	0.55	0.41
	(58)	(60)		
10	88	82*	0.53	0.39
	(62)	(65)		
11	78	83	0.51	0.37
	(66)	(67)		
12	90	103*	0.51	0.37
	(67)	(70)		
13	91	94	0.50	0.36
	(69)	(72)		
14	103	75	0.49	0.35
	(70)	(74)		
15	123	113	0.49	0.36
	(74)	(77)		
16	163**	145*	0.48	0.35
	(77)	(80)		
17	210***	183**	0.48	0.35
	(77)	(82)		
18	266***	302***	0.48	0.35
	(77)	(86)		
19	283***	274***	0.47	0.34
	(78)	(88)		
20	308***	226**	0.47	0.34
	(79)	(89)		
21	302***	325***	0.47	0.34
	(80)	(91)		
22	320***	289***	0.46	0.33
	(82)	(92)		
23	307***	248***	0.46	0.33
	(84)	(93)		
24	313***	326***	0.47	0.34
	(84)	(93)		

[a]γ comes from a model that controls for race/ethnicity, age when the GED was last attempted, year and quarter when earnings were measured, and GED test scores and their square.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

to do with unobserved heterogeneity among GED candidates that is time-dependent. Do successful and unsuccessful candidates experience different, unobserved pre-attempt changes that are related to both eventual passing status and post-attempt labor market potential, but are not captured by the variables in X or S? While this is possible, to the extent that these different changes are related to pre-attempt earnings and employment dynamics in the E vector, estimates from (2) should give correct inferences, and the estimates based on (2) should be different from the estimates generated by (1) and (3). However, the results based on (2) are very close to estimates using (1) and at least qualitatively similar to the fixed effects estimates from (3). Thus, any bias-inducing heterogeneity in these data must

- exist among dropouts who currently seek the credential;
- be time-varying;
- be related to passage of the GED exams and post-attempt quarterly earnings; and yet
- not be related to GED test scores and pre-attempt earnings dynamics.

Discussion and Conclusion

The central finding of this study is that the earnings of GED candidates who successfully obtained a GED grew faster in the years after the GED attempt than did the earnings of unsuccessful candidates. This finding is robust with respect to the use of several different estimation strategies that attempt to account for unobserved differences among GED candidates that may be related both to passing the GED exams and to later quarterly earnings. The results indicate that 24 quarters after obtaining a GED, successful candidates are expected to earn from 13% to 20% more than comparable GED candidates who do not obtain the credential. Analysis based on a decomposition of Tobit estimates indicates that as much as two-thirds of these earnings gains are due to the increased employment of GED candidates who pass the exams and obtain the credential.

The estimates in this paper are similar to the results from other recent studies of the earnings effects of the GED that use different data and methods. In particular, they are very close to results from the Tyler, Murnane, and Willett (2000) study that used national data, Social Security earnings, and a natural experiment research design. The results presented here are also consistent with prior research indicating that it takes time for substantial GED-related earnings differences to occur.[20] As discussed earlier, a gradual increase in the relative earnings of dropouts is consistent with either a human capital story in which employers take time to learn about the skills of their dropout employees or a signaling story in which GED holders are matched to jobs that have lower initial earnings and a steeper earnings profile than the jobs in which uncredentialed dropouts are employed. The delayed earnings gains can also be explained by a model in which dropouts who obtain a GED engage in a more intensive and extended job search than do dropouts who unsuccessfully attempt to acquire the credential.

The predicted percentage increases in earnings I find in this study are larger than predicted percentage increases in hourly wages estimated by Heckman, Hsse, and Rubinstein (2000) in a sample of dropouts who were in the second quartile of the AFQT test score distribution. There are three potential explanations for this divergence. First, to be in HHR's sample required non-zero hourly or weekly wages. Thus, HHR's estimates did not capture any of the effect the GED might have had in

[20]See, for example, Murnane, Willett, and Boudett (1995); Murnane, Willett, and Boudett (1999); Murnane, Willett, and Tyler (2000); and Tyler, Murnane, and Willett (2000). Also see Murnane, Willett, and Tyler (2000) for a discussion that attempts to reconcile the more positive GED results that have emerged recently in the literature with less positive findings from the well-known Cameron and Heckman study (1993).

moving dropouts from nonemployment to employment. The Tobit-based results in this paper suggest that a substantial portion of the GED effect on earnings may result from this mechanism. Second, HHR's comparison group consisted of all uncredentialed dropouts who fit the relevant sample selection criteria, whereas the comparison group in this study consists only of dropouts who had taken but failed the GED exams. If the conditional mean labor market outcomes of all uncredentialed dropouts are generally more positive than the conditional mean outcomes of unsuccessful GED candidates, then this could also explain the differences between the estimates in this paper and those in HHR. Finally, the HHR specification controlled for accumulated work experience. If increasing work experience were one of the mechanisms through which a GED affects hourly wages, then controlling for this mechanism would reduce any estimated impact of a GED on wages.

The generally positive results associated with a GED found in this paper must be tempered by two important considerations. The first is that obtaining a GED is no quick fix for low earnings, since the earliest that any statistically discernible differences in earnings appear is in the 10th post-attempt quarter. Second, and even more sobering, is the fact that the mean earnings of the GED holders in these data are very low. For example, fixed effects estimates from equation (3) predict that a 26-year-old GED holder will earn about 19% more than a similar uncredentialed dropout six years after acquiring a GED. While this is a quite substantial economic effect, it must be remembered that it occurs on top of very low annual earnings. The predicted earnings at age 26 of a male dropout who obtained his GED at age 20 are about $11,200. This is $2,000 below the 1999 poverty line for a couple with one child. These earnings figures underscore two important facts. The first is that individuals lacking a regular high school diploma generally have very low average earnings, in part because of low employment rates. The second fact is that even if the high economic returns estimated in this paper represent the causal impact of the GED, acquisition of this credential can only partially ameliorate the harsh economic realities associated with being a dropout in this country.

Finally, it is not possible, using these data, to make inferences concerning how GED candidates would fare in a regime that lacks a GED program. For example, if the GED credentialing program tends to encourage students to drop out of school who otherwise would have benefited from more schooling, then the net private and social benefits of obtaining a GED could be zero or negative.

Appendix Table A1
Coefficient Estimates in the 5th and 24th Quarters after the GED Attempt Based on Equations (1), (2), and (4). (Standard Errors in Parentheses)

	5th Quarter after the GED Attempt			*24th Quarter after the GED Attempt*		
Variable	*Equation (1) Estimates*	*Equation (2) Estimates*	*Equation (4) Estimates of γ′*	*Equation (1) Estimates*	*Equation (2) Estimates*	*Equation (4) Estimates of γ′*
GED	31	56	143	313***	325***	461**
	(49)	(44)	(104)	(84)	(81)	(190)
Age	226***	29	153***	222***	–0.5	293**
	(30)	(26)	(54)	(55)	(53)	(98)
Age-Squared	–3***	–0.03	–1	–3***	0.3	–3.8**
	(0.5)	(0.5)	(1)	(0.9)	(0.8)	(1.5)
Black	–378***	–224***	–511***	–778***	–630***	–988***
	(38)	(34)	(54)	(70)	(68)	(111)
Hispanic	61	42	20	258***	238***	249**
	(40)	(36)	(63)	(71)	(69)	(118)
Schooling	175**	169**	240**	–193	–199	–251
	(102)	(84)	(101)	(220)	(238)	(325)
Schooling-Squared	–5	–7	–10*	20	18	19
	(5)	(4)	(5.5)	(11)	(12)	(17)
Multiple GED Attempts	141***	36	186**	267***	164**	331***
	(48)	(42)	(54)	(84)	(81)	(103)
Five Initial GED Test Scores	Yes	Yes	—	Yes	Yes	—
Five Final GED Test Scores	Yes	Yes	—	Yes	Yes	—
Testing Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Year and Quarter of Earnings Measurement	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Employment Earnings and Employment	—	Yes	—	—	Yes	—
Mean GED Test Score	—	—	–24	—	—	–50
			(20)			(41)
R^2	0.04	0.20	0.05	0.03	0.07	0.03

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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