Changes in the Labor Market Returns to Obtaining a GED after the 2002 Test
Revisions*

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Abstract

General Educational Development (GED) credential recipients comprise an increasingly large share of the population of Americans with high school credentials. Why is their labor market performance so dissimilar from those with conventional high school diplomas? Capitalizing on 2002 policy changes that mandated the introduction of more rigorous GED passing requirements, we assess whether the “new” GED—which requires additional human capital to acquire but which retains the same market signal and credential value—improved the earnings of GED recipients (relative to diploma recipients). Using data from the U.S. National Longitudinal Survey of Youth 1997 we find no evidence that the relative labor market returns to the GED improved after the 2002 changes. This finding runs counter to expectations derived from human capital theory and is in line with signaling and credential theories.
Changes in the Labor Market Returns to Obtaining a GED after the 2002 Test Revisions

In the ten years between 1992 and 2002 the General Educational Development (GED) Testing Service issued 5.6 million GEDs, constituting roughly 16 percent of all high school-leaving credentials in the United States. Although returns to a GED may be beneficial for certain portions of the population, particularly high school dropouts with fewer cognitive skills (Tyler et al. 2000), most social scientists agree that the credential is not a profitable alternative to standard high school diplomas for most youth and young adults (Cameron and Heckman 1993). As GED holders make up an increasingly large proportion of the population, it is natural to ask why their labor market performance is so dissimilar from those with conventional high school diplomas. More specifically, what mechanisms link GEDs to economic outcomes?

Capitalizing on policy changes in 2002 that mandated the implementation of more rigorous GED passing standards, we ask whether and to what extent the “new” GED affected the relative earnings of GED recipients (as compared to high school diploma recipients). Contrasting the outcomes of recipients who passed the test before and after implementation of the new standards allows us to formulate conclusions regarding the mechanisms through which GEDs affect labor market outcomes.

Research on social mobility and socioeconomic attainment often supposes that differentials in earnings are due to variations in human capital accumulation (Becker 1993; Schultz 1961; Mincer 1958) and/or phenomena related to market signaling theory (Spence 1973). Within the context of high school equivalency tests, the former perspective presumes that expending the passing thresholds and skill sets required to obtain a GED should improve recipients’ work-relevant skills to the point of altering productivity and earnings. The latter
perspective suggests that GEDs act as an indicator which employers use to determine whether applicants possess a desired but unobservable set of skills, attitudes, and traits. In general, previous efforts to isolate and empirically test the relative merits of these theoretical positions on labor market returns to the GED have proven difficult for a variety of reasons, not least being that both explanations anticipate the same labor market outcomes for GED holders (Tyler 2003). As described below, hypotheses based on signaling theory concerning the wage effects of the 2002 changes to GED passing thresholds vary depending on the assumptions that we make about whether employers are aware of the 2002 changes.

In contrast to human capital and signaling perspectives, Collins’ theory of credentialism (1971; 1979) posits that employers achieve no productive ends by hiring employees with GEDs or other educational credentials. Instead, such credentials serve as indicators of whether potential employees’ status cultures match their own and whether those potential employees will conform to organizational norms. Since the 2002 changes to GED presumably did nothing to change the status cultures of test takers, this perspective suggests that there should have been no improvement in labor market returns to the GED after 2002.

The American Council on Education, which operates the GED Testing Service, worked with employers to revise the GED for 2002; one important goal of the revision was to ensure that individuals obtaining GEDs were prepared for the 21st century workplace. Implicit in the redesign was the assumption that raising GED passing thresholds would improve the workplace productivity and preparedness of GED recipients. Is this assumption warranted?

Identifying the mechanisms that link the GED to economic outcomes is thus more than a simple academic exercise. Aside from the number of people who take the GED test each year, a considerable amount of federal funding is directed to GED preparation programs. In fiscal year
2005-2006, the Adult Education and Family Literacy Act authorized over 560 million dollars in grants to state and local educational agencies—a large portion of which were used for the purpose of GED test preparation (United States Department of Education 2005). These costs might be justified if the relationship between GED receipt and earnings is one that is best characterized by human capital theory; that is, if preparing for the GED improves skills and capabilities that together generate non-trivial increases in worker productivity. On the other hand, if GEDs do not improve workers’ productivity (at least as evidenced by their earnings) then it would perhaps be appropriate to reevaluate the current level of federal funding.

This paper is divided into five sections. After briefly outlining the history of the GED—a discussion that pays special attention to policy changes that concern minimum passing standards—we describe the theoretical foundations that motivate our analysis. We then relate the present analysis to the few empirical studies that have sought to identify the mechanisms that link GED acquisition and labor market outcomes, emphasizing in particular those studies that exploit natural variation in testing standards. In the fourth section we describe our data and analytic approach. Finally, we conclude with a review of the findings and the main theoretical implications.

A brief history of GED test requirements

Originally designed to provide World War II service members with an alternative route to college admission, the GED has a long history as a “second-chance” credential. In 1954 there were roughly 42,000 GED test takers in the United States and Canada (Tyler et al. 2003); by the early 2000s this figure surpassed 1 million (GED Testing Service 2001). The test itself consists of a more than seven-hour battery of five multiple-choice and short-answer exams, which cover
mathematics, science, social studies, literature, and writing. The American Council on Education’s (ACE) GED Testing Service produces and administers the tests, and its Commission on Educational Credit and Credentials determines the minimum passing standards. Beyond the minimums, state educational agencies have the jurisdiction to implement additional requirements for acquiring the credential.

The initial wartime standards remained in force until 1978, at which time the ACE commission released the second edition of the GED test. In the original edition the “cut score” between failing and passing was aligned to the point at which 20 percent of graduating high school seniors could not pass the test. This corresponded to a score of at least 35 out of 80 in each subject area or an average of 45 across the entire battery. Presumably in response to the charge that acquiring a GED was too easy, the second edition raised the cut score to 40 on each subsection or a mean of 45 for all five. Passing requirements were raised again with the release of the 1988 series (which was subsequently revised in 1997), when the Commission established a new minimum score of 40 and 45—a standard that was designed to be prohibitive for 3 out of 10 high school graduates. Finally, and most importantly for the purposes of the present analysis, in January of 2002 the ACE released the fourth and current generation of the GED test. In the 2002 series, the passing scores are aligned to a level sufficient to ensure that 40 percent of graduating seniors would not pass (GED Testing Service 2003).

Figure 1 plots the relationship between these policy changes and yearly pass rates among GED test takers of all ages. By no means may the data be characterized as demonstrating trendless fluctuation over time. Pass rates declined markedly during the years in which the ACE raised minimum cut scores (e.g., 1978, 1988, 1997, and 2002), with the downtrends ranging in

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1 The ACE determines the appropriate cut score by fielding the test to a nationally stratified random sample of graduating seniors.
magnitude from 4.0 percent in 1997 to 15.5 percent in 1978. Though not conclusive, this suggests that ACE-mandated changes to the minimum passing standards are associated with the difficulty of acquiring a GED. Obtaining the credential has become progressively more challenging with the release of each new edition of the test, a trend that necessarily corresponds to progressively higher overall levels of human capital among GED recipients. In the next section, we consider what effect, if any, these changes might have for the earning potential of GED holders.

Three theories linking GED acquisition and earnings

Despite the exceedingly large amount of research within sociology, economics, and education policy that considers the economic value of a GED credential (Cameron and Heckman 1993; Heckman and LaFontaine 2006; Kerckhoff et al. 2001; Murnane et al. 1999; Murnane et al. 2000; Tyler et al. 1998; Tyler et al. 2003), there has been little more than speculation about why GEDs command the labor market returns that they do. Most common are interpretations that cite variations of market signaling theory or human capital theory (Boesel et al. 1998; Tyler 2003; Tyler et al. 2000). While for quite different—although not mutually exclusive—reasons, both predict GED holders to earn relatively higher wages than non-credentialed high school dropouts, but less than conventional high school diploma recipients.

Human capital theory provides the most compelling and politically viable rationale for the presence of testing programs like the GED. In its most basic form the theory asserts that individuals and society derive economic benefits from investments in people (Becker 1993; Mincer 1958; Schultz 1961; Schultz 1963). Education is viewed as a means to acquiring additional knowledge and skills; an investment that in turn encourages worker productivity, spurs micro- and macroeconomic growth, and permits individuals to command relatively higher...
wages. Baldwin (1990) found that GED candidates spend an average of 30 hours preparing for the test. However, there is substantial variation around the mean, suggesting that some individuals—presumably those with lower skills and/or those who speak English as a second language—may spend considerably more time studying for the exam, and thus may enjoy considerable increases in human capital-type skills. To the extent that successfully obtaining a GED requires the development of some non-trivial amount of work-relevant skills, we would expect that those with the credential earn more than otherwise possible. According to human capital theory, we would also expect that productivity—and thus earnings—will be highest among those recipients who obtained their degree after the implementation of the newer, more rigorous standards.

Market signaling theory anticipates similar labor market outcomes for GED holders. Originally articulated by Spence (1973) and Arrow (1973), and later elaborated by Stiglitz (1975), the standard signaling model contends that a substantial portion of workers’ ability is not immediately observable to the employer at the time that hiring decisions are made. Indicators like applicants’ educational attainment allow prospective employers to more expediently and effectively make hiring decisions within an informational structure characterized by uncertainty. This suggests that the returns that an individual receives for acquiring additional education do not owe directly to the acquisition of ability and productive skills, but rather to the scarcity of information that prospective employers have at their disposal and their more general regard for education per se (Benson 1978). Provided that GED receipt signals productive attributes like a mastery of basic skills and motivation, it is reasonable to expect GED holders to generally command relatively better wage offers than their non-credentialed counterparts.
In the case of our research question, however, hypotheses derived from the market signaling perspective hinge on whether we believe that employers (1) are aware that obtaining a GED became more difficult in 2002 and (2) know what year potential employees obtained their GEDs. If both of these things are true, then signaling theory would expect greater labor market returns to the GED after 2002. If either of these things is false, then signaling theory would not expect increased returns to the GED after 2002 because the post-revision GED would not send any different signal than the pre-revision GED. That is, the increased GED passing requirements would only affect the strength of a well-established market signal if employers were aware of the change in requirements and if they knew when applicants obtained their GEDs. We contend that it is unlikely that many employers are aware of this little-publicized change in GED passing thresholds, and thus our hypothesis based on market signaling theory is that the 2002 changes did not affect the relative wages of GED recipients.

In contrast to human capital theory and signaling theory, Collins’ theory of credentialism (1971; 1979) holds that employers value educational qualifications for reasons that have little or nothing to do with potential employees’ observed or unobserved skills or capacity for productivity. Instead, employers seek employees whose status cultures match their own and who will conform to organizational norms. Employers are engaged in an act of social closure whereby the ‘other’—those lacking the status distinction that educational agents are equipped to provide—is denied access to positions of power and prestige. According to Collins, the main activity of schools (and by extension, GED training programs) is not to impart knowledge or skills, as is commonly believed, but instead “to teach particular status cultures, both in and outside of the classroom” (1971: 1010). The GED is, of course, not deigned to measure status culture. Since credential theory views the skills and knowledge that are measured on the GED as
of relatively little consequence in the labor market, it would not anticipate any effect of the change in GED passing standards on labor market outcomes.

To summarize, if we find that the labor market rewards associated with earning a GED improved after the 2002 revisions, then this would provide evidence that GEDs affect labor market outcomes via the human capital required to obtain the GED. If we find no change in the labor market returns to the GED after the revision to the passing standards in 2002 then this would provide evidence that the labor market value of the GED (1) arises purely out of its value as a credential or (2) is derived from the signaling value of that credential, which did not change in 2002 because employers were unaware of the increased standards and/or do not know when potential employees obtained their GEDs.

Identifying the effects of the GED

Since human capital theory and market signaling theory both predict comparable labor market outcomes for GED holders, convincingly adjudicating between the two theories has proven challenging. As Frazis (2002) notes, “It is difficult to distinguish empirically between the effects of ability unobservable to the employer and that observable to the employer and not the analyst.” Unfortunately, even the research that does attempt to isolate the mechanisms linking GED acquisition to labor market outcomes tends to be limited in important ways. Using a data set constructed by the Social Security Administration and the GED Testing Service, for example, Tyler, Murnane, and Willett (Tyler et al. 2000) took advantage of variation in GED status generated by interstate differentials in passing standards. The creative research design paired individuals who passed the test in one state to individuals with identical scores—and presumably identical levels of human capital—who failed to meet more rigorous standards in another state. At least in theory, this approach allowed the authors to attribute differences in earnings to the
signaling effect of the GED net of human capital.

The findings presented by Tyler, Murnane, and Willett (2000) indicate that the GED market signal increases the earnings of young white dropouts by 10 to 19 percent, but has no observable effect among minorities. Their identification strategy and the inferences that follow, however, are only valid if the pool of potential GED candidates (e.g., high school dropouts) in a given state does not vary systematically on the basis of GED difficulty. Chaplin (1999) and Lillard (2004) have shown that this assumption does not hold: state GED policies have a measurable impact on rates of high school continuation and dropout. For example, if states increase the minimum passing score threshold beyond what is recommended by the GED Testing Service, the high school dropout rate falls by 9.8 to 12.5 percent (Lillard 2004), holding all else constant. As a result, it may be that the apparent signaling effect that Tyler and colleagues observed was, at least in part, a byproduct of a contaminated experiment, in which assignment to the “treatment” and “control” groups was not fully randomized.

In a separate but methodologically related paper, Lofstrom and Tyler (2004) exploited the implementation of more rigorous passing standards to compare earnings among those who failed the 1997 edition of the GED—but whose score would have been sufficient to pass the previous version—to similarly scoring individuals who took and passed the earlier version. Here, again, the authors attributed variation in earnings to the presence (or absence) of the GED market signal. Although their strategy alleviates some concerns about the validity of the experimental design by focusing on a single state, the nature of the data set did not allow the authors to completely control for confounders such as post-secondary educational enrollment and/or training. In addition to having a direct effect, there is considerable empirical evidence to suggest that GED holders benefit \textit{indirectly} from the credential by gaining access to otherwise
unobtainable educational opportunities (Berktold et al. 1998; Murnane et al. 1997; Murnane et al. 1999). Indeed, Lofstrom and Tyler (2004: 21) themselves note that “newly minted GED holders [may be] engaged in more post-attempt, post-secondary education, training, or job search than are comparison group members lacking a GED.” If this is in fact the case—that is, if individuals in the study’s treatment and control groups vary with respect to GED receipt and post-test human capital accumulation—then Lofstrom and Tyler’s estimates of the market signaling value of the credential may be spurious.

Our analysis—which considers the impact of an exogenous shift in GED testing policy on the wages of random sample of individuals from adjacent birth cohorts, some of whom obtained their educational credentials before 2002 and some of whom obtained their educational credential in 2002 or later—is designed to provide fresh evidence about the mechanisms through which GEDs affect labor market outcomes. As described above, if the change in GED passing thresholds positively affected the wages of GED recipients (relative to diploma holders) then this would be evidence in support of human capital theory. If the changes in passing thresholds produced no such effect, then this is evidence in support of signaling and/or credential theory.

Data and methods

This analysis will resolve many of the technical limitations outlined above by using geocoded data from the U.S. National Longitudinal Survey of Youth 1997 (NLSY97). Designed to characterize the school to work transition of U.S. residents born between 1980 and 1984, the nationally representative survey contains detailed information pertaining to labor market experiences, educational attainment and training, and income and assets. The NLSY97 includes a randomly chosen sample of 8,984 respondents who originated from 6,819 households. Members of the cohort were ages 12-16 at the outset of the survey and have been interviewed
annually since 1997. Through the eighth wave of the survey, which was fielded and completed between October 2003 and July 2004, 569 respondents reported that they obtained a GED. Using information pertaining to the date of GED receipt, we determined that 59 percent acquired their credential prior to implementation of the new standards; the other 41 percent, or 236 respondents, obtained their GED under the mandate of the newer, more difficult passing requirements.

Unlike the Current Population Survey’s October Education and Enrollment Supplement (CPS), which Chaplin (2002) and others have shown to severely underestimate the number of GEDs issued annually, the NLSY97 offers a relatively accurate measure of GED receipt and timing of passage. Table 1 compares NLSY97 and CPS-based estimates of the number of GED credentials issued to 16-19 year olds in calendar years 1999 through 2001 to official counts published by the ACE. Whereas the CPS underestimated official counts by as much as 67 percent, the NLSY97 estimates diverge by no more than 8 percent in any given year. This finding provides strong evidence that the NLSY97 is able to reliably portray GED acquisition within the population of interest.

Our analyses are initially restricted to 7,503 respondents to the 2004 NLSY (so that we can observe labor market outcomes). We further restrict the sample to the 6,878 respondents with complete information on educational attainment, labor force participation in 2003, and wages in 2003 (if employed at any point in 2003). All analyses are weighted by the BLS-provided NLSY wave eight sampling weight, which is to be used to account for differential probabilities of selection into the sample and for differential attrition from the sample. Because

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2 The NLSY97 has enjoyed relatively high retention rates, with 86.3 percent of the original sample taking part in the most recent round.
3 For both the CPS and NLSY97 sampling weights were used to inflate the sample to the total population size.
the NLSY97 is made up of a multi-stage stratified random sample instead of a simple random sample, Taylor series approximation is used to calculate correct standard errors. We estimate all models using the SVY commands in STATA 9.1.

Table 2 describes the key measures utilized in our analyses. For reasons outlined below, we estimate successive models that predict whether respondents have completed any post-secondary schooling; whether respondents worked at least one hour for pay in 2003; the total number of hours that respondents worked for pay in 2003; and respondents’ (logged) hourly wage rate in 2003. We also include a measure of experience, which equals the number of months that elapsed between when respondents left secondary education for the last time (as dropouts, with GEDs, or with diplomas) and the date of their 2004 survey. As shown in the first row of Table 2, diploma recipients are far more likely than GED recipients to have completed any post-secondary schooling and (partly as a function of their ages) respondents who earned GEDs prior to 2002 are more likely that those who received their GEDs later to have completed any post-secondary schooling. Diploma recipients enjoy more favorable labor market outcomes (with respect to labor force participation and wages) than GED recipients, and pre-2002 GED recipients fare better in the labor market than individuals with no credentials. At least partly as a function of their younger ages (and thus less experience), recent GED recipients fare worse in the labor market than any other group. Thus it is necessary to control for experience in our models of labor force outcomes.

Results

Our investigation of the relationship between GED acquisition, changes in passing standards, and recipients’ earnings proceeds in three stages. As noted above, a primary concern involves minimizing the amount of bias introduced by indirect returns to GED acquisition that occur via
post-secondary educational attainment. Thus stage one involves regressing the log-odds of completing any post-secondary schooling on (1) a dummy variable indicating whether respondents obtained a GED in any year; (2) a dummy variable indicating whether respondents obtained a GED in 2002 or later; and (3) a measure of experience. This parameterization allows us to formally test the null hypothesis that the relationship between obtaining a GED and completing any post-secondary schooling did not change after the implementation of the new GED passing standards. A null finding for the “GED in 2002 or beyond” variable would indicate that the level of difficulty of the GED is orthogonal to post-secondary schooling. If the data support such a conclusion, then it is permissible to subsequently constrain the sample to the 3,424 respondents with no post-secondary education—a restriction that effectively purges the sample of the potential for indirect effects.

In Table 3 we present the results of this model of post-secondary schooling, both for the full sample and separately by sex. Respondents who obtained a GED were substantially less likely \(e^{-2.45} = 91.2\) percent less likely) to have completed any post-secondary schooling by 2004. However, the model provides no evidence that the relationship between obtaining a GED and completing post-secondary schooling changed after the implementation of the more rigorous GED requirements.

In stage two of the analysis we draw on subset of 3,424 individuals with no post-secondary schooling to model the association between GED passing standards and labor force participation. Recognizing that any examination of earnings prompts important questions regarding selectivity, the principal objective of stage two is to establish whether it is necessary to

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4 The experience measure acts as a proxy for age in this model. Without controlling for experience we would conflate the timing of respondents’ GED receipt with their age and/or experience. Given the structure of the sample, individuals obtaining a GED in 2002 or beyond are younger and have thus had less time to complete post-secondary schooling (and, in subsequent models, to enter the labor market and increase their wages).
enter a correction term into the wage model specified below. If it can be demonstrated that increased GED passing standards do not significantly affect the likelihood (or level) of participation in the labor market, then it is possible to further restrict the sample to the economically active, and to proceed without explicitly adjusting for selection into the labor force. To do so, we estimate two models. In the first, the dependent variable expresses the log odds of working at least one hour for pay in 2003. In the second, the dependent variable expresses respondents’ total number of hours worked in 2003.

The results of the “any hours worked in 2003” model are shown in Table 4. Dropouts—defined as individuals with no secondary credential—are much less likely to have worked in 2003, but GED recipients are no less (or more) likely to have worked than diploma recipients, regardless of when they obtained their GED. Table 5 depicts a similar set of results for the “total number of hours worked in 2003” measure. Dropouts worked fewer hours in 2003 than diploma recipients, but GED recipients did not differ from diploma recipients with respect to this measure of labor force participation. The results presented in Tables 4 and 5 provide evidence that changes in GED passing standards are orthogonal to labor force participation. We thus precede with our wage models after limiting the sample to the 2,786 respondents who worked at least one hour for pay in 2003 (and who thus earned wages in that year) and without including any further correction for selection into the labor force.

Operating under the sampling criteria outlined above, in stage three we utilize ordinary-least squares regression (OLS) to estimate the extent to which GED passing requirements influence logged hourly wages. The results of this model are presented in Table 6, again for the full sample and separately for women and men. We find no evidence that the wage returns to the GED changed for the better once the new passing standards were implemented in 2002.
To check the robustness of our results we repeated all of our analyses again in three different ways. First, we re-estimated all of our models after selecting respondents in the bottom third of the distribution of scores on the Armed Services Vocational Aptitude Battery (ASVAB). Second, we re-estimated all of our models after selecting respondents in the middle third of the distribution of ASVAB scores. These supplementary analyses—the results of which are available upon request—were designed to understand whether the effects of changes in GED passing standards are concentrated among low- and middle-achieving students, respectively. Third, we re-estimated all of our models after including covariates for parents’ education, number of siblings, racial/ethnic background, sex, and local unemployment rate. The latter models were estimated to check for the possibility of suppressor effects. However, the results of all three sets of supplementary analyses mirror those presented in Tables 3 through 6.

Discussion

Alternative high-school leaving credentials like the GED offer an increasingly well-traveled route to the labor market. In fact, taking the GED examination has become so commonplace that in 2001—the year before the enactment of more demanding passing standards—the ratio of twelfth graders to GED candidates for the first time reached 3 to 1. The factors motivating such growth are numerous and suggest a variety of sociologically-relevant research questions.

In this paper we ask an empirical question that is designed to shed light on the mechanisms through which educational attainment (in this case, GED receipt) influences labor market outcomes. Our basic empirical question concerns changes in wage returns to the GED after that credential became more difficult to get in 2002. The corresponding sociological question addresses theories about whether the labor market principally rewards observable skills and knowledge (human capital theory), unobservable skills and knowledge that are presumed to
be signaled by educational credentials (market signaling theory), or evidence of status group membership (credentialing theory). Because we find no evidence that increasing the difficulty level of the GED exam improved the labor market value of that credential, we find support for credentialing theory; if we presume that employers may be unaware of the 2002 changes to the GED (or of when potential employees obtained their GEDs) then our results are also consistent with market signaling theory. The GED test may have become more difficult in 2002, but it would appear that neither the signaling value nor the credential value of the GED has increased.

Although we have checked the robustness of our results in a number of ways, properly identifying the “treatment effect” parameterized by the coefficient on the GED variables in Tables 3 through 6 requires that two important assumptions are met. The first is that GED candidates are randomly assigned to the pre- and post-2002 test groups, at least conditional on observed individual, familial, and state-level controls. There are a number of reasons to suspect that this might not be the case; perhaps most prominent among them being the potential for candidates to self-select into the pre-2002 group on the basis of the exam’s anticipated difficulty level. The second assumption is that the NLSY97 sample provides adequate statistical power to reject the key null hypotheses. This is problematic given the relatively small sample size, and is further complicated by the possibility that increased GED passing standards may only be consequential for a small subset of all GED passers—for example, test-takers who find themselves on the margin between passing and failing. In future work we will consider whether there is empirical evidence that these assumptions have been violated and we will attempt to establish through a series of sensitivity analyses the extent to which any resulting biases impact our substantive findings.
The answer to our basic empirical question has a number of practical implications. Perhaps the most important involves the way researchers operationalize educational attainment in their analyses. Should we treat GEDs as functional equivalents to high school diplomas? Or should we continue to treat GED recipients as closer to high school dropouts with respect to their labor market potential? Cameron (1992) and Cameron and Heckman (1991; 1993) settled this debate in a series of papers in the early 1990s, but scholars have begun to question whether their results remain relevant under the “new GED” testing regime (Mishel and Roy 2006). The results of our analyses suggest that the new GED holds the same labor market value as its predecessor, suggesting that we would at least do well to continue to distinguish GED recipients from regular diploma recipients in models of labor market (and likely other) outcomes.
References

### Table 1. Annual Number of 16- through 19-Year-Olds Receiving a GED, by Data Source: 1999-2001

<table>
<thead>
<tr>
<th>Year</th>
<th>(1) GED Testing Service Count</th>
<th>(2) 1997 National Longitudinal Study of Youth (NLSY97)(^1) Count</th>
<th>(3) October Current Population Survey (CPS)(^1) Count</th>
</tr>
</thead>
<tbody>
<tr>
<td>1999</td>
<td>225,601</td>
<td>226,464</td>
<td>105,168</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[(2)-(1)]/(1) = 0.004</td>
<td>[(3)-(1)]/(1) = -0.534</td>
</tr>
<tr>
<td>2000</td>
<td>211,844</td>
<td>207,811</td>
<td>68,917</td>
</tr>
<tr>
<td></td>
<td></td>
<td>-0.019</td>
<td>-0.675</td>
</tr>
<tr>
<td>2001</td>
<td>263,745</td>
<td>242,077</td>
<td>95,977</td>
</tr>
<tr>
<td></td>
<td></td>
<td>-0.082</td>
<td>-0.636</td>
</tr>
</tbody>
</table>

\(^1\) Counts for the NLSY97 and CPS represent weighted estimates.

Table 2. Descriptive Statistics for Educational and Labor Force Variables

<table>
<thead>
<tr>
<th></th>
<th>All Respondents (n=6,878)</th>
<th>Diploma Recipients (n=5,692)</th>
<th>Dropouts (No Credentials) (n=687)</th>
<th>GED (Pre-2002) (n=311)</th>
<th>GED (2002-04) (n=188)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Any Post-Secondary Schooling Completed?</td>
<td>53.9%</td>
<td>64.3%</td>
<td>0.0%</td>
<td>10.7%</td>
<td>6.1%</td>
</tr>
<tr>
<td>Worked at a Paid Job Any Hours in 2003?</td>
<td>87.6%</td>
<td>89.5%</td>
<td>75.1%</td>
<td>84.7%</td>
<td>81.6%</td>
</tr>
<tr>
<td>Total Hours Worked at All Paid Jobs in 2003</td>
<td>1,410.2 (849.3)</td>
<td>1,403.6 (840.6)</td>
<td>1,404.1 (899.2)</td>
<td>1,584.5 (894.2)</td>
<td>1,350.8 (859.4)</td>
</tr>
<tr>
<td>Effective Hourly Wage Rate for 2003 (Logged)</td>
<td>1.36 (2.19)</td>
<td>1.46 (2.08)</td>
<td>0.67 (2.69)</td>
<td>1.23 (2.45)</td>
<td>0.50 (2.80)</td>
</tr>
<tr>
<td>Experience (Months Since Leaving Secondary School)</td>
<td>619.69 (245.25)</td>
<td>598.45 (222.45)</td>
<td>873.62 (263.26)</td>
<td>670.61 (163.65)</td>
<td>249.32 (120.43)</td>
</tr>
</tbody>
</table>

Note: Sample restricted to individuals who responded to the 2004 NLSY and with complete information on all measures. See text for a description of these measures, as well as a discussion of the rate at which respondents were missing data on them. All analyses are weighted by NLSY 2004 (Round 8) sampling weight (cumulative cases methods).
Table 3. Logistic Regression Models of Whether Respondents Completed Any Post-Secondary Schooling (n=6878)

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b</td>
<td>b/se</td>
<td>b</td>
</tr>
<tr>
<td>GED (Obtained Any Year)</td>
<td>-2.45 (10.82)**</td>
<td>-2.33 (7.38)**</td>
<td>-2.56 (9.15)**</td>
</tr>
<tr>
<td>GED (Obtained 2002-2004)</td>
<td>-0.55 (1.47)</td>
<td>-0.37 (0.70)</td>
<td>-0.81 (1.41)</td>
</tr>
<tr>
<td>Experience</td>
<td>0.04 (2.22)*</td>
<td>0.05 (1.60)</td>
<td>0.04 (1.62)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.69 (1.54)</td>
<td>-0.60 (0.87)</td>
<td>-0.80 (1.42)</td>
</tr>
</tbody>
</table>

Note: Reference category is high school graduates with no experience. Sample restricted to individuals who responded to the 2004 NLSY and with complete information on all measures. See text for a description of these measures. All analyses are weighted by NLSY 2004 (Round 8) sampling weight (cumulative cases methods). Models estimated using the SVY commands in STATA in order to account for the complex sampling design in the NLSY.

** p < 0.01 ; * p < 0.05
Table 4. Logistic Regression Models of Whether Respondents Worked Any Hours in 2003 (n=3424)

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th></th>
<th>Women</th>
<th></th>
<th>Men</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b</td>
<td>[b/se]</td>
<td>b</td>
<td>[b/se]</td>
<td>b</td>
<td>[b/se]</td>
</tr>
<tr>
<td>Dropout</td>
<td>-0.76</td>
<td>(5.59) **</td>
<td>-1.01</td>
<td>(5.15) **</td>
<td>-0.50</td>
<td>(2.69) **</td>
</tr>
<tr>
<td>GED (Obtained Any Year)</td>
<td>-0.15</td>
<td>(0.81)</td>
<td>-0.21</td>
<td>(0.74)</td>
<td>-0.11</td>
<td>(0.42)</td>
</tr>
<tr>
<td>GED (Obtained 2002-2004)</td>
<td>-0.23</td>
<td>(0.98)</td>
<td>-0.34</td>
<td>(0.78)</td>
<td>-0.16</td>
<td>(0.47)</td>
</tr>
<tr>
<td>Experience</td>
<td>0.00</td>
<td>(0.46)</td>
<td>0.00</td>
<td>(0.21)</td>
<td>0.00</td>
<td>(0.80)</td>
</tr>
<tr>
<td>Constant</td>
<td>1.94</td>
<td>(13.32) **</td>
<td>1.82</td>
<td>(8.38) **</td>
<td>2.05</td>
<td>(10.77) **</td>
</tr>
</tbody>
</table>

Note: Reference category is high school graduates with no experience. Sample restricted individuals with no post-secondary schooling who responded to the 2004 NLSY and with complete information on all measures. See text for a description of these measures. All analyses are weighted by NLSY 2004 (Round 8) sampling weight (cumulative cases methods). Models estimated using the SVY commands in STATA in order to account for the complex sampling design in the NLSY.

** p < 0.01 ; * p < 0.05
Table 5. OLS Regression Models of the Number of Hours That Respondents Worked in 2003 (n=3424)

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b</td>
<td>[b/se]</td>
<td>b</td>
<td>[b/se]</td>
</tr>
<tr>
<td>Dropout</td>
<td>-447.11</td>
<td>(8.83)**</td>
<td>-426.74</td>
<td>(7.39)**</td>
</tr>
<tr>
<td>GED (Obtained Any Year)</td>
<td>-117.03</td>
<td>(1.66)</td>
<td>-85.55</td>
<td>(0.88)</td>
</tr>
<tr>
<td>GED (Obtained 2002-2004)</td>
<td>30.31</td>
<td>(0.29)</td>
<td>-5.97</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Experience</td>
<td>0.70</td>
<td>(8.60)**</td>
<td>0.46</td>
<td>(3.80)**</td>
</tr>
<tr>
<td>Constant</td>
<td>1246.57</td>
<td>(26.07)**</td>
<td>1210.36</td>
<td>(15.71)**</td>
</tr>
</tbody>
</table>

**Note:** Reference category is high school graduates with no experience. Sample restricted individuals with no post-secondary schooling who responded to the 2004 NLSY and with complete information on all measures. See text for a description of these measures. All analyses are weighted by NLSY 2004 (Round 8) sampling weight (cumulative cases methods). Models estimated using the SVY commands in STATA in order to account for the complex sampling design in the NLSY.

** p < 0.01 ; * p < 0.05
Table 6. OLS Regression Models of Respondents’ Logged Wage Rates in 2003 (n=2786)

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b</td>
<td>b/se</td>
<td>b</td>
</tr>
<tr>
<td>Dropout</td>
<td>-1.15 (7.25) **</td>
<td>-1.40 (5.65) **</td>
<td>-0.94 (5.37) **</td>
</tr>
<tr>
<td>GED (Obtained Any Year)</td>
<td>-0.30 (1.81)</td>
<td>-0.45 (1.36)</td>
<td>-0.21 (1.08)</td>
</tr>
<tr>
<td>GED (Obtained 2002-2004)</td>
<td>-0.21 (0.72)</td>
<td>-0.08 (0.15)</td>
<td>-0.28 (0.78)</td>
</tr>
<tr>
<td>Experience</td>
<td>0.00 (5.79) **</td>
<td>0.00 (3.23) **</td>
<td>0.00 (5.05) **</td>
</tr>
<tr>
<td>Constant</td>
<td>0.73 (5.29) **</td>
<td>0.69 (3.24) **</td>
<td>0.75 (4.28) **</td>
</tr>
</tbody>
</table>

Note: Reference category is high school graduates with no experience. Sample restricted individuals with no post-secondary schooling who worked at least one hour in 2003, who responded to the 2004 NLSY, and with complete information on all measures. See text for a description of these measures. All analyses are weighted by NLSY 2004 (Round 8) sampling weight (cumulative cases methods). Models estimated using the SVY commands in STATA in order to account for the complex sampling design in the NLSY.

** p < 0.01 ; * p < 0.05
Figure 1. GED Pass Rates, 1974-2003