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September 2004

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ABSTRACT

Measuring the Returns to the GED: Using an Exogenous Change in GED Passing Standards as a Natural Experiment*

In this paper, we exploit an exogenous change in the passing standard required to obtain a General Educational Development (GED) credential to identify the impact of the GED on the quarterly earnings of male dropouts, utilizing the Texas Schools Micro Data Panel (TSMP). These unique data contain demographic and GED test score information from the Texas Education Agency linked to pre- and post-test taking Unemployment Insurance quarterly wage records from the Texas Workforce Commission. Comparing Texas dropouts who acquired a GED before the passing standard was raised in 1997 to dropouts with the same test scores who failed the GED exams after the passing standard hike, we find no evidence of a positive “GED effect” on earnings. The finding of no significant difference in pre-test taking earnings between the treatment and control group support the validity of the natural experiment. Our results are robust to a number of specifications and sub-samples of our general sample population of 16-40 year old males.

JEL Classification: I2, J31

Keywords: GED, returns to education, natural experiment

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1. Introduction

The General Educational Development certificate (GED) is an exam-based credential awarded to about 500,000 high school dropouts in the U.S. each year, representing almost 15 percent of all high school diplomas, or credentials, issued. Lacking a high school diploma to certify successful completion of their secondary schooling experience, the GED is the first, and often only, credential high school dropouts receive. One reason many dropouts seek a GED certificate is a belief that it will lead to greater labor market success. Several studies over the last decade have attempted to determine whether this is indeed the case, with the research questions breaking along two lines: How do GED holders compare to regular high school graduates? And, how do GED holders compare to other, uncredentialed high school dropouts?

Most observers agree that the first question has been answered rather convincingly by Cameron and Heckman (1993). They find that GED holders fare consistently worse than regular high school graduates on any number of labor market outcomes. An answer to the second question is somewhat more contentious, at least partly due to the lack of true exogenous variation in GED status and the accompanying lack of suitable instruments that could address this problem.

The research strategy in this paper utilizes 1997 changes in Texas in the GED passing standards to identify the causal effect of the GED on labor market outcomes. We argue that the changes in Texas gives rise to a natural experiment that can be exploited to identify the causal effects of the GED on earnings, reducing endogeneity due to correlation between the outcome of passing/not passing and unobserved individual heterogeneity that may bias Ordinary Least Squares (OLS) estimates.

The American Council on Education (ACE) administers the nation's GED testing program and sets the minimum requirements for obtaining the credential. States can, and regularly have, set state level passing standards above the ACE-mandated minimum. Prior to 1997, Texas was one of the few states whose passing standard was at the ACE minimum level. As a result, Texas had one of the country's least stringent GED passing standards. In 1997 the ACE mandated a nationwide increase in the minimum passing standard, a change that was arguably exogenous to Texas. Given these changes and data on individuals containing GED test scores, we can identify an "affected score group" of GED candidates whose eventual GED status is "affected" by the change in the passing standard. Members of the "affected score group" who took the GED exams before 1997 had high enough scores to be awarded a GED, while dropouts in this group with the exact same scores, but who attempted the GED exams after the passing standard hike in January 1, 1997, would have scores below the passing threshold and hence would not be awarded the credential. Since only the passing threshold and not the GED exams themselves changed in January of 1997, and since the raising of the threshold was by the ACE in Washington, DC, rather than by Texas policy makers, this occurrence provides us with a clearly defined natural experiment that can be exploited to estimate the causal effect of the GED on labor market outcomes. Because we are comparing individuals who have the same test scores but differ in GED status according to the year in which they attempted the exams, a strict interpretation of our results is that they estimate the labor market signaling value of the GED.

2. The GED and the Earnings of Dropouts

The GED is an exam-based credential that is awarded based on the scores on five different exams: math, science, social studies, reading, and writing. All of the test items in the GED exam battery are multiple choice except for a section in the writing exam that requires GED candidates to write an essay.¹ The total exam battery takes about seven and one-half hours, and GED examinees who fail to score sufficiently high to “pass” may, under certain circumstances retake any or all of the exams in the battery. The growth in this education credential has coincided with substantial research efforts over the last decade.

Cameron and Heckman (1993) first drew attention to the GED credential and the fact that male GED-holders are not the labor market equivalents of regular high school graduates. Studies that followed their research focused on the question of how GED holders compare to dropouts in the labor market who lack the credential. That is, once the drop out decision has been made, does the GED buy you anything in the labor market? While the answer has been somewhat mixed, two recent studies—each of which uses a different data set and a different empirical strategy—offer evidence that lower 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) offer a discussion of how these more recent results can be reconciled with the earlier Cameron and Heckman findings. Later work by Heckman, Hsueh, and Rubinstein (2000) finds that failing to control for time-invariant heterogeneity between low-skilled credentialed and uncredentialed dropouts may lend an

¹ GED candidates are not required to take all of the five exams in one sitting.

upward bias to estimates of a GED treatment effect. However, estimates from both individual fixed effects models and a regression discontinuity design (Tyler 2003) provide additional evidence that acquisition of a GED can improve the earnings of dropouts. To summarize recent research, it is fair to say that while there is still some ambiguity about the causal effect of the GED on labor market outcomes, the recent research tends to support a view that the credential has beneficial effects for the least skilled dropouts.

The data in this paper contain information on the quarterly earnings of dropouts who attempt to acquire a GED credential. For those dropouts who pass the GED exams, there are several mechanisms through which acquisition of a GED could impact the quarterly earnings of dropouts. 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 were the case, then we would expect to see higher employment rates among the GED holders. Also, conditional on being employed, employers may use the GED as a positive signal of productivity and hence offering and assigning higher wages and/or more hours of work. In all cases we would expect the earnings of GED holders to be higher than the earnings of uncredentialed dropouts.

Second, there could be a human capital impact on the 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. To the extent that school dropouts have to work and prepare to increase their cognitive skills in order to achieve a passing score on the GED exams, then the opportunity to be awarded the credential could lead to human capital gains. However, the most recent data on GED preparation time indicates that the median

study time was only about 30 hours (Baldwin 1990). This appears to be too little for meaningful human capital accumulation.²

Third, GED holders can use the credential to gain access to, and funding for, postsecondary education. Most degree granting postsecondary education programs require applicants to possess a high school diploma or a GED. Also, Pell grants and guaranteed federal student loans for postsecondary 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. It should be pointed out that previous research indicates that relatively few GED holders obtain substantial amounts of post-secondary education. Murnane, Willett and Boudett (1997) report that only 12 percent receive at least 1 year of college schooling and 3 percent obtained an Associate degree.

As with any program evaluation in which the selection process cannot be adequately modeled, we are concerned about the role of individual unobserved heterogeneity. We address this problem by exploiting the natural experiment that resulted when the passing standard required to obtain a GED in Texas was raised for everyone who took the GED exams on or after January 1, 1997. In simplest terms, we will compare the labor market outcomes of GED candidates who have the same GED test scores, but who vary in GED status depending on whether they tested in the years before the hike in the passing standard or in the years after the hike. We detail our data, research design and empirical models in the next sections.

² However, the study did not distinguish between successful and unsuccessful candidates, nor did it include the time that dropouts might have spent in Adult Basic Education, English Second Language, or pre-GED classes in the estimate. As a result, the time spent by successful candidates could be longer than the study

3. Data

This paper brings new and unique data to bear on the GED question in an attempt to better estimate the counterfactual and provide answers to questions about the causal economic impact of the GED. We 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 male dropouts who all attempted the GED exams in Texas either in 1995 or 1997. The key feature of these data is that we have data on GED testers in Texas before and after January 1, 1997, the date on which the passing threshold for the GED in Texas (and several other states) was raised.

The data utilized in this paper is generated from the Texas Schools Microdata Panel (TSMP). We extracted and linked data from two sources, the Texas Education Agency (TEA) and the Texas Workforce Commission (TWC). The TEA data contain information on GED test date, GED test scores, age at test attempt, highest grade attained prior to dropping out of school, gender, ethnicity, GED test language and GED test center. The TWC data contain information on employer reported Unemployment Insurance quarterly earnings. In these data we have quarterly earnings records from the first quarter of 1989 through the last quarter of 2002. When there is no wage record in a quarter, we impute a value of zero. Although our analysis is of the first 20 quarters following the test taking date, we also generate pre-test taking work experience and earnings for the six years prior to attempting the GED.

estimate. It may also be the case that today's dropouts, especially foreign schooled immigrants, may spend more time in GED preparation than the survey respondents in the 1990 study.

The sample utilized in this paper is restricted to males who attempted the GED in either 1995 or 1997, were between the ages of 16 and 40 at the time of the test and did not attempt the GED while incarcerated. We choose 1995 as the period to examine before the passing standard hike because there is some evidence of a change in testing behavior in 1996 in anticipation of the 1997 increase in the scores required to pass the GED exams. Simply put, there appears to be less of a rush in 1995 to test before the passing standard changed than is the case in 1996. The sample restrictions we impose yield a sample of 52,251 dropouts who last attempted the GED exams in either 1995 or 1997.³

4. Descriptive Statistics

Sample descriptive statistics and graphs are presented in Tables 1 and 2, and Figures 1 and 2. Figure 1 plots the quarterly earnings by quarters from the time individuals attempted the GED, both before and after, by GED status. The raw quarterly earnings of eventual GED holders and GED candidates who will, eventually, fail the exams appear similar in the quarters prior to attempting the GED, with some relatively weak indication of slightly higher earnings in the quarters immediately before taking the GED among eventual passers. About a year after the GED attempt, the unadjusted earnings of those who passed the exams, and hence were awarded the credential, begin to diverge from the uncredentialed GED candidates. The patterns in the Texas data are similar to what has been observed in other UI quarterly earnings data on GED candidates. In particular, they are very close to what Tyler (2004) found using data from Florida.

³ We say “last attempted,” because upon failing the GED exams, a dropout can, with certain minimal restrictions, retake the GED battery. We classify individuals into last-attempt years based on GED data as of December 31, 2000.

Two differences are the flattening of the earnings profile of both groups just prior to attempting the GED and the increase in mean quarterly earnings for both groups immediately after the GED attempt. One possible reason for the pre-test flattening of the profile is test preparation, in which individuals spend less time working in the quarter before, and of taking, the GED test. This is consistent with the observed proportion of individuals with positive earnings, i.e. roughly employment rates, which we discuss next.

Figure 2 shows the percentage in each group, successful and unsuccessful GED candidates, who have positive earnings in each quarter around the GED attempt. Unlike the raw mean earnings profiles of Figure 1, the profiles in Figure 2 indicate that eventual GED holders were more likely to be employed in the quarters *before* the GED attempt than were candidates who would eventually fail the GED exams. Also, the employment advantage does not appear to widen after the successful candidates were awarded their GED. The higher pre-test taking employment rates among eventual GED passers suggest that this group have accumulated greater work experience *prior* to taking the GED. This may partially explain their relatively greater earnings growth, and levels, in the period after having received a GED credential.

Table 1 provides information on GED candidates in Texas in the two testing years, 1995 and 1997. The central message of the table is that the two cohorts of candidates appear similar on all observables except, as might be expected, the percentage who passed the exams—ten percent fewer candidates passed in 1997, after the passing standard was raised, than in 1995. One factor that should not go unnoticed is that the number of dropouts who attempted the GED exams in 1997 was substantially lower than

in 1995. This observation has potential implications for our identification strategy, a point we address in a later section.

Table 2 displays information for the six distinct groups created by the passing standard change. A GED candidate is defined to be in the affected score group if his scores are such that either the minimum score on the five GED exams is at least 40 *or* the average the five scores is at least 45, while simultaneously the candidate does not have a minimum score of 40 *and* an average of at least 45. The defined groups are:

1. The group of individuals who tested under the old passing standard and whose scores were too low to pass under either standard. This group attempted the GED in 1995.
2. The group of individuals who tested under the new passing standard, but whose scores were too low to pass under either standard, including the old standard. This group attempted the GED in 1997.
3. The treatment group of individuals who barely passed the GED exams before the change in the passing standard. This group attempted the GED in 1995.
4. The comparison, or control, group of individuals with the same GED test scores as the treatment group, but who lack a GED because they tested under the new, higher standard. This group attempted the GED in 1997.
5. The group of individuals who tested under the old regime, but whose scores were high enough so that they would have passed under the new standard. This group attempted the GED in 1995.

6. The group of individuals who tested under the new regime and whose scores would have been passing under either passing standard. This group attempted the GED in 1997.

The groups of particular importance, given our empirical strategy, are clearly the treatment and comparison groups, i.e. those in the “affected score group”. To validate our identification strategy, the assignment into these groups needs to be random, or more specifically, in the models defined below, assignment into the groups, controlling for observables, including differences across the other pre- and post-passing standard change groups, must be uncorrelated with the disturbance term.

According to Table 2, the treatment and comparison groups appear quite similar across the dimensions on which we have data.⁴ There are however some differences. For example, the percentage African-American is slightly larger in the comparison group. The comparison group also has slightly greater pre-test taking work experience, about two-thirds of a quarter, and higher pre-test taking quarterly earnings. A difference between the 1997 and 1995 group pre-test taking earning also exists for the passing groups, i.e. groups 5 and 6, in the three years prior to attempting the GED, although it is smaller.

It should also be pointed out that the sample size of the treatment and control groups are quite different, with the latter being smaller. One plausible and partial reason for the difference in sample size, besides the overall decline in number of test takers, lies in the necessary definition and restriction of test takers. We have restricted our sample to individuals who *last* attempted the GED in either 1995 or 1997. A test taker in this score

group who took the exam in 1995 has no incentive to come back and test again since he has already received high enough scores to obtain the credential. However, a 1997 test taker in this score group is quite close to passing but did not receive the credential. Some individuals with scores in this range are likely to return later to try again. If these individuals returned for another attempt in 1998 or later, they would be excluded from our sample. To address this potential complication, we will perform robustness tests, including a restriction of our sample to individuals who *only* attempted the GED in either 1995 or 1997.

5. Empirical Specifications

OLS Specification:

As a first step in the empirical work, we fit simple OLS models to establish that the Texas data do not generate unique patterns, and is similar to previously used data, and that our main results and findings can be generalized. The first specification, shown as equation (1);

$$(1) \quad y_{it} = \mathbf{b}_0 + \mathbf{b}_1 \text{GED}_i + \mathbf{b}_2 \text{D1995}_i + \mathbf{Yrs Post}_{it} \mathbf{g} + \mathbf{Yrs Post}_{it} * \text{GED}_i \mathbf{t}_t + \mathbf{X}_{it} \mathbf{n} + \mathbf{Time}_t \mathbf{a} + \mathbf{e}_{it}$$

where y is the quarterly earnings for individual i in year-quarter, t is the quarter after the GED attempt in which earnings are measured and $t \in (1, 2, \dots, 20)$, GED is an indicator variable for possessing a GED, $D1995$ is an indicator variable for attempting the GED in 1995, $\mathbf{YrsPost}$ is a vector of dummy variables indicating whether earnings were

⁴ Of course, these two groups have similar test scores by construction.

measured in the second, third, fourth or fifth years after the GED attempt. The interaction between *GED* and **YrsPost** allows earnings to grow differently for GED-holders and non-GED-holders. The matrix **X** contains a set of individual control variables including age, highest grade completed, race/ethnicity, pre-test taking earnings and experience,⁵ and GED scores. **Time** is a vector of year-quarter dummy variables, i.e. time fixed effects, that control for macro economic conditions.

There are two interpretations that can be given to estimates based on specification (1). First, to the extent that the **X** matrix, including GED test scores, captures an individual's stock of human capital, \mathbf{b}_1 is an estimate of the signaling value of a GED on first year earnings, and $\mathbf{b}_1 + \mathbf{t}_t$ is an estimate of the signaling value of the GED on earnings in the t^{th} year. If one thinks that the matrix **X** and GED test scores capture only some portion of an individual's level of human capital, then \mathbf{b}_1 is an estimate of some combination of the signaling value plus the return to the unexplained (by **X** and GED test scores) portion of human capital of a GED on first year earnings, and $\mathbf{b}_1 + \mathbf{t}_t$ are estimates of the same for the t^{th} year. The important point here is that under either interpretation, \mathbf{b}_1 and $\mathbf{b}_1 + \mathbf{t}_t$ are unbiased estimates of the causal impact of the GED only if the variable *GED* is uncorrelated with \mathbf{e} .

Fixed Effects Specification:

The next two specifications are comparable to those used in earlier research, specifically Tyler (2004). These specifications attempt to address the potential

⁵ Pre-test experience is measured as the number of quarters in the six years prior to testing in which non-zero earnings are recorded. Pre-test earnings is a vector of six variables representing the annual earnings (sum of the year's four quarters of earnings) in each of the six years prior to testing.

endogeneity of the GED outcome, and we include them here to establish that the Texas data yield similar results to Tyler (2004). The first of these specifications is a fixed effects model which controls for time invariant individual unobserved heterogeneity. The specification is shown in equation (2);

$$(2) \quad y_{it} = \mathbf{b}_i + \mathbf{b}_1 \text{GED}_i + \mathbf{Yrs Post}_{it} \mathbf{g} + \mathbf{Yrs Post}_{it} * \text{GED}_i \mathbf{t}_t + \text{Age}_{it} \mathbf{l}_0 + \text{Age}_{it}^2 \mathbf{l}_1 + \mathbf{Time}_t \mathbf{a} + \mathbf{e}_{it}$$

Note that in this specification, to be able to identify the GED effect, we include observations prior to the GED attempt. As opposed to all other models estimated, where $t=1, 2, \dots, 20$, we now add in observation for the six quarters before the person took the GED, i.e. $t = -6, -5, \dots, 20$. This implies that the variable *GED* is set to zero in all quarters before the GED attempt for all individuals and one in the quarters after the GED attempt for those who receive passing scores. In this specification \mathbf{b}_i is the individual fixed-effect. The vector **YrsPost** is defined as in the above simple OLS specification. The model also includes time fixed effects and controls for age, which is the only time varying individual variable in our data. If unobserved individual heterogeneity is time invariant, the estimates of \mathbf{b}_i and $\mathbf{b}_i + \mathbf{t}_t$ will be unbiased. On the other hand, if unobserved heterogeneity is time-varying, the fixed-effects specification will not accurately estimate the causal impact of the credential.

Regression Discontinuity Specification:

Tyler (2004) also takes advantage of the fact that there is a cutoff, or discontinuity, in the average score, holding minimum score at 40 on each of the five

components, in order to obtain the GED credential. In our Texas data this strategy can only be applied to the individuals who took the GED in 1997 since there does not exist a well defined discontinuity prior to the passing standard hike. Hence in this specification, we limit our sample to individuals who attempted the GED in 1997.

Equation (3) shows the estimated specification.

$$(3) \quad y_{it} = \mathbf{b}_0 + \mathbf{b}_1 \text{GED}_i + \mathbf{b}_2 \text{MeanScore}_i + \mathbf{Yrs Post}_{it} \mathbf{g} + \mathbf{Yrs Post}_{it} * \text{GED}_i \mathbf{t}_t + \mathbf{X}_{it} \mathbf{n} + \mathbf{Time}_t \mathbf{a} + \mathbf{e}_{it}$$

The model to be estimated given our third specification is similar to specification (1) except that we include mean GED score, as opposed to all individual scores. Furthermore, the model is fitted using test takers who scored a minimum of 40 and whose mean score was between 40 and 50. The latter restriction is imposed since the relationship between earnings and average scores may not be linear over the full range of mean scores. Unbiased estimates of \mathbf{b}_1 and $\mathbf{b}_1 + \mathbf{t}_t$ in this model rest on the assumption that in a narrow range around the passing cutoff, the conditional earnings–mean-score relationship is linear and that any vertical shift in the regression line at the cutoff is due solely to acquisition of the credential. We note that while the fixed effects specification of equation 2 estimates the total impact of the GED, regression discontinuity estimates based on equation 2 capture the signaling value of the credential.

Natural Experiment:

A concern is that in the above specifications, a correlation between *GED* and \mathbf{e} remains, yielding biased estimates of the impact of the GED on earnings. For example, in

specification (1), where we control for an individual's test scores, the GED scores may not adequately capture unobserved productivity related characteristics of the individual. If the unobserved heterogeneity is not time invariant, the fixed effects model, specification (2), will not yield unbiased estimates of the GED effect. The regression discontinuity approach, specification (3), may also fail to correctly reveal the causal effect of the GED on earnings if those who are on either side of the passing cutoff of a 45 mean score are different in unobservable ways, controlling for the effects of the GED mean score.⁶

Lacking a true experiment that would generate true exogenous variation in GED status, we exploit the 1997 change in GED passing standards in Texas as a natural experiment that simulates random assignment of the credential. We will utilize the six groups defined above.

There are two key assumptions in our identifying strategy. The first is that any difference in outcomes between the individuals in the treatment and comparison groups, are solely the result of differences in GED status and in the years in which they tested and, hence, entered the labor market. The second key assumption is that the individuals in group 1 relative to group 2 and those in group 5 relative to group 6 differ only in the years in which they attempted the GED exams, and as a result these individuals can be used to purge the treatment group versus comparison group contrast of any differences related to whether they attempted the GED before or after the change in the passing standard. These assumptions, or restrictions, can be relaxed somewhat and still yield an unbiased estimate of the causal effect. As long as the assignment into the defined score

⁶ Or, alternatively, the mean score variable does not adequately control for unobserved heterogeneity.

groups is uncorrelated with the disturbance term, the estimates of the GED effect are unbiased.

The difference-in-differences estimator that captures this idea is given by specification (4);

$$(4) \quad y_{it} = \mathbf{b}_0 + \mathbf{b}_1 AllPass_i + \mathbf{b}_2 ASG_i + \mathbf{b}_3 Before_i + \mathbf{b}_4 ASG_i * Before_i + YrsPost_{it} \mathbf{g} + YrsPost_{it} * AllPass_i \mathbf{d} + YrsPost_{it} * ASG_i \mathbf{h} + YrsPost_{it} * Before_i \mathbf{l} + YrsPost_{it} * ASG_i * Before_i \mathbf{t}_t + \mathbf{X}_{it} \mathbf{n} + \mathbf{Time}_t \mathbf{a} + u_{it}$$

where i and t index as before and,

- y_{it} = Quarterly earnings
- $AllPass_i$ = dummy variable indicator for those whose score would place them above the passing threshold either before or after the change in the passing standard,
- ASG_i = dummy variable indicator for those whose scores place them in the affected score group,
- $Before_i$ = dummy variable indicator for having taken the GED exams before the passing standard was raised, i.e. 1995 test takers,
- $ASG_i * Before_i$ = identifies individuals in the treatment group—individuals in the affected score group who have a GED,
- $YrsPost_{it}$ = as before,
- \mathbf{X}_{it} = as before, excluding GED test scores
- \mathbf{Time}_t = vector capturing time fixed effect.

The parameters of interest in this specification are \mathbf{b}_4 and $\mathbf{b}_4 + \mathbf{t}_t$, the first of which is the difference in mean outcomes between the treatment and comparison individuals in the first year after the GED attempt, after any 1995 to 1997 time effects

have been differenced out. In other words, this is the difference-in-differences estimator for the first post-attempt year's earnings. The parameter $\mathbf{b}_4 + \mathbf{t}_t$ is the difference-in-differences estimator for the t^{th} year's earnings. OLS estimates of the parameters from specification (4) will be unbiased if, controlling for time effects, the comparison group accurately estimates the counterfactual of what would have happened to the treatment group had they not been awarded a GED. This is, of course, equivalent to saying that the OLS estimate of the parameters of interest is unbiased if $ASG*Before$ is uncorrelated with the disturbance term u .

It is worth reemphasizing here that by construction, the members of the treatment and comparison groups have the same GED test scores. Thus, to the extent that GED test scores capture dropouts' levels of human capital, \mathbf{b}_4 and $\mathbf{b}_4 + \mathbf{t}_t$ estimate the signaling value of the GED. This is similar to the estimation strategy used by Tyler, Murnane, and Willett (2000), who utilize state variation in passing standards. To the extent that there is a human capital component to the GED credential, the difference-in-differences estimator will underestimate the total impact of the GED on earnings. This argument can also be made for the Tyler, Murnane and Willett (2000) research approach.

6. Empirical Results

We begin the empirical investigation by fitting models based on variations of specification (1). Coefficient estimates and test statistics are displayed in Table 3. The estimated GED effects in Table 3 are summarized in Table 4 across the years, including tests of the null hypothesis of no GED effect.

Results from Model 1 in column 1 are from a simple model that includes time fixed effects, to control for differences in macro economic conditions, and controls for highest grade attained, age, race/ethnicity, whether or not the Spanish language version of the GED was taken and whether the test was taken in 1995 or 1997. These baseline results indicate that in the first year following the GED attempt, the mean quarterly earnings of GED holders are somewhat higher than those of dropouts who attempted, but failed, the exams. However, by the fifth year following the GED attempt, the mean quarterly earnings of GED holders are about \$423 ($\$60 + \363) higher than those of the individuals who failed the GED. A rather substantial change in the estimates appears in Model 2 when controls for pre-test taking work experience and earnings are included. The estimates now indicate significantly lower earnings for GED holders in the first year after taking the GED, by \$76. This is reversed in the preceding years and in the fifth year following the GED attempt, the quarterly earnings advantage is about \$288. This suggests that almost 1/3 of the estimated GED earnings premium is due to differences in pre-test taking labor market outcomes between men who pass and fail the test.

Model 3 in Table 3 add controls for GED test scores. When GED test scores are added, the estimated GED effect falls by approximately 20 percent. Nevertheless, Model 3 estimates still indicate that by the fifth year after the test attempt, GED holders earn about \$234 more per quarter than do observationally similar dropouts lacking the credential. To the extent that GED test scores capture the human capital of GED examinees, these results can be interpreted as the signaling value of the GED. Under this interpretation, roughly 80 percent of the fifth year GED advantage estimated using Model 2 is due to the signaling component of the GED. Of course, since we have no information

on pre-GED test scores, we do not know how much, if any, of the remaining 20 percent is the result of human capital related to GED preparation versus human capital that GED examinees already possessed before they decided to attempt to acquire the credential.

The fixed effects model specification is presented as Model 4 in Tables 3 and 4. The results indicate a strong GED effect on earnings. In fact, the estimates suggest a statistically significant immediate earnings advantage in the first year since taking the GED of about \$220. By the fifth year the GED premium has grown to roughly \$426.

The last model, Model 5 in Tables 3 and 4, utilizes the regression discontinuity strategy, i.e. specification (3). The estimated impact of the GED on earnings is smaller than in the fixed effects model but greater than the simple OLS estimates indicate. There appear to be no effect in the first year after taking the GED but by the fifth year GED holders earn roughly \$340 more per quarter compared to dropouts who did not obtain the credential.

We next turn to a comparison of the results in tables 3 and 4 to previous findings of the impact of the GED on earnings, primarily to determine if the Texas data can generate similar results to earlier studies. In particular, we want to see if our results are close to those in Tyler (2004) that are based on GED examinees in Florida. Like our Texas data, the Florida data used in Tyler (2004) also contain demographic information, GED test scores, and UI quarterly earnings. Two comparisons are important. First, across all of the specifications represented in Tables 3 and 4, our estimates in Texas are quite similar to those found by Tyler. In general, four to five years after taking the GED, individuals who passed the exam display higher quarterly earnings by about 10-17 percent. Data used in the Tyler paper had the slight advantage of an additional 4 quarters

of post-GED attempt UI wage information, and the estimated differences were \$266 and \$310 in the fifth and sixth years following the GED attempt, respectively. Second, similar to Tyler and others (Tyler, Murnane, and Willett 2000), we find that examining the first years after the GED attempt will typically fail to reveal any substantive GED earnings advantage; it typically takes at least three years for substantively large and statistically significant results to appear.⁷ These results indicate that estimates based on the Texas data quite closely replicate results using similar data from Florida.

Of course regardless of the data source, estimates based on any of the three specifications represented in equations 1-3 are only as good as the assumptions upon which they rest. The simple OLS estimates may be biased if the GED scores variables do not adequately capture productivity related unobserved heterogeneity, including ability and motivation. Estimates based on the fixed effects specification will be biased if there are time-varying, unobserved differences between GED candidates who do and do not obtain the credential. The validity of the regression discontinuity approach hinges on whether or not the mean score in the vicinity of the passing score cutoff adequately captures unobserved differences in GED candidates who score on either side of that cutoff. In short, all of these approaches lack a clear, exogenous source of variation in GED status. A value of natural experiments is the clear source of variation in the policy variable of interest. The change in GED passing standards in Texas generates this type of clear variation in GED status, and, we argue, generates unbiased estimates of the impact of the GED.

⁷ Tyler, Murnane, and Willett (2000) also find that GED effects grow over time and are often negligible in the first years after the attempt.

Natural Experiment Results

Model 1 in Table 5 presents estimates from the “pure” natural experiment. Based on Model 1, GED holders in the treatment group earn approximately \$287 less in the first post-attempt year than do members of the comparison group who have the same GED test scores, a result that is statistically significant. This may be because newly minted GED holders are engaged in more post-attempt post-secondary education, training, or job search than are comparison group members lacking a GED.⁹ As the companion results in the first column of Table 6 show, this negative effect becomes small enough in subsequent years so that it is not statistically significant. The overarching message of column one is, however, that estimates of the impact of the GED on earnings from the natural experiment fail to find any positive effect.

If the natural experiment is doing a good job at pseudo-randomization, estimates should change little when observable control variables are added. The evidence on this in Table 5 is somewhat mixed. Estimates from Model 1 change little when time fixed effects are added in Model 2 or when age, schooling, and ethnicity are added in Model 3. The largest changes in the estimates occur when pre-test-taking work experience and earnings are added in models 4, 5, and 6, and all of the change associated with adding these controls accrues to the first year GED effect. From Model 3 to Model 6 the first year negative GED “effect” increases by about a 33 percent, while at the same time there is no increase in any of the β 's. Even with this less negative first year effect, however, inferences about the impact of the GED remain unchanged from Model 3 to Model 6. In

⁹ We note that while previous work suggests that GED holders only accumulate 0.4 mean total years of post-secondary education (Murnane, Willett, and Tyler 2000), this same work shows that up to 30 percent of the GED holders in the data earned at least one college credit.

fact, the conclusion one would draw from the estimates in Model 1, that GED holders display lower earning in the first year after attempting the GED and earn no premium from the credential in subsequent years, holds across all models. As discussed above, the lower earnings in the first year may be due to a composition effect where relatively more productive GED holders enroll in some schooling in the first year after receiving the GED diploma. It is also possible that upon successfully obtaining the GED credential, an individual's reservation wage may increase. As a result, the job search process is likely to take longer as the person is now requiring a higher wage to accept a job offer. Nonetheless, the results strongly indicate that there is no evidence of a positive impact of the GED on the earnings of male dropouts when the effect is identified through the natural experiment of the passing standard change.

Robustness Checks

Clearly, the above presented results warrant thorough robustness checks. We do this by first validating the natural experiment. Next, we see if the results can be generalized or appear to be specific to some groups in our sample population.

As stated earlier, the difference in the sample size between the treatment and comparison group suggests that there may be some non-random assignment into these groups. There are two potential explanations for the smaller relative size of the comparison group, and we examine the potential impact of each in turn. First, the 1995 test takers in the affected score group had no incentive to take the GED in later years since their score qualifies them for the credential. However, 1997 testers whose scores would place them in the affected score group failed to obtain a credential and thus had an

incentive to retake the GED exams. Any of these potential comparison group members who chose to retest in 1998 or later would not be included in our sample. Thus, the 1997 affected score group members may be a selective group of individuals since they did *not* return to try to obtain the credential again.

To address this concern we re-estimated the models shown in Table 5 and 6 using a restricted sample including only individuals who attempted the GED once, either in 1995 or in 1997. The results for Model specification 6 are presented in Tables 7 and 8. The results are quite similar to the ones shown in Tables 5 and 6. For example, the estimated first year difference between the treatment and control group for Model 6 specification is -\$190 for the sub sample of test takers who only attempted the GED once, compared to -\$167 in the “full” sample. The estimated GED effect in the fifth year now becomes positive, \$45, but is still statistically insignificant.

An important advantage with our data is that we can investigate the validity of the experiment by analyzing pre-intervention differences between treatment and control groups. To do this, we estimate a difference-in-difference model similar to Model 6 in Table 5 using observation for the four quarters *prior* to the GED attempt. The estimated earnings difference the year *before* taking the GED between the treatment and control group indicate that the treatment group earned roughly \$49 more than the comparison group. However, the standard error is quite large, \$44, suggesting no pre-intervention difference between the two groups. The results are virtually unchanged if we extend the pre-test taking period to the immediate two years before the test was taken. These results lend strong support to the validity of our natural experiment.

A second, and not mutually exclusive, reason for the smaller comparison group has to do with potential behavioral changes associated with the change in passing standard. While the imposition of the passing standard hike in Texas was surely exogenous to Texas, the behavior of GED preparation providers, GED examinees, and potential GED examinees may have been influenced by the passing standard change in ways that influence our results.

Figure 3 plots trends over time in the number of GED examinees in Texas and in Florida. Florida, of course, is a state that did not face a passing standard hike in 1997 as did Texas, though the standard was raised slightly in Florida in June of 1998, again in July of 1999, and again in July of 2000.¹⁰ The trend line in Texas offers some evidence that there was a substantial increase in the number of GED examinees in Texas in 1996, the year prior to the passing standard hike. To the extent that the 1996 data point is above the trend line for Texas, this is consistent with anecdotal evidence that GED preparation providers were encouraging GED candidates to attempt to acquire the credential before the higher standards went into effect. This is the primary reason that we choose to use 1995 as a “before” period in our difference-in-differences estimates; we were concerned that the pool of GED examinees in Texas was different in unobservable ways in 1996 as the run up to the January 1, 1997 deadline loomed.

What we are not able to account for is the potentially different pool of GED examinees from which we draw our comparison group—those who chose to attempt the GED exams *after* the passing standard hike was in place in 1997. The sharp drop in the number of testers in Texas that occurred between 1994-1996 and 1997 suggests that those

who decided to attempt to obtain a GED in 1997 may be different than GED examinees of the near prior years. Of primary concern for us is whether those who tested and just failed in 1997, our comparison group, are different in unobservable ways from those who tested and passed with the same scores in 1995, our treatment group. In particular, if the reduced pool of dropouts who did decide to attempt a GED in 1997, in spite of the higher standard, have on average more productive unobservable traits than the pool of 1995 examinees, then our natural experiment estimates would be downwardly biased. Thus, if individuals in the 1997 comparison group have more motivation, more determination, are less deterred, etc., then we would expect our results to be downwardly biased.

It is important to note that if these types of unobservable differences exist for everyone in the 1995 and 1997 groups, then there is not a problem, as the difference-in-differences estimator will account for them. What is a problem, however, is if the individuals who are nearest the passing cutoff tend to have more unobservable productivity in 1997 than in 1995 than do those who are farther from the cutoff. For example, if it is the case that most of the reduction in testing numbers that occurred in Texas between 1995 and 1997 occurred because of the decision not to test by those who would tend to score close to the passing standard, while there were few changes at the upper and lower ends of the skill distribution in the propensity to test, then our results could be biased. The observation, from Table 2, that there appear to be some differences specific to the affected score groups in pre-test taking experience and earnings is consistent with this.

¹⁰ In June of 1998 Florida GED candidates had to score a minimum of 42 and a mean of 45 to be awarded a GED. In July of 1999 the minimum allowable score was raised to a 44, and in July of 2000 the minimum allowable score was a 45 giving Florida the highest GED passing standards in the nation.

We address this issue by restricting the other groups, i.e. the ones who were not affected by the passing standard change, to be less heterogeneous and to more closely resemble the treatment and control groups. We do this by restring the “failing” group to individuals who scored a minimum of at least 35 or an average of at least 40 (this is our “High Fail” sample in tables 7 and 8) and the “all passing” group to test takers who scored a minimum of less than 45 and an average score of less than 50 (“Low Pass” sample). The difference-in-difference estimator using these sub-samples should better handle the possible “marginal” effects from the passing standards change since the groups are more similar to each other.

The estimates suggest that the results presented in Table 5 and 6 may marginally underestimate the positive earnings impact of the GED. The estimated first year difference drops to -\$118 and -\$151 respectively for the two sub-samples, still statistically significant, and the estimates for the fifth year GED effects are approximately \$35 and -\$40 respectively but, as before, not significantly different from zero. Again, the results appear to hold up quite well to the robustness check. We next turn to investigating whether the finding of no positive GED effect may be specific to certain groups in our sample.

Previous research, Tyler, Murnane and Willett (2000) finds no evidence that non-white dropouts who obtain the GED credential earn more than statistically similar non-whites who failed to receive passing scores. To address whether our results are driven by the relatively large proportion of non-white minorities in our sample, close to 60 percent, we estimated Model 6 separately for whites and non-whites. The results are presented in the fourth and fifth result columns in Tables 7 and 8. The results for the two groups are

sufficiently similar that one would draw the same conclusions regarding the impact of the GED on earnings for both groups, lower earnings in the first year and positive effect in the subsequent years.

Since the sample used in Tyler, Murnane and Willett (2000) consisted of young dropouts, between the ages of 16-21, a positive GED effect may be specific to this age group. To test whether our data, and approach, may mask these effects since we include males between the ages of 16 and 40, we fitted Model 6 for a sample of 16-21 year old males. The results are shown in column six, tables 7 and 8. The estimates do not suggest that there are any differences between the relatively younger dropouts and their older counterparts since the results are very similar to the ones for the 16-40 age group.¹¹

Lastly, we fit Model 6 to a sample replacing the 1997 test takers with 1998 test takers. Given that our data include earnings information up until the fourth quarter in 2002, we can only track 1998 test takers post-test taking earnings for four years. As is shown, in the last column in tables 7 and 8, the results using 1998 test takers are quite similar to the ones we obtained using 1997 test takers.

The above sensitivity analysis quite strongly indicates that the strategy using the natural experiment to identify the causal earnings effect of the GED credential is reasonably robust. Also, the finding of no statistical difference in pre-test taking earnings between the treatment and comparison groups validates the strategy.

¹¹ In analysis not shown here we examined results for white, 16-21 year olds, the exact group used in Tyler, Murnane, and Willett (2000). Estimates based on this more refined sample also show no evidence of a GED effect on earnings.

8. Summary and Conclusions

Reconciling differences

The natural experiment estimates differ from recent results in the literature which indicate a positive impact of the GED on the earnings of low skilled dropouts. In particular, Tyler, Murnane, and Willett (2000) used national data and a natural experiment similar to that used in this paper to estimate the impact of the GED on annual earnings from the federal Social Security earnings file. Exploiting interstate variation in GED passing standards that existed across states in 1990 as an identification strategy, their difference-in-differences estimates place the impact of the GED on annual earnings at around 15 percent. What are the potential explanations that could reconcile these different results?

First, it could be the case that the research design employed by Tyler et al (2000) failed to adequately control for endogenous GED passing standard policies across states in a way that lead to an upward bias in their estimates. A potential strength of the research design used in this paper is that the mechanism behind the passing standard change that occurred in Texas is more transparent, and hence, arguably more likely to be exogenous than differences in GED passing standards that occur across states.

A second explanation concerns the signaling interpretation of the natural experiment estimates. As stated earlier, since the treatment and comparison groups are balanced on both the motivation to attempt the GED exams and on the GED test scores, it is likely that what is primarily being estimated is not the total impact of the GED, but rather the signaling value of this education credential. Texas, along with only three other

states, has long had the lowest passing standard allowable in the GED program.¹² It could be that with such a low passing standard, the GED is a relatively useless signal of productivity to Texas employers. Evidence that supports this interpretation comes from regression discontinuity estimates of the impact of the GED using data from Florida (Tyler 2004). In the Florida study examinees whose GED test scores placed them just a few points on either side of the passing standard were compared. Because they were so close in score, it is reasonable to interpret the results as primarily estimates of the signaling value of the GED in Florida. The estimated impact of the GED from this regression discontinuity design was about a 15 percent increase in earnings, very similar to the Tyler et al results using national data. At the time of this study, Florida had the highest GED passing standard in the nation, except for Wisconsin and New Jersey. Thus, a possible inference is that having a high GED passing standard, as in Florida, results in a separating equilibrium that provides a meaningful signal to employers about the productivity of those possessing the signal. Meanwhile, if the passing standard is too low, such as might have been the case in Texas prior to 1997, the mean productivity of GED holders will not be sufficiently different from that of uncredentialed dropouts resulting in a credential that conveys no useful information to employers.

To the extent that this explanation is the correct one for our results, they suggest that policy makers should think carefully about the role that passing cutoffs on high stakes tests play in determining the composition of the differing pools of “passers” and “failers,” and how the composition of these pools affects the distribution of earnings. This is a lesson that others, including Betts and Costrell (2001), have effectively

¹² The other states that had a similarly low passing standard in 1996 were Louisiana, Mississippi, and Nebraska.

illustrated. If one goal of the GED program is to allow more motivated and skilled dropouts to distinguish themselves to employers, then the hike in passing standards mandated in 1997 may well have been one of the most important decisions the American Council on Education has made in recent years. However, since we are able to quite closely replicate the results obtained in Tyler (2004) utilizing our Texas data, we fail to find strong support for the low passing standard hypothesis. Nonetheless, it should be noted that our regression discontinuity results that utilize a sample of test takers who faced the higher passing standards, namely the 1997 test takers, are consistent with this hypothesis.

A third possibility has to do with the different samples used in Tyler, Murnane, and Willett (2000) and in this paper. Estimates in Tyler, Murnane, and Willett are based on a pooled sample of males and females, and their estimated GED effect is a weighted average of the effect for males and the effect for females. Meanwhile, our estimates are based on a sample containing only males. A scenario where there are heterogeneous signaling effects of the GED by gender with positive effects for females and small or no effects for males would explain the differences between our results and those of Tyler, Murnane, and Willett (2000). Support for this possibility is found in Texas data similar to that used in this paper. Preliminary work we have done with samples of females from the Texas Schools Micro Data Panel indicate positive and statistically significant GED effects based on natural experimental specifications similar to those used for males in this paper. Exploring the extent to which heterogeneous returns by gender can reconcile the different results across papers is an important area for future research.

A final explanation for the different findings is that the natural experiment-based estimates in this paper are biased downward. As discussed earlier, while the passing standard change is surely exogenous to Texas, selection into the comparison group may not be, and under certain conditions discussed above, estimates based on the natural experiment may mis-estimate the causal impact of the GED on earnings. We note in summary, however, that our extensive robustness checks reveal no evidence that would question the validity of the Texas natural experiment.

Why obtain a GED if no effect on earnings?

If the estimates in this paper represent the causal impact of the GED on earnings, the question raised is why would dropouts continue to acquire this credential if there is no economic benefit? There are several potential and non-mutually exclusive answers to this question. We first note that to the extent that we are only estimating the signaling value of the credential, we may be underestimating the total potential impact on earnings, composed of the returns to the signaling value and the returns to human capital component of the credential. Putting that caveat aside, however, one explanation is that there are non-economic benefits that dropouts derive from acquiring a GED. Anecdotally, GED preparation providers report that many of the “graduates” of their programs report that studying for and acquiring a GED is the first thing that they have accomplished on their own, and they proudly bring family to GED graduation ceremonies. Thus, feelings of pride and accomplishment could bring dropouts to the GED program even if there were no economic payoffs to the credential.

A second explanation for why we might see dropouts pursuing the credential is that at least in terms of direct costs, it is a relatively cheap credential. Direct costs for taking the GED exams can be as low as \$50 and even free in some cases. Also, GED preparation classes are often subsidized by the government and therefore of very low cost to participants. Furthermore, if the dropout attempting to obtain a GED is unemployed or out of the labor force, then there are no opportunity costs from lost employment to participate in GED preparation classes. Less clear are the psychic costs dropouts with a history of poor academic performance incur as they sit in GED preparation classes and subsequently take examinations that take up to seven and three-quarters hours. Nevertheless, government subsidization of GED preparation courses appears to make this a very cheap credential.

Conclusions

Each year some three-quarters of a million individuals in this nation take the GED exams in hopes of passing and acquiring their GED certificate. Our results suggest that the returns to this credential for male dropouts are substantially less than what was found in other studies. In fact, our findings strongly suggest an earnings effect close to zero for male dropouts. Our results are particularly applicable to estimates in Tyler, Murnane, and Willett (2000) that are based on a similar natural experiment. In both cases the results are most accurately interpreted as the returns to the labor market signaling value of the GED. Our results indicate that Texas employers gave little credence to the GED as a signal of productivity, at least for males. One reason for this may be that the low GED passing

threshold that existed in Texas for so long resulted in a credential that conveyed little information to employers. However, we find no support for this interpretation.

On the other hand, it may be that the GED has no signaling value, at least for males, regardless of the state and the historical passing threshold. This story is plausibly consistent with the Tyler, Murnane, and Willett (2000) results, provided there is a positive return to the GED signal for females, an area that needs further research. In the meantime we argue that our results are the most definitive to date regarding the value of the GED credential as a labor market signal for male dropouts.

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Table 1.
Summary Statistics by Year of Attempting the GED.

	Year Attempted the GED		Difference (1997-1995)
	1995	1997	
Highest Grade Attained	9.82	9.75	-0.06
Age when took GED	21.41	21.31	-0.10
Non-Hispanic White	42.0%	42.6%	0.5%
Hispanic	39.1%	38.9%	-0.1%
African American	14.1%	13.0%	-1.1%
Asian	1.2%	1.3%	0.1%
All Other	3.7%	4.3%	0.6%
GED in Spanish	3.9%	5.0%	1.1%
<u>Test Scores:</u>			
Writing Skills	45.45	45.70	0.25
Social Studies	48.60	49.77	1.17
Science	49.47	49.94	0.47
Interpreting Lit. & the Arts	47.69	48.61	0.92
Mathematics.	47.38	48.42	1.04
Average Score	47.72	48.49	0.77
Min Score	42.98	43.66	0.68
Passed the GED	78.7%	68.7%	-10.0%
<u>Pre-Test Taking Employment and Earnings:</u>			
Experience (Quarters)	7.56	7.59	0.03
<u>Average Quarterly Earnings</u>			
6 Years Prior to Attempting the GED	\$528	\$490	-\$39
5 Years Prior to Attempting the GED	\$623	\$568	-\$55
4 Years Prior to Attempting the GED	\$697	\$681	-\$16
3 Years Prior to Attempting the GED	\$824	\$852	\$28
2 Years Prior to Attempting the GED	\$1,025	\$1,050	\$25
1 Year Prior to Attempting the GED	\$1,214	\$1,241	\$27
Number of Individuals	28,449	24,734	

Table 2.
Summary Statistics by GED Outcome Group.

	GED Outcome Group					
	Failed		Treatment	Comparison	Passed	
	1995	1997	Passed 1995	Failed 1997	1995	1997
Highest Grade Attained	9.62	9.52	9.67	9.59	9.94	9.85
Age when took GED	22.57	22.16	21.22	21.53	21.03	20.97
Non-Hispanic White	27.1%	27.6%	31.9%	30.7%	51.1%	49.3%
Hispanic	46.4%	45.3%	44.7%	42.7%	34.4%	36.2%
African American	20.5%	20.5%	18.5%	20.4%	10.2%	9.6%
Asian	1.3%	1.6%	1.3%	1.4%	1.2%	1.2%
All Other	4.7%	5.1%	3.9%	4.8%	3.2%	4.0%
Took the GED in Spanish	2.5%	2.2%	1.8%	2.7%	5.1%	6.2%
<u>Test Scores:</u>						
Writing Skills	39.35	39.48	42.69	42.23	48.72	48.33
Social Studies	40.95	41.42	44.59	44.63	52.88	53.35
Science	41.83	41.85	45.42	45.25	53.75	53.39
Interpreting Lit. & the Arts	40.38	41.03	44.02	44.05	51.73	51.86
Mathematics.	40.20	40.40	43.87	44.11	51.30	51.80
Average Score	40.54	40.84	44.12	44.06	51.67	51.75
Min Score	36.86	37.16	39.88	40.13	46.36	46.41
<u>Pre-Test Taking:</u>						
Experience (Quarters)	7.82	7.60	7.19	7.85	7.59	7.56
<u>Average Quarterly Earnings</u>						
6 Years Prior to Attempting the GED	\$656	\$549	\$451	\$464	\$506	\$470
5 Years Prior to Attempting the GED	\$763	\$609	\$528	\$564	\$602	\$553
4 Years Prior to Attempting the GED	\$815	\$709	\$601	\$692	\$684	\$670
3 Years Prior to Attempting the GED	\$918	\$859	\$727	\$866	\$820	\$848
2 Years Prior to Attempting the GED	\$1,104	\$1,004	\$914	\$1,072	\$1,032	\$1,065
1 Year Prior to Attempting the GED	\$1,229	\$1,152	\$1,089	\$1,247	\$1,251	\$1,274
Number of Individuals	6,311	6,303	5,590	1,532	16,548	16,899

Table 3.

Estimated Quarterly Earnings based on Specifications (1), (2) and (3).

Variable	Specification	Model 1	Model 2	Model 3	Model 4	Model 5
		Simple OLS	Simple OLS	Simple OLS	Fixed Effect	Discontinuity
Intercept		-6441.09 (27.85)	1178.41 (6.04)	-60.35 (0.18)	-3857.60 (41.21)	2750.41 (4.02)
GED Holder		60.26 (2.74)	-76.12 (4.72)	-129.93 (5.53)	222.04 (26.08)	50.48 (0.97)
<u>Years Since Took the GED:</u>						
2nd Year*GED Holder		124.67 (8.25)	126.26 (8.36)	126.28 (8.36)	-18.99 (1.51)	168.13 (4.67)
3rd Year*GED Holder		213.10 (10.38)	214.72 (10.46)	214.67 (10.46)	60.94 (4.87)	218.07 (4.55)
4th Year*GED Holder		296.66 (12.25)	298.39 (12.33)	298.28 (12.32)	141.31 (11.27)	250.57 (4.43)
5th Year*GED Holder		363.00 (13.20)	364.28 (13.27)	364.44 (13.27)	204.37 (16.30)	289.58 (4.56)
2nd Year		38.52 (2.09)	119.79 (7.13)	120.44 (7.17)	157.74 (12.84)	-59.81 (1.38)
3rd Year		74.83 (2.44)	233.73 (8.69)	234.86 (8.74)	147.59 (10.03)	9.39 (0.13)
4th Year		89.66 (2.07)	324.53 (8.77)	326.20 (8.82)	131.68 (7.23)	76.18 (0.72)
5th Year		142.88 (2.57)	457.26 (9.77)	458.27 (9.80)	159.24 (7.47)	182.65 (1.29)
Attempted GED in 1995		-49.53 (2.27)	15.67 (0.89)	26.03 (1.45)		
<u>Other Control Variables:</u>						
Highest Grade Attained		139.92 (17.73)	72.83 (11.84)	69.96 (11.30)		62.07 (4.83)
Age		303.49 (16.46)	218.43 (12.47)	219.15 (12.51)	373.75 (71.88)	173.70 (3.87)
Age Squared/100		-348.15 (9.96)	-344.34 (10.51)	-345.55 (10.55)	-536.78 (71.62)	-273.91 (3.45)
Age when Took GED		167.27 (6.48)	-328.74 (14.57)	-324.08 (14.39)		-262.22 (4.94)
Age when Took GED ² /100		-314.07 (6.05)	524.70 (11.81)	517.57 (11.67)		406.85 (4.04)
Hispanic		-29.96 (1.23)	90.83 (4.65)	97.78 (4.91)		136.89 (3.11)
African- American		-1273.55 (43.83)	-506.25 (21.35)	-473.20 (19.45)		-468.41 (9.11)
Asian		-324.31 (3.12)	151.14 (1.68)	82.74 (0.92)		274.04 (1.33)
Other Ethnicity		-399.84 (7.25)	-74.79 (1.65)	-64.62 (1.43)		-10.96 (0.11)
Took GED in Spanish		-380.71 (5.89)	64.38 (1.27)	54.86 (1.04)		

Continued...

Table 3, Continued

	Model 1	Model 2	Model 3	Model 4	Model 5
Time Fixed Effect Included	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Experience	No	Yes	Yes	No	Yes
Pre-Test Taking Earnings	No	Yes	Yes	No	Yes
GED Test Scores	No	No	All Tests	No	Average
Number of Observations		1,044,604		1,414,535	200,583
Number of Individuals		52,251		52,251	10,033
R-squared	0.080	0.277	0.278	0.100	0.286

Note: t-values based on robust standard errors are shown in parentheses. Furthermore, the standard errors are obtained relaxing the independence across individual observations assumption.

Table 4.

Estimated Quarterly Earnings Differences, GED Holders and Non-GED Holders, based on Results in Table 3.

Year Since Attempted the GED	Estimated Earnings Difference: GED Holders - Non-GED Holders				
	Model 1 Simple OLS	Model 2 Simple OLS	Model 3 Simple OLS	Model 4 Fixed Effect	Model 5 Discontinuity
First Year	60.26	-76.12	-129.93	222.04	50.48
Second Year	184.92	50.15	-3.65	203.05	218.62
Third Year	273.35	138.60	84.74	282.98	268.56
Fourth Year	356.91	222.27	168.36	363.35	301.06
Fifth Year	423.26	288.16	234.51	426.41	340.06
<u>Test H₀: GED Effect=0, P-value.</u>					
First Year (t-test)	0.006	<0.001	<0.001	<0.001	0.33
Second Year (F-test)	<0.001	0.012	0.890	<0.001	<0.001
Third Year (F-test)	<0.001	<0.001	0.004	<0.001	<0.001
Fourth Year (F-test)	<0.001	<0.001	<0.001	<0.001	<0.001
Fifth Year (F-test)	<0.001	<0.001	<0.001	<0.001	<0.001
<u>Included Controls</u>					
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Age, Schooling, Ethnicity	Yes	Yes	Yes	Age	Yes
Pre-Test Taking Experience	No	Yes	Yes	No	Yes
Pre-Test Taking Earnings	No	Yes	Yes	No	Yes
GED Test Scores	No	No	All Tests	No	Average
Number of Observations		1,044,604		1,414,535	200,583
Number of Individuals		52,251		52,251	10,033
R-squared	0.080	0.277	0.278	0.100	0.286

Table 5.

Estimated Quarterly Earnings based on Specification (4) – Natural Experiment.

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept	1781.44 (78.50)	1037.98 (15.95)	-6798.64 (28.64)	2102.53 (9.16)	-315.99 (1.55)	765.26 (3.86)
All Passing Group (Pre and Post)	77.45 (3.26)	75.29 (3.16)	97.46 (4.17)	5.88 (0.29)	-48.75 (2.84)	-68.91 (4.01)
Affected Score Group (ASG)	190.97 (3.06)	185.11 (2.97)	264.50 (4.54)	188.36 (3.75)	157.62 (3.51)	138.30 (3.11)
Before	-88.62 (4.18)	423.76 (8.30)	217.57 (3.92)	345.54 (7.04)	314.38 (7.32)	327.59 (7.63)
Before*ASG	-286.63 (4.24)	-285.83 (4.23)	-246.39 (3.89)	-182.86 (3.34)	-175.78 (3.64)	-166.93 (3.48)
<u>Years Since Took the GED:</u>						
2nd Year	305.30 (20.14)	66.23 (2.51)	15.41 (0.63)	52.25 (2.24)	56.47 (2.52)	61.31 (2.75)
3rd Year	531.16 (25.68)	120.71 (2.50)	21.58 (0.49)	95.13 (2.30)	103.76 (2.63)	113.39 (2.89)
4th Year	640.56 (26.77)	194.64 (2.76)	46.36 (0.73)	146.04 (2.46)	160.33 (2.85)	171.55 (3.06)
5th Year	578.51 (21.33)	345.21 (3.53)	149.77 (1.69)	270.28 (3.29)	290.29 (3.72)	300.20 (3.86)
2nd Year*All Passing	126.70 (7.97)	127.98 (8.05)	117.78 (7.39)	118.66 (7.45)	117.73 (7.39)	117.98 (7.41)
3rd Year*All Passing	238.71 (11.01)	240.14 (11.08)	219.35 (10.07)	220.91 (10.15)	219.08 (10.07)	219.55 (10.09)
4th Year*All Passing	346.87 (13.60)	349.82 (13.72)	318.90 (12.42)	321.16 (12.51)	318.44 (12.41)	319.12 (12.44)
5th Year*All Passing	440.58 (15.19)	443.98 (15.32)	402.39 (13.76)	405.43 (13.87)	401.78 (13.75)	402.69 (13.78)
2nd Year*ASG	-90.96 (2.05)	-86.86 (1.96)	-91.41 (2.07)	-90.74 (2.05)	-91.14 (2.06)	-91.03 (2.06)
3rd Year*ASG	-17.50 (0.31)	-13.83 (0.25)	-24.51 (0.43)	-23.13 (0.41)	-23.94 (0.42)	-23.72 (0.42)
4th Year*ASG	346.87 (13.60)	82.55 (1.26)	67.22 (1.02)	68.51 (1.04)	67.18 (1.02)	67.46 (1.03)
5th Year*ASG	116.05 (1.62)	125.35 (1.75)	104.15 (1.46)	105.79 (1.48)	103.94 (1.45)	104.34 (1.46)
2nd Year* Before	-101.64 (6.87)	-83.91 (3.21)	-70.97 (2.77)	-58.71 (2.39)	-58.48 (2.49)	-53.68 (2.28)
3rd Year* Before	-68.67 (3.33)	-160.74 (3.86)	-137.91 (3.38)	-118.50 (3.01)	-118.30 (3.11)	-111.04 (2.92)
4th Year* Before	79.61 (3.25)	-223.86 (4.15)	-186.50 (3.52)	-157.39 (3.11)	-157.25 (3.22)	-147.08 (3.02)
5th Year* Before	358.63 (12.83)	-326.83 (4.33)	-276.70 (3.77)	-233.00 (3.37)	-232.70 (3.51)	-216.81 (3.28)
Continued...						

Table 5, Continued	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
2nd Year*Before*ASG	203.16 (4.20)	200.91 (4.15)	198.69 (4.10)	198.78 (4.10)	198.28 (4.09)	198.39 (4.10)
3rd Year*Before*ASG	202.33 (3.22)	198.07 (3.16)	195.20 (3.11)	194.86 (3.10)	194.07 (3.09)	194.20 (3.09)
4th Year*Before*ASG	156.53 (2.12)	153.36 (2.09)	145.93 (1.98)	146.48 (1.99)	145.29 (1.97)	145.56 (1.98)
5th Year*Before*ASG	137.36 (1.69)	132.12 (1.63)	122.96 (1.51)	123.82 (1.52)	122.44 (1.51)	122.80 (1.51)
<u>Other Control variables:</u>						
Highest Grade Attained			138.20 (17.51)	92.49 (13.15)	87.30 (14.03)	72.56 (11.78)
Age			285.46 (13.65)	171.68 (8.54)	202.13 (10.39)	183.91 (9.51)
Age Squared/100			-342.14 (9.78)	-314.75 (9.33)	-347.19 (10.57)	-338.81 (10.33)
Age when Took the GED			185.21 (6.75)	-417.96 (16.08)	-214.61 (9.08)	-296.08 (12.48)
Age when Took the GED^2/100			-320.09 (6.15)	789.22 (16.19)	355.34 (8.10)	522.17 (11.73)
Hispanic			-19.40 (0.79)	20.10 (0.91)	68.17 (3.46)	92.72 (4.72)
African- American			-1260.76 (43.06)	-735.73 (28.34)	-572.17 (23.41)	-505.13 (21.19)
Asian			-317.04 (3.05)	249.11 (2.56)	40.88 (0.45)	152.67 (1.69)
Other Ethnicity			-392.69 (7.12)	-153.20 (3.04)	-116.17 (2.55)	-73.07 (1.62)
Took GED in Spanish			-396.88 (6.13)	86.67 (1.51)	-50.70 (1.00)	61.68 (1.21)
Time Fixed Effect Included	No	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Experience	No	No	No	Yes	No	Yes
Pre-Test Taking Earnings	No	No	No	No	Yes	Yes
Number of Observations			1,044,604			
Number of Individuals			52,251			
R-squared	0.018	0.020	0.080	0.193	0.273	0.277

Note: t-values based on robust standard errors are shown in parentheses. Furthermore, the standard errors are obtained relaxing the independence across individual observations assumption.

Table 6.

Estimated Quarterly Earnings Differences, GED Holders and Non-GED Holders, based on Results in Table 5.

Year Since Attempted the GED	Estimated Earnings Difference: GED Holders - Non-GED Holders					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
First Year	-286.63	-285.83	-246.39	-182.86	-175.78	-166.93
Second Year	-83.48	-84.92	-47.71	15.92	22.51	31.46
Third Year	-84.30	-87.76	-51.19	12.00	18.30	27.28
Fourth Year	-130.10	-132.47	-100.46	-36.38	-30.49	-21.36
Fifth Year	-149.28	-153.71	-123.43	-59.04	-53.33	-44.13
<u>Test H₀:GED Effect=0 (P-values)</u>						
First Year (t-test)	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Second Year (F-test)	0.269	0.261	0.504	0.804	0.706	0.595
Third Year (F-test)	0.324	0.304	0.532	0.872	0.795	0.697
Fourth Year (F-test)	0.169	0.161	0.270	0.665	0.706	0.790
Fifth Year (F-test)	0.136	0.124	0.203	0.515	0.542	0.612
<u>Included Controls</u>						
Time Fixed Effect	No	Yes	Yes	Yes	Yes	Yes
Age, Schooling, Ethnicity	No	No	Yes	Yes	Yes	Yes
Pre-Test Taking Experience	No	No	No	Yes	No	Yes
Pre-Test Taking Earnings	No	No	No	No	Yes	Yes
Number of Observations			1,044,604			
Number of Individuals			52,251			
R-squared	0.018	0.020	0.080	0.193	0.273	0.277

Table 7.
Robustness of Estimated GED Effect based on Natural Experiment.

Alternative Sub-Samples, Based on Model 6 Specification, Table 5							
Sample	<i>Test Once</i>	<i>Low Pass</i>	<i>High Fail</i>	<i>White</i>	<i>Non-White</i>	<i>Age 16-21</i>	<i>1998 Takers</i>
Intercept	800.92 (3.17)	1041.28 (3.44)	1680.67 (5.58)	1177.49 (3.18)	1214.64 (5.29)	-12418.06 (7.62)	815.22 (4.26)
All Passing Group (Pre and Post)	-62.50 (2.93)			-65.68 (2.10)	-20.19 (1.01)	-64.03 (3.59)	-34.06 (2.03)
Affected Score Group (ASG)	165.71 (2.31)	137.75 (2.99)	123.95 (2.62)	135.20 (1.62)	126.07 (2.41)	147.57 (3.23)	157.53 (3.91)
Before	310.70 (5.90)	145.99 (2.91)	195.07 (3.44)	243.35 (4.68)	129.41 (3.17)	313.50 (6.98)	247.94 (5.42)
Before*ASG	-190.15 (2.47)	-117.99 (2.25)	-151.31 (2.74)	-174.41 (1.97)	-118.96 (2.09)	-160.44 (3.27)	-164.68 (3.74)
<u>Years Since Took the GED:</u>							
2nd Year	70.79 (2.57)	193.30 (6.40)	130.28 (3.98)	65.62 (1.69)	121.99 (4.84)	111.54 (4.54)	162.21 (7.65)
3rd Year	159.50 (3.29)	321.04 (6.14)	235.52 (4.14)	112.89 (1.77)	233.84 (5.45)	187.85 (4.38)	300.40 (8.03)
4th Year	231.39 (3.33)	479.20 (6.94)	353.30 (4.68)	226.58 (2.70)	358.70 (6.27)	297.08 (4.82)	405.91 (7.47)
5th Year	355.09 (3.68)	736.91 (8.22)	603.69 (6.12)	436.50 (4.07)	549.36 (7.38)	503.50 (5.89)	
2nd Year*All Passing	120.26 (6.23)			82.30 (2.65)	139.46 (7.38)	79.93 (4.54)	101.65 (6.37)
3rd Year*All Passing	219.13 (8.29)			182.02 (4.29)	242.87 (9.45)	178.50 (7.46)	196.58 (9.14)
4th Year*All Passing	305.19 (9.68)			288.16 (5.68)	315.79 (10.47)	284.75 (9.80)	271.37 (10.66)
5th Year*All Passing	387.70 (10.77)			411.61 (7.15)	378.68 (11.04)	380.36 (11.47)	
Continued...							

Table 7, Continued

	Sample	<i>Test Once</i>	<i>Low Pass</i>	<i>High Fail</i>	<i>White</i>	<i>Non-White</i>	<i>Age 16-21</i>	<i>1998 Takers</i>
2nd Year*ASG		-102.64 (1.44)	-215.60 (4.69)	-76.83 (1.65)	-41.75 (0.47)	-112.53 (2.25)	-142.35 (3.03)	22.73 (0.56)
3rd Year*ASG		-41.98 (0.44)	-234.84 (4.00)	-7.60 (0.13)	-20.39 (0.18)	-23.97 (0.38)	-51.52 (0.82)	-20.49 (0.39)
4th Year*ASG		61.17 (0.58)	-213.97 (3.11)	95.27 (1.37)	288.16 (5.68)	93.97 (1.25)	-10.63 (0.14)	59.08 (0.99)
5th Year*ASG		39.82 (0.35)	-232.52 (3.11)	99.40 (1.31)	89.50 (0.62)	107.15 (1.32)	33.55 (0.41)	
2nd Year* Before		-63.05 (2.19)	-12.99 (0.33)	-34.43 (0.80)	-7.64 (0.18)	-40.99 (1.33)	-45.83 (1.81)	-93.63 (3.47)
3rd Year* Before		-139.62 (2.98)	-34.46 (0.54)	-75.70 (1.12)	-16.13 (0.24)	-104.60 (2.07)	-113.60 (2.73)	-172.21 (3.17)
4th Year* Before		-148.82 (2.47)	-132.54 (1.70)	-80.90 (0.97)	-88.64 (1.06)	-158.01 (2.54)	-151.57 (2.80)	-201.47 (2.53)
5th Year* Before		-197.93 (2.42)	-278.52 (2.73)	-225.40 (2.04)	-265.53 (2.48)	-224.34 (2.80)	-293.70 (3.98)	
2nd Year*Before*ASG		211.30 (2.72)	196.01 (3.68)	175.00 (3.18)	141.63 (1.49)	216.44 (3.89)	219.73 (4.25)	72.88 (1.61)
3rd Year*Before*ASG		243.93 (2.31)	187.64 (2.69)	158.09 (2.20)	183.53 (1.47)	189.69 (2.65)	233.73 (3.33)	175.21 (2.96)
4th Year*Before*ASG		210.37 (1.75)	181.49 (2.21)	91.78 (1.09)	195.69 (1.40)	124.16 (1.45)	220.40 (2.62)	119.30 (1.76)
5th Year*Before*ASG		235.25 (1.80)	152.83 (1.68)	111.12 (1.19)	122.92 (0.79)	125.69 (1.33)	214.43 (2.32)	

Continued...

Table 7, Continued

	Sample	Test Once	Low Pass	High Fail	White	Non-White	Age 16-21	1998 Takers
<u>Other Control variables:</u>								
Highest Grade Attained	84.04 (10.46)	75.32 (7.77)	70.64 (7.35)	106.18 (9.68)	36.64 (5.37)	78.48 (11.05)	54.53 (9.70)	
Age	176.70 (7.29)	247.01 (8.77)	190.76 (6.57)	240.65 (7.66)	172.73 (8.07)	172.04 (3.35)	192.33 (10.35)	
Age Squared/100	-312.82 (7.46)	-404.02 (7.66)	-370.25 (7.60)	-403.17 (6.62)	-284.21 (7.66)	-338.67 (2.73)	-289.79 (7.83)	
Age when Took the GED	-300.63 (10.01)	-359.41 (10.00)	-359.34 (10.20)	-389.41 (9.65)	-274.80 (10.15)	1158.37 (6.34)	-277.11 (12.24)	
Age when Took the GED ² /100	518.66 (9.12)	589.13 (8.23)	655.40 (10.04)	645.17 (7.90)	444.12 (8.70)	-3401.60 (6.87)	425.17 (9.08)	
Hispanic	77.62 (3.18)	118.82 (3.91)	99.52 (2.86)			78.71 (3.72)	85.30 (4.71)	
African- American	-552.71 (18.55)	-528.20 (14.34)	-443.21 (11.61)			-613.70 (23.58)	-449.78 (20.36)	
Asian	98.15 (0.93)	210.38 (1.42)	432.32 (2.48)			-23.54 (0.24)	137.41 (1.79)	
Other Ethnicity	-148.03 (2.68)	-131.89 (1.82)	-47.59 (0.65)			-106.15 (2.17)	-13.40 (0.31)	
Took GED in Spanish	111.08 (1.75)	-114.80 (1.31)	129.79 (1.13)	201.83 (0.17)	154.83 (3.05)	137.85 (1.98)	95.65 (1.99)	
Time Fixed Effect Included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Earnings	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	706,638	407,817	335,821	448,500	599,305	727,978	883,507	
Number of Individuals	35,332	20,400	16,799	22,430	29,982	36,404	55,244	
R-squared	0.277	0.284	0.314	0.251	0.294	0.175	0.308	

Note: t-values based on robust standard errors are shown in parentheses. Furthermore, the standard errors are obtained relaxing the independence across individual observations assumption.

Table 8.

Estimated Quarterly Earnings Differences, GED Holders and Non-GED Holders, based on Results in Table 7.

Sample	Estimated Earnings Difference: GED Holders - Non-GED Holders						
	(Alternative Sub-Samples, Based on Model 6 Specification, Table 5)						
Years Since Attempted the GED	Test Once	Low Pass	High Fail	White	Non-White	Age 16-21	1998 Takers
First Year	-190.15	-117.99	-151.31	-174.41	-118.96	-160.44	-164.68
Second Year	21.16	78.02	23.69	-32.78	97.48	59.29	-91.79
Third Year	53.78	69.64	6.78	9.12	70.74	73.29	10.54
Fourth Year	20.22	63.50	-59.53	21.29	5.20	59.96	-45.37
Fifth Year	45.10	34.84	-40.19	-51.49	6.74	53.99	N/A
<u>Test H₀:GED Effect=0 (P-values)</u>							
First Year (t-test)	0.014	0.025	0.006	0.048	0.036	0.001	<0.001
Second Year (F-test)	0.820	0.232	0.728	0.772	0.159	0.351	0.104
Third Year (F-test)	0.642	0.368	0.933	0.947	0.381	0.347	0.870
Fourth Year (F-test)	0.879	0.473	0.513	0.890	0.956	0.511	0.524
Fifth Year (F-test)	0.751	0.718	0.686	0.758	0.947	0.588	N/A
<u>Included Controls</u>							
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age, Schooling, Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Test Taking Earnings	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	706,638	407,817	335,821	448,500	599,305	727,978	883,507
Number of Individuals	35,332	20,400	16,799	22,430	29,982	36,404	55,244
R-squared	0.277	0.284	0.314	0.251	0.294	0.175	0.308

Figure 1.

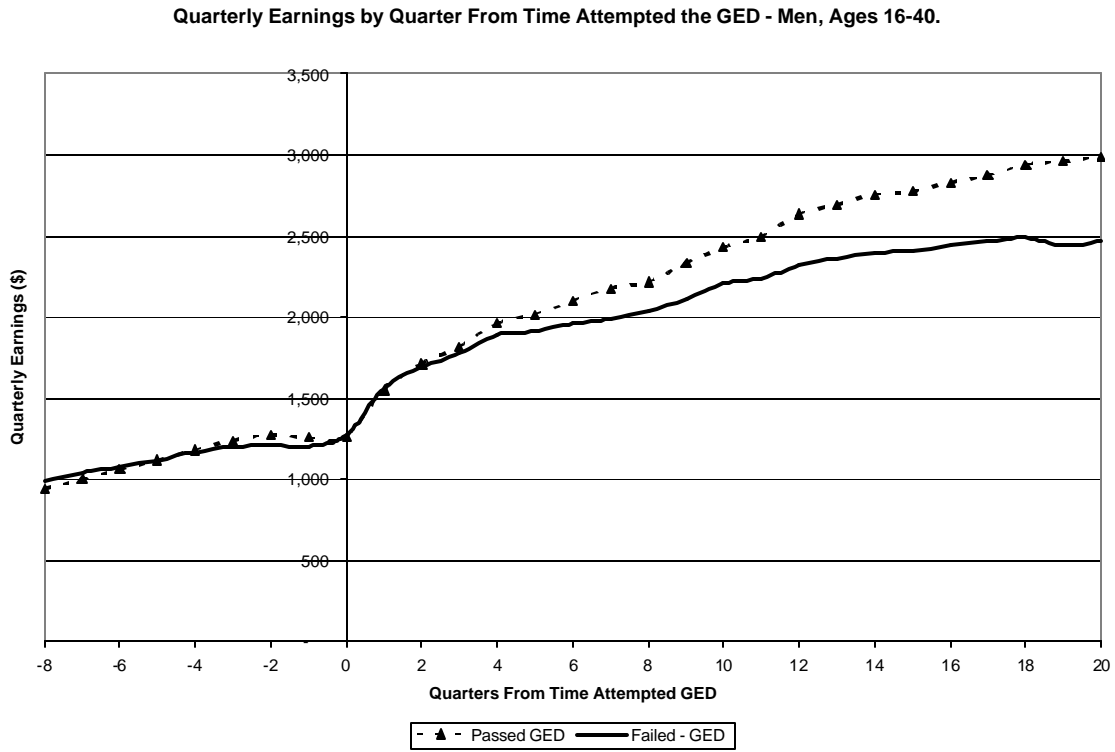


Figure 2.

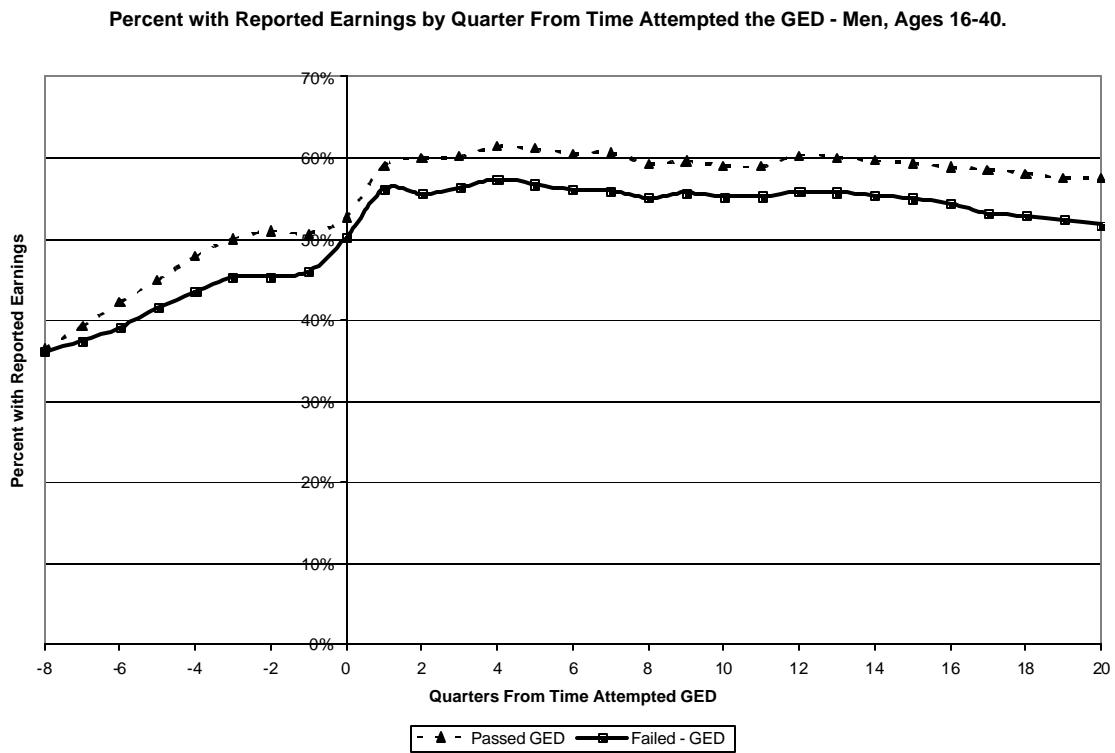


Figure 3.

