The Signaling Value of a High School Diploma

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This paper distinguishes between the human capital and signaling theories by estimating the earnings return to a high school diploma. Unlike most indicators of education (e.g., a year of school), a diploma is essentially a piece of paper and, hence, by itself cannot affect productivity. Any earnings return to holding a diploma must therefore reflect the diploma's signaling value. Using regression discontinuity methods to compare the earnings of workers who barely passed and barely failed high school exit exams—standardized tests that students must pass to earn a high school diploma—we find little evidence of diploma signaling effects.

I. Introduction

According to the theory of human capital, individuals invest in education to increase their productivity and, therefore, their wages (Becker 1964). Human capital theory has been used to explain the life cycle profile of wages (Mincer 1974) and the distribution of wages (Becker 1967). It also underpins explanations for wage and productivity differences across cities (Moretti 2010) and for differences in the level and growth of productivity across countries (Lucas 1988; Romer 1990). Signaling theory provides an alternative rationale for educational investments. According to signaling theory, firms have imperfect information about worker productivity, and individuals invest in education to signal their productivity to firms and thereby increase their wages (Spence 1973). If signaling theory is important, it undermines explanations for economic phenomena based on human capital theory. It also implies that the social returns to education could be lower than the private returns to education. Since this implication contrasts with recent research suggesting that the social returns to education might be higher than the private returns to education, it has important policy implications (Moretti 2006).

The signaling and human capital theories are difficult to differentiate empirically, and this has made it hard to determine the practical importance of education-based signaling.[1] The basic problem is that both theories imply that there will be a positive effect of education on wages. The reason is that most types of education (e.g., years of schooling, school quality) could increase wages by improving productivity or by acting as productivity signals.

This paper aims to distinguish between the human capital and signaling theories by estimating the signaling value of a high school diploma. There are two reasons why a high school diploma is an interesting credential to analyze. First, it is the most commonly held credential in the United States.[2] Second, unlike other indicators of education such as years

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[1] Previous approaches to distinguishing these theories have tested whether the private returns to education exceed the social returns to education (as they would if signaling dominated any positive externalities associated with education), whether education policy changes affect the educational decisions of students they do not directly affect (as they would if those students wished to differentiate themselves from the directly affected students), and whether wage equations fit better for workers in occupations in which productivity cannot be observed (as they would if productivity expectations and hence wages in those occupations were based on signals such as education). See Lange and Topel (2006) for a review and critique of the relevant papers.

[2] In 2009, among individuals in the United States aged 25 and older, the highest degree attained is an associate's or higher for 39 percent, a high school diploma for 44 percent, and less than a high school diploma for 13 percent. The most common educational attainment categories are high school diploma (31 percent) and 4-year college degree (19 percent). These figures are authors' calculations based on census tabulations from http://www.census.gov/population/www/socdemo/education/cps2009.html.

of schooling, it cannot affect productivity because it is essentially only a piece of paper, despite the strong correlation between productivity and diploma receipt, precisely the reason why it could act as a productivity signal.[3] This implies that we could, conceivably, estimate the signaling value of a diploma by randomly assigning it among a small group of workers. By virtue of the random assignment, the signaling value of the diploma would be captured by the earnings advantage enjoyed by the workers randomly assigned to receive it. Of course, in the broader population, the measured earnings advantage enjoyed by workers with diplomas reflects this signaling value plus any productivity differences that firms observe.[4]

To mimic the random assignment of diplomas, we use high school exit exams, tests that students must pass in order to graduate from high school. These were first used in the 1980s and are now used in around half of US states.[5] Typically, students in these states are first administered these exams in grade 10 or 11; those who fail can retake them in grades 11 and 12. We focus on individuals who retook the exam at the end of grade 12 and compare those who barely passed and barely failed. Barely passers are much more likely to receive a diploma since retaking options are limited for students who fail at this stage. Barely passers and barely failers should, however, be similar in all other dimensions that matter for productivity since, under certain conditions (Lee 2008), passing status can be viewed as effectively randomly assigned for individuals with scores close to the passing cutoff. This implies that an estimate of the signaling value of a diploma can be based on the earnings differences between these two groups. We exploit this insight by applying fuzzy regression discontinuity methods (Hahn, Todd, and van der Klaauw 2001) to a large administrative data set that links individual-level high school records to information on postsecondary schooling and earnings for up to 11 years after high school graduation.

Our findings suggest that a high school diploma has little signaling value. Across a variety of specifications, the estimated diploma signaling values are close to zero and statistically insignificant. We obtain similar

[3] We find in our data that among workers who completed grade 12 and did not attend college, the diploma earnings differential—which will reflect the diploma productivity differential—is between 10 and 20 percent. Similar wage differentials are found in a variety of other data sets (e.g., Heckman and LaFontaine 2006).

[4] We are assuming that firms do not know which workers are in the experimental group in which diploma status is randomly assigned.

[5] By 2012, 26 states (containing 76 percent of high school students) had high school exit exams (Zabala et al. 2007). These exams were designed to create incentives to improve student achievement and to increase the value of a high school diploma. They are, however, controversial because of concerns that they hurt students from disadvantaged backgrounds (Peterson 2005). Dee and Jacob (2007) summarize the recent literature on exit exams.

results when we split the sample by sex and race and when we examine the time profile of signaling values. We also obtain similar results when we generate parallel estimates for another state (Florida) that operates a similar exit exam policy (see the online Appendix for details). Since a high school diploma could signal both high school completion (i.e., perseverance) and passing the exam (i.e., cognitive skills), we interpret our results as evidence that, at least among our sample of twelfth-grade exit exam retakers, neither high school completion nor high school diploma receipt is used to signal either cognitive or noncognitive skills.[6] This finding cannot be explained by high school diplomas being unrelated to productivity. Indeed, we find a strong correlation between diploma status and earnings both in our analysis sample consisting of twelfth-grade exit exam retakers and more generally among those who completed twelfth grade and did not go to college.

These findings differ from those obtained by previous studies, several of which estimate that high school diplomas and completion of twelfth grade are associated with large wage increases (Hungerford and Solon 1987; Jaeger and Page 1996; Park 1999; Frazis 2002). We argue that these earlier studies did not adequately control for observable (to firms but not to the econometrician) productivity differences between workers with and without these credentials.[7]

We conclude that our results provide a strong challenge to those who contend that employers use high school completion and high school diplomas to make inferences about unobserved productivity.

II.   Institutional Setting

This section describes the key features of the relevant institutions. First, we briefly describe how exit exams operate in Texas, the site for our study (the Appendix provides a more detailed description). We then describe

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[6] This statement assumes that workers are not able to signal high school completion independently of high school graduation. In principle, workers without a diploma could inform employers that they completed high school. However, as we explain below, since most students in our sample did not have a mechanism for signaling completion independently of graduation (e.g., via a certificate of completion) and since employers find it difficult to obtain grade information from schools (e.g., Bishop 1988), presenting employers with a diploma is likely to be the easiest and most convenient way for students to convey both high school graduation and completion of twelfth grade.

[7] Our results also differ from those reported in Tyler, Murnane, and Willett (2000), who used a difference-in-difference approach to estimating the return to a General Educational Development (GED) certificate, a credential that is typically pursued by high school dropouts that is intended to be equivalent to a high school diploma (Heckman, Humphries, and Mader 2010). Although they estimate large returns for some types of workers (between 10 and 20 percent), Jepsen, Mueser, and Troske (2010) argue that some of these estimates may be influenced by selective retaking of the GED exams. They find much smaller GED returns, as do some other studies, including Cameron and Heckman (1993) and Heckman and LaFontaine (2006).

exactly what the high school diploma represents and how diploma information can be conveyed to employers.

A.   *High School Exit Exams in Texas*

In order to earn a high school diploma in Texas, students must take and pass the minimum required courses set by state law. Aside from a relatively small number of students who receive special education waivers, students must also pass a standardized test known as a high school exit exam.[8] In Texas, the exit exam includes math, reading, and writing sections, and students without special education waivers must pass all three sections in order to graduate.

Students first take the exit exam in the spring of tenth grade or the fall of eleventh grade (this varies across cohorts during our study period). Following the initial attempt, there are retests administered periodically for students who failed. On the day of a retest administration, students who have not passed all sections of the exam are required to retake the unpassed sections.[9] Students can retake the exam after the end of twelfth grade (e.g., in the summer following twelfth grade or by returning to school for a thirteenth grade). However, doing so is relatively uncommon, as shown below by the strong relationship between the outcome of the final retest given in twelfth grade and eventual high school diploma status.

B.   *High School Diplomas in Texas*

We refer throughout to high school diplomas. Technically, however, students who graduate from high school earn a high school degree. According to Texas state law, the official indicator that a student earned a high school degree is a high school graduation seal (which is common throughout the state) on a student's high school transcript. The transcript—which has to conform to guidelines set by the state—includes information on courses completed, grades awarded, and the dates on which the exit exams were passed (if they were). Appendix figure A1 shows a sample Texas high school transcript.[10] Typically, students also receive paper diplomas at a school's commencement ceremony. While these diplomas

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[8] In Texas during our study period, about 7 percent of high school students who reached tenth grade received exemptions for some portion of the exit exam (Martorell 2005).

[9] Retests are administered once in the fall, spring, and summer of each year. There is also a retest given late in the spring for twelfth graders who have not yet passed the exam. In practice, students may not retake the exit exam if they are absent on the day the retests are administered or if they have already dropped out of school. Retaking is also much less common for the summer retest administrations, presumably because students do not attend school during the summer.

[10] This form and other information about high school transcripts can be found at http://www.tea.state.tx.us/index2.aspx?id=5974.

are not legally recognized proof of graduation, as we discuss in the next section, they could be used by workers as an indication that they graduated from high school.

State law grants districts the option of issuing certificates of completion for students who met all graduation requirements except passing the exit exam. Districts that do this must also indicate on the high school transcript that a student received a certificate of completion. This differs from the state seal that denotes whether a student earned a high school degree. Most districts do not appear to offer certificates of completion. According to a survey that we conducted, of the 35 largest districts in our sample (which account for nearly half of the students in our sample), only nine offer certificates of completion (31 percent when weighted by the number of students in our analysis sample in these districts).[11]

C. How Can Firms Acquire Information about Diploma Status and Other Educational Indicators?

There are two ways in which firms can acquire information about diploma status and other education indicators. First, they (or a firm acting on their behalf) can request transcript information from the worker's school. This would ensure that information obtained was accurate, but it could be time consuming and expensive. Even with the written consent of the worker, schools do not have to respond to these inquiries;[12] the evidence (Bishop 1988) suggests that responses may be slow or nonexistent.[13] In the event that they do respond, transcript information may be incomplete (e.g., in Texas, exit exam scores are not included on the transcript and may or may not be sent with the transcript) or difficult to interpret (e.g., course and grade information may be too terse to understand; exit exam scores cannot be understood without knowledge of the scale and passing thresholds, which have changed over time).[14]

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[11] The 35 districts for which we collected this information contain 48 percent of the students used in our analysis sample. The fraction of these districts that offer certificates (29 percent) is nearly identical to the student-weighted fraction that offer certificates (31 percent). Since this implies that there is no clear relationship between district size and offering certificates, we have no reason to suspect that certificates are more common in smaller districts and hence no reason to suspect that certificates are more or less likely in the subsample for which we collected this information. We also asked whether districts allow students to participate in graduation ceremonies if they fulfill all graduation requirements other than passing the exit exam. Ten out of the 35 districts in our sample do; 21 do not. We could not determine this information for the remaining four.

[12] Without the written consent of the worker, only "directory information" (which contains degrees awarded and dates of enrollment) can be requested.

[13] A study by Bishop (1988) found that Nationwide Insurance, "one of Columbus, Ohio's most respected employers," obtained permission to get all high school records for its applicants. Despite sending over 1,200 requests, it received only 93 responses.

[14] To learn about the information employers might receive when requesting a transcript, we requested an actual transcript of a student who attended a Texas public high school

Second, firms could ask workers to provide education information themselves. In this case, it seems likely that they would ask for a smaller amount of more easily interpretable information, with the high school diploma being of particular importance. The reason is that for students educated in most Texas districts, a high school diploma distinguishes a worker from the set of workers who did not complete high school and from the set of workers who completed high school but did not meet the other graduation requirements including passing the exit exam.[15] As such, if firms value high school completion and graduation as signals of unobserved productivity, then one might expect that holding a diploma would result in a significant earnings premium.

III.   Data and Descriptive Statistics

In this section we describe our data sources and report descriptive statistics for the analysis samples that we use. We provide a more detailed description of the data in the Appendix.

A.   Data Sources

We use a statewide data set from Texas that links administrative high school records to administrative postsecondary schooling records and Unemployment Insurance (UI) earnings records. These data contain information on demographic characteristics, high school enrollment and attendance, exit exam performance, high school graduation, postsecondary enrollment and attainment in the state's public colleges and universities, and earnings. We have these linked data for five cohorts: students who were in tenth grade in spring 1991–95 and who took the last-chance exam in 1993–97. The earnings data we use go through 2004 and information on postsecondary schooling goes through 2005. Thus, for all cohorts, we have labor market outcomes that go through at least 7 years and up to 11 years after taking the last-chance exam. Postsecondary schooling outcomes are available for all cohorts for at least 8 years. To the best of our knowledge, this is the first paper to use US data that link statewide

during our study period (i.e., eleventh grade between fall 1990 and 1994). In addition to the Academic Achievement Record (see App. fig. A1), the school sent an additional sheet that included standardized test results. In addition to scores on various other tests, this included the scores received for each exit exam subject, whether the student passed, whether the student mastered all objectives (a higher standard than passing), and the test date. It did not contain any information that would allow firms to interpret these scores (e.g., the minimum, maximum, mean, or passing threshold). We do not show this document because of poor image quality, but it is available on request.

[15] In districts that offer them, certificates of completion would also be a convenient way of allowing nongraduates who completed high school to distinguish themselves from dropouts who did not complete high school. However, as noted above, the evidence we collected suggests that only a minority of students in our sample attended schools in districts that offered certificates of completion.

administrative high school records to information on long-run outcomes. The obvious strength of these administrative data sets is that they contain large samples. In addition, they allow for a fairly long follow-up and contain information on GED receipt and postsecondary schooling. This allows us to assess the importance of potential threats to our research design stemming from effects of exit exam passing status on GED receipt and postsecondary schooling.

The data are limited in two ways. First, the UI data do not include measures of hourly wages, the best proxy for productivity. Instead, we examine various measures of total earnings. Second, three types of workers are not covered by the UI data: those paid "under the table" by employers who do not report their earnings to the state, those working out of state, and those working for the federal government. We characterize workers who have positive earnings but not in jobs covered by UI, and hence not observed in our earnings data, as having "false zero earnings." If passing the exam increases the probability of having false zero earnings, our estimates of the diploma effect on total earnings could be biased down. In Section IV.B, we discuss how we address the distinction between hourly wages and total earnings as well as the possible bias resulting from false zero earnings.

B. Analysis Samples and Descriptive Statistics

We report descriptive statistics in table 1. Columns 1–3 show sample means for the cohorts of students in tenth grade in spring 1991–95 (i.e., our study period), stratified by performance on the exit exam. Column 4 shows sample means for the primary analysis sample: the 37,571 students who took the last-chance exam. We label this the "last-chance sample."[16]

As seen by comparing columns 1 and 4 of table 1, students in the last-chance sample are more disadvantaged and have lower test scores than the full sample of exit exam takers. They are also more disadvantaged than the average student who failed the initial exam (col. 2) and the average student who failed the initial exam but passed prior to the last-chance exam (col. 3). Consistent with generally high completion rates among students still in school when the exit exam is first administered, the vast majority of students in these groups complete high school. This implies that the last-chance sample is not unusual in this respect. It also implies a role for the diploma in helping employers distinguish among workers who complete 12 grades of high school (but do not necessarily graduate). Compared to the other groups, the last-chance sample students

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[16] We also restrict the analysis to students who took the exam the first time it was administered to their cohort. This excludes a small number of students who missed the initial exam through illness or because they moved into the state after it was first offered. We make this restriction because it is useful to condition on the initial exam score.

TABLE 1
DESCRIPTIVE STATISTICS

	FULL SAMPLE (1)	FAIL INITIAL ATTEMPT (2)	FAIL INITIAL BUT PASS BEFORE LAST-CHANCE EXAM (3)	LAST-CHANCE SAMPLE		
				Mean (4)	Coefficient (5)	Standard Error (6)
Sample size	777,892	378,388	238,414	37,571		
Demographics:						
Male	.487	.474	.460	.421	−.020	.012
Black	.117	.170	.154	.246	−.003	.010
Hispanic	.289	.383	.363	.478	−.001	.012
Free or reduced-price lunch	.213	.303	.266	.409	−.004	.012
Limited English proficient	.040	.073	.052	.147	−.011	.008
Special education	.034	.060	.025	.034	−.006	.004
At grade level (initial attempt)	.770	.635	.746	.541	−.031	.012
Cohort 1 (grade 11, fall 1991)	.177	.183	.168	.356	−.006	.011
Cohort 2 (grade 11, fall 1992)	.174	.160	.168	.156	.006	.009
Cohort 3 (grade 10, spring 1993)	.214	.232	.226	.185	−.017	.009
Cohort 4 (grade 10, spring 1994)	.211	.210	.212	.157	.009	.008
Initial exam:						
Took all sections	.949	.895	.920	.956	−.004	.005
Reading (mean)	3.8	−1.4	.9	−5.7		
	(7.5)	(7.4)	(6.0)	(6.8)		
Math (mean)	.9	−8.7	−5.5	−14.9	.021	.021
	(11.7)	(9.3)	(7.7)	(7.9)		
Pass (%)	.514	0	0	0	−.005	.021
Total exam attempts in high school	2.052	3.159	3.054	5.727	−.008	.030
Predicted earnings	70,438.2	67,371.9	72,651.5	66,305.5	−323.194	328.563
High school completion:						
Complete grade 12	.812	.724	.880	.952		
Graduate	.768	.654	.848	.578		

NOTE.—See text for further description of the samples. Column 2 includes students who did not pass all three sections on the initial exit exam attempt, and col. 3 is further limited to students who retook and passed all sections of the test before their "last-chance" administration. Columns 5 and 6 report the estimated coefficients (and robust standard errors) on a "pass" dummy variable in regressions of these variables on the pass dummy and a second-order polynomial in the last-chance exam scores (interacted with the pass dummy). For the "high school completion" rows, "complete grade 12" refers to whether a student appeared in the final 6-week attendance period of grade 12, and "graduate" refers to whether a student earned a high school diploma within 2 years of the last-chance exam. The "predicted earnings" row refers to the fitted values from a regression of cumulative earnings 7 years after the last-chance exam on all of the demographics in the first panel, initial reading and math scores (missing values set to zero), and dummies for which sections were taken as part of the last-chance exam. This is the standard set of covariates used in the paper. Numbers in parentheses in the section on initial exam are standard deviations.

are much less likely to receive a diploma. Since they have not passed the exit exam before the last-chance administration, this is not surprising.

In column 5 we report the estimated discontinuities in baseline covariates for the last-chance sample; in column 6 we report the associated standard errors. These estimates suggest that among last-chance sample students with scores close to the passing threshold, passing status appears approximately randomly assigned. In particular, these estimated discontinuities are small and, except for one variable, statistically indistinguishable from zero.[17] This supports the assumption that there is not systematic sorting around the passing cutoff. Figure 1 is also consistent with this assumption since it suggests that the density of last-chance scores is continuous at the passing cutoff (the test proposed by McCrary [2008] fails to reject the null hypothesis of a continuous distribution).

IV.   Empirical Strategy

As noted in the introduction, we use a discontinuity strategy to estimate the signaling value of a diploma—leveraging the earnings difference between those who barely pass and barely fail high school exit exams. Because the exams are administered multiple times, there are several exams on which a discontinuity strategy could be based. We focus on individuals who took the last-chance exam administered at the end of twelfth grade. There are two advantages to focusing on this exam. First, unlike earlier exams, this exam cannot affect high school outcomes determined before the end of twelfth grade (e.g., whether or when students drop out of high school, the curriculum studied in high school). As such, there should be no differences in these outcomes by diploma status. Second, the outcome of this exam has a strong effect on the likelihood that students earn a high school diploma. The reason is that retake opportunities for students who fail this exam are limited and that students who pass this exam have typically met the other graduation requirements.

Figure 1 confirms that for students taking the last-chance exam, passing status has a strong effect on whether students eventually receive a high school diploma. This graph shows the distribution of last-chance scores and the relationship between last-chance scores and the likelihood of obtaining a high school diploma within 2 years of the last-chance exam. Because the exams test multiple subjects, we normalize these scores in relation to the relevant passing thresholds and define a

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[17] Since we conduct multiple tests, it is not surprising that we find one statistically significant discontinuity. The smoothness of these characteristics is demonstrated most clearly in the final row, which shows estimated discontinuities in the fitted values of a regression of total earnings over the first 7 years since the last-chance exam on the other covariates used in this table. The estimates suggest that this single-index summary measure of the baseline covariates is smooth through the passing cutoff.
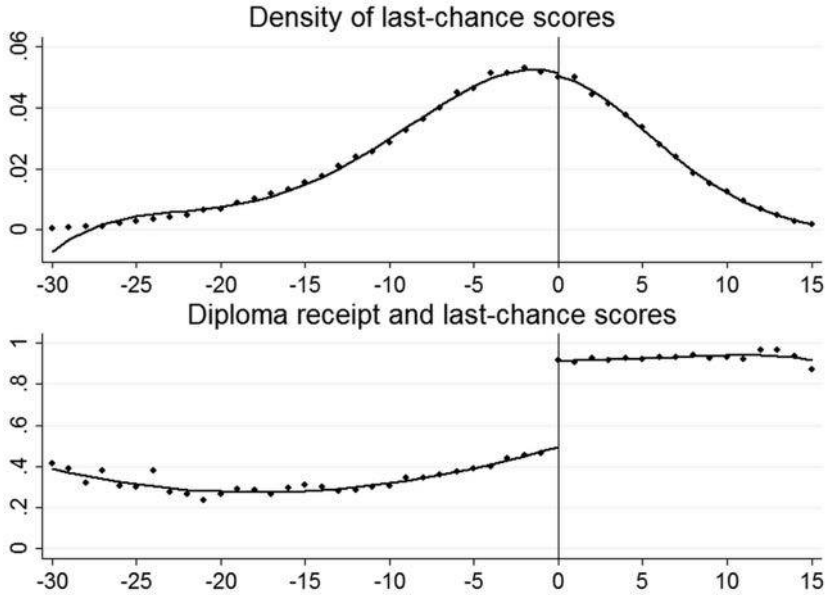
F‌IG. 1.—Last-chance exam scores and diploma receipt. The graphs are based on the last-chance sample. See table 1 and the text. Dots are test score cell means. The scores on the *x*-axis are the minimum of the section scores (recentered to be zero at the passing cutoff) that are taken in the last-chance exam. Lines are fourth-order polynomials fitted separately on either side of the passing threshold.

student's score to be the minimum of these normalized scores. As such, students pass if and only if this normalized score is nonnegative. The dots are cell means, and the lines are fitted values from a regression of diploma receipt on a fourth-order polynomial in the score (estimated separately on either side of the passing cutoff). The fraction of students with a diploma increases sharply as scores cross the passing threshold, from around 0.4 to 0.9. This implies that barely passing the last-chance exam substantially increases the probability of earning a diploma.

A. Main Estimates

We use fuzzy regression discontinuity methods (Angrist and Lavy 1999; Hahn et al. 2001) to exploit this discontinuity. In particular, we use passing status on the last-chance exam as an instrumental variable for diploma receipt in models that control for flexible functions of the exam scores (i.e., the variable on the horizontal axis in fig. 1). More formally, we estimate the following equations:

$$Y_i = \beta_0 + \beta_1 D_i + f(p_i) + \varepsilon_i, \tag{1}$$

$$D_i = \alpha_0 + \alpha_1 \text{PASS}_i + g(p_i) + \omega_i, \tag{2}$$

where Y_i represents a given labor market outcome (e.g., earnings) for individual i, D_i denotes high school diploma status, $f(p_i)$ captures the relationship between the outcomes and last-chance scores (p_i), PASS_i is an indicator for passing the exam, and ε_i and ω_i are error terms. The parameter α_1 in the first-stage equation is the discontinuity seen in figure 1. Provided that PASS_i and ε_i are orthogonal, PASS_i is a valid instrumental variable for D_i. This will be true provided that passing status near the cutoff is quasi-randomly assigned and that passing affects earnings only by changing the likelihood of earning a high school diploma. It also requires that differences in the earnings measure we use are informative about differences in lifetime earnings. We discuss these assumptions below.

As with any regression discontinuity application (Lee and Lemieux 2010), we must choose a method for modeling the relationship between the outcomes and the last-chance scores. We use "global polynomial" methods that exploit the full range of scores and specify f and g to be low-order polynomials.[18] One set of estimates use polynomials that can take different shapes on either side of the passing cutoff (i.e., fully interacted polynomials with a passing dummy). Since noninteracted polynomials fit the reduced-form earnings-score relationship well and since these generate more precise estimates, we also present results based on these more restrictive specifications.

When estimating the earnings discontinuity associated with passing the exam (the parameter α_1 in eq. [2]), we interpret the test score polynomial $f(p)$ as a statistical control designed to ensure that those discontinuity estimates capture the "jump" at the passing threshold. In Section VI, when we consider the various explanations for our findings, we will give an economic interpretation to this estimated earnings-score relationship.

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[18] As suggested by Lee and Lemieux (2010), the choice of polynomial was guided by minimizing the Akaike information criterion (AIC) statistic. The AIC statistic helps choose a functional form that balances the trade-off between generating a good fit and generating precise estimates. The AIC can suggest different functions for the first-stage and the reduced-form relationships, so we present estimates from models that use the more flexible of the suggested polynomials for both the first stage and reduced form. The other common approach to obtaining regression discontinuity estimates is to use "local linear" methods, which use data within only a narrow bandwidth of the passing cutoff and where f and g are linear functions (interacted with PASS). When the cross-validation method proposed by Imbens and Lemieux (2008) is used, the optimal bandwidth for the reduced form is fairly wide since the test score–earnings relationship is approximately linear. However, the optimal bandwidth for the first stage is much narrower because of the curvature in the test score–diploma relationship. With bandwidths this narrow, the estimated signaling effects are less precise. We focus, therefore, on the global polynomial results. Local linear methods generate similar results (available on request).

B.   Validity Checks

As noted, this strategy will deliver valid estimates of β_1 under three assumptions: first, the usual regression discontinuity assumption that passing status is quasi-randomly assigned close to the passing cutoff; second, that barely passing or failing the exit exam can affect earnings only through an effect on high school diploma receipt (i.e., the exclusion restriction necessary for PASS to be a valid instrument for D); and third, that differences in the earnings variables we use must be informative about differences in actual lifetime earnings. We now discuss our strategies for assessing these assumptions.

Quasi randomness of passing status on the last-chance exit exam.—As shown by Lee (2008), the key issue here is whether students have precise control over test scores. If not, then passing status among students with scores in the neighborhood of the passing threshold can be considered random. It seems plausible to suppose that students cannot control their scores on these tests. Moreover, the data are consistent with this assertion. As noted above, the distribution of scores is smooth around the passing cutoff, and we find little evidence of discontinuities in predetermined characteristics.

Exclusion restriction.—The second assumption underlying our approach is that diploma status can affect earnings only by affecting the likelihood of earning a high school diploma. The primary concern here is that passing status may affect earnings by changing the likelihood of earning a GED. To address this concern, we use the framework represented by equations (1) and (2) to estimate the effects of passing status on GED receipt. To preview the results, while passing status does have a statistically significant effect on this outcome, the magnitude of this effect is too small to undermine our main findings.

Validity of the earnings measures.—One issue with the earnings data we use is that earnings differences appearing shortly after the last-chance exam might reflect differences in college enrollment. We address this by showing that passing the exit exam has no effects on college attainment and only very short-lived effects on college enrollment.[19] A second issue is that the UI data contain information on total earnings, not hourly wages. To the extent that diploma effects on total earnings could be driven by diploma effects on hourly wages, employment, and hours worked, they could differ from diploma effects on hourly wages. There are two reasons why we feel comfortable using total earnings to measure diploma effects. First, since workers on either side of the passing cutoff should have sim-

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[19] The exclusion restriction could also be violated if passing the last-chance exit exam affects college enrollment and attainment and this has a labor market return independent of any effect operating through high school diplomas. This concern is also addressed with the empirical evidence showing small effects on college attainment.

ilar labor supply preferences, any differences in employment or hours worked likely reflect demand-driven employer decisions and hence could be argued to capture the full effect of diploma receipt. For example, if firms use credential information when making hiring decisions, there might be diploma effects on employment. Similarly, if the uncompensated wage elasticity of labor supply is positive (Blundell and MaCurdy 1999), any diploma wage premium will generate diploma effects on hours worked.[20] Second, total earnings measures enable comparisons with earlier signaling studies that used total earnings measures (Tyler et al. 2000; Tyler 2004; Lofstrum and Tyler 2007; Jepsen et al. 2010).

A third issue is that, if diploma receipt affects the likelihood of having false zero earnings, our estimates could be biased. We assess this possibility in three ways. First, we consider the plausibility and possible magnitude of each reason for false zero earnings. Second, we estimate diploma effects for several subgroups of workers for whom the false zero problem should be small (e.g., those living in counties with low federal employment and out-of-state mobility rates). Third, we assume that all workers with zero earnings have false zero earnings but that true earnings for these workers are below the conditional quantile (e.g., median) of earnings among workers with their observed characteristics. We then estimate regression discontinuity versions of an instrumental variables quantile regression (IVQR) model that will, under this assumption, generate consistent estimates of the quantile treatment effects on total earnings.[21] We also estimate these IVQR models for the subsamples that we think are unlikely to have false zero earnings.

V.   Results

In this section we present our main estimates of the signaling value of a high school diploma. We begin by reporting estimates of the impacts of passing the last-chance exam on the probability of receiving a high school diploma. We then report estimates of the impacts of passing the

[20] One caveat to this point is that young adults with higher learning ability may invest in skill acquisition (e.g., postsecondary schooling). However, we present evidence below that earning a high school diploma does not affect college attainment, which suggests that earning a high school diploma is unlikely to reduce labor supply via effects on post–high school education.

[21] It is worth noting that this approach takes us part of the way toward estimating effects on hourly wages. In particular, if these subsamples exclude workers with false zero earnings, then we will be assigning zero earnings to workers with true zero earnings. Assuming that these workers face potential earnings opportunities below the conditional quantile of earnings given their observed characteristics (even more plausible when we restrict the sample to men), the IVQR model will identify diploma effects on earnings opportunities. The only difference between this approach and similar approaches that focus on hourly wages (Johnson, Kitamura, and Neal 2000; Neal 2004) is that the positive earnings analyzed here also depend on hours worked.

last-chance exam on earnings and estimates of the impacts of receiving a high school diploma on earnings. Finally, we present results that support the validity of our estimates.

A. First-Stage Estimates

As shown in figure 1, students who pass the last-chance exam are much more likely to obtain a high school diploma. To investigate this relationship in more detail, table 2 reports estimates of the high school diploma effects of passing the last-chance exam. The first row shows the effect of passing the exam on the likelihood of receiving a high school diploma by the end of the summer after the last-chance exam. Once we move beyond a linear specification, these estimates are robust to the test score polynomial used (compare cols. 2–4). The preferred polynomial used in columns 2 and 5 (chosen using goodness-of-fit statistics) suggests estimates of around 0.5. As expected, these are robust to the inclusion of baseline covariates (compare cols. 2 and 5).

One year after the exam the first-stage discontinuity falls to around 0.42 and remains stable at longer intervals (2 and 3 years). The discontinuity falls because some students who fail the last-chance exam pass during the following year or receive a special education waiver. Comparisons across the different rows of table 2 make clear that we would obtain similar results if we considered high school diplomas received within 1 year or within 3 years of the last-chance exam.

B. Estimates of the Effect of a High School Diploma on Earnings

Figure 2 shows earnings by the last-chance exam score. As before, the dots are cell means, and the lines are fitted values from a regression of earnings on a fourth-order polynomial in the score (estimated separately on either side of the passing cutoff). The figure shows the present discounted value (PDV) of earnings (using all available years and a discount rate of 0.05, as in Cunha and Heckman [2007], as well as earnings in years 1–3, 4–6, and 7–11 after the last-chance exam).[22] We examine PDV earnings or earnings pooled across years to streamline the presentation of our results and to generate more precise estimates of the earnings effects of a high school diploma. As discussed below, there is no evidence to suggest that we lose information by aggregating the data in this way. We chose these particular groupings with a view to capturing earnings effects in the short, medium, and long run. Although 11 years after high

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[22] We obtain very similar results when using a discount rate of 0.02.

TABLE 2
IMPACT OF PASSING THE LAST-CHANCE EXAM ON THE PROBABILITY
OF EARNING A DIPLOMA

Receive High School Diploma	(1)	(2)	(3)	(4)	(5)
By end of summer after 12th grade (sample mean = .363)	.545 (.007)	.484 (.009)	.481 (.012)	.475 (.016)	.486 (.009)
Within 1 year of last-chance exam (sample mean = .452)	.480 (.007)	.420 (.009)	.425 (.012)	.424 (.016)	.422 (.009)
Within 2 years of last-chance exam (sample mean = .465)	.472 (.007)	.415 (.009)	.419 (.012)	.417 (.016)	.417 (.009)
Within 3 years of last-chance exam (sample mean = .468)	.468 (.007)	.412 (.009)	.416 (.012)	.414 (.016)	.414 (.009)
Baseline covariates?	No	No	No	No	Yes
Degree of test score polynomial	1	2	3	4	2

NOTE.—The table is based on last-chance samples (see table 1 and the text). "Degree of test score polynomial" refers to the test score polynomials controlled for in these regressions (all interacted with a dummy for passing the exam). Column 5 presents estimates based on models that also control for covariates (see note to table 1). Robust standard errors are in parentheses. There are 37,571 observations in each panel.

school (when most individuals are around 30 years old) may seem too early to capture long-run effects, for workers with 12 years of schooling, the experience profile in annual earnings is fairly flat beyond age 30 (Heckman, Lochner, and Todd 2006). As such, it is unlikely that there are important signaling effects that appear beyond but not within our observation window.

Three features of these graphs stand out. First, from inspection of the *y*-axes, it is clear that average earnings are low in these years. This is expected: our sample consists of lower-ability individuals early in their careers, and these graphs include workers with zero earnings.[23] Second, there is a strong positive correlation between earnings and last-chance exam scores (i.e., the fitted lines are upward sloping). This is consistent with a strong positive correlation between earnings and high school diploma sta-

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[23] To check that these mean earnings numbers are reasonable, we analyzed data from the 1997 National Longitudinal Survey of Youth (NLSY). To make the NLSY as comparable as possible to the last-chance sample, we limited the sample to individuals who enrolled in at least grade 10 but did not earn a college degree. We also reweighted the NLSY sample so that it matched the distribution across gender, race, and test score percentiles (using the Armed Forces Qualification Test [AFQT] score in the NLSY and the initial exit exam score in the last-chance sample). Earnings in the reweighted NLSY data were broadly in line with earnings in the state-level administrative data set (details available on request). Note that since we have fewer follow-up years for the more recent cohorts, these figures are based on an "unbalanced panel" in years 7–11.
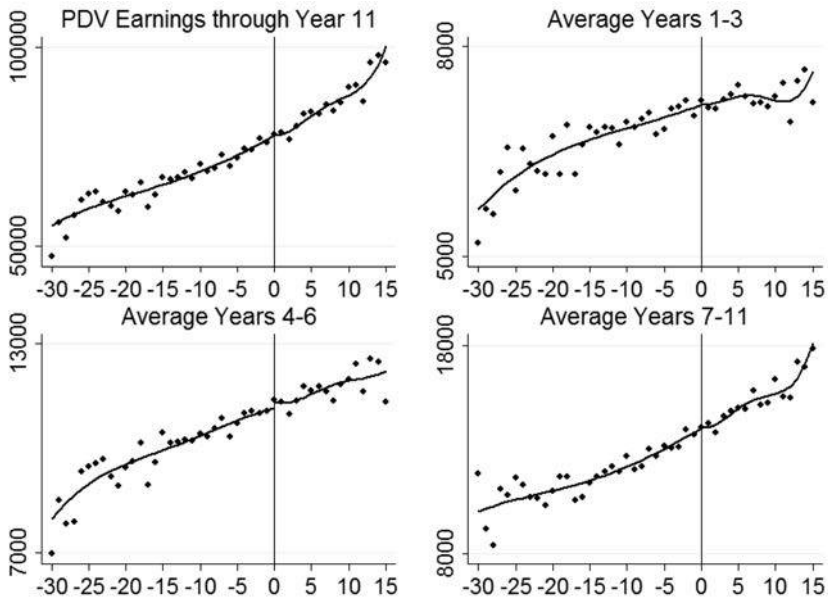
Fɪɢ. 2.—Earnings by last-chance exam scores. The graphs are based on the last-chance samples. See table 1 and the text. Dots are test score cell means. The scores on the *x*-axis are the minimum of the section scores (recentered to be zero at the passing cutoff) that are taken in the last-chance exam. Lines are fourth-order polynomials fitted separately on either side of the passing threshold.

tus even in the last-chance sample of students who remain in school until the end of grade 12. We return to this point in our discussion of the findings. Third, there is no indication of any jump in earnings at the passing cutoff.

The estimated discontinuities reported in table 3 are consistent with this last assertion. For each earnings outcome (i.e., for each year grouping), columns 1–4 report estimated discontinuities for first- through fourth-order polynomials, where the polynomials are fully interacted with an indicator for passing the last-chance exam. For each outcome, the estimated discontinuities are small in magnitude, small relative to the mean earnings of those who barely failed the exam (col. 1) and statistically indistinguishable from zero. Moreover, the estimates are robust to the choice of polynomial. Goodness-of-fit statistics suggest that the second-order polynomial is the preferred specification, and column 5 reports estimates from a model that uses this preferred polynomial and controls for baseline covariates. In column 6 we report estimates from a model in which the coefficients of the polynomial are restricted to be the same on either side of the passing cutoff. These estimates are more pre-

cise than those in column 5, and we cannot reject the hypothesis that the coefficients are the same to the left and right of the passing cutoff. Finally, we obtain similar results when we produce estimates for earnings in single years rather than earnings in particular groups of years.[24]

Table 4 reports instrumental variables estimates of the earnings effects of a high school diploma. Because the reduced-form discontinuities reported in table 3 are robust across a variety of specifications, we focus on estimates that use the preferred polynomial and adjust for baseline covariates. We report estimates based on the restricted polynomial and unrestricted polynomial specifications. The estimated effects are all positive but never statistically distinguishable from zero and always less than 3 percent of the mean earnings of individuals with exit exam scores just below the passing cutoff. When we focus on the results for PDV earnings, which combines information across all years, the estimated signaling value is only 2 percent of mean PDV earnings in the unrestricted polynomial specification and less than 1 percent of mean earnings in the restricted polynomial specification.[25]

While these estimates suggest that the signaling value of a high school diploma is, at most, small, it is possible that signaling values vary across different types of workers. To investigate this possibility, the first two panels of table 5 report separate estimates by race and gender. To save space, we report estimates only from models that use the restricted version of the preferred polynomial specification.[26] For all subgroups, the estimated returns to a high school diploma are small and not statistically distinguishable from zero. Moreover, for any subgroup comparison (e.g., men compared to women), we can never reject the hypothesis that the signaling effects are the same.

C.   Validity Checks

Quasi randomness of passing status on the last-chance exit exam.—As discussed in Section IV, our approach requires that there be uncertainty in

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[24] The estimates in years 1–11 are −32 (135), 45 (166), 124 (193), 265 (214), 118 (234), 34 (260), −44 (281), −38 (325), 23 (376), 244 (476), and 206 (623). Across all specifications, we can never reject the hypothesis that the earnings effect of passing the last-chance exam is the same in every year.

[25] The 95 percent confidence intervals exclude effects larger than about 10 percent of mean earnings. In an online appendix, we also report results using data from Florida, which has an exit exam policy similar to that in Texas. The results for Florida are very similar, although the data allow us to examine earnings only up to 6 years after taking the last-chance exam. When we combined the state-specific estimates using the variance-minimizing weights, i.e., the ratio of the inverse variance of the estimate for states to the sum of the inverse variances, to generate more efficient estimates of the earnings effects of a high school diploma, we obtain point estimates that are essentially zero and that rule out effects larger than 5–6 percent of mean earnings (details available on request).

[26] Estimates based on the unrestricted version (available on request) are similar.

TABLE 3
Impact of Passing the Last-Chance Exam on Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Years 1–3 (mean earnings: 7,006)	9.7	39.2	−29.4	8.3	45.7	50.0
	(110.8)	(151.0)	(191.1)	(233.5)	(146.8)	(118.1)
Years 4–6 (mean earnings: 11,055)	18.2	72.1	70.2	160.8	138.9	64.6
	(165.4)	(222.5)	(281.1)	(343.9)	(215.9)	(175.9)
Years 7–11 (mean earnings: 13,732)	134.6	−26.5	−138.8	87.7	40.3	21.2
	(237.6)	(319.7)	(401.7)	(496.2)	(311.0)	(257.5)
All years pooled (mean earnings: 10,743)	58.0	25.8	−37.4	85.7	73.3	44.0
	(151.4)	(203.5)	(255.4)	(312.9)	(196.4)	(162.3)
PDV earnings through year 11 (mean: 75,986)	318.6	1.5	−519.6	169.2	680.5	249.7
	(1,084.9)	(1,456.3)	(1,824.5)	(2,224.7)	(1,375.8)	(1,143.8)
Baseline covariates?	No	No	No	No	Yes	Yes
Degree of test score polynomial	1	2	3	4	2	2
Polynomial specification	Unrestricted	Unrestricted	Unrestricted	Unrestricted	Unrestricted	Restricted

Note.—Estimates in the first four rows were generated from a panel data set in which each observation represents a person-year, where year denotes time since the last-chance exam, and standard errors adjusted for clustering at the individual level are in parentheses. Estimates in the fifth row are from an individual-level data set in which the outcome is the PDV of earnings ($r = .05$) through the last available year (year 7 for the latest cohort, year 11 for the earliest cohort), and robust standard errors are in parentheses. Mean earnings refers to the mean just to the left of the passing cutoff (score = −1). Estimates in cols. 1–4 are the coefficients on an indicator for passing the last-chance exam controlling for a polynomial in the last-chance exam (fully interacted with an indicator for passing) that is fully interacted with year dummies. Estimates in col. 5 include baseline covariates (same as those used in table 2) fully interacted with year dummies. Estimates in col. 6 use the preferred polynomial (quadratic), where the polynomial is not interacted with a dummy for passing.

TABLE 4
IMPACT OF RECEIVING A HIGH SCHOOL DIPLOMA ON EARNINGS

	(1)	(2)
Years 1–3	109.5	119.8
	(352.0)	(283.2)
	1.6%	1.7%
Years 4–6	333.1	155.0
	(517.9)	(421.7)
	3.0%	1.4%
Years 7–11	99.3	52.2
	(767.0)	(634.0)
	.7%	.4%
All years pooled	177.7	106.4
	(475.9)	(392.9)
	1.7%	1.0%
PDV earnings through year 11	1,632.1	598.8
	(3,299.5)	(2,742.3)
	2.1%	.8%
Polynomial specification	Unrestricted	Restricted

NOTE.—Estimates use the same samples and specification as cols. 5 and 6 of table 3. Unrestricted specification estimates are from two-stage least-squares models in which the test score polynomial is fully interacted with an indicator for passing the last-chance exam. Results from the "restricted" polynomial specification are the ratio of the reduced-form and first-stage estimates, where these are estimated separately and the standard errors are calculated using the delta method. The reduced form is the estimated discontinuity in earnings using the polynomial specification in which the slopes are constrained to be equal on either side of the passing cutoff (col. 6 in table 3) and the first stage is estimated using a polynomial that is fully interacted with the passing dummy. All standard errrors are adjusted for clustering at the individual level. For each set of estimates, the third row represents the point estimate expressed as a percentage of mean earnings just to the left of the passing cutoff.

exit exam scores that prevents students from sorting around the passing cutoff. As discussed above, the evidence in table 1 and figure 1 suggests that this assumption is satisfied.

Exclusion restriction.—Our approach also requires that last-chance exit exam passing status affect earnings only by affecting high school diploma receipt. As noted above, the chief concern here is that passing status might affect GED receipt. One may worry that effects of last-chance exam passing status on GED receipt could explain our zero diploma effect estimates if failing the exam causes students to obtain a GED and if the GED has a positive return in the labor market. To examine the empirical significance of this possibility, Appendix figure A2 shows the relationship between last-chance exam scores and this outcome. These results suggest that students are indeed more likely to obtain a GED if they do not receive a diploma (lower-right panel of fig. A2) but that this effect is too small to affect our estimates. A back-of-the-envelope calculation shows that even under optimistic assumptions about the earnings

TABLE 5
Signaling Values of a High School Diploma for Subgroups

	By Gender		By Race		By County: Mobility and Federal Employment	
	Male	Female	White	Nonwhite	Low	High
Years 1–3	188.1	91.1	−8.9	154.1	137.9	89.7
	(546.8)	(303.6)	(561.1)	(322.6)	(393.7)	(407.4)
Years 4–6	452.0	8.7	−228.2	247.0	556.0	−276.6
	(814.2)	(449.9)	(838.7)	(480.9)	(584.9)	(607.5)
Years 7–11	492.0	−138.6	−124.4	19.8	−149.7	329.1
	(1,224.5)	(655.6)	(1,269.6)	(703.3)	(878.2)	(916.1)
Years pooled	383.5	−17.7	−120.7	135.8	163.7	57.3
	(765.5)	(407.2)	(791.3)	(438.5)	(551.6)	(557.7)
PDV earnings through year 11	2,473.8	−297.0	938.5	−1,457.0	1,037.9	114.7
	(5,391.4)	(2,824.2)	(3,035.7)	(5,621.4)	(3,885.9)	(3,849.0)

Note.—Estimates are from the preferred restricted polynomial specification with baseline covariates. See the text and note to table 4 for additional details.

effects of a GED, these GED effects would affect our diploma earnings estimates by, at most, 1 or 2 percentage points.[27]

Validity of the earnings measures.—Even if exit exam passing status close to the passing cutoff is quasi-random and the exclusion restriction holds, the estimated diploma effects we report will be valid only insofar as differences in the earnings measures we use are reasonable proxies for differences in lifetime earnings. One concern is that strong diploma effects on college enrollment could explain our zero diploma effect estimates since college students are less likely to be working, at least during their

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[27] For instance, in our data, having a GED relative to being an uncredentialed dropout is associated with about 7 percent higher earnings (this estimate is based on last-chance sample members who did not earn a high school diploma). This, coupled with the estimated impact of a diploma on the probability of receiving a GED (about 15 percentage points), suggests that our estimates may be reduced by about 1 percent of mean earnings. Of course this calculation assumes that the correlation between GED receipt and earnings is causal, while much of the evidence suggests that the GED has little effect on labor market performance (Heckman et al. 2010). In that case, any bias would be even smaller. Another way of thinking about the implications of GED acquisition is to note that acquiring a GED likely has little direct impact on productivity (Heckman et al. 2010). As such, its earnings return should reflect its signaling value. One possible assumption is that the signaling value of a GED is the same as the signaling value of a diploma (i.e., firms view GEDs as equivalent to high school diplomas). In that case, to estimate this signaling value, we need only redefine the credential variable D to be "high school diploma or GED." Since the impact of passing the last-chance exam on this alternative credential measure is about 0.35 (compared to the impact of receiving a regular diploma, which is 0.42) and since the "reduced-form" discontinuities in table 3 are close to zero, our main conclusions would be unaffected.

college years. Our results suggest that there are diploma effects on college enrollment but that these are short-lived. In particular, figure A2 shows that there are effects on enrollment in the year after the last-chance exam as well as on enrollment at any point in the 8 years following the last-chance exam. However, we see no effects on enrollment in the second year after the last-chance exam, and as discussed above, we find no evidence of effects on college attainment.[28]

Perhaps the most serious threat to our results is the possibility that receiving a high school diploma increases the likelihood of having false zero earnings, such that our estimates of the signaling effect of a high school diploma are downward biased. In analyses not reported, we found no evidence to suggest that high school diplomas affected the probability of having observed zero earnings, but this could still be consistent with impacts on the probability of having false zero earnings. For instance, diploma receipt could increase the likelihood of being employed but increase the likelihood of moving out of state. If these effects were the same size, we would find no impact on the probability of having observed zero effects, despite an effect on the probability of having false zero earnings.

There are three reasons why we do not think that false zero earnings are driving our results. First, a consideration of each source of false zeros suggests that these are unlikely to be quantitatively important. With regard to federal employment, the most relevant concern for workers in the age range we examine is military enlistment. However, individuals in the last-chance sample are unlikely to meet the military's aptitude requirements, at least assuming a rough correspondence between exit exam and AFQT scores.[29] Moreover, our estimates are similar for men and women, even though men are much more likely to enlist in the military. With regard to out-of-state mobility, we note that census data on out-of-state mobility suggest that any mobility effects are likely small. Among those in the relevant age group, the out-of-state mobility rate was only 3.4 percent higher for those with a high school diploma than for those without one.[30]

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[28] In results not shown, we also find no effect on enrollment in years 3–8 after the last-chance exam.

[29] The military accepts very few applicants below the 31st percentile of the national AFQT distribution (Asch et al. 2008). In contrast, the median initial-attempt score in the Texas last-chance sample is at the 12th percentile of the full-sample distribution.

[30] Calculations are based on the 5 percent 2000 Census Public Use Microdata Sample. The out-of-state mobility rate pertains to individuals who were living in Texas 5 years prior to the census and is defined as the percentage of individuals in this group who were living outside of Texas as of the 2000 census. The sample is restricted to (1) 23–24-year-olds (who were aged 18–19 5 years ago, roughly the age at which the last-chance exam is taken) and (2) US natives or individuals who were no older than 14 when they immigrated to the United States. We obtained similar results when we restricted the sample to workers who completed grade 12 but did not enroll in college.

With regard to the "black" economy, if high school diplomas have a positive signaling value, diploma holders would have incentives to avoid self-employment and the black economy.[31]

Second, we estimated effects separately by whether students did or did not attend high schools in counties with low federal employment and out-of-state mobility rates. Since these factors are related to the likelihood of having false zero earnings, then to the extent that false zeros were driving our results, the estimates for these two groups should be different. The evidence in the lower panel of table 5 provides no indication of such a difference.[32] Third, estimates based on a regression discontinuity instrumental variable quantile treatment effects estimator proposed by Frandsen, Frolich, and Melly (2010) show no evidence of diploma signaling effects. Provided that zero observed earnings imply that true earnings are below the relevant quantile (conditional on a worker's observed characteristics), estimates of the quantile treatment effect will be consistent.[33] We think that this condition is especially plausible for the low-mobility, low–federal employment sample, and especially for men. Both overall and for various subsamples in which we think that observed zero earnings are likely to be indicative of below-quantile earnings, we

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[31] Self-employment rates are slightly higher among workers with a high school diploma than among those without one (Georgellis and Wall 2000). However, since it is hard to see how a diploma benefits self-employed workers, this is unlikely to reflect a causal effect of diploma status on self-employment. Similarly, it is plausible that diplomas have no causal effect or possibly a negative effect on employment in jobs in which the pay is "under the table."

[32] To identify high out-of-state mobility counties in Texas, we used data from the 2000 census on county-level population outflows between 1995 and 2000 and intercensal estimates of the population of Texas counties in 1995. Counties with out-of-state mobility rates below the state median (weighted by population), which is about 6 percent, were classified as "low-mobility" counties. To identify counties with high federal employment rates, we obtained county-level data on the number of civilian workers employed by the federal government from the Office of Personnel Management (http://www.opm.gov/feddata/geograph /00geogra.pdf). To calculate the federal employment rate, we divided the number of workers employed by the federal government by the total number of workers in a county (collected from the Bureau of Labor Statistics' Local Area Unemployment Statistics series). We classified counties as low–federal employment counties if the federal employment rate was below 2 percent (which covers about two-thirds of Texas counties). One limitation of the federal employment data is that they include only civilian employment. However, most counties with military bases also have a relatively high percentage of civilian employees working for the federal government. There are only three counties in Texas with military bases that have federal employment rates below 2 percent, and these represent only 0.8 percent of the last-chance sample and also have relatively high mobility rates (and so are excluded from the analysis when we limit the sample to "low-mobility, low–federal employment" counties).

[33] This is also the idea behind the analysis of the black-white wage gap in Johnson et al. (2000) and Neal (2004). These papers argue that wage offers for women with certain observable characteristics (such as long spells on public assistance) are likely to be below the quantile regression line and that imputing values of zero for these individuals will mitigate the bias resulting from restricting the sample to employed individuals.

found no instances of statistically significant positive signaling effects for quantiles 0.5, 0.6, and 0.75.[34]

VI. Discussion

A. Implications and Explanations

We now address two questions raised by our findings. First, what are their implications for the potential signaling role of school completion? Although our estimates of the signaling value of a diploma are based on only a sample of high school completers (hence do not speak to this question directly), the question is important because completion is closely related to diploma receipt (in the setting we consider, school completion is a necessary condition for diploma receipt) and has been the focus of much of the previous literature, going back to the earliest models of labor market signaling (Spence 1973). Second, what might explain our findings?

With respect to completion, the key point is that most districts do not offer certificates of completion to students who complete high school but do not pass the exam. In these districts, a diploma therefore allows students to signal both their completion (perseverance, etc.) and their score (cognitive skill) cheaply. In districts that offer certificates of completion, a diploma allows students to signal only their cognitive skill. It follows that if there is a signaling value to completion, then the diploma signaling value should be larger in the districts that do not offer certificates of completion than in the districts that do.

To test this hypothesis, we generated separate estimates of the diploma signaling value in the two sets of districts. As seen in Appendix tables A1 and A2, the evidence is not consistent with the above hypothesis. In fact, the estimates for noncertificate districts are smaller than those for certificate districts. For instance, in the latest follow-up period (years 7–11), the estimates in noncertificate districts range from −738 to 2,009 and are never statistically significant; the certificate district estimates range from 2,849 to 4,355. While some of these are on the margins of statistical significance, these are best interpreted as noisy zeros. To summarize, the

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[34] These results are available on request. We also produced estimates for these subgroups after restricting the sample to men, since men are less likely to exit the labor force than women. The main limitation of this approach in this context is that the estimates are imprecise, and hence the confidence intervals are very wide. However, the point estimates are frequently negative and offer little indication that there are positive signaling effects masked by statistical imprecision. For instance, the estimated median regression estimates using PDV earnings as the outcome were −7,452 (23,477) for the low-mobility and low–federal employment county sample and −11,900 (11,135) for the high-mobility or high–federal employment county sample.

contrast between estimates in certificate and noncertificate districts is difficult to reconcile with a scenario in which school completion has an important signaling value but the diploma does not. Moreover, the magnitude and sign of the estimates for noncertificate districts are difficult to reconcile with the hypothesis that employers use high school completion or diploma receipt as a signal of unobserved productivity.

With respect to explanations, a useful starting point is the observation that diploma signaling values will be positive if (1) the diploma contains information about relevant productivity differences, (2) diploma receipt can be observed and verified, and (3) firms cannot obtain this productivity information from other sources.[35] A violation of the first condition could explain our findings but would leave open the possibility that other educational indicators that are related to productivity do have a signaling value. However, the data are not consistent with this view. Even if we assume that the diploma can signal only passing the exam (i.e., if we focus on completers), the evidence reported in panel A of table 6 suggests that there is considerable earnings variation among school completers and that this variation is correlated with diploma receipt. Moreover, panel B of table 6 shows that even among workers in the last-chance sample, earnings are strongly related to last-chance exam scores (see also fig. 2) and hence diploma status. With regard to the second condition, although firms may not be able to verify diploma status (as discussed in Sec. II.C), we might still expect physical possession of a diploma to have real value in a world in which completing high school and passing the exit exam were valuable signals of skills.

This leaves the third condition as the most likely explanation for our findings, although it is difficult to pinpoint how firms acquire other productivity information. Clearly, some will be revealed when applying for a job (e.g., interviews, tests of an applicant's suitability) and more will be revealed from a short tryout period. Note that these three explanations are likely connected: the smaller the productivity differences between workers with and without diplomas that firms cannot infer from other sources, the weaker will be their incentives to verify diploma status.

B.   Relationship to the Previous Literature

Our main findings, that high school completion and diploma receipt have little signaling value in our setting, differ from those of an older literature concerned with the returns to completing twelfth grade conditional on completing eleventh grade (Hungerford and Solon 1987;

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[35] The other key assumption is that the labor market is competitive. Given this and the other assumptions, diploma status will predict productivity, and firms will therefore pay a wage premium for the diploma.

TABLE 6
Associations between Diploma and Test Scores and Earnings

	Last-Chance Sample (1)	Complete Grade 12, No College			
		All (2)	T1 (3)	T2 (4)	T3 (5)
A. Mean Differences by Diploma Status					
Earnings years 7–11	1,814.7	2,867.8	1,780.3	1,752.0	2,385.3
	(138.1)	(79.3)	(111.8)	(176.1)	(228.5)
Observations	128,460	992,031	210,793	193,970	194,896
Mean earnings without diploma	12,400	12,673	11,858	13,301	13,538
Difference (%)	14.6	22.6	15.0	13.2	17.6
PDV earnings	8,054.5	8,731.0	7,280.7	7,459.4	10,546.3
	(632.3)	(341.9)	(501.9)	(779.8)	(951.4)
Observations	37,571	340,028	74,490	63,652	64,548
Mean earnings without diploma	70,280	69,992	66,466	74,216	73,860
Difference (%)	11.5	12.5	11.0	10.1	14.3

	B. Correlation between Test Score and PDV Earnings in Last-Chance Sample			
	(1)	(2)	(3)	(4)
Last-chance score	870.6	983.2	968.8	962.7
	(40.0)	(60.6)	(61.0)	(103.6)
Last-chance score^2		11.3	14.3	14.1
		(3.8)	(7.9)	(7.4)
Last-chance score^3			.2	.2
			(.3)	(1.0)
Last-chance score^4				.0
				(.0)
Polynomial degree	1	2	3	4
p-value slopes = 0	.00	.00	.00	.00
R^2	.0126	.0129	.0129	.0129
Adjusted R^2	.0126	.0128	.0128	.0128

Note.—Panel A shows mean differences in earnings by high school diploma status. T1, T2, and T3 refer to bottom, second-bottom, and third-bottom tertiles of the ability distribution as measured by initial exam scores. No restrictions are placed on the last-chance sample (i.e., we do not restrict to those with no college). Panel B shows estimates of a regression of PDV earnings through year 11 ($r = .05$) on a polynomial in the last-chance exam score (each column represents a separate regression) for students in the last-chance sample. All models are estimated with no additional covariates beyond those listed in the table (high school diploma in panel A and the test score polynomial terms in panel B) and an intercept.

Park 1999).[36] One possible explanation is that these estimates are biased upward by omitted variables. Omitted variable bias could also explain why other studies find large returns to diploma receipt conditional on

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[36] Since these studies measure signaling values using hourly wages, it is plausible to suppose that they would have found even higher signaling values measured using annual earnings (the metric used in this paper). The reason is that these hourly wage differences are likely magnified by labor supply responses.

completing twelfth grade (Jaeger and Page 1996; Frazis 2002). The main concern is that firms observe information about workers (e.g., interview outcomes) not captured in the data sets used in these studies, such that the estimates conflate the relevant signaling values with productivity differences between workers with different levels of education.[37] This concern is especially relevant since these estimates are typically based on data sets such as the Current Population Survey or census, which have limited information on workers (Heckman and LaFontaine 2006).

Our findings also differ from some of those found in the GED literature. For example, Tyler et al. (2000) estimate that for some workers, the signaling value of a GED is between 10 and 20 percent. However, recent work by Jepsen et al. (2010) estimates GED signaling values closer to zero and other estimates of the return to a GED, including the Tyler et al. estimates for nonwhites, are much smaller (Cameron and Heckman 1993; Heckman and LaFontaine 2006; Lofstrum and Tyler 2007; Jepsen et al. 2010).

VII.   Conclusion

What is the best framework for thinking about the relationship between education, productivity, and wages? This is one of the oldest questions in economics. The answer has important implications for our understanding of various economic phenomena. Since it has implications for the difference between the private and social returns to education, it also has important policy implications.

This paper shed light on this question by estimating the signaling value of a high school diploma, the most commonly held educational credential in the United States. Since a diploma is a piece of paper, it cannot affect productivity. Any wage return to a diploma must, therefore, capture its value as a signal of productivity. We use linked administrative data from Texas to estimate the signaling value of a diploma among students who take the high school exit exam for the last time. For these students, we find that a diploma has a signaling value close to zero.

Our empirical results do not allow us to draw conclusions about the signaling value of completing high school and obtaining a diploma for

[37] In an important critique of using the wage premium associated with completing 12 vs. 11 years of school, Lange and Topel (2006) note that if the productivity returns to education are heterogeneous and initially unknown, workers who drop out before completing grade 12 may be those for whom the returns to education are low. This implies that the observed returns to completing grade 12 will exceed the observed return to completing grade 11 even when there is no job market signaling. Riley (2001) makes the more general point that without a theory of why some workers complete twelfth grade while others do not, it is difficult to interpret these estimates. A similar criticism applies to studies that compare wages across workers with and without diplomas conditional on highest grade completed (Jaeger and Page 1996; Park 1999): the wage premium may simply reflect the correlation between diploma status and characteristics firms but not researchers observe.

other populations. Nevertheless, we have shown that, among this sample of last-chance exam takers, there is little or no value to the random assignment of a low-cost opportunity to signal completing high school and passing the exit exam. While creative theorists may be able to construct some variant of the signaling model that accounts for this result, we view this evidence as a strong challenge to those who contend that high school completion is valued by employers as a signal of productivity. Given the evidence in Bishop (1988) concerning the response rates of schools to inquiries about students' academic records, one expects physical possession of a diploma to have real value in a world in which high school completion is a valuable signal. We find no evidence that Texas students live in such a world.

Appendix

A. *Exit Exams and High School Graduation Requirements*

The Texas class of 1987 was the first that was subject to the state's exit exam requirement.[38] In 1990, changes in state law prompted the adoption of a new, harder set of exams called the Texas Assessment of Academic Skills (TAAS). The stated purpose of the TAAS was to assess "higher-order thinking skills and problem solving" (Texas Education Agency 2003).[39]

The exit-level TAAS consisted of three sections (reading, math, and writing), all of which had to be passed to satisfy the testing requirement. The math and reading sections had 48 and 60 multiple-choice questions, respectively, and state law set the passing standard to be 70 percent. The writing section had a multiple-choice component (40 questions) and an essay component, which was scored on a four-point scale. The score for the writing section was computed by summing the multiple-choice items answered correctly and 10 times the essay section, and a passing score was set to be 48.[40] The tests are designed to be of equal difficulty across administrations, but the passing standards are still adjusted to be equivalent to the passing standard on the first (fall 1990) exam.[41] Students receive a score report describing their performance on the exam. It contains the scores received on the exam, the standard required to pass each section, and whether the student satisfied the graduation requirement.

Students who failed their first attempt could retake the exam during a subsequent administration (students had to retake only the sections they did not pass

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[38] This section draws on Texas Education Agency (2003).

[39] Students could also take course-specific exams called end-of-course (EOC) exams to satisfy the testing requirement. The first class for which the EOC exams could be a substitute for the TAAS was 1999 (they were not fully phased in until the fall of 1998). Consequently, the EOC exams are not relevant for the period in this study.

[40] A minimum score of 2 on the writing section was also necessary, which implied that a student could still fail the writing section with a writing score of 48 or greater if he received a 1 on the essay and got 38 out of 40 (or more) items correct in the multiple-choice section. In practice, very few students scored a 1 on the essay and correctly answered 38 or more multiple-choice items.

[41] These adjustments are typically no more than plus or minus one correct answer.

on an earlier attempt). The timing of the initial exam and retake administrations changed over the study period. Students in tenth grade in the spring of 1991 or 1992 first took the exam in the fall of eleventh grade. The 1993–95 tenth-grade cohorts began taking the TAAS in the spring of tenth grade. Students in all cohorts could retake the exam during administrations given in the fall, spring, and summer. Beginning with the 1992 tenth-grade cohort, seniors who had not yet passed could take the exam one more time before graduation during a special administration given in April or May. Thus the number of chances to retake the exam before the end of twelfth grade (for students not held back) increased from five (for students in the 1991 tenth-grade cohort) to eight (for students in the 1993–95 tenth-grade cohorts). Texas law requires that districts provide remedial services for students who fail the exam (but does not mandate specific interventions).

To receive the "minimum high school program" diploma (the easiest high school diploma to obtain), students had to meet the exit exam requirement and earn a minimum number of course credits across a required distribution of subject areas. Students could receive special education exemptions from the exit exam (or certain sections of the exit exam) and still earn high school diplomas. To receive an exemption, a student's admission, review, and dismissal (ARD) committee has to determine that the TAAS is not an appropriate measure of his or her academic progress.[42] In Texas during this period, about 7 percent of students received exemptions from at least one section of the exam (Martorell 2005). Texas does not offer a certificate of completion to students who completed all other graduation requirements aside from the exit exam. Individual districts, however, can issue these.

Students who complete all other graduation requirements but fail the exam and do not receive an exemption can attempt the exam after they leave school. If they pass, they are awarded a diploma. To help prepare to retake the TAAS, students who have not graduated are also eligible to enroll in school for all or part of an academic year following their twelfth-grade year (a "thirteenth year").

Texas does not have statewide college admissions standards. However, there are several ways in which not having a high school diploma could affect college attendance. First, there may be explicit admissions barriers. Four-year colleges typically do not admit students who did not graduate from high school (unless they earned a GED degree or a similar "equivalency" credential). Most community colleges are "open enrollment" and will admit anyone who wants to enroll, but some colleges have additional hurdles that students without a high school diploma or GED must overcome. For instance, Austin Community College will admit students without a high school diploma or GED only if they "can demonstrate skill proficiencies that support an ability to benefit from college-level instruction." Second, there may be informational barriers. Students might not know that they could enroll in a community college without a diploma even if they could. Finally, students without a diploma or GED are ineligible for federal student financial aid.[43]

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[42] An ARD committee is made up of teachers, parents, and individuals with expertise working with special education students.

[43] See http://studentaid.ed.gov/eligibility/infographic-accessible for federal student financial aid eligibility information.

B. Data

The Texas data used in this paper come from the Texas Schools Project (TSP), a collection of administrative records from various Texas state agencies. These permit a longitudinal analysis of individuals as they proceed through high school and later as they enter college or the workforce.

For this study we used five cohorts of students, defined in terms of the year in which they first took the exit exam. These include students who took the initial exam in eleventh grade in the fall of 1991 and 1992 and students who took the initial exam in tenth grade in the spring of 1993–95. Assuming no grade repetition, these correspond to the 1993–97 graduation classes. The initial exam record forms the basis for the sample, and for each student in the cohorts we examine, we merged in data from the following Texas Education Agency (TEA) files:

1. Exam files: There are separate files for each exit exam administration. We used these to generate longitudinal exam-taking histories through 2002. One important limitation is that only scores from the spring 1998 administration are included in the 1997–98 school year. Thus, we have incomplete data on retakes in the year after the final cohort of students was in twelfth grade.

2. K–12 enrollment files: The TSP contains annual enrollment files from 1990–2002. These files include fields for school, grade level, special education status, limited English proficiency, and economic disadvantage status.

3. K–12 attendance files: These contain data on 6-week attendance periods (six per school year) starting in 1993. These files contain the number of days a student attended a given school in each attendance period.

4. GED files: These are annual files that contain GED scores for individuals who attended Texas public schools and who earned a GED certificate through 2002. Beginning in 1995, records for all GED test takers are included regardless of eventual passing status.

5. Graduation files: These are annual rosters of students receiving a high school diploma from a Texas public school between 1992 and 2001. The data distinguish between basic and more advanced diplomas. Since the exit exam is a requirement for the most basic diploma, we do not make use of information on degree type.

We also merged in data from the following Texas Higher Education Coordinating Board (TEA) files (all files available 1990–2005):

1. Report 1: These files, created every semester, include basic information for students enrolled in Texas colleges including the school, semester, and year in which a student was enrolled.

2. Report 2: These files, created every semester, include information on admissions tests, remediation status, placement exam results, and academic credits attempted in each semester. Academic credits are those that count toward a degree, and "attempted" credits refer to credits in courses in which a student earns a grade (i.e., he did not drop out of the course) but did not necessarily pass.

3. Report 9: These are annual rosters of awards issued by public colleges in Texas. These files identify the type of degree (associate degree or baccalaureate degree).

We also merged in data from Texas Workforce Commission Files (available 1990–2004:Q3). These quarterly files contain earnings reported by employers covered by the state's UI system. "Year 1" earnings are computed as the sum of earnings received in quarter 4 of the year of the last-chance exam and quarters 1–3 of the following year. Annual earnings for subsequent years are defined in a similar fashion.

Records were linked across files using a unique scrambled student identification used by the TEA. Records were linked across agencies (e.g., TEA to Texas Workforce Commission matches) using scrambled Social Security numbers. We checked to see if records matched on the basis of scrambled student identifications or Social Security numbers were in agreement on variables such as gender and date of birth. In general, these fields were almost always in agreement.

C. Additional Figures and Tables

STATE OF TEXAS
ACADEMIC ACHIEVEMENT RECORD

STUDENT/DISTRICT INFORMATION
(Full Legal Name)
Student ID Number:
SSN/State ID Number:
Date of Birth:
Sex:
Ethnicity:

(Parent/Guardian)
(Parent's Name)
(Address)
(City, State Zip)

(District Name)
(Name of School)
(School Address)
(City, State Zip)
(Phone Number)
(CB/ACT Campus Number)

EXIT-LEVEL ASSESSMENT

		DAP ADVANCED MEASURES	
English Language Arts	(Mo/Yr)	(measure earned)	(date)
Mathematics	(Mo/Yr)	(measure earned)	(date)
Science	(Mo/Yr)	(measure earned)	(date)
Social Studies	(Mo/Yr)	(measure earned)	(date)

SCHOOLS AWARDING CREDIT
2008-09 (CDCN)
2009-10 (CDCN)

2010-11 (CDCN)
2011-12 (CDCN)

Course Credit - Please Add Rows As Needed

	SE	S1	S2	Av	Cr		SE	S1	S2	Av	Cr
English Language Arts						PE/Equivalent					
						Other Languages					
Speech											
Mathematics											
						Fine Arts					
Science											
						Career & Technical Ed					
						Other Electives					
Social Studies											
						Local Credit					
Econ/Free Enterprise											
						Credit Totals					
Health						State					
						Local					

Date of Class Rank:
Rank: Class Size:
GPA:
Quartile:

Date of Certificate of Coursework Completion:
Date of Graduation:
Graduation Program Type:
Texas Grant Indicator:

(Seal)

Signature and Title of School Official

NOTES: CDCN=County/District/Campus Number; A passing grade is 70 or above. P=Pass F=Fail; Texas Grant Indicator 1, 2, & 5=Eligible; SE=Special Explanation Code; A=articulated credit course; C=correspondence course; D=dual credit; E=credit by exam (90%); G=gifted/talented course; H=honors course; I=IB course; J=course completed prior to Grade 9; K=pre-IB course; L=local credit; M=magnet course; P=AP course; Q=pre-AP course; R=summer school, night school, or other non-school day/year instructional arrangement; T=credit by exam (70%); V=content modified per ARD committee decision; X=innovative course; Z=distance learning course; 1=course providing PE equivalency or PE waiver; 2=part of a coherent sequence of CTE courses for Technology Applications credit; 3=transfer credit from non-Texas public school; 4=CTE course that satisfies another specific graduation requirement   Refer to *2008 Minimum Standards* for appropriate use.

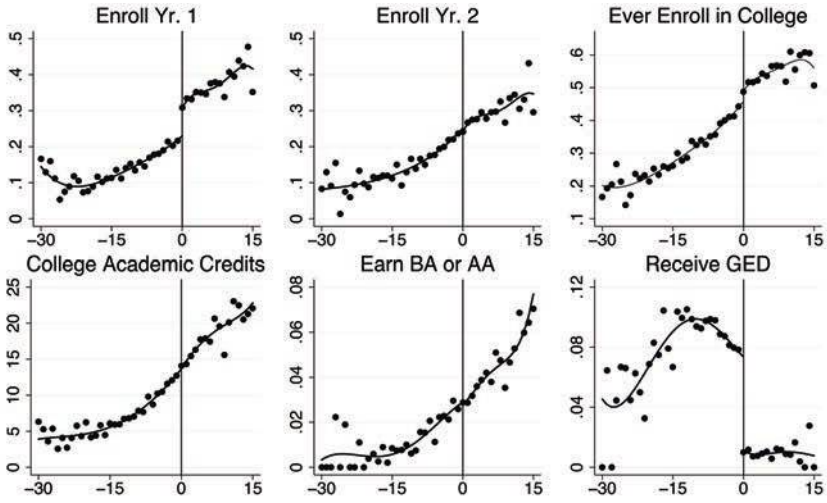Fɪɢ. A1.—Sample Texas high school transcript

Fig. A2.—Postsecondary outcomes by last-chance exam scores. The graphs are based on the last-chance sample. See table 1 and the text. Dots are exam score cell averages. Lines are fourth-order polynomials fitted separately on either side of the passing threshold. Estimated discontinuities (using a fully interacted quadratic in the test score) are 0.086 (SE = 0.010) for enrolled in college in year 1, 0.005 (SE = 0.010) for enrolled in college in year 2, 0.042 (SE = 0.012) for ever enrolled in college, 0.332 (SE = 0.677 for total college academic credits, -0.002 (SE = 0.004) for earning a bachelor or associate degree, and -0.062 (SE = 0.005) for received GED certificate. We observe postsecondary information for these cohorts for 7 years after the last-chance exam. Instrumental variable estimates of diploma impacts on these outcomes would be roughly 2.5 times as large.

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TABLE A1

IMPACT OF RECEIVING A HIGH SCHOOL DIPLOMA ON EARNINGS IN DISTRICTS THAT DO NOT ISSUE CERTIFICATES OF COMPLETION

	No Baseline Covariates						Baseline Covariates					
	Unrestricted (1)	Restricted (2)	Unrestricted (3)	Restricted (4)	Unrestricted (5)	Restricted (6)	Unrestricted (7)	Restricted (8)	Unrestricted (9)	Restricted (10)	Unrestricted (11)	Restricted (12)
Years 1–3*	−535.4	−484.8	−23.9	−508.9	79.1	−476.1	−536.2	−521.1	−63.5	−518.4	−72.9	−472.1
	(704.8)	(556.0)	(890.7)	(517.6)	(1,158.0)	(700.8)	(689.3)	(545.3)	(894.1)	(515.1)	(1,139.3)	(686.1)
Years 4–6	−1,063.2	−967.7	−583.9	−1,006.6	−505.8	−1,068.3	−893.0	−975.9	−689.6	−906.3	−558.6	−1,032.2
	(1,030.3)	(832.5)	(1,285.2)	(765.3)	(1,668.3)	(1,031.9)	(1,006.1)	(814.9)	(1,285.8)	(760.1)	(1,632.3)	(1,007.1)
Years 7–11	−529.8	−467.6	111.4	−678.6	1,945.5	−122.5	−484.8	−643.5	−126.8	−737.6	2,009.1	−202.8
	(1,470.0)	(1,223.4)	(1,786.4)	(1,095.7)	(2,395.8)	(1,485.9)	(1,423.7)	(1,187.0)	(1,773.0)	(1,091.7)	(2,301.8)	(1,436.0)
All years pooled	−704.7	−633.8	−157.4	−730.0	526.9	−542.9	−633.9	−711.0	−288.4	−721.3	487.3	−558.1
	(929.9)	(760.2)	(1,146.8)	(690.7)	(1,503.9)	(935.8)	(900.7)	(738.3)	(1,137.7)	(683.2)	(1,453.0)	(905.6)
PDV earnings through year 11	−4,854.2	−7,045.9	−4,854.2	−6,443.5	−4,854.2	−4,198.7	−4,015.3	−5,488.9	−4,015.3	−4,980.8	−4,015.3	−4,029.6
	(6,549.3)	(5,391.5)	(6,549.3)	(5,473.4)	(6,549.3)	(6,589.8)	(6,222.1)	(5,121.1)	(6,222.1)	(5,199.8)	(6,222.1)	(6,250.8)
Degree of test score polynomial	2		3		4		2		3		4	

NOTE.—The sample is based on students who were in districts that did not offer certificates of completion and were in the largest 35 districts (i.e., the districts for which we have information on certificates of completion). Unrestricted specification estimates are from two-stage least-squares models in which the test score polynomial is fully interacted with an indicator for passing the last-chance exam. Results from the restricted polynomial specification are the ratio of the reduced-form and first-stage estimates, where these are estimated separately and the standard errors are calculated using the delta method. The reduced form is the estimated discontinuity in earnings using the polynomial specification in which the slopes are constrained to be equal on either side of the passing cutoff and the first stage is estimated using a polynomial that is fully interacted with the passing dummy. All standard errors are adjusted for clustering at the individual level. See the notes to tables 3 and 4 for additional details on the sample used and the regression specifications.

* Mean earnings are as follows: for years 1–3, $6,923; for years 4–6, $11,518; for years 7–11, $14,234; for all years pooled, $11,023; and for PDV earnings through year 11, $77,383.

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TABLE A2

IMPACT OF RECEIVING A HIGH SCHOOL DIPLOMA ON EARNINGS IN DISTRICTS THAT ISSUE CERTIFICATES OF COMPLETION

	No Baseline Covariates						Baseline Covariates					
	Unrestricted (1)	Restricted (2)	Unrestricted (3)	Restricted (4)	Unrestricted (5)	Restricted (6)	Unrestricted (7)	Restricted (8)	Unrestricted (9)	Restricted (10)	Unrestricted (11)	Restricted (12)
Years 1–3*	1,885.8	1,241.9	1,402.5	1,561.5	1,783.4	1,217.8	1,925.6	1,515.6	1,477.3	1,810.5	1,782.8	1,288.5
	(1,002.0)	(832.1)	(1,212.6)	(700.6)	(1,374.0)	(987.1)	(955.8)	(797.5)	(1,165.6)	(677.4)	(1,306.1)	(940.1)
Years 4–6	2,728.2	2,045.4	2,569.5	2,608.6	2,534.6	1,668.0	2,840.0	2,290.6	2,681.3	2,871.4	2,838.2	1,730.5
	(1,492.4)	(1,263.9)	(1,816.4)	(1,058.7)	(2,052.3)	(1,477.9)	(1,430.8)	(1,207.9)	(1,753.5)	(1,024.1)	(1,965.5)	(1,411.6)
Years 7–11	3,292.4	3,053.7	4,355.0	2,962.2	2,950.4	2,848.8	3,377.6	3,309.4	4,426.8	3,308.9	2,936.7	2,954.0
	(2,174.0)	(1,905.5)	(2,591.8)	(1,549.5)	(2,888.8)	(2,193.1)	(2,113.4)	(1,846.5)	(2,532.3)	(1,502.3)	(2,796.8)	(2,116.3)
All years pooled	2,654.6	2,144.1	2,828.8	2,393.9	2,442.6	1,940.2	2,731.1	2,398.5	2,908.8	2,679.1	2,532.7	2,016.7
	(1,371.0)	(1,185.2)	(1,662.8)	(976.9)	(1,879.3)	(1,369.1)	(1,311.5)	(1,133.6)	(1,599.1)	(939.7)	(1,790.5)	(1,302.7)
PDV earnings through year 11	14,641.5	11,496.9	14,641.5	13,710.8	14,641.5	8,685.4	19,768.0	16,883.5	19,768.0	19,326.0	19,768.0	14,326.1
	(9,672.1)	(8,346.4)	(9,672.1)	(8,372.8)	(9,672.1)	(9,635.1)	(9,020.1)	(7,779.3)	(9,020.1)	(7,813.8)	(9,020.1)	(8,951.9)
Degree of test score polynomial	2		3		4		2		3		4	

Note.—The sample is based on students who were in districts that offered certificates of completion and were in the largest 35 districts (i.e., the districts for which we have information on certificates of completion). Unrestricted specification estimates are from two-stage least-squares models in which the test score polynomial is fully interacted with an indicator for passing the last-chance exam. Results from the restricted polynomial specification are the ratio of the reduced-form and first-stage estimates, where these are estimated separately and the standard errors are calculated using the delta method. The reduced form is the estimated discontinuity in earnings using the polynomial specification in which the slopes are constrained to be equal on either side of the passing cutoff and the first stage is estimated using a polynomial that is fully interacted with the passing dummy. All standard errors are adjusted for clustering at the individual level. See the notes to tables 3 and 4 for additional details on the sample used and the regression specifications.

* Mean earnings are as follows: for years 1–3, $6,514; for years 4–6, $10,567; for years 7–11, $12,942; for all years pooled, $10,119; and for PDV earnings through year 11, $71,109.

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