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Do High School Graduation Exams Matter? A Regression Discontinuity Approach

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Abstract

Nearly half of all states now require students to pass an exit exam to earn a high school diploma. Typically, students take the exam for the first time in 10th or 11th grade, and can try again if they fail. Critics of the policy argue that exit exams lower graduation rates by denying diplomas to students who complete all the other requirements for a degree, and by inducing some students who fail the exam initially to drop out rather than try again. In this paper I use longitudinal micro data for a large sample of Texas high school students to evaluate the effect of exit exams. Since students who score below the passing threshold are more likely to drop out even in the absence of an exit exam, inferences are based on a regression-discontinuity approach, using comparisons between students who barely pass and barely fail the test. Failing the test in 10th or 11th grade does not cause students to drop out early. However, some students meet all other requirements for graduation but fail the "last chance" exam in their senior year of high school. Students who barely fail this exam are more likely to earn a GED, but have significantly lower chances of holding either a diploma or a GED, and are less likely to attend post-secondary schooling than those who barely pass. Overall, I estimate that just over 1 percent of Texas students do not graduate because they cannot pass the test. I also find that for students with positive earnings, failing the test reduces earnings in the first several years after high school, though the effect fades over time.

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1 Introduction

More than half of all public high school students in the United States are now required to pass an exit exam to earn a high school diploma. By 2009, this number will increase to 70 percent overall, and to 80 percent for minority students (Center on Education Policy, 2004, p. 5). While common in other countries, sharp controversy surrounds the use of graduation exams in the United States. Supporters argue that by successfully completing an exam, students demonstrate a mastery of a minimum set of skills. Critics counter that exams prevent many students, particularly minority and economically disadvantaged students, from earning a high school degree. Indeed, in Texas, the Mexican-American Legal Defense and Education Fund (MALDEF) filed a lawsuit alleging the state's exit exam had an "illegal discriminatory impact on Black and Hispanic students" (Haney, 2000).[1,2]

There is clearly some risk that exit exams will lower graduation rates. Students may find themselves at the end of high school having completed all other requirements but unable to pass the exit exam. A subtler, but potentially more serious concern, is that exit exams cause some to quit high school early. Exams are typically first administered in the 10th or 11th grade. Students who fail these exams may become discouraged and give up on school, completing one to two years less schooling than they would have had they passed (Center on Education Policy, 2002, p. 25; Griffin and Heidorn, 1996). Given the evidence on the long-run impact of years of schooling (Card 2001), any discouragement effect could have large economic costs.

Mirroring the deeply felt disagreement between proponents and critics of these tests, research findings conflict as to whether or not exit exams affect graduation rates. A recent report by the Center on Education Policy concludes "no consensus has emerged among researchers or policymakers about the effect of exit exams on dropout rates" (2004, p. 41). The fundamental problem is that students who fail the exit exam are arguably less likely to graduate anyway. Unfortunately, existing studies have been unable to credibly address this issue. In this paper I use longitudinal data from the Texas Schools Microdata Panel coupled with a regression discontinuity (RD) econometric approach to provide new evidence on the effects of exit exams. The idea behind the RD strategy is to compare students who barely fail and barely pass the test. Provided

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[1] MALDEF argued the differences in passing rates were so large that using the exam as a graduation requirement violated, among other laws and regulations, the Equal Protection and Due Process provisions of the 14th Amendment of the plaintiffs. The State of Texas successfully maintained that the adverse impact suffered by minority students was legally permissible given the State's interest in promoting high educational standards (READ Institute, 2000).

[2] In addition to the MALDEF lawsuit, the Texas chapter of the National Association of Colored People filed a complaint with the Department of Education Office of Civil Rights also alleging that the state's exit exam had a discriminatory impact on Hispanic and Black students.

that there is some randomness in the test outcomes, this comparison generates credible estimates of the effect of failing the exam.

The richness and scope of the TSMP enables me to extend the analysis beyond simply estimating the effect of failing on the likelihood of graduation. In particular, by tracking students over time and examining dropout behavior after the initial test administrations it is possible to assess the empirical relevance of the discouragement effect stressed by testing opponents. Moreover, since the TSMP links high school records to college enrollment and labor market earnings data, I can also measure the effect of exit exam performance on the likelihood of acquiring post-secondary schooling and on earnings. An analysis of this type is not feasible with the publicly available datasets most exit exam studies use and to my knowledge has yet to be attempted.[3]

This paper finds that failing the exit exam reduces the probability of normal high school graduation and increases the likelihood of acquiring a General Educational Development (GED) degree. The GED result buttresses the graduation finding since it suggests some students fall back on this credential if they are unable to earn a conventional diploma. Many non-graduates, however, do not earn a GED; failing lowers the rate of receiving any credential, be it a standard high school degree or a GED.

The estimated fraction of students not graduating as a consequence of failing ranges from 1.1 to 1.4 percent with larger estimates for disadvantaged and low-achieving students. At the same time, the results do not indicate failing induces students to quit school early. The non-graduation effect instead arises from an inability to pass the exam rather than the discouragement effect that worries testing critics.

Not graduating because of failing the exit exam carries economic consequences. For students arriving at the end of 12th grade without having passed the exam, failing the "last chance" test lowers the rate of enrollment in a Texas public two or four-year college by nearly six percentage points (relative to a base level of 38 percent). Had these students gone to college, however, they likely would not have completed their degree program as no effect on the receipt of post-secondary credentials is found. The estimates suggest instead that students whose college attendance is affected by failing the exit exam would have enrolled in approximately 40 semester credit hours (roughly the equivalent of 1.3 years in college).

[3] Betts and Grogger (2000) examine the impact of the stringency of a school's grading standards on college attendance and earnings (in addition to high school outcomes). They find no evidence that grading standards affect college attendance and at best a small effect on earnings.

I also find that failing lowers earnings, but only among individuals whose work experience indicates a strong attachment to the Texas labor market.[4] The data only allows workers to be followed into their early twenties, and at this age, many will still be in school or will not yet have settled into permanent jobs. Focusing on individuals with an uninterrupted string of earnings is therefore sensible since their observed earnings likely provide a more reliable forecast of future labor market performance than those of someone with an earnings history characterized by periods of not working. The estimated effect on earnings falls over time suggesting the labor market premium enjoyed by passers eventually wears off.

These results provide some important new evidence on the role of signaling in the labor market. This is because any difference in earnings between barely failers and barely passers will be driven by differences in the likelihood of receiving a high school credential rather than in productivity.[5] The findings suggest that an educational credential does have a causal impact on an employer's estimation of a jobseeker's productivity. That this effect dissipates is consistent with employers learning about a worker's actual productivity, which in turn renders the credential uninformative.[6]

An important caveat to the paper is that I cannot address the potential benefits of exit exams, since all the data pertain to an environment with an exit exam requirement. As such, the results reported here are not meant to provide a conclusive account of the relative merits of this policy. Instead, this paper examines whether exit exams prevent students from graduating and if this has economically meaningful consequences. Whether and how exit exams benefit students, and if these positive effects outweigh the costs borne by students who do not graduate as a consequence of the policy, is left for future research.

2 Background

2.1 Review of Existing Research

Existing studies of high school exit exams fall into three categories. The fist consists of cross-sectional comparisons of the graduation outcomes of students living in states with and without exit exams. Typically, these studies use the National Educational Longitudinal Study of

[4] More precisely, failing is found to lower earnings among workers with positive earnings reported in the Texas unemployment insurance records in each quarter in the relevant observation window.
[5] This would not be true if failing drove students to quit school early. It would also not be true if the differences in post-secondary schooling induced by failing lead to productivity differences. But college attendance is actually related to lower average earnings among individuals in their early to mid twenties (the age at which earnings are observed in this paper).
[6] Altonji and Pierret (1996) take this position regarding labor market signaling.

1988 (NELS).[7] Mueller (1998) and Mueller and Schiller (2000) use the NELS to examine dropout rates between 10th and 12th grade, and find no relationship between dropout rates and living in an exit exam state. Warren and Edwards (2004) also find no link between exit exam requirements and high school completion or GED acquisition rates even for low achieving students. In contrast, Jacob (2001) finds that exit exams increase the likelihood of dropping out by 25 percent among students in the bottom quintile of academic performance.

A second set of studies uses variation in the timing of the introduction of exit exams. Warren and Jenkins (2004) use the October CPS to see if the implementation and changes in the difficulty of exit exams in Florida and Texas increase the number of dropouts. They find that neither the existence nor the level of difficulty is correlated with dropout rates. Due to relatively small sample sizes, however, any underlying effects may simply be too small to detect. Amrein and Berliner (2003) find that a majority of states experienced higher dropout rates, lower graduation rates, and higher enrollments in GED programs after imposing exit exams than what persisted nationally. This finding has been criticized by a number of authors, notably Carnoy and Loeb (2003) who use a similar research design but do not find any relationship between the use of high-stakes testing (of which exit exams are an example) and state-level high school survival rates.

A carefully done study by Warren, Jenkins, and Kullick (2004) examines the impact of imposing a testing regime on state-level high school completion and GED test taking rates. Fixed effect estimates suggest that instituting an exit exam requirement lowers high school completion rates and increases the rate of GED test taking. The effect is stronger in high poverty states. This is consistent with the criticism levied against exit exams that says they will disproportionately affect the most vulnerable students.

Like this paper, a third group of studies uses administrative data and compares the outcomes of students by exit exam passing status. Griffin and Heidorn (1996) find that even after controlling for demographic characteristics and past academic performance, Florida students who failed the exit exam were more likely to leave school. This effect is most pronounced for relatively high achieving students and does not vary by race or socioeconomic status. Davenport et al. (2002) look at what happened when Minnesota began requiring high school students to pass an exam in order to graduate. They find essentially the same dropout rate in the first cohort of students subject to the exit exam as that of the preceding cohort and conclude that the exit exam did not have an effect on high school completion rates. Since the Minnesota exit exam tests only

[7] An exception is Coates and Wilson-Sadberry (1994) who use the High School and Beyond survey. They find that exit exams do increase dropout rates and that the effect is largest for white and Asian students.

basic skills, it may present a smaller barrier to graduation than the more difficult test given in Texas.[8]

In summary, the existing research has produced conflicting results on the impact of graduation tests. The primary limitation lies in the lack of a design that effectively teases out the true effect of the exams. For instance, explicit and implicit changes in education policy often accompany the adoption of exit exams and these may mask or exacerbate the exit exam's true impact when identifying the effects from variation over time or across states.[9] Moreover, they are uninformative regarding the mechanism through which exit exams affect graduation rates. Finally, none of them directly address what, if any, effect will be had on the economic fortunes of students who face an exit exam requirement.

2.2 Exit Exams in Texas

Texas has been at the forefront of the national movement to use statewide testing to improve academic standards, and introduced standardized testing for public school students in the early 1980's (Achieve, 2002, p.9). The class of 1987 was the first subject to the state's exit exam requirement. 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" (TEA 2003).[10]

The exit-level TAAS consists of three multiple-choice sections – reading, math, and writing – all of which must be passed to satisfy the testing requirement.[11] The writing section also includes an open-ended essay component. State law sets the passing standard for the reading and math sections to be a score of 70 percent. The passing criterion for the writing section combines the multiple-choice and essay components.[12] The tests are designed to be of equal difficulty from administration to administration, but for tests that are idiosyncratically difficult or easy, the passing standard for that test is adjusted to make it equivalent to the version given in the fall of 1990. These adjustments are typically no more than plus or minus one correct answer.

[8] The initial passing rate in Minnesota is 63 percent while it is 54 percent in Texas. Warren et al. (2004) stress the relevance of the distinction between basic and "more difficult" exams.

[9] Some have argued that in an effort to keep failure rates, which are often highly publicized, low, administrators have incentives to increase retention rates in the grade preceding the testing year (Haney 2000). Such allegations are highly controversial, however.

[10] 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 exam are not relevant for the period I study.

[11] Each section has 48, 60, and 40 questions, respectively.

[12] The multiple-choice score plus ten times the essay score must be at least 48. In addition, the essay is scored on a four-point scale and a score of 2 is required to pass. Passing the essay is required to pass the section.

Students who failed their first attempt could retake the test during a subsequent test administration.[13] Table 1 lists the tests administered to students by academic cohort (assuming no grade repetition after initially taking the exit exam). Students in 10th grade in the spring of 1991 or 1992 first took the exam in the fall of 11th grade. The 1993-1995 cohorts began taking the TAAS in the spring of 10th grade. For all cohorts, students could retake the test during administrations given in the fall, spring, and summer. Beginning with the 1992 cohort, seniors who had not yet passed could take the test one more time before graduation during a special administration given in May or April. Thus the number of chances a student who was not held back had to pass the TAAS before graduation increased over time. Students in the 1991 cohort had only five such opportunities while those in the 1993-1995 cohorts had eight. At the same time, state regulations do not limit the number of times the TAAS can be retaken. Students who do not pass the exit exam by the time they are slated to graduate but who complete all other graduation requirements can attempt the exam even after they leave school and are awarded diplomas if they pass it.

Not all students have to pass the TAAS to graduate. Special education students may receive exemptions from the entire exam or selected sections of it. To receive an exemption, a student's admission, review, and dismissal (ARD) committee must determine that the TAAS is not an appropriate measure of their academic progress.[14] In the five cohorts studied in this paper, 4.5 percent of students were exempted from all sections of the test and 7.2 percent received an exemption from at least one section.[15]

Table 2 presents cumulative passing and exemption rates for the 1991-1994 cohorts.[16] Overall, 87.2 percent satisfy the testing requirement through a combination of passing and receiving exemptions for all three sections of the TAAS (84.8 pass all sections).[17] Not surprisingly, non-graduates perform much worse on the exit exam: 43.8 percent of non-graduates pass all sections as compared to 95.7 percent for graduates. Almost all graduates (98.3 percent) pass or are exempt from the TAAS. Measurement error is one reason why this figure is less than

[13] All three sections need not be passed during the same test administration; only the failed sections need to be retaken.

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

[15] 0.5 percent of the students were initially exempt from part of the exam (and some eventually received exemptions for the entire test). 2.2 percent received exemptions for some section after taking at least one section.

[16] The 1995 cohort is not included here because some of these students were still retaking the TAAS in the 1997-98 school year and the TSMP's collection of exit exam data in that year is incomplete.

[17] Unlike the samples used in the analysis that follows, students who take at least one section of the TAAS are included, even if they never took all unpassed sections during any single test administration. See the data appendix for an enumeration of the sample selection restrictions. Students initially exempt from any section of the TAAS are not included in these calculations

100 percent. For instance, a student may be incorrectly classified as not having passed the TAAS when they in fact did.[18] Alternatively, some school administrators may not comply with the exit exam policy. However, even among graduates who fail at least once, the rate of eventually satisfying the testing requirement exceeds 96 percent so noncompliance does not appear to be widespread.

Although none of the students included in Table 2 were initially exempt from any section of the TAAS, some eventually obtain exemptions. Among the graduates who fail the TAAS at least once, 6.4 percent receive exemptions from part or the entire test. This suggests some students who have trouble passing the exam adopt acquiring exemptions as a strategy to circumvent the testing requirement.

Table 2 also reveals important variation in passing rates across different ethnic groups. 90.2 percent of whites eventually pass the TAAS as compared to only 78.1 percent of non-whites. Whites who initially fail are also nearly fifty percent more likely to receive exemptions than are non-whites (6.1 percent for whites and 4.1 percent for non-whites). At the same time, since whites have higher initial pass rates, only 2.3 percent of graduating whites receive exemptions compared with 3.0 percent for non-whites.[19] These findings are notable in terms of the fairness of the implementation of the exit exam policy. As Davenport et al. note, "uniform tests with uniform passing scores do not ensure uniform application of educational expectations for all students" (2002, p. 13).

3. A Model of Exit Exam Performance and High School Completion

This section develops a simple analytical framework to analyze the effect of exit exam performance on progression through high school and graduation under different behavioral assumptions. Specifically, it generates testable implications regarding the pattern of regression discontinuities expected if failing elicits a direct behavioral response, what I call the discouragement effect, or if it matters only insofar as it reduces the likelihood of eventually passing.

3.1 Institutional Setting

To earn a high school diploma, or equivalently, to graduate, students must complete high school and also pass an exit exam. There are $T+1$ periods, $t=0\ldots T$. Students initially take the exam in $t=0$ and the end of high school is in $t=T$. In each period before T, a student still in

[18] This could occur if a student's test records could not be successfully linked due to errors in the identifiers used to match multiple records. A student who initially failed but subsequently passed can only be recognized as having ultimately passed if the two test records are linked.

[19] Davenport et al. (2002) find a similar patter in Minnesota.

school decides whether or not to dropout. Denoting this decision by D_t, $D_t = 1$ indicates the student dropped out after period *t* while $D_t = 0$ reflects the student remains in school until *t*+1. All dropout decisions are assumed to be permanent. A_t denotes whether or not the student still attends school in period *t* so that $A_t \equiv 1(D_0 = 0,..., D_{t-1} = 0)$. A student is considered to have completed high school if $A_T = 1$.

All students initially take the exam and receive a score of Y_0 where the passing score is 0. In each period *t*, students still in school and who have not yet passed the exam retake it and receive a score of Y_t. As in Texas, students who do not pass the exam before the end of high school (so that $\max_{t \le T} Y_t < 0$) have the option of retaking the exam after period *T*. Students who complete high school but pass the exit exam only after leaving school are allowed to graduate.

3.2 Dropout Decision and Test Score Process

Assume that the probability that student *i* drops out of school in period *t* is a combination of a fixed component common to all time periods and students and the direct effect of failing the exam:

$$P\left(D_{ti} = 1 \mid A_{ti} = 1, \max_{j<t}(Y_{ji}) \right) = \begin{cases} d & \text{if } \max_{j<t}(Y_{ji}) \ge 0 \\ d + \delta_{ti} * 1(Y_{ti} < 0) & \text{if } \max_{j<t}(Y_{ji}) < 0 \end{cases}$$

where $\delta_{ti} \ge 0$ measures the student *i*-specific discouragement effect of failing the t^{th} retest (or $t+1^{th}$ attempt overall). The probability of staying in school until *t*+1 is thus $1-d$ for students who pass the exam by the end of period *t* while it is $1-d-\delta_{ti}$ for those who fail it in *t*. The assumption of a common baseline dropout hazard is made for convenience and is not crucial. The key assumption is that students on either side of the passing cutoff have the same dropout rate in the absence of a discouragement effect.

In period *t*>0, students still in school who have not passed the exam (i.e. those with A_{ti}=1 and $Y_{t-1,i} < 0$) retake the exam and receive a score equal to $Y_{ti} = Y_{0i} + \Delta Y_{ti}$. The only assumption placed on ΔY_{ti} is that p_{ti}, the probability of passing the period *t* exam conditional on taking it, lies between 0 and 1 so that failing is at no point ruled out nor guaranteed.

3.3 Properties of the Implied Regression Discontinuities

Dropping Out

The regression discontinuity for dropping out of high school (and the other outcomes) is the probability of dropping out corresponding to barely passing (so that the test score is a small distance ε above the passing score) minus the probability of dropping out corresponding to barely failing. The period *t* discontinuity for dropping out equals:

$$\begin{aligned}\theta_{ti}^{dropout} &= P(D_{ti}=1 \mid Y_{ti}=\varepsilon) - P(D_{ti}=1 \mid Y_{ti}=-\varepsilon) \\ &= d-(d+\delta_{ti}) \\ &= -\delta_{ti}\end{aligned}$$

The discontinuity is simply the size of the discouragement effect in period *t* for student *i*. The observed discontinuity will be a weighted average of the student-specific discontinuities were observations near the passing score receive the largest weights.[20]

Completing High School

The discontinuity in the probability of completing high school will be:

$$\begin{aligned}\theta_{ti}^{complete} &= P(A_{Ti}=1 \mid Y_{ti}=\varepsilon) - P(A_{Ti}=1 \mid Y_{ti}=-\varepsilon) \\ &= (1-d)^{T-t} - \prod_{k=t}^{T-1} P(D_k=0 \mid Y_{ti}=-\varepsilon, A_{ki}=1)\end{aligned}$$

If there is no discouragement effect, i.e. $\delta_{ji}=0$ for $t \le j < T$, failing does not affect the probability of dropping out so $P(D_k=0 \mid Y_{ti}=-\varepsilon, A_{ki}=1)=1-d$ for all *k* and $\theta_t^{complete}=0$. If there is a discouragement effect, that is, $\delta_{ji}>0$ for some $t \le j < T$, then $\theta_t^{complete}>0$. If $\delta_{ti}>0$, failing the period *t* exam directly reduces the likelihood of completing school by making dropping out before period *t*+1 more likely. But failing in period *t* also introduces the possibility that a subsequent exam will also be failed. So if failing a later exam has a discouragement effect, i.e. $\delta_{ji}>0$ for $t<j<T$, failing the period *t* exam indirectly reduces the probability of completing high school. A formal proof that $\theta_t^{complete} \ge 0$ and that $\theta_t^{complete}>0$ if and only if $\delta_{ji}>0$ for some $t \le j < T$ is given in Appendix C.

Graduation

Define G_i to be an indicator variable denoting whether a student graduates. By definition, $G_i \equiv 1(A_T=1)*1(\max_{t\le T}(Y_{ti})>0 \text{ or } \textit{pass after } T)$. The discontinuity in the graduation rate is equal to:

[20] Lee (2003) shows that the regression discontinuity gap is an average treatment effect where the weights involved in the calculation of the average effect depend positively of an observation's proximity to the cutoff.

$$\begin{aligned}\theta_{ti}^{graduate} &= P(G_i = 1 \mid Y_{ti} = \varepsilon) - P(G_i = 1 \mid Y_{ti} = -\varepsilon) \\ &= (1-d)^{T-t} - P(\max_{t<j\le T}(Y_{ji}) > 0 \text{ or } \textit{pass after } T \mid A_{Ti} = 1, Y_{ti} = -\varepsilon) \\ &\qquad * P(A_{Ti} = 1 \mid Y_{ti} = -\varepsilon)\end{aligned}$$

As with the completing high school outcome, a discouragement effect of failing any exam starting in period *t* would generate a positive discontinuity in the fraction graduating.[21]

In contrast to the dropping out and completing high school outcomes, a discontinuity in the graduation rate will also exist even when $\delta_{ji} = 0$ for all $j \ge t$. Under the assumption of no discouragement effect, $\theta_{ti}^{graduate}$ can be written as:

$$\begin{aligned}\theta_{ti}^{graduate} &= (1-d)^{T-t} - (1-d)^{T-t} P(\max_{t<j\le T}(Y_{ji}) > 0 \text{ or } \textit{pass after } T \mid A_{Ti} = 1, Y_{ti} = -\varepsilon) \\ &= (1-d)^{T-t}\left[1 - P(\max_{t<j\le T}(Y_{ji}) > 0 \text{ or } \textit{pass after } T \mid A_{Ti} = 1, Y_{ti} = -\varepsilon)\right] \\ &= (1-d)^{T-t}\left[(1-\rho_i)\prod_{j=t+1}^{T}(1-p_{ji})\right]\end{aligned}$$

where ρ_i is the probability of passing the exam after time *T* conditional on failing the period *T* exam. The term in brackets is the probability of never passing the exam conditional on completing high school.[22] As long as $\rho_i < 1$, completing high school does not guarantee eventually passing the exam implying that failing at any point lowers the likelihood of graduating.

At the same time, if *T* is large enough, failing during an early test administration will have a minimal effect on the likelihood of graduating. Consider the initial attempt when *T*=7 (as in Texas). Because students just below the passing cutoff are at the cusp of being able to pass, their probability of passing on any retest attempt is likely to be at least ½.[23] In the extreme case where the period *T* test is binding (so that ρ_i=0), $\theta_{0i}^{graduate} \approx (1-d)^7(\tfrac{1}{2})^7 \approx (1-d)^7 * .007 \approx 0$.

But as the end of high school nears, the discontinuity in the graduation rate will grow. For instance, the discontinuity for the *T*-1 exam equals $(1-d)(1-p_{Ti})(1-\rho_i)$. Assuming that a

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[21] This can be seen by noting that $\theta_{ti}^{graduate} \ge \theta_{ti}^{complete}$ and that $\theta_{ti}^{complete} > 0$ when $\delta_{ji} > 0$ for some $t \le j < T$.

[22] The conditional probability of never passing the exam can be written this way because the no discouragement effect assumption implies that completing high school reveals no information about a student's test performance. With a discouragement effect, completing high school would be more likely if the test had been passed before period *T* and therefore the conditional probability of failing on the j^{th} attempt (for *j*<*T*) would be lower than $(1-p_{ji})$.

[23] The probability of passing could be even better than ½ if students acquire test-specific knowledge over time. Alternatively, students who initially receive "bad" draws from the test score distribution could do better in the future as a result of mean reversion.

student just below the passing cutoff on the period T-1 exam has a 50-50 chance of passing in period T and setting d =0.1 and ρ_i =0.4 (values higher than the averages actually seen in Texas) yields $\theta_{T-1}^{graduate} = 0.675 * \frac{1}{2}$ and the discontinuity will be about 0.27.

Summary of Predictions

The following table summarizes the model's qualitative predictions of the discontinuities for each outcome:

	No Discouragement Effect		Discouragement Effect	
	early t	late t	early t	late t
Dropout	0	0	<0	<0
Complete High School	0	0	>0	>0
Graduate	≈ 0	>0	>0	>0

Note that information on graduation alone does not identify the discouragement effect. Finding discontinuities in the graduation rates only for the late test rounds is consistent with no discouragement effect, but it is also consistent with a discouragement effects that appears only towards the end of high school. Finding in addition no discontinuity in the dropout rate or in the fraction of students completing high school, however, rules out the discouragement effect. The TSMP data includes information on graduation but also school leaving and completion which makes testing for the discouragement effect possible.

As noted earlier, the regression discontinuity gap provides an estimate of the average effect of failing that places the most weight on students nearest the passing cutoff. If the behavioral response is limited only to students with scores far below the passing cutoff, no discontinuity in the fraction dropping out or completing high school will be seen. Identification of the effect of failing for the average student (or average failer) requires making strong assumptions about the counterfactual dropout rate and the test score process.

4 Empirical Strategy

This paper uses a regression discontinuity design to distinguish the causal effect of failing the exit exam on student outcomes from the influence of confounding factors. The motivation for the strategy lies in the sharp cutoff demarcating failing and passing students. If passing status has an effect, the mean of the dependent variable will jump at the passing threshold. Interpreting such a discontinuity as evidence of a causal effect is valid provided that unobservable determinants of the student outcome exhibit no discontinuous behavior at the passing cutoff.[24]

In the present context, such a condition seems sensible. Endogenous sorting of students around the passing cutoff is unlikely given teachers and students do not know ex ante which students will be right at that score. Even if teachers cheat (as some allege to be the case, see Jacob and Levitt, 2002), scores at the passing cutoff would be distorted no more than would neighboring scores. Not only is the underlying identification assumption intuitively appealing, it can be tested. The continuity assumption implies that all observed student characteristics trend smoothly through the passing cutoff. Finding this to be true would provide reassurance that the data satisfies the identification conditions.

The estimation problem in the RD model consists of obtaining estimates of the discontinuity in the conditional expectation of the dependent variable at the passing cutoff. Carrying out this task can be done several ways. One possibility is to take the difference in mean outcomes between passers and failers whose scores lie within some bandwidth of the passing score. Aside from throwing away all of the information provided by the rest of the sample, including that from observations with scores very close to the passing cutoff, such a strategy would be inappropriate in this application. As long as there is a monotonic gradient in the mean outcome (with respect to the test score) near the passing cutoff, simply comparing the average for the students at and just below the cutoff will overstate the effect of failing the test.[25]

An alternative is to adopt a specific functional form for the conditional expectation of the dropout rate as a function of the test score. In practice, I estimate the following equation for each outcome Y_i:

$$Y_i = \begin{cases} \theta Pass_i + f(MinScore_i) + X_i\beta + \varepsilon_i & \text{if } Y_i \text{ is continuous} \\ 1(\theta Pass_i + f(MinScore_i) + X_i\beta + \varepsilon_i > 0) & \text{if } Y_i \text{ is dichotomous} \end{cases}$$

where

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[24] For a discussion of the precise conditions under which an RD design is valid, see Porter (2003). Other recent applications of the RD approach can be found in McCrary and Royer (2003), DiNardo and Lee (2004), and also in the context of test scores and student outcomes, Jacob (2004). Lee (2003) interprets the RD design in the framework of classical randomized trials.

[25] The ability of the test score to identify comparable passers and failers is limited by the fact that it is fundamentally discrete. While students at the passing cutoff and those one point below it are likely to be far more similar than students, say, 5 points apart, it remains true that students with higher scores will tend to have "better" characteristics than those with lower scores, regardless of the proximity of their scores. It is therefore inappropriate to attribute difference in dropout rates simply to failing the exam. With a truly continuous selection index, the ability to compare arbitrarily similar observations on either side of the cutoff is limited only by the density of the data near that cutoff.

$$MinScore_i = \underset{j}{\text{Min}}\left(dScore_{ij}\right), \quad j \in \{\text{Math}, \text{Reading}, \text{Writing}\}$$
$$Pass_i = 1\left(MinScore_i \geq 0\right)$$
$$dScore_{ij} = Score_{ij} - PassScore_j$$

and $Score_{ij}$ is student *i*'s raw score on test section *j*, $PassScore_j$ is the passing score for section *j*, X_i is a vector of covariates (not included in all specifications), $f(MinScore_i)$ is a flexible function of the student's test score, and ε_i is a random disturbance term.[26] I specify *f* as a fourth-order polynomial in the test score with an interaction between the passing dummy and the test score. Because the estimates will be biased if *f* is misspecified, the robustness of the results is assessed by comparing the estimates to those in which *f* is a second-order polynomial with the linear and quadratic term interacted with the passing dummy. Equation 1 is estimated via least squares for continuous dependent variables while a probit regression is used for binary outcomes. The parameter of interest is $\lim_{Minscore \to 0^+} E(Y_i \mid MinScore_i) - \lim_{Minscore \to 0^-} E(Y_i \mid MinScore_i)$, or the difference in the conditional mean of Y_i between passers and failers at the passing cutoff.[27] For continuous outcomes, this parameter equals θ. For dichotomous outcomes, the parameter of interest is the difference at the passing threshold in the probability that Y=1 after "turning on" the passing dummy: $\Phi(\theta + f(0))$-$\Phi(f(0))$ where Φ is the c.d.f of a standard normal random variable.

5 Data

5.1 Dataset Construction

The data used in this paper come from the Texas Schools Microdata Panel (TSMP), a database housed at the Green Center for the Study of Science and Society on the University of Texas–Dallas campus.[28] The TSMP is a collection of administrative records from various Texas state agencies that permits a longitudinal analysis of individuals as they proceed through high school and later as they enter college or the workforce. As such, it offers a unique opportunity to

[26] The test score variable used here is the lowest rescaled score a student receives. The scores are centered at the passing level so that a rescaled score of at least zero is needed to pass a given section. For the initial attempt, the variable is the minimum rescaled score of all three sections of the TAAS. For subsequent attempts, taken only by students who have yet to pass, the variable is the minimum rescaled score of the unpassed sections. Because all three sections must be passed, a student passes the entire test if and only if they pass their worst section.

[27] Under the assumption that the X's behave smoothly at the passing cutoff, the difference in unconditional means will be the same as the difference in the means conditional on the X'.

[28] For a (somewhat out of date) description of TSMP and examples of its use, see John Kain's 2001 paper: http://www.utdallas.edu/research/greenctr/Papers/pdfpapers/paper28.html.

assess the impact of exit exams on a broader set of outcomes than that considered in any existing study.

The backbone of the datasets employed in this analysis comes from the Texas Education Agency's (TEA) records.[29] These files have information on enrollment, attendance, dropout status, GED acquisition, and performance on the exit-level TAAS.[30] I examine 5 cohorts of test takers, students in 10th grade in the spring of 1991 through 1995.[31] Linking multiple test records on the basis of a unique student identifier, I construct the test-taking history for each student. The resulting panel includes the test scores as well as the student's ultimate passing or exemption status.

From the full test score panel, I produce the two principal estimation samples. The first consists of the students in the 1993-1995 cohorts and is used to analyze how the impact of failing the exit exam changes as the number of failed attempts grows.[32] For instance, the effect of failing the initial attempt can be compared to the effect of failing again in the fall of 12th grade (by which point a student could have taken and failed the exam up to five times).

The second estimation sample covers all five cohorts and includes the students who had not passed the exit exam by the final time it was offered before the expected date of the end of high school.[33] I refer to this administration as the "last chance" test. As seen in Table 1, the "last chance test" for the 1992-1995 cohorts is the special test given in May or April to seniors who have yet to pass. No such test was available for the 1991 cohort so their "last chance" test is the exam administered in the spring of their senior year. It is important to recognize this test really is not a student's last chance to pass since they can always take it at some point after finishing high school. Nonetheless, because the impact of failing the "last chance" test on graduating from high

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[29] See Appendix A for a detailed description of the construction of the datasets used in this paper.

[30] The TEA collects information on GED acquisition to determine whether or not a student who has left high school without graduating will count towards a school or district's dropout count for the purposes of assigning accountability ratings (students receiving a GED do not count towards the official dropout count).

[31] Additional cohorts are available in the TSMP. The 1990 11th grade cohort is excluded because those students faced a lower passing standard than did subsequent test takers. Younger cohorts are not included because the TSMP's test score data from the 1997-98 school year is incomplete making it impossible to construct complete test-taking histories.

[32] The earlier (1991 and 1992) cohorts are not included because in those years students started taking the exit exam in 11th grade and had fewer chances to take the exam before the end of high school than did later cohorts.

[33] For students initially taking the test in 11th grade (the 1991 and 1992 cohorts), the expected end of high school takes place in the academic year following that in which the TAAS was initially taken. For those taking it in 10th grade (1993, 1994, 1995), it is two years after initially taking the test. A student who graduates but is retained in grade following the initial test attempt would graduate one year later. I refer to the expected high school completion date as the "on time" graduation year.

school is so large, I use this sample to see if not graduating due to an inability to pass the exit exam affects post-secondary schooling and earnings in early adulthood.[34]

The final step in creating the datasets involves merging the test data to the TSMP files that have information on student outcomes. Details concerning these matches and other aspects of the dataset construction can be found in Appendix B. Sample means of both samples are reported in Appendix Tables 1 and 2.

5.2 Outcomes

Graduation: Information on a student's graduation status comes from the TEA's roster of students who receive a high school diploma from a Texas public school during an academic year. In this paper, a student is classified as a graduate if a matching record appears in these files indicating the student earned a diploma within two years of the year in which they would graduate if they were not held back after initially taking the test. I refer to this year as the expected date of high school completion.[35]

GED Test Taking and Certification: The TSMP's GED data includes a record for each time an individual in Texas who earned a GED certificate took the GED qualifying exam. Beginning in 1995, it includes records of all GED test takers regardless of eventual passing status. I construct two outcomes using these data. The first is whether or not the student earned a GED within five years of the expected high school completion year. The second is simply whether or not the student ever took the GED exam within five years of this date.[36]

High School Attendance After Taking the Exit Exam: I consider two outcomes based on student attendance that are used to test for the presence of a discouragement effect.[37] The TEA's attendance data for a given academic year consist of six files corresponding to separate six-week periods. Each of these files documents the number of days attended by all enrolled students during the period. The first attendance-based outcome simply denotes whether the student stopped attending school after taking the exit exam. Someone is considered to have stopped attending school if no matching records in the attendance files are found starting with the first six-

[34] The "last chance" test is not truly the final opportunity a student has to pass the exit exam. Anyone who fails it but has completed all other high school requirements can earn a diploma if they pass the test during a future administration. Moreover, this test will not even be the final test before for someone's scheduled graduation if they are held back at some point after initially taking the exit exam.

[35] Over 99% of graduates graduate within one year of their expected graduation date, or what I call the expected date of high school completion. Virtually no one graduates more than two years after this time.

[36] The second variable is somewhat problematic because the pre-1995 data does not include GED test takers who did not earn the certification and students who took and failed the GED test before 1995 will thus be misclassified. For the 1993-95 sample, this does not represent a serious problem since even the students in the earliest cohort would be scheduled to graduate after the data began covering all GED test takers.

[37] Similar results are obtained when considering a third outcome, whether or not the TEA officially designated the student as a dropout.

week period after the test.[38] It is intended to capture an immediate response to failing the exit exam and corresponds to the dropout outcome from the model presented in Section 3. The second is whether a matching record is found indicating attendance in the final six-week period of 12th grade. It is designed to reflect whether a student eventually reached the end of high school allowing for the possibility of temporarily dropping out along the way. It resembles the high school completion outcome from the model.

Post-Secondary Schooling: The TSMP's data on post-secondary education comes from the administrative records of the Texas Higher Education Coordinating Board (THECB). Included in the THECB's files is a list of all students enrolled in public two and four-year colleges in Texas in a given academic term as well as the number of semester credit hours in which they are enrolled. I consider two measures of college attendance – the first based on ever enrolling in college within five years of expected high school completion and the second based on ever enrolling as a full time student within the same five-year window.[39]

In addition to looking at attendance, I also examine the amount of post-secondary schooling acquired. Using the THECB's data on degrees conferred by Texas post-secondary institutions, I construct an outcome that denotes a student earning a bachelor degree, associate degree, or certificate (such as a nursing or teaching certificate). Since the available data stops in 2001, only degrees awarded four years after the expected end of high school are included in this measure. Lastly, I look at the number of semester credit hours a student enrolled in up to five years after the expected high school completion date. I consider credits from two and four-year colleges separately.

Earnings: The data on earnings comes from the Unemployment Insurance (UI) tax reports submitted to the Texas Workforce Commission by employers subject to the state's UI law. Subject employers are required to report, on a quarterly basis, the wages paid to each employee in order to determine the firm's tax liability.[40] The use of administrative earnings data presents advantages and drawbacks relative to more commonly used survey data such as the CPS or NLSY. A key strength of administrative data is the relative lack of measurement error that plagues survey

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[38] The relevant six-week period changes with each retest attempt. For instance, a student retesting in the summer before 11th grade is considered to have stopped attending school if they never show up in the attendance data starting in the first six-week period of the following school year.

[39] A student is considered full time if they take at least 12 semester credit hours.

[40] Someone working more than two covered jobs in a quarter will have multiple entries in the TWC file for that quarter. The earnings variables used here are the sum of the earnings in a quarter from records with matching encrypted SSN's.

data (Jacobson, Lalonde, and Sullivan, 1993).[41] The main limitation is the inability to distinguish individuals with no observed earnings due to unemployment from those who are employed in a job not covered by the UI system, who have moved out of Texas, or who are enrolled in college.[42]

The possibility that enrollment in school could lead to someone having no observed earnings or temporarily depressed earnings is especially important given that college attendance may be endogenous to failing the exit exam. With this concern in mind, I consider earnings in three different periods: (1), the first four quarters after a student was last enrolled in school (with a one quarter gap between the end of school and the start of the earnings window) (2), years 4 and 5 and (3) years 2 through 5 after the (expected) end of high school. The first two periods are chosen with the intent of being late enough so as to minimize the bias arising from reduced labor supply while attending college. For each period, I will use a sample of students with any earnings in the observation window as well as one restricted to students with positive observed earnings in each quarter. The latter sample is designed to focus on individuals with a strong attachment to the labor market whose observed earnings are likely to be a better predictor of lifetime economic success than are those of someone with periods spent not working. And while UI earnings data understate the true earnings of individuals working outside of Texas or in jobs uncovered by the UI system, the observed earnings of individuals in this sample are less likely to suffer from this problem.[43]

5.3 Matching and Misclassification

Because the outcomes described in the preceding section depend entirely on linking records across various administrative data sets, they are likely to be measured with error. For instance someone graduating from high school will only be classified as such if the test and graduation records are successfully matched. Simple typographical errors in the identifiers present the most obvious reason for failed matches. The presence of such errors may be systematic; the record keeping in resource-rich schools is likely to be more reliable than it is in resource-poor schools. However, misclassification arising from this type of error is not likely to vary

[41] Bound and Krueger (1991) estimate the error variance of CPS earnings to be 27.6% of the total variance for men and 8.9% for women.

[42] Federal employees, persons working "under the table" or who are self-employed, and students working for the school they attend are relevant examples of individuals whose employment is not covered by the UI law in Texas.

[43] This restriction is similar in spirit to that made by Jacobson, LaLonde and Sullivan (1993). In their study of the earnings losses of displaced workers, they require displaced workers to have positive earnings in every calendar year in their data. In a context similar to that of this paper, Betts and Grogger (2000) use earnings from individuals working full-time at least nine months out of the year when examining the effect of high school grading standards on earnings.

systematically between students who barely fail and those who barely pass the exam. As such, it should not lead to biased regression-discontinuity estimates.

A more worrisome possibility is that misclassification is itself correlated with failing the TAAS. For example, a student who fails the "last chance" test cannot graduate with their class and, at best, must retake the exam and graduate at some point in the future. These atypical graduates could be more likely to slip through their school's record keeping cracks and not appear on the roster of graduates. This type of misclassification would lead to an overstatement of the impact of failing the exit exam on not graduating.

Endogenous misclassification of this sort is unlikely to pose a problem for outcomes using non-TEA data. It is difficult to envision a scenario in which a student's TAAS-passing status would affect the likelihood of, say, incorrectly appearing in or failing to appear in the list of a community college's enrollees. Moreover, the bias associated with endogenous misclassification is likely to work in opposing directions depending on the outcome. Suppose that the records of students who barely fail the TAAS are systematically less likely to match to other TEA files than are those of students who barely pass. As noted in the preceding example, such misclassification would yield an overstated estimate of the effect of failing on not graduating. However, it would also lead to an *understated* estimate of the effect on earning a GED since individuals induced to get a GED due to failing the exit exam will face higher rates of misclassification than will GED recipients who pass the exit exam. The availability of multiple outcomes therefore provides some reassurance that the main results are not being driven solely by misclassification.

6 Results for High School Attendance and Completion

6.1 Does Failing the Exit Exam Reduce the Likelihood of Graduating?

The first empirical question to be resolved is whether the testing requirement actually prevents anyone from graduating from high school. Figure 1 sheds light on the relationship between test performance and graduation. The organization of many of the graphs that follow mirrors that of Figure 1 and the following description also applies to them. Each panel corresponds to a separate test administration. The upper-left panel depicts the association between graduation and the score on a student's initial attempt. The remaining panels represent the analogous relationship for subsequent administrations taken by students who had not passed by that point.[44] The open circles represent the average graduation rate for students with a particular test score. The superimposed curve is the fitted probability from a probit regression of

[44] Some students still needing to pass the test may not retake it each time it is offered. Someone retesting in the spring of 11th grade would only have taken the test a total of three times if they did not retake the exam in the summer of 10th grade.

the graduation dummy on a dummy for passing the test, a fourth order polynomial in the test score and an interaction between the passing dummy and a linear term in the test score.[45] The size of the discontinuity in the fitted probability curve at the passing threshold is listed at the top of each panel (with the estimated standard error adjusted for clustering within test score cells in parentheses) and it equals the estimated marginal effect of "turning on" the passing dummy.[46] Table 3 reports the estimated magnitude of the discontinuities. Note that the number of observations varies by test administration since students who pass the exam do not need to retake it and fewer students retest in the summer.

The first panel reveals that the likelihood of graduating does not jump up as the score on the initial attempt crosses the passing cutoff (the estimated discontinuity is 0.001 with a standard error of 0.004).[47] However, as the expected graduation date nears, students still unable to pass the exit exam have markedly lower graduation rates. A small but discernible discontinuity appears by the summer after 11th grade (0.020 with a standard error of 0.006) and the jump in the graduation rate increases steadily for the remaining test administrations. Failing the "last chance" test, if only by one question, has an enormous negative impact on the probability of graduating – the point estimate is 0.444 with a standard error of 0.012. Note that this is the pattern predicted by the model from Section 3 under the no discouragement effect assumption.

I now look to see if failing the exit exam causes an increase in GED acquisition. Such an effect would be expected if this test really precludes students from earning a conventional diploma because some of the induced non-graduates might earn a GED as a substitute credential. Figures 2a and 2b show little evidence of a discontinuity at the exit exam passing score in the rate of taking the GED certification exam or in receiving a GED for tests taken through summer of 11th grade. But as with graduation, failing late in high school appears to cause distinctly higher rates of GED acquisition. The probit estimates in Table 3 reveal that barely failing the "last chance" test increases the rate of taking the GED test by 0.074 with a standard error of 0.004 and of receiving a GED certificate by 0.044 with a standard error of 0.003.

At the same time, the GED does not fully substitute for the diplomas denied to students who fail the exit exam; most of these individuals never get a GED. Figure 3 shows a clear discontinuity in the rate of receiving any credential, be it a GED or a standard high school degree,

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[45] For completeness, I also report estimates (in the tables) in which the test score function is parameterized as a quadratic allowing for different linear and quadratic terms to the left and right of the passing score.
[46] All marginal effects are evaluated at the passing cutoff.
[47] Unless otherwise noted, the estimates reported in the text of the paper are obtained using the quartic polynomial in the test score with no additional covariates included as controls.

at the passing score for tests starting in the fall of 12^{th} grade. For the "last chance" test, the estimated discontinuity reported in Table 3 is 0.410 with a standard error of 0.012.

The consistency of the GED and graduation results offer reassurance that the evidence in Figure 1 really does reflect a true diploma-denial effect and not a spurious artifact of matching-related misclassification. Successfully matching a test and graduation record is taken to be evidence the student received a high school degree while matching a test and GED record has the opposite interpretation. Even if the probability of misclassification of graduation is correlated with passing status (which results below suggest may be true), it is unlikely to fully explain the observed impact of failing on graduating.

The validity of the RD estimates in Table 3 rests on two assumptions – the polynomial approximation to the underlying conditional expectation function is sufficiently close and that confounding factors do not behave discontinuously at the passing cutoff. A straightforward way to test the first assumption is to see how closely the fitted probability curve tracks the local averages. For both GED variables and especially for graduation, the predicted probabilities are close to the local averages (at least in the relevant range of scores near the passing cutoff). The similarity of the point estimates obtained using the quartic versus the quadratic polynomial in the test score provides further evidence that functional form concerns are not of first-order importance. For instance, for the "last chance" test, the point estimate of the effect on graduation using the quadratic specification is 0.438 as opposed to 0.444 when a quartic polynomial is used (the respective standard errors are both 0.012).

The second assumption has the testable implication that the conditional expectation functions of predetermined characteristics exhibit no jumps at the passing score. Appendix Table 3 reports the estimated discontinuities for a set of baseline student characteristics. These estimates tend to be relatively small and statistically insignificant. One exception is the dummy variable for needing to pass only one section – the point estimates range between 0.022 and 0.026 (with standard errors ranging between 0.005 and 0.007) in some of the early administrations.[48] Figure 4, however, reveals essentially no evidence of a striking discontinuity in this variable at the passing cutoff. One other notable result is for Limited English Proficient (LEP) in the fall of 11^{th} grade. The point estimate is –0.012 with a standard error of 0.002 suggesting that barely passers are somewhat less likely to be LEP than are barely failers (this pattern does not appear in any of the other administrations).

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[48] For the initial attempt, all students are required to take all three sections (students exempt from any section are not included) so it is not included in Figure 4.

Another implication of the second assumption is that determinants of the outcome will be orthogonal to passing status conditional on the test score. This implies that including baseline characteristics in the probit regressions ought not affect the estimated discontinuities. The point estimates in Table 3 are largely unaffected by the inclusion of covariates.

6.2 The Effect of Failing the Exit Exam on Quitting School Prematurely

The preceding section established the result that failing the exit exam does reduce the likelihood of graduating. Moreover, the absence of a discontinuity in the graduation rate for the first few attempts combined with a growing discontinuity as the end of high school nears matches the pattern predicted by the model from Section 3 with no discouragement effect. I now directly assess the validity of the discouragement hypothesis by looking to see if failing causes students to leave school before completing high school. To address this issue, I first see if failing causes higher rates of permanent school leaving. The discouragement hypothesis predicts a jump downward since students who pass do not receive the discouragement "treatment". Figure 5 reveals little evidence of such a pattern. The largest jump downward occurs for the test given in summer of 11th grade with an estimated discontinuity (found in Table 4) of -.006 and a standard error of 0.003. For the other administrations, the estimated discontinuities are both small and statistically insignificant or are positive.

Interestingly, the results indicate that failing the "last chance" test increases the probability of attending high school again by 0.063 with a standard error of 0.007. Apparently, some students return to school for another year if they have not passed the TAAS by the end of 12th grade.[49] This finding is reasonable given that students in Texas are eligible for free schooling provided they are younger than 21 and have not graduated from high school. Returning to school may be an attractive option for students whose only unmet graduation requirement is passing the exit exam.

Figure 6 replaces the never attend again variable with an indicator for attendance in the final six-week period of 12th grade. This new outcome would pick up a discouragement effect whereby students quit school sometime after taking the test but not necessarily immediately thereafter. However, the results offer no evidence that the rate of attendance at the end of 12th grade jumps up for students barely passing the exam. The largest estimated discontinuity

[49] A similar but less pronounced pattern can also be seen for the fall of 12th grade (the estimated discontinuity is 0.009 with a standard error of 0.002). This is likely due to students who completed all non-TAAS graduation requirements but did not pass the exit exam the first time they were in 12th grade. After passing the TAAS the following fall, they earned a diploma and did not return to school in the spring. To confirm this explanation, I replaced the dependent variable with an indicator for not attend & not graduate in the fall (the vast majority of graduates do so in the spring), the estimated discontinuity falls to –0.001 with a standard error of 0.002.

(reported in Table 4) is for the test given in the summer of 11th grade and equals 0.012 with a standard error of 0.008.

Taken together, the results in this section do not support the view that exit exams drive frustrated students to drop out of school. As with the graduation results, these findings are most consistent with a model with no discouragement effect. It must be remembered, however, that RD estimates inherently compare barely failers to barely passers and will not uncover the discouragement effect if it operates primarily on students scoring nowhere near the passing standard. That said, no effect is found even for students who have failed a number of times. And finding no effect for barely failers at a minimum suggests the absence of a sizable discouragement effect in the population at large. Barely failing, after all, is still failing.

6.3 Estimates of the Effect of Failing the Exit Exam on the Total Number of Non-Graduates

The preceding subsections imply that exit exams do prevent students from graduating but not because they drive students to quit school early. This conclusion suggests focusing exclusively on the "last chance" test is appropriate for estimating the total number of students not graduating as a result of the exit exam policy.[50,51] Of course, these estimates understate the true effect to the extent that the exit exam causes students to dropout before the "last chance" test. This section provides a back-of-the-envelope calculation of the effect on aggregate non-graduation rates making the conservative assumption that no discouragement effect exists.

The strategy simply uses the RD estimates as a measure of the causal effect of failing on not graduating and multiplying that by the number of students who fail. The RD estimate may understate the effect for students well below the passing cutoff. Their weak academic skills make them less able to eventually pass relative to students who barely fail.[52]

Naturally, this approach works only if the RD estimate provides a valid measure of the effect of failing. As has been noted, it may not if passing status affects not only the true probability of graduating but also the prevalence of misclassification in the graduation variable. One way to gauge this problem's importance is to perform the analysis on a set of students for whom failing the "last chance" test should not affect the likelihood of graduating – those who

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[50] The "last chance" sample includes students from the 1991 and 1992 cohorts. Recall that during this period, the test was first administered later in high school – in the fall of 11th grade. Focusing on the "last chance" administration renders this distinction unimportant. A smaller fraction of students from the 1993-95 cohorts (4.0 percent) appear in the "last chance" sample than do those from the 1991-92 cohorts (8.2 percent) in part because the late cohorts took the test earlier in high school and had more chances to pass it.

[51] Appendix B discusses the evidence for the validity of the RD design for the "last chance" sample.

[52] For the very lowest scoring students, on the other hand, the RD estimate may overstate the effect. These students are more likely to have unmet graduation requirements beyond passing the exit exam and therefore would not graduate even without the testing requirement.

pass the TAAS at some point in the future. Figure 7 shows instead clear evidence of a discontinuity in the classified-as-graduate rate even for this subset of students. The magnitude of the discontinuity, seen in Table 5, is 0.162 with a standard error of 0.009 – less than half the size obtained using the full sample, but still fairly large.

It is implausible that this pattern reflects a true effect on graduation rates. Students failing this test may still have unmet diploma requirements but students who barely pass should as well and at roughly the same rate as do those who barely fail. And unlike the results for the full sample, no discontinuity in the fraction acquiring a GED appears.[53]

I account for the apparent presence of endogenous misclassification in two ways. The first simply excludes the students eventually passing from the count of students failing the "last chance" test when estimating the number of induced non-graduates. This adjustment is likely to be conservative because the causal effect for those not eventually passing is likely to be larger than that reported in Table 3.[54] The second adjustment attempts to directly account for the misclassification bias. I calculate the overestimate in the number of induced non-graduates by multiplying the RD estimate that uses only the eventual-passers by the number of students failing the "last chance" test but eventually passing. This is then subtracted from the unadjusted figure. I calculate the overall effect using the following three estimates of the number of induced non-graduates:

(1) No Adjustment $\theta_{full} * N^{fail}$

(2) Conservative Adjustment $\theta_{full} * \left(N^{fail} - N^{fail}_{eventuallypass}\right)$

(3) Direct Adjustment $\theta_{full} * N^{fail} - \theta_{eventuallypass} * N^{fail}_{eventuallypass}$

where θ_{full} and $\theta_{eventuallypass}$ are the RD estimates obtained using the full sample and the subset of students eventually passing, respectively. N^{fail} is the number of students who fail the "last chance" test and $N^{fail}_{eventuallypass}$ denotes the number of failers who eventually pass. Dividing each figure by the total number of students gives the fraction of students not graduating due to the testing requirement. Dividing instead by the number of non-graduates gives the fraction of non-graduates who do not graduate because of the exit exam.

[53] The difference in the levels of GED acquisition also supports this claim. The fraction receiving a GED certificate conditioning on eventually passing is only 1.1 percent while it is 5.2 percent in the full sample.

[54] Another way to see this is to think of the RD estimate from the full sample as a weighted average of the effect of failing for students who never eventually pass and for those who do. The latter ought to equal zero. Even though misclassification error makes this not the case, it will still be smaller than the effect for the never-passers.

Table 6 reports the estimates of the total effect on non-graduation. The estimated fraction of students not graduating ranges from 0.011 to 0.014. The effect for non-white and economically disadvantaged students is larger – ranging from 0.019 to 0.025 for non-whites and from 0.021 to 0.026 for the economically disadvantaged. Differences in passing rates account for the larger effects found among these groups. 45 percent of the first-time test takers are non-white and 25% are economically disadvantaged, but as seen in Appendix Table 2, these figures rise to 77 percent and 45%, respectively, for the "last chance" test.[55]

Figure 8 reveals considerable heterogeneity in the magnitude of the effect on non-graduation.[56] Failing has virtually no effect for those with initial scores near passing. This result is consistent with the earlier finding of no discontinuity in the graduation rate for students at the passing cutoff on the initial attempt. The inability to pass the test poses a much larger hurdle for very low-achieving students. The estimated effect grows steadily and tops out, depending on the misclassification adjustment, between 7 and 9 percent roughly at a score of 25 questions below passing (or the 5th percentile of the conditional-on-failing test score distribution).

7 Results for Post-High School Outcomes

The results presented thus far demonstrate that a segment of Texas high school students do not graduate because they cannot pass the exit exam. I now assess what this fact implies about the economic future of these students. A sizable literature documents the higher earnings of high school graduates relative to non-graduates (for instance, Oreopoulos, 2003) but it is not clear that these findings apply in this context. This paper's results show no evidence that exit exams increase the rate of leaving school early. Students induced to not graduate as a result of scoring just under the passing standard on the "last chance" test finish their high school careers with the same educational attainment (and presumably human capital) as do those with scores just above the cutoff.

But not earning a diploma may still have adverse economic consequences. One reason is that admission into college requires a high school degree.[57] This implies that failing the exit exam and subsequently not graduating from high school may indeed lower educational attainment, just

[55] The subgroup calculations use group-specific RD estimates. The RD estimates do not vary much by subgroup – for nonwhites, the estimated discontinuity in graduation rates is 0.405 (0.009) and for economically disadvantaged students it is 0.394 (0.008).

[56] To produce the estimates separately by test score, the procedure used for the full sample is replicated but the counts of graduates, students, and number of failers is done conditional on initial test score. For instance, suppose there were 1000 induced non-graduates overall and 100 with a score of –10 on the initial test. If there are 2000 students with an initial test score equal to 2000, the score-specific rate of induced non-graduation would be 100/2000=.05 for a score of –10.

[57] Many schools accept a GED in lieu of a high school diploma.

not at the pre-graduation level that receives most of the attention in the literature. Not graduating could itself also prove costly. According to the models that view education as a sorting mechanism, a degree can serve as a signal to prospective employers of unobserved ability (Spence, 1973; Jaeger and Page, 1996). If this signal has value in the labor market, then not earning a diploma due to failing the exit exam (or for any reason) would lower potential earnings. It should be noted that these results are useful not only for understanding the consequences of failing an exit exam but also because the identification strategy offers a unique opportunity to assess the empirical importance of the signaling value of a degree.[58] Because students on either side of the passing cutoff essentially have the same level of human capital but markedly different high school credential rates, differences in earnings can plausibly be attributed to the receipt of the degree.[59,60]

The GED results presented earlier offer indirect evidence that not receiving a diploma matters. If a graduate and a non-graduate with otherwise identical characteristics (which would be the case right at the exit exam passing cutoff) enjoyed equally promising labor market prospects, the non-graduate would have no incentive to incur the costs associated with earning a GED. These students apparently view their lack of a high school degree as a deficiency and the GED as a way to ameliorate it.

This section examines the direct economic cost of failing the exit exam by looking at the impact of failing on post-secondary schooling and earnings. To be clear, the estimated effects refer to students whose graduation status is affected by failing. Potentially offsetting some of these costs are benefits to students for whom the likelihood of graduation remains unaffected by the policy. Because the exit exam reshuffles some students from the graduate to the non-graduate category, the average ability of each group will rise.[61] If the wages earned by members of the two groups respond to changes in the average "quality" of the group (as they would in a signaling model of the labor market), anyone whose graduation outcome is unaffected by the policy stands

[58] Tyler (2001) uses a regression discontinuity design (along with several other identification strategies) to estimate the effect of receiving the GED credential on earnings. Tyler, Murnane, and Willett (2000) use variation in the passing standard across states to compare the earnings of credentialed and uncredentialed GED test-takers who have the same test score. Both papers estimate substantial returns to earning a GED.

[59] Since passers are more likely to go on to college, they will have higher levels of human capital ex post. As will be seen later, however, college attendance is actually associated with lower earnings in the years immediately after completing high school. Thus the earnings impacts estimated without accounting for differential rates of college attendance may understate the diploma effect.

[60] Failing affects the likelihood of not only earning a high school diploma, but also rates of GED acquisition and college attendance. The RD estimates that follow are reduced form in nature since they capture the effect failing has on earnings through its effect on the intermediate outcomes.

[61] Mean ability levels of both groups will rise if the students whose graduation outcomes are affected by failing have higher ability than the "never graduates" but lower ability than the "always graduates".

to gain.[62] While this type of feedback effect may exist, in practice it is likely to be small given that failing only reduces the likelihood of graduating for a modest number of students. Any change in the distribution of ability for high school graduates and non-graduates will likely be imperceptible to employers.

7.1 Estimates of the Effect of Failing the Exit Exam on Post-Secondary Schooling[63]

Figure 9 presents evidence that failing the "last chance" test does reduce the likelihood of attending college. The upper-left panel shows that the rate of attending a two or four-year college jumps up at the passing score and the upper-right panel reveals a similar result for ever taking a full-time load of semester credit hours at either a two or four-year college. The estimated discontinuities listed in the first column of Table 7a are 0.057 with a standard error of 0.007 for ever attending a 2 or 4 year college and 0.042 with a standard error of 0.008 for attending full time.

Smaller discontinuities emerge when focusing exclusively on four-year colleges. Failing reduces the likelihood of ever enrolling in a four-year institution by an estimated 0.008 with a standard error of 0.003. It reduces full time attendance in a four-year school by 0.012 with a standard error of 0.003. These results imply most of the effect on college enrollment comes from a reduction in community college (two year institutions) attendance. This is not surprising given that students in the "last chance" sample tend to be low achieving and come from relatively disadvantaged backgrounds. As such, these students would likely be choosing along the no college – community college margin.[64]

It remains possible that this result is an artifact of only using the college attendance data for the first five years after the expected graduation date. Students who fail the TAAS and do not earn a conventional diploma must first receive a GED before being admitted and may therefore enter college later than do students graduating from high school. The discontinuities seen in Figure 9 may therefore simply reflect lower rates of starting college within five years rather than a true difference in post-secondary schooling.

It is true that among students ever attending college within five years, those who fail the "last chance" test tend to start somewhat later. However, first entering college in the fifth year after leaving high school is rare for both groups. Among students attending community college, 4.8 percent who fail the "last chance" exam enter in the fifth year as compared to 2.9 percent of

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[62] I am indebted to Jesse Rothstein for pointing this out to me.

[63] The estimates on post-secondary schooling and earnings use only students with valid SSN's. Appendix B presents evidence that the frequency of valid SSN's (along with other characteristics) does not jump at the passing score.

[64] Kane and Rouse (1995) describe of the characteristics of students attending two and four-year colleges.

passers. For those attending a four-year college, the rates are 11.5 percent and 7.5 percent for failers and passers, respectively. Since a large majority of both groups begin college before year five, censoring at this date is unlikely to drive the result.

Another sensitivity test is to use only the earlier cohorts and widen the observation window. In Table 7b, I limit the sample to the 1991-1993 cohorts. The first four columns use the same variables used in Table 7a while columns 5 through 8 consider college attendance up to seven years after the expected end of high school. Comparing the results in columns 1 to those in column 5, expanding the time interval to seven years leads to a decrease in two of the college attendance estimates and an increase in the other two. The estimated discontinuity for attending a two or four-year college falls after from 0.057 to 0.050 and that for attending a four-year college full time goes from 0.014 to 0.010. For attending a two or four-year college full time, the point estimate increases from 0.046 to 0.050 and for ever attending a four-year college it grows from 0.009 to 0.010. The small size of these changes and the fact that they are not in a uniform direction points to the relative unimportance of differences in college start-times between exit exam passers and failers.

Turning to the results on post-secondary school attainment, the upper-left panel of Figure 10 shows no evidence that failing the exit exam affects the likelihood of earning a post-secondary degree. The point estimate of -0.002 in the first column of Table 8 has the "wrong" sign but is not statistically significant (the standard error is 0.002). This variable only captures degrees earned within the first four years after leaving high school since the THECB data includes information only on degrees awarded through 2001, or four years after the expected graduation year for the youngest (1995) cohort. But even after expanding the window to six years and using only the 1991-1993 cohorts, the estimated discontinuity (not reported here) is -.005 with a standard error of 0.005.[65] The exit exam apparently affects the college attendance of students who would be unlikely to complete the degree requirements.

The remaining panels of Figure 10 do show that failing the exit exam reduces the average number of college credits in which a student enrolls. The estimated discontinuity for two-year college credits is 1.225 with a standard error of 0.472 and for four-year college credits it is 1.104 with a standard error of 0.228. Considering the maximum college credits enrolled in, be it from a two or four-year college, yields an estimated discontinuity of 2.289 with a standard error of 0.424.

[65] 28.5 percent of students in the 1991-1993 cohorts ever attending college were still enrolled in the sixth year after their expected end of high school.

These estimates are small; thirty credit hours correspond approximately to one year in college.[66] It must be remembered, however, that failing the exit exam affects a limited number of students and that the effect on college credits for students whose college attendance is affected by failing the exit exam is much larger. A simple way to estimate the impact on college credits for these students is to divide the RD estimate for college credits by the RD estimate for college attendance.[67] Dividing the estimate for maximum credits by the estimate for ever attending a two or four-year college suggests students induced not to attend college by failing the exit exam would have enrolled in 2.288/.057=40.1 credits, or about 1.3 years, had they instead passed.

It should be borne in mind that these estimates pertain only to enrollment and degrees from Texas public colleges and universities. Since some students in the sample undoubtedly attend college outside of Texas or at a private college in Texas, the estimates of the fraction of students attending college or receiving a degree will be biased. However, the estimates of the test-failing impacts will be biased only to the extent that passers and failers have different propensities of attending private college or going to school out-of-state. While this may be the case for the average passer and failer, mobility and private college enrollment is likely to be similar for students just on either side of the passing cutoff so the regression-discontinuity estimates are unlikely to be affected by this issue.

7.2 Exit Exam Performance and Earnings

Reduced Form Impact of Failing on Earnigns

To estimate the earnings impacts, I focus on wages received during the three periods described in Section 4. In the interest of brevity, I refer to the first four quarters after leaving school as Period 1, years four and five after the end of high school as Period 2, and years two through five as Period 3. For each window, two estimation samples are used. One uses individuals with positive earnings in at least one quarter while the second requires positive earnings in all quarters in the period.

As noted in Section 4, the reason why someone has zero earnings in administrative earnings data typically cannot be determined. This can be a problem when sample restrictions are made, as they are here, on the basis of having positive earnings. If selection into the sample is

[66] A typical associate's degree program offered at a two-year college requires 60 credit hours while 120 are necessary for a bachelor's degree.

[67] This is analogous to using passing status (conditional on the test score) as an instrument for college attendance in a regression of credit hours on ever attending college. The effect of failing on credit hours is like the reduced form and the impact on college attendance is like the first stage effect.

endogenous to passing the exit exam, the continuity-of-confounders assumption that underlies the RD design could be violated.[68]

I address this concern by seeing if the probability of selection into each sample used to estimate the earnings impacts is discontinuous at the passing cutoff. Figure 11 shows no striking jump in the fraction with any positive earnings for all of the periods considered. The estimated discontinuities (reported in Table 9), however, are uniformly negative, and that for Period 2 is statistically significant (-0.013 with a standard error of 0.005). The lower-right panel indicates that most students show up in the TWC files at least once; for almost all test scores, the fraction with any positive earnings within the five years after leaving high school is over 90 percent.

Turning to the samples that require positive earnings in all quarters, the upper-right and lower-left panels of Figure 12 reveal little evidence of a discontinuity in the percent with positive earnings during all of Period 2 or Period 3. In contrast, students barely passing the "last chance" test are somewhat less likely to have positive earnings throughout all of Period 1. The estimated discontinuity reported in the second panel of Table 9 is -0.024 with a standard error of 0.009. This is not surprising since passing the exit exam increases the probability of going to college. Anyone in college at the last point in which they can be observed, five years after leaving high school, cannot be used in the analysis based on Period 1.[69]

While differential rates of selection for students just above and below the passing cutoff do not appear to be a major concern, all of the point estimates suggest students who pass are somewhat less likely to be in the earnings sample, particularly for Period 1. Again, this makes sense considering that students passing the exit exam are more likely to go to college and might not be fulltime members of the labor force during the period in which they are observed. Caution should thus be taken when interpreting the estimated earnings impacts. In particular, it will be important to see how the estimates change when controlling for baseline characteristics given the possible violation of the usual RD assumptions.

Turning to the results, the analysis begins with the samples that only require one quarter with positive earnings in the relevant window. Figure 13 shows that average log earnings do not jump up at the passing cutoff for any of the periods (the dependent variable is the log of total earnings received in the period). The corresponding discontinuity estimates in the first column in Table 10 (panel a) are all small and statistically insignificant. In particular, no effect is seen even

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[68] A possible scenario in which this could be true would be if individuals in college were more likely to have no earnings. Failing the exit exam reduces the likelihood of attending college and could thus increase the probability of being selected into the sample used to estimate the earnings impacts.

[69] 2002 is the last year the TWC data is available. The "on time" graduation year for the 1995 cohort is 1997.

when considering earnings in the year after permanently leaving school (Period 1) suggesting that differential college attendance rates do not account for the absence of an effect. Adding the baseline covariates in column 2 hardly changes the point estimates suggesting that selection into the samples, at least based on pre-determined characteristics, is not driving the results. The regressions in column 3 control for prior work experience by including the number of quarters before the start of the observation window with positive earnings.[70] The estimates remain statistically insignificant and retain the same sign. The estimated discontinuities for Periods 2 and 3 increase from 0.004 to 0.018 and from -0.018 to -0.005, respectively, while it falls for Period 1 from 0.015 to less than 0.001.

Controlling for experience may be inappropriate since it may itself be endogenous to passing the exit exam. Students passing the exam may have less work experience before Periods 2 and 3 since they are more likely to be in college at that point. This relative inexperience would depress the earnings of passers and conditioning on experience would be expected to increase the estimated effect of passing on earnings. The same reasoning implies that passers ought to have higher levels of experience before Period 1 since students going to college can accumulate work experience in the time between high school and leaving college.[71] For Period 1, passers would thus tend to be relatively experienced and controlling for quarters worked should reduce the estimated effects. The results just discussed are consistent with the predicted effect of adding experience to the regressions (although the change in the point estimates is modest).

Now consider the samples that exclude observations with zero observed earnings in any quarter during the relevant period. For these students, Figure 14 presents evidence that failing the "last chance" test does lower earnings. Average log earnings jump up at the passing score for all three periods considered. The estimated discontinuities in column 1 of Table 10 (panel b) range from 0.036 with a standard error of 0.008 for Period 1 to 0.040 with a standard error of 0.012 for Period 3. Controlling for baseline covariates in column 2 reduces the estimated effects but by a small amount – the largest change is for Period 3 in which the estimate falls from 0.040 to 0.035 – and they all remain statistically significant. This is reassuring since it implies the fact that these samples are highly selected (the sample for Period 3 uses just over a quarter of all available observations) may not create a serious violation of the identifying RD assumption. As anticipated, including experience (column 3) reduces the estimate for Period 1 and increases them for Periods 2 and 3. The changes are again small: the estimated discontinuity for Period 1 falls from 0.035 to

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[70] Experience squared is also included.

[71] Only quarters with positive earnings beginning in the year of the "last chance" test contribute to the experience measure.

0.030 while they go up from 0.035 to 0.038 and from 0.035 to 0.041 for Periods 2 and 3, respectively.

There is reason to believe that the observed earnings of individuals in the more restrictive sample better reflect current and future economic wellbeing. The earnings observed in administrative data understate true earnings if someone works at a job uncovered by the Texas UI system. The extent of this bias is likely smaller among individuals with positive covered earnings in consecutive quarters (this is the reasoning behind Jacobson, LaLonde, and Sullivan's requirement that displaced workers have covered earnings in each year in their sample). Similarly, observing a stretch of uninterrupted earnings is suggestive that the worker has completed the transition from school to work and is relatively established in the labor market. The income observed for a young person fully engaged in the labor market could be a better predictor of future economic welfare than it would for someone working sporadically.

If this last claim is true, the earnings observed for someone with a continuous stretch of positive earnings may provide a more useful forecast of future labor market performance than would those of someone with intermittent periods of zero (observed) earnings. To test this proposition, I compare the correlation between someone's earnings in two different years by whether or not the individual had positive earnings in each quarter of the earlier year. Table 11 reports the correlation between average quarterly earnings in the seventh year after the expected end of high school and in earlier years separately by having positive earnings throughout the entire early year. [72] The results show considerably stronger correlations among individuals with positive earnings in all four quarters of the early year. For instance, the correlation between average earnings in the second and seventh years after "on time graduation" is 0.482 for those with earnings in all four quarters in the second year but only 0.313 among the individuals with at least one quarter with zero earnings. This evidence suggests the estimates obtained from the samples that include individuals only if their earnings histories contain no gaps may more accurately reflect the true impact of failing the exit exam on labor market outcomes.[73]

Effect on Earnings Over Time

[72] The averages are calculated using only quarters with positive earnings (i.e. the denominator will be equal to the number of quarters with positive earnings in a given year). Only the 1991-1993 cohorts are used in this analysis because the 7th year after "on time" graduation for subsequent cohorts is after the final year of earnings data (2002).

[73] Betts and Grogger (2000) make a similar argument when investigating the impact of higher grading standards on early-adulthood earnings in the High School and Beyond survey. They restrict attention to earnings received in years in which the respondent worked full-time in at least nine months.

The results just reported refer to the effect of failing on earnings at a single point in time. However, the literature on labor market signaling stresses the dynamic nature of the return to a credential. Altonji and Pierret (1998) argue that the pure credential effect of education declines as employers learn about a worker's underlying productivity and the signal becomes redundant. On the other hand Tyler (2000) finds that the effect of getting a GED increases in the five years following the receipt of the credential. He notes this finding is consistent with employers assigning credentialed workers to jobs involving a period of on-the-job training so that the estimated GED effects arise from the resulting acquisition of human capital.[74]

To examine the evolution of the test-failing impact on earnings, I estimate the effect of failing on log earnings in each year after the end of high school. For each year, the sample is limited to individuals with positive earnings in each quarter. Only the 1991-1994 cohorts are used in order to observe earnings up to six years after the expected completion of high school. Figure 15 reveals that the estimated discontinuities in years 1 and 2 are small and statistically insignificant. In years 3 and 4, individuals who barely failed receive earnings that are on average about 3.4 log points below those of barely passers. By year 6, however, the estimated premium associated with passing essentially disappears. Table 12 reports regression-adjusted discontinuity estimates. Adding baseline controls in column 2 increases the estimated effects except in year 5 (where it falls from 0.023 to 0.019) although the changes are small. Column 3 controls for experience (measured by the number of prior quarters with positive earnings) and the estimated effects increase somewhat, but again, not dramatically. In all specifications, only the estimated effect in years 3 and 4 are statistically distinguishable from zero.

Higher rates of college attendance among passers help explain why the estimated earnings impacts are small in the years immediately after high school. As noted below, college attendance is associated with lower earnings in the years following high school and this may offset any earnings boost enjoyed by passers due to signaling. Figure 16 repeats the analysis but excludes individuals who attend college within 6 years of the end of high school. For each year considered, the estimated discontinuities go up, in some cases considerably. In particular, the estimates for years 1 and 2 increase to 0.038 and 0.053, respectively. The time profile of the estimates mirrors that of Figure 15. The effect increases at first but by year 6, the estimated effect is only 0.018 and is not statistically significant. It is not surprising that these estimated discontinuities, reported in columns

[74] Heisz and Oreopoulos (2002) find that the return from graduating from a better law school increases over time but it does not increase when conditioning on initial firm quality. They interpret this as evidence that lawyers with better expected ability are sorted into firms that engage in more training, but that conditional on the initial firm assignment, the value of the signal (quality of law school) does not increase over time.

4-6 of Table 12, are more sensitive to the inclusion of covariates than are those in the less selected samples that include college attendees. For instance, the estimate for year 3 falls from 0.068 in column 4 to 0.060 when controlling for baseline covariates in column 5 and increases to 0.072 after including experience in the regression. The qualitative shape of the time profile is the same in all specifications, however.

The finding that the earnings effect eventually dissipates is consistent with the Altonji-Pierret hypothesis that employer learning reduces the signaling value of education. But their framework also predicts that the pure credential effect decreases monotonically. In contrast, the estimated test failing impact initially grows. A potential explanation is that it takes time for some high school graduates to acquire the labor market savvy necessary to take advantage of their credential. Alternatively, individuals unsure of their future plans may settle temporarily for relatively low-paying jobs while deciding upon things like where to live or whether or not to get married or attend college. Only after starting the first "real" job would the diploma premium materialize. A second possibility is that the upward slope is explained by sorting of credentialed jobseekers into positions that involve some training aspect whereby productivity increases. The results indicate, however, that the return to any such activity is fleeting.

Effect on Earnings via Reduced Post-Secondary Schooling

The preceding estimates likely do not capture the effect of failing on earnings arising from changes in post-secondary schooling. Figure 17 demonstrates the premium for attending college (using the attend a two or four-year college definition from Table 7a) is negative until the sixth year after the expected final year of high school.[75] Since the estimates in Table 10 only use earnings through year 5, they likely understate the average effect of failing on an individual's eventual economic prospects.

The findings of Kane and Rouse (1995) can be used to gauge the earnings impact of failing the exit exam arising from reduced college attendance. The estimates in the preceding section suggest failing the "last chance" test reduces post-secondary school attainment by about 2.3 semester credit hours for the average student and by roughly 40 credit hours among students whose college attendance is affected by failing. Using the Kane and Rouse estimate that the return per 30 credit hours is between 4 and 6 percent, the implied test-failing effect on earnings is approximately 0.3-0.5 percent overall and 5.3-8.0 percent for the set of students induced not to

[75] This figure uses only the 1991-1993 cohorts so that earnings could be observed up to seven years after "on time" graduation.

attend college.[76] With earnings histories that extended farther into an individual's working life, future research will be able to estimate this effect directly.

It is important to note that these effects are not likely to be temporary. This is because differences between exit exam passers and failers in attainment of post-secondary schooling presumably yield permanent differences in human capital levels. The literature on the long run impact of education on earnings suggests that this human capital differential enhances productivity and is rewarded in the labor market (Card, 2001). In contrast, the evidence presented here indicates that any effect of failing that operates via labor market signaling does not last.

8 Conclusion

Despite the growing use of graduation tests, their effects on student outcomes remain uncertain. This paper uses a new data source, consisting of longitudinal records of more than 500,000 Texas students, together with a regression discontinuity econometric approach to assess the short and longer-term effect of exit exam performance. I find that that just over 1 percent of Texas students do not graduate due to an inability to pass the test. As expected, this rate is much higher rates for nonwhite, economically disadvantaged, and low-achieving students. At the same time, the results do not support the view that exit exam testing will increase early dropout rates. In this sense, the exams may be operating as intended. Students who initially fail are not driven to dropout before the end of high school and most eventually go on to pass the exam. Students unable to demonstrate the level of achievement required by policymakers do not earn a diploma.

Nonetheless, the evidence strongly suggests that being denied a diploma matters. Failing the exit exam reduces the amount of post-secondary school attainment (although it does not affect the likelihood of earning a post-secondary degree). Among the individuals whose string of uninterrupted labor market activity suggests permanent membership in the workforce, failing the test also lowers observed earnings.

Since exit exam performance affects the likelihood of earning a high school degree but not the amount of completed high school, this effect suggests educational credentials serve as a valued signal in the labor market. The estimated earnings impact falls over time, suggesting that the value of the credential falls as labor market participants learn about the true abilities of those who barely failed and barely passed the exam.

The usual caveat that the results only pertain to the region and time period actually studied applies here. The initial failure rates are rather high in this paper's sample. Clearly, the percentage

[76] This figure may be overstated because the estimate of the return to time spent in college obtained by Kane and Rouse refers to completed semester credit hours while the THECB data refers instead to semester credit hours enrolled in and not necessarily completed.

negatively affected by an exit exam would be lower with a test students find easier to pass. In recent years, in fact, the widely publicized improvement in passing rates could mean that fewer Texas students have been denied a diploma due to failing the exit exam than what is reported here.[77] Another important aspect of the specific policy adopted in Texas is the provision of multiple chances to retake the test. This feature may explain the absence of a discouragement effect and it also helps limit the number of students who do not graduate because of failing. The findings of this paper suggest that designing an exit exam policy with numerous chances to retry the test is one way to mitigate the exam's effects testing opponents find worrisome.[78]

Although the results here help to quantify the costs of an exit exam policy, future research is needed to measure the benefits. In particular, testing supporters argue that exit exams increase the relative value of high school education. Given the growing use of exit exams, the significance of this question will only grow.

[77] The TEA reports that TAAS passing rates among 10th graders grew from 50 percent in 1994 to 85 percent in 2002.

[78] Tyler, Murnane, and Willett (2004) find that many GED recipients initially failed the GED exam and argue that is evidence that the option to retake an exit exam would be an important determinant of the number of students who eventually pass it. The evidence reported here corroborates that view.

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Appendix A – Proof $\theta_{ti}^{complete} > 0$ if and only if $\delta_j > 0$ for some $t \leq j < T$

As noted in the text, $\theta_{ti}^{complete} = (1-d)^{T-t} - \prod_{k=t}^{T-1} P(D_{ki} = 0 \mid Y_{ti} = -\varepsilon, A_{ki} = 1)$ and if $\delta_j = 0$ for all $t \leq j < T$, then $\theta_{ti}^{complete} = 0$. To prove that the discouragement effect implies $\theta_{ti}^{complete} > 0$ it suffices to show that $P(D_k = 0 \mid Y_{ti} = -\varepsilon, A_{ki} = 1) \leq (1-d)$ for all $t \leq k < T$ and that if $\delta_j > 0$ for some $t \leq j < T$, then this inequality is strict for at least one period.

Begin by observing that the $\phi_{ki} \equiv P(D_k = 0 \mid Y_{ti} = -\varepsilon, A_{ki} = 1)$ can be re-expressed as $\phi_{ki} = \widetilde{P}(D_k = 0 \mid \max_{l<k}(Y_{li}) \geq 0)\widetilde{P}(\max_{l<k}(Y_{li}) \geq 0) + \widetilde{P}(D_k = 0 \mid \max_{l<k}(Y_{li}) < 0)\widetilde{P}(\max_{l<k}(Y_{li}) < 0)$ where $\widetilde{P}(\cdot)$ represents probability conditional on $Y_{ti} = -\varepsilon$ and $A_{ki} = 1$. The assumptions about the test process and the probability of dropping out imply that:

$$\begin{aligned}
\phi_{ki} &= (1-d) + \widetilde{P}(\max_{l<k}(Y_{li}) < 0)\left(\widetilde{P}(D_k = 0 \mid \max_{l<k}(Y_{li}) < 0) - (1-d)\right) \\
&= (1-d) + \widetilde{P}(\max_{l<k}(Y_{li}) < 0)\begin{pmatrix} (1-d)\widetilde{P}(Y_{ki} \geq 0 \mid \max_{l<k}(Y_{li}) < 0) + \\ (1-d-\delta_k)\widetilde{P}(Y_{ki} < 0 \mid \max_{l<k}(Y_{li}) < 0) - (1-d) \end{pmatrix} \\
&= (1-d) - \delta_k \widetilde{P}(\max_{l<k}(Y_{li}) < 0)(1 - p_{ki}) \\
&\leq (1-d).
\end{aligned}$$

Now assume that $\delta_j > 0$ for some $t \leq j < T$. If j=t, then $\phi_{ti} = 1 - d - \delta_t < 1 - d$. Now suppose j>t. Without loss of generality, assume $\delta_l = 0$ for l<j so that period j is the first period with a nonzero discouragement effect. This implies that remaining in school until period j confers no information about a student's test performance so $\widetilde{P}(\max_{l<j}(Y_{ji}) < 0) = \prod_{l=t+1}^{j-1}(1 - p_{li}) > 0$ since $0 < p_{li} < 1$ by assumption. Thus $\phi_{ji} = (1-d) - \delta_j \prod_{l=t+1}^{j}(1 - p_{li}) < (1-d)$.

Appendix B – Dataset Construction

B.1 Test History Panel

I first create a dataset of the first TAAS record for each student who took the test anytime between 1991 and 1999.[79] I identify multiple records from the same student by grouping all records with the same encrypted social security number (SSN) and sex. [80] To assess the validity of these matches, I calculated the proportion of SSN-sex groups with a common birth date and ethnicity; 98.9 percent of groups with more than one record had a common ethnicity and 98.5 percent had a common birth date. As explained in the main text, the estimation sample uses students who first took the TAAS between the fall of 1991 and the spring of 1995. In this group there are 1,043,977 records indicating the student took at least one section of the TAAS.

Next I create each student's test-taking history. For students who did not pass all three sections of the exam on the first try, I sequentially merge (on encrypted SSN and sex) the records from subsequent tests taken up to four years after the initial attempt. For each match indicating a student

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[79] More precisely, I select the first record for each student with a valid score. That is, if two records for the same student are found but only the second has valid scores, I keep the second record. If none of the student's records have valid scores, I simply keep the first one.

[80] The TEA assigns an alternative identification number in lieu of the SSN to students who refuse to give, or who do not have, a SSN.

retested during a certain administration, the scores and exemption status are recorded. After linking the retest information to the initial test record, the student's ultimate passing and exemption status can be determined, as can whether or not a student took the test again after failing a given retest attempt.

B.2 Test – Attendance Crosswalk

Creating outcomes involves matching the TAAS records to records in other TSMP files on the basis of the two available student identifiers – the encrypted SSN and the encrypted TEA student ID number. The test records only have the encrypted SSN while some of the other TEA files only have encrypted TEA student ID. The TEA's attendance files have both identifiers, and thus serve as a crosswalk between the test and non-test TEA data.

I make this crosswalk separately for cohorts initially taking the test in 11th grade (1991 and 1992) and then for those taking it in 10th grade (1993-1995). A parallel procedure is followed for each set of cohorts so only that for the 1993-1995 students is described here. The first step takes all 10th grade records in the attendance files and selects, for each student, the first record. As with the test data, groups of records with the same encrypted SSN and sex identifies students with multiple 10th grade records.[81] The frequency with which the birth date and ethnicity of matched records agree is slightly higher than the rates for the test records.

I then merge the TAAS and 10th grade files by encrypted SSN and sex. I find matches in the 10th grade attendance data for about 87.1 percent of the observations.[82] Validating the reliability of the matches entails comparing the ethnicity and birth date of the two records. Specifically, "good" matches are those in which two of the day, month, and year of the birth dates agree or one of the day, month, and year of the birth dates agree and the ethnicities agree. Over 99.7 of the matches satisfy this condition.

A test record may be unmatched if the student never attended 10th grade in a Texas public school or if the student ID contained an error in the 10th grade attendance data. I therefore look for a matching attendance record from some other grade for the unmatched test records. I start by taking the unmatched records from some year and merging them to the set of high school attendance records from that year (excluding grade 10 attendance records). I successively repeat this procedure for test records that remain unmatched using attendance data from adjacent years.[83]

B.3 Sample Selection

Sometimes multiple test records are matched to observations in the attendance files with the same TEA student ID. After deleting 1,553 of these observations, 1,042,424 records remain in the sample. I then delete 9,597 students who faced the lower passing standard that existed before 1991.[84] After making these preliminary sample selection restrictions, the 97,864 test records (9.5 percent) that remain unmatched to the attendance data or have nonnumeric (and unusable) TEA student ID's are deleted.[85]

124,703 additional observations are dropped for a variety of reasons. 2,403 have valid scores for at least one section but a score code indicating that section was not taken. 5,261 have score codes for some section signifying that section had already been passed.[86] Because those at least partially exempt from the testing requirement are likely to respond to failing much differently from the non-exempt population, the 3,321 students exempt from at least one section are dropped. 2,393 are

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[81] The most common reason for multiple 10th grade records is grade retention.

[82] For the 1991 and 1992 cohorts, the match rate is only 81.7%.

[83] For year t, the order of the years of data I use to find matches for the 1993-1995 cohorts is t, t+1, t-1, $t+2$, t-2. For the 1991 and 1992 cohorts the order is t, t-1, t-2, $t+1$, t+2.

[84] A student in the 1991 test-taking cohort would face the 1990 passing standard if she were absent during the 1990 administration and first took the test in 1991. A students is classified as facing the 1990 standard if the test record lists the grade as 12, if they were enrolled in 11th grade or higher in an earlier year or if they were enrolled in 12th grade in 1991.

[85] 81 percent of these deletions arise from failing to match the test records to the attendance data. Unmatched records are more likely to be from African American or Hispanic students.

[86] This could happen if the earlier test record is not successfully matched to the record incorrectly identified as the one from the initial attempt.

discarded because the match to the attendance files does not meet the validation condition described in the preceding section. 34,613 observations have missing values for at least one variable used in the analysis. 1,591 students have the passing status from the test record disagreeing with the passing status implied by the raw test score.[87] Since the regression discontinuity approach relies on a comparison of those who barely fail and barely pass, these observations are dropped. Similarly, only observations where all unpassed sections are taken during a given attempt can be used (otherwise a student may appear to barely pass the exam when in fact the untaken section remains unpassed). I therefore delete the 16,882 students who always have a missing score for at least one unpassed section in all of their test attempts.

Lastly, I only use students who, when initially taking the exam, did so during the "main" administration and were enrolled in the appropriate grade for their respective cohort (the fall of 11th grade for the 1991 and 1992 students and the spring of 10th grade for the 1993-1995 cohorts).[88] 58,239 observations do not meet this condition. This restriction is made so that analyzing, for example, the "last chance" test involves examining the effect of failing for first-time 12th graders and not for students returning to retake the exam after leaving high school. It should be noted that I had originally not made this restriction and none of the paper's main results were appreciably different.

After making these deletions, 810,260 observations remain in the full 1991-1995 sample. The two estimation datasets are subsets of the full sample. The 1993-1995 sample includes 529,395 observations (although the estimates for any particular test attempt will only include the students taking all unpassed sections during that attempt). The "last chance" sample includes 42,957 observations.

B.4 Linking the Test and Other TSMP Records

To create the outcomes concerning high school graduation, GED acquisition, and high school attendance, I merge the test records to the appropriate data files with the encrypted TEA student ID. Most of the actual outcome variables are dummies for whether or not a matching record was found for a particular test record. Casual assessment of the reliability of these matches (based on the degree to which the sex, birth date, and ethnicity agreed) suggested that linking two records on the basis of the student ID did in fact reflect a "true" match in the overwhelming majority (over 99 percent) of cases.

Because the post-secondary schooling and quarterly earnings data do not include the TEA student ID, matches involving these data rely on the encrypted SSN.[89] For the college attendance outcomes, I only classify a student as having enrolled if the match between the test and the THECB records is validated on the basis of two out of three of the birth date (month and year only), ethnicity, and sex agree. The TWC files do not have any demographic information so validation of test-TWC matches cannot be done.

Appendix C – Testing the Continuity of Observable Characteristics in the "Last Chance" Sample

Because the analyzing the post-secondary schooling and earnings outcomes limits the sample to records with valid SSN's, I check to see if the rate of usable SSN's is similar just to the right and to the left of the passing cutoff. The upper-left panel of Appendix Figure 1 shows little evidence of a discontinuity in the fraction of students with a valid SSN. The estimated discontinuity (reported in Appendix Table 4) is 0.011 and with a standard error of 0.004 is statistically significant. Specifying the

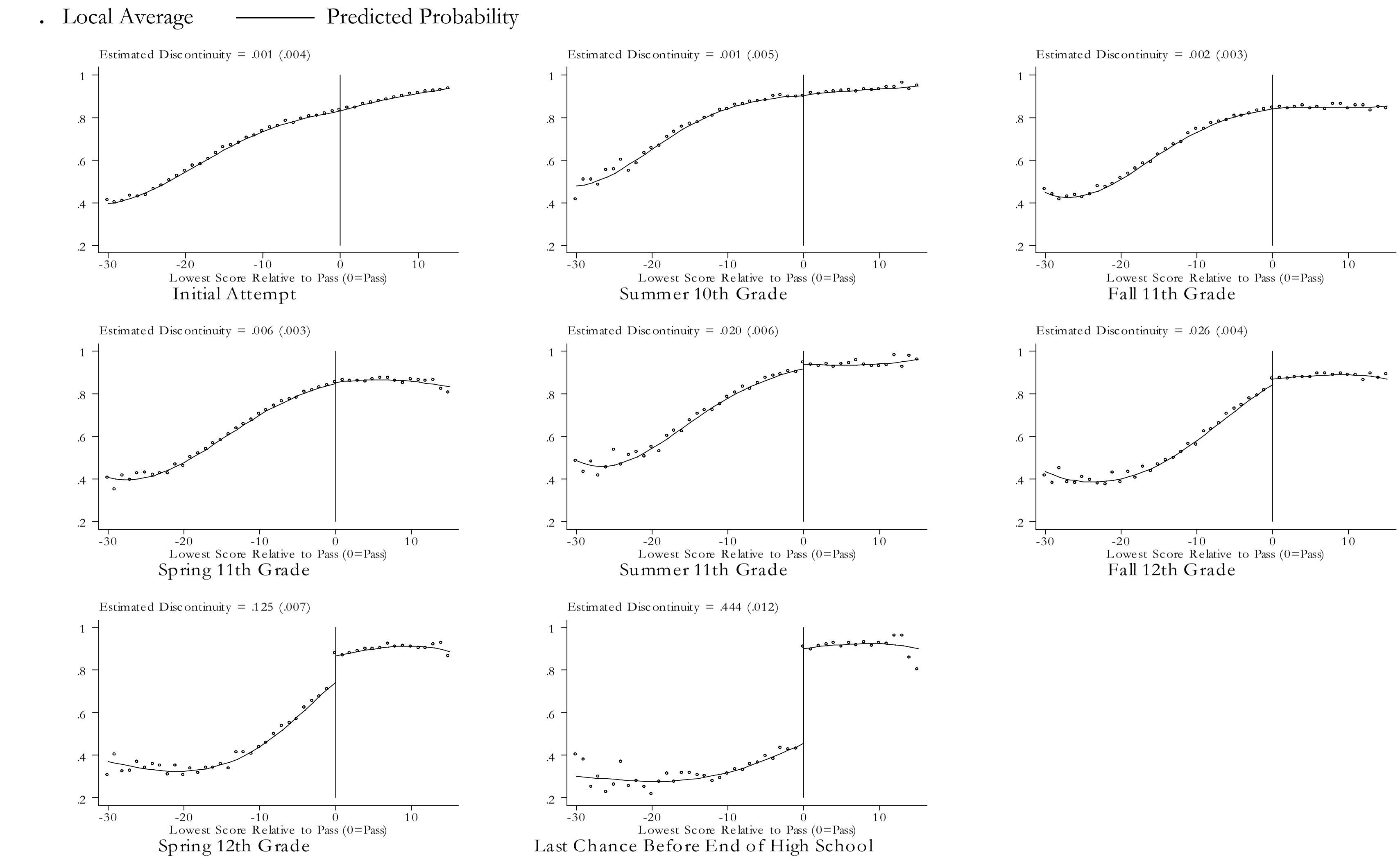
[87] All but 119 of these are from students whose combined writing multiple choice and essay score places them above the passing threshold (10*essay+multiple choice > passing cutoff) but who still fail the section due to an essay score of zero or one.

[88] The grade appears on the test and attendance records and only observations where the two agree are kept.

[89] Because the TEA assigns an alternative ID number to students whose SSN does not exist or is unknown, test records can be linked to other TEA files on the basis of the encrypted SSN even when that variable does not reflect a true SSN. In contrast, matching a test record to an observation in either the post-secondary schooling or earnings data can only be done if both records have an encrypted SSN generated from an actual SSN.

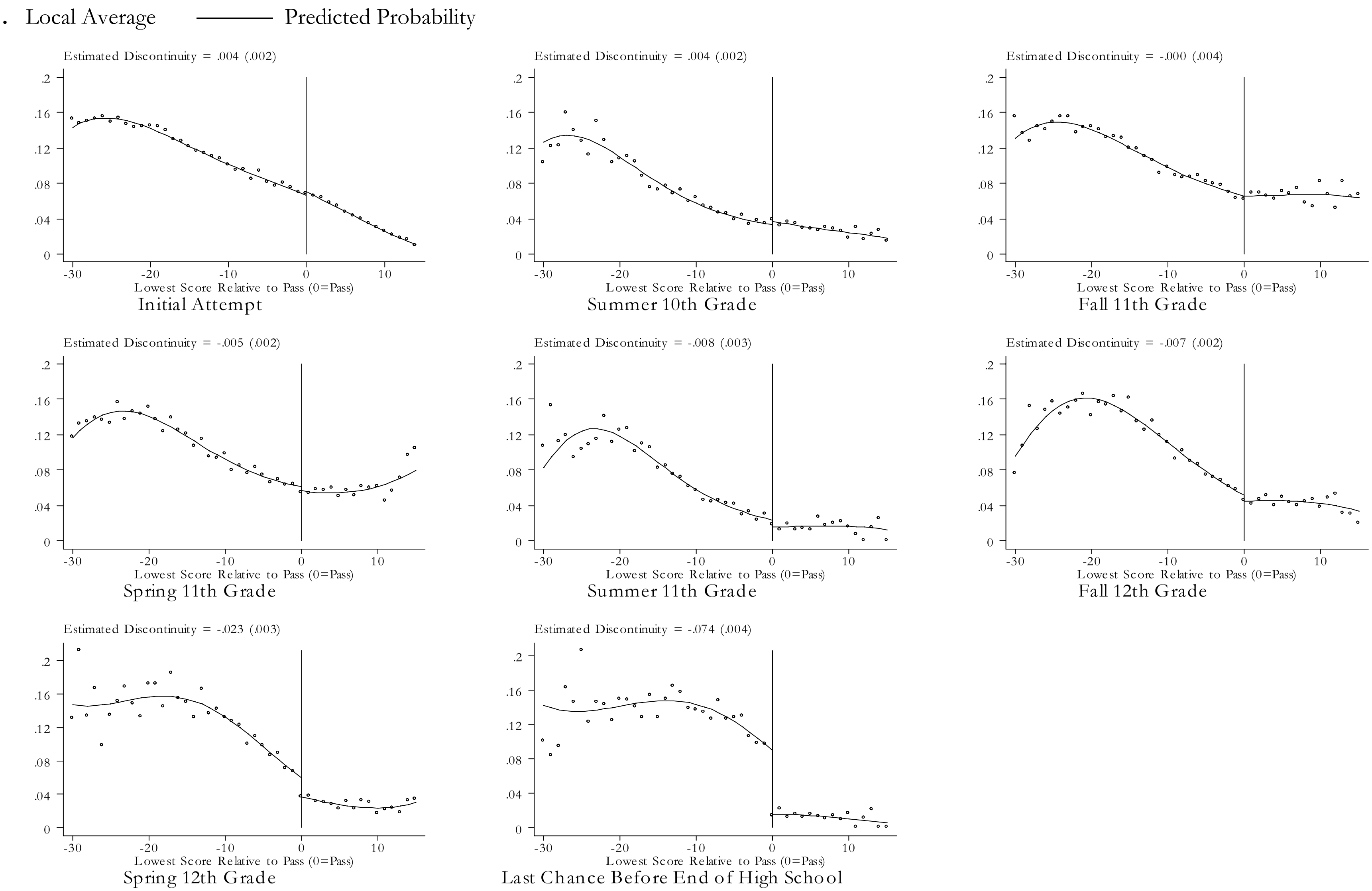
test score polynomial as a quadratic, however, yields a small and insignificant estimate of 0.006 (0.004). Similarly, no clear discontinuity is seen for any of the other three characteristics in the Figure (one section remaining to be passed, at risk of dropping out, and have not passed writing) although the point estimates are statistically significant. The estimated discontinuities of the remaining characteristics listed in Appendix Table 4 are small and generally statistically insignificant. Overall, these results suggest that restricting the sample to students with valid SSN's poses little danger of biasing the RD estimates and support the assumption that confounding factors behave smoothly at the passing cutoff.

Figure 1: Graduation Rates by Test Score and Administration



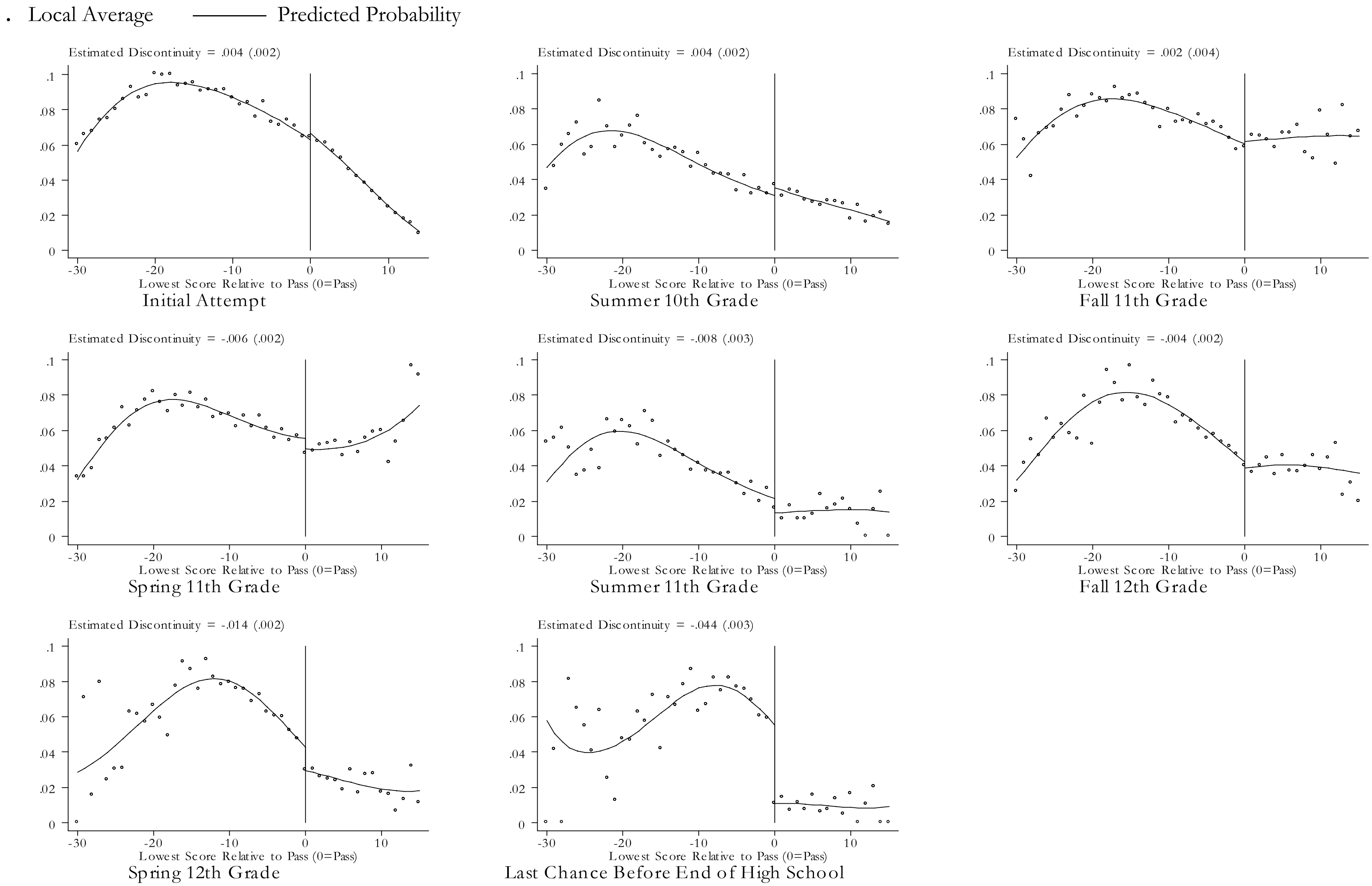
Notes: For each test administration, students not retaking all unpassed sections and whose reported grade level is higher than that implied by normal grade progression excluded. See Table 3 for sample sizes. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The predicted probabilities are from a probit regression of not graduating on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in the test score. The end of high school refers to the spring in which a student takes the "last chance" test.

Figure 2a: Rates of Taking the GED Exam by Test Score and Administration



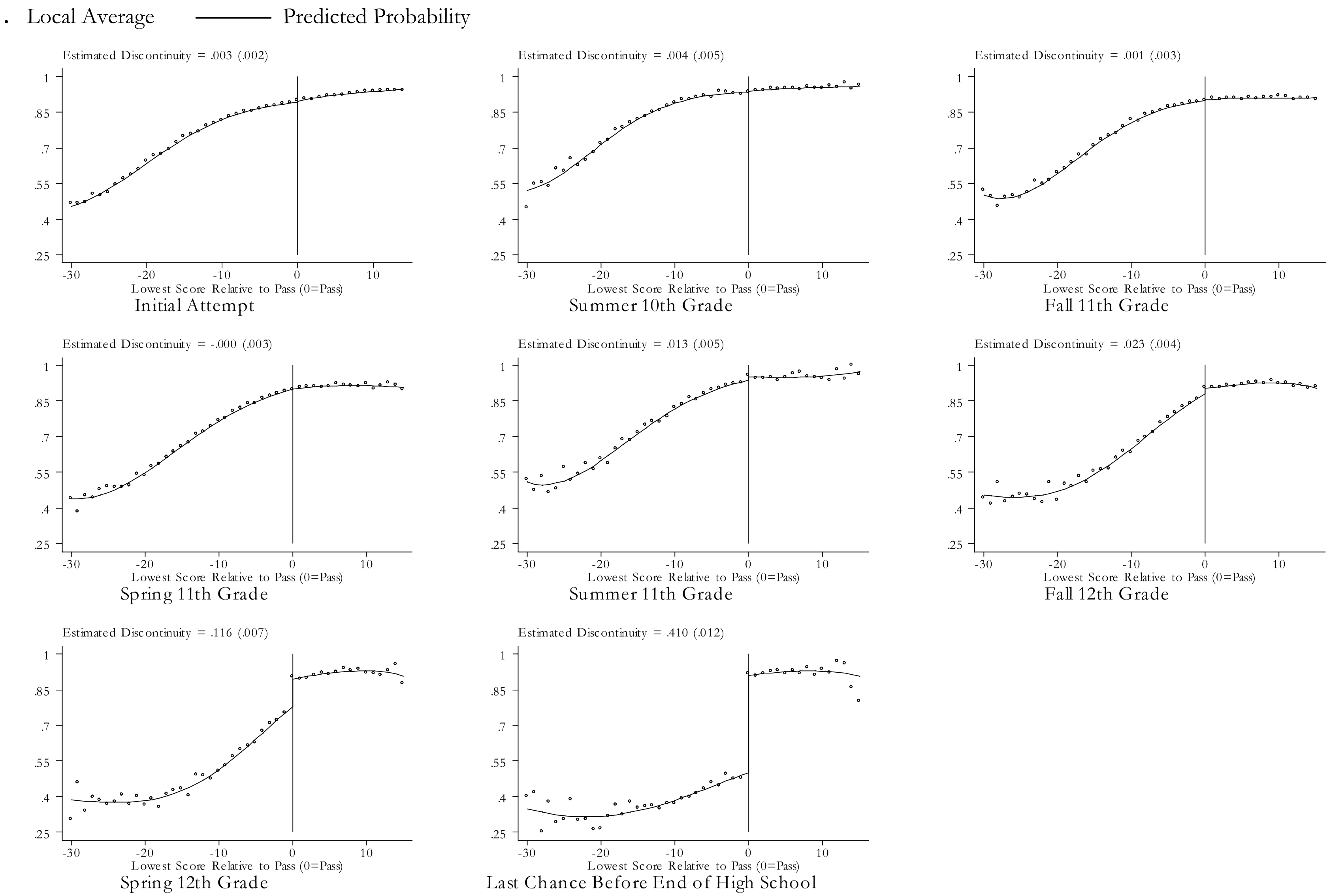
Notes: See notes to Figure 1.

Figure 2b: Rates of Receiving GED Certificate by Test Score and Administration



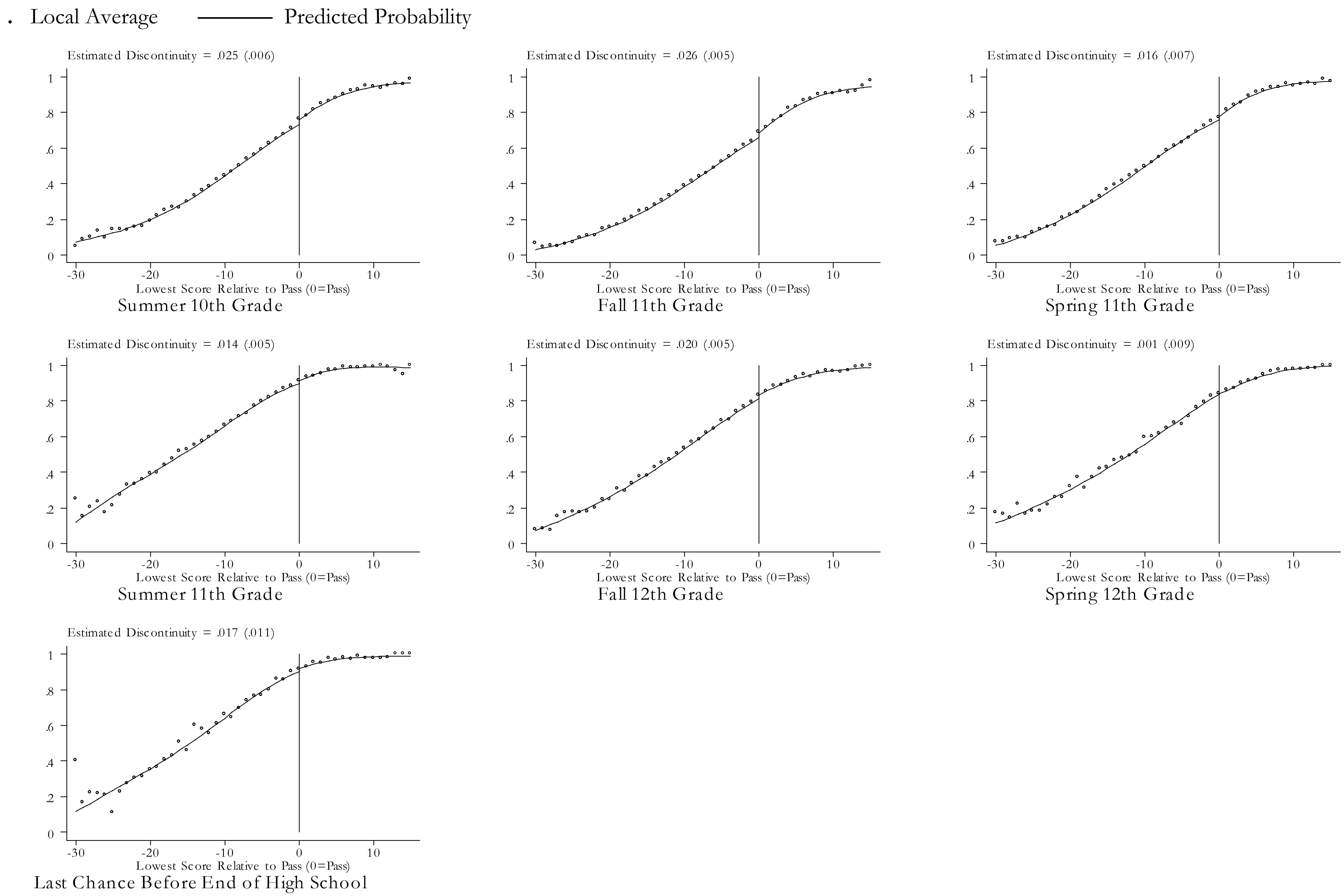
Notes: See notes to Figure 1.

Figure 3: Fraction Graduating or Receiving a GED by Test Score and Administration



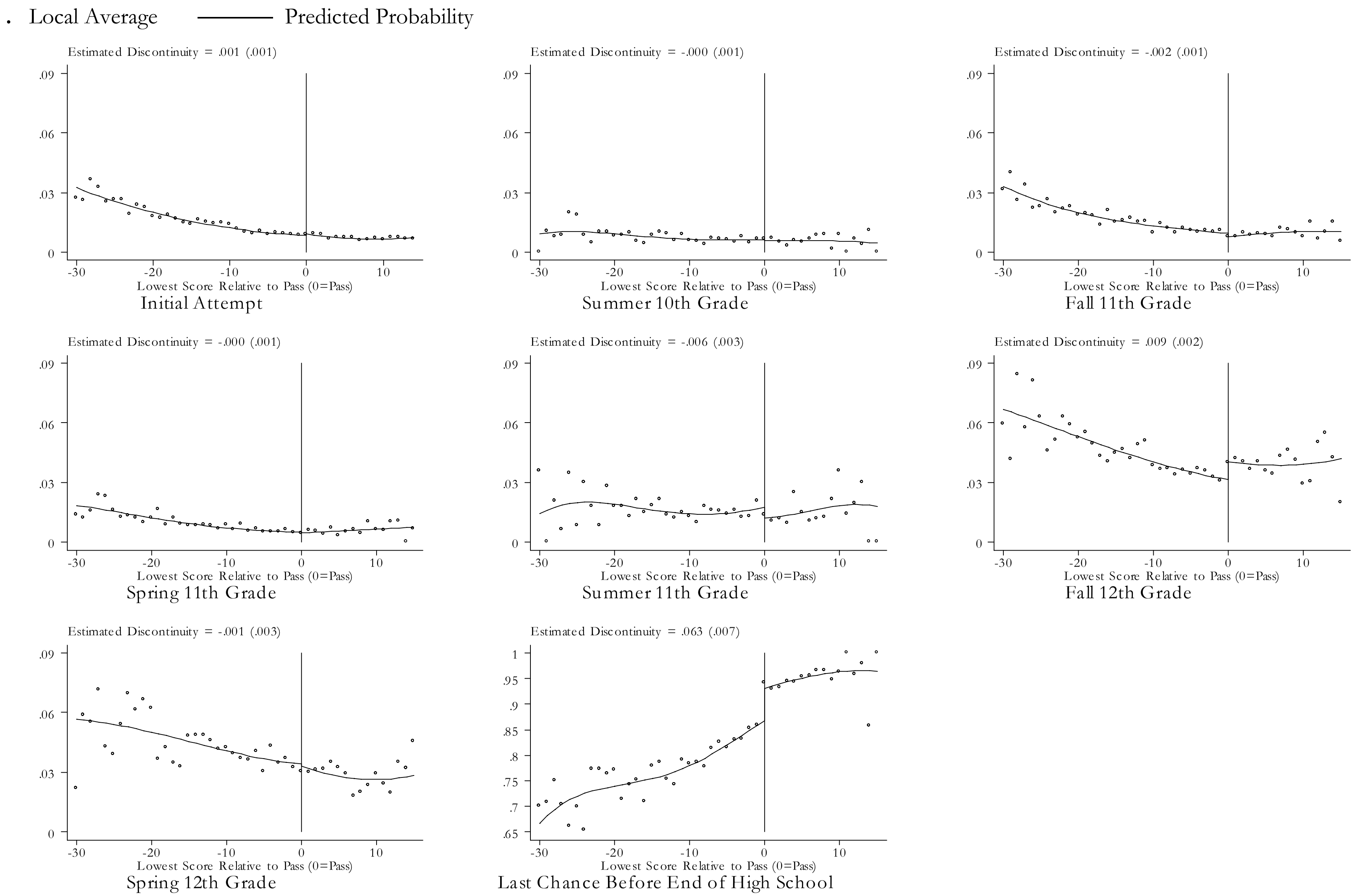
Notes: See notes to Figure 1.

Figure 4: Fraction with Only One Section that Remains Unpassed by Test Score and Administration



Notes: See notes to Figure 1.

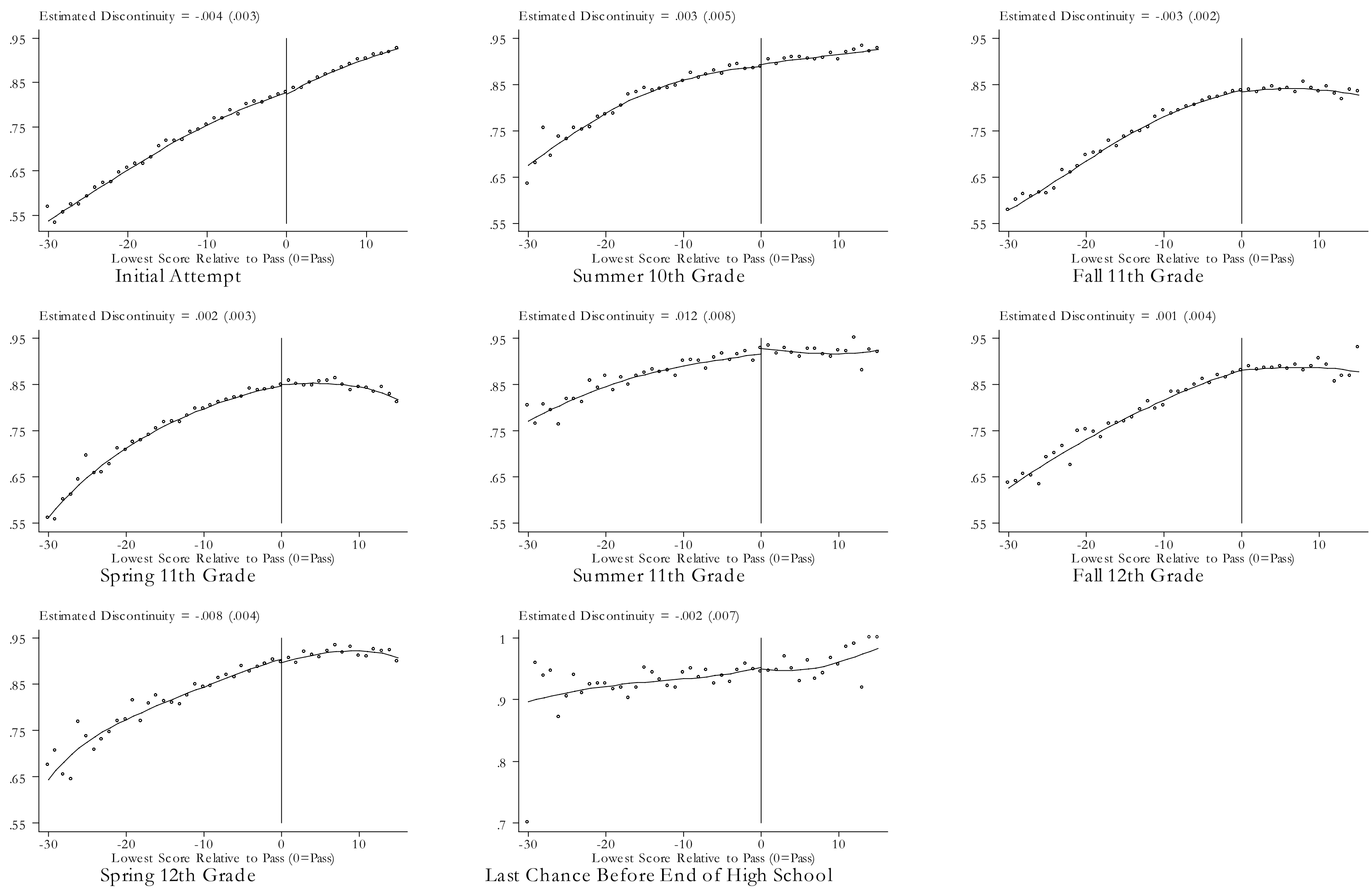
Figure 5: Fraction Never Again Attending School After Exam by Test Score and Administration



Notes: See notes to Figure 1.

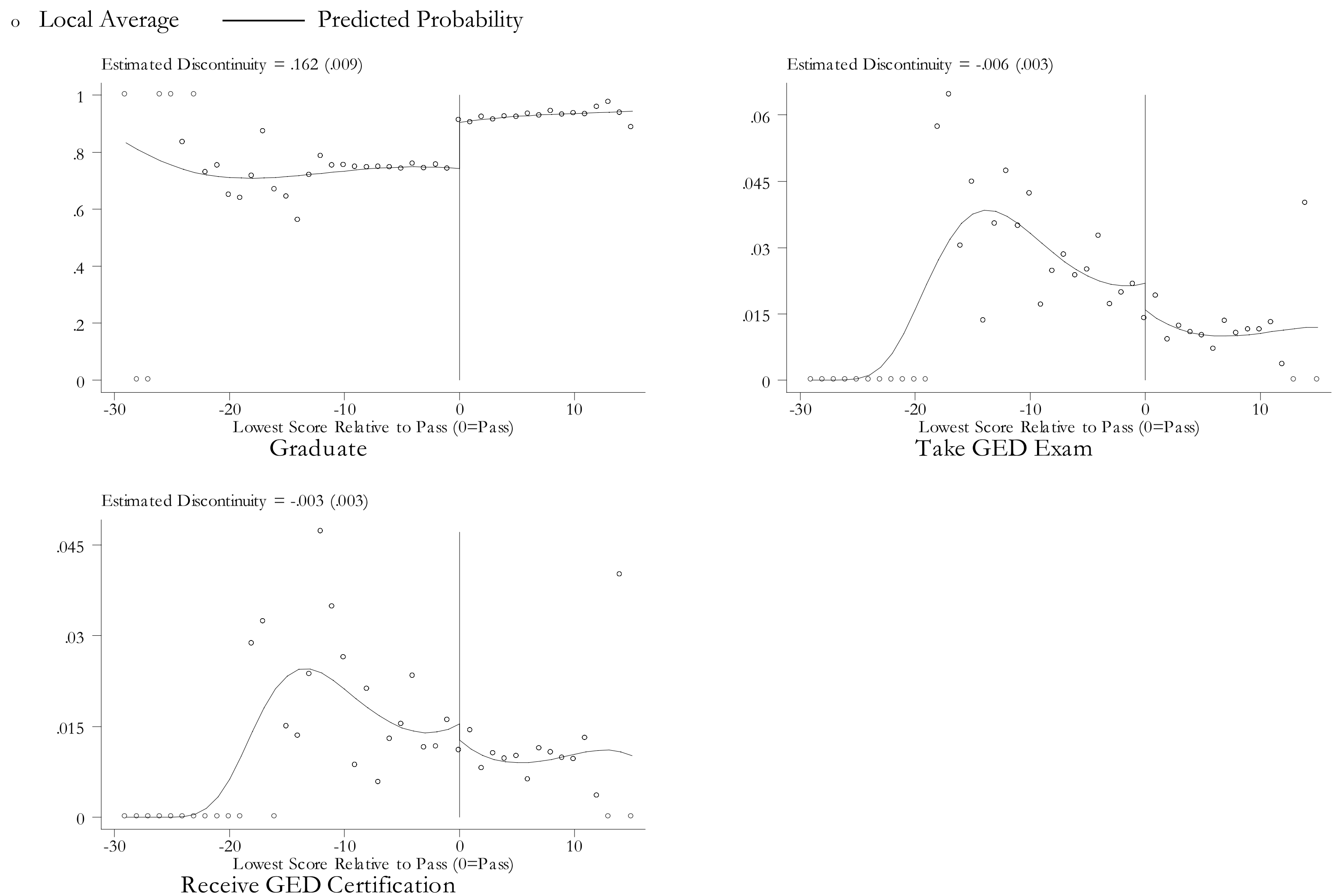
Figure 6: Fraction Attending the Final Six-Week Period of 12[th] Grade by Test Score and Administration

. Local Average ——— Predicted Probability



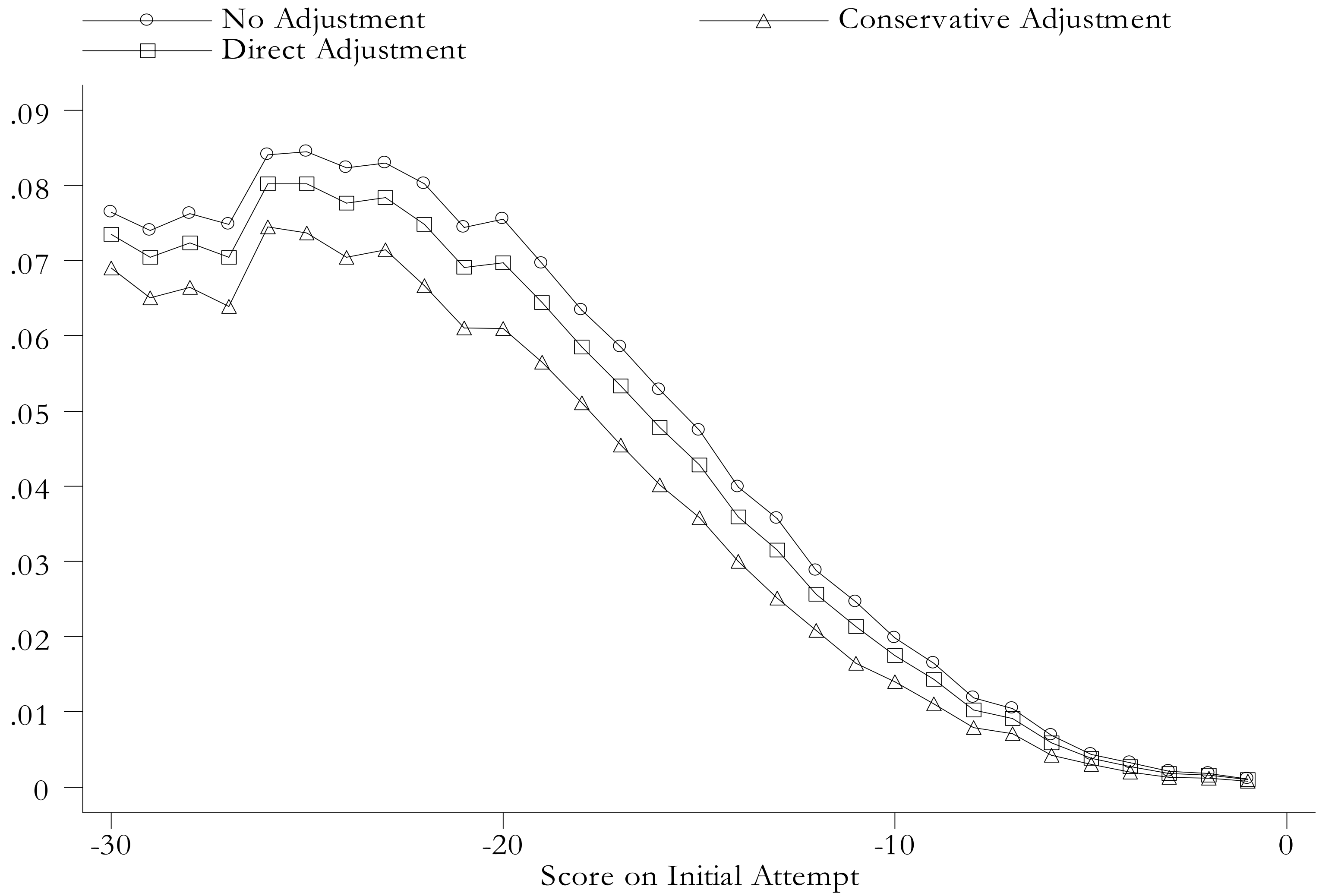
Notes: See notes to Figure 1.

Figure 7: Graduation and GED Acquisition Rates by Score on "Last Chance" Test, Only Students Eventually Passing



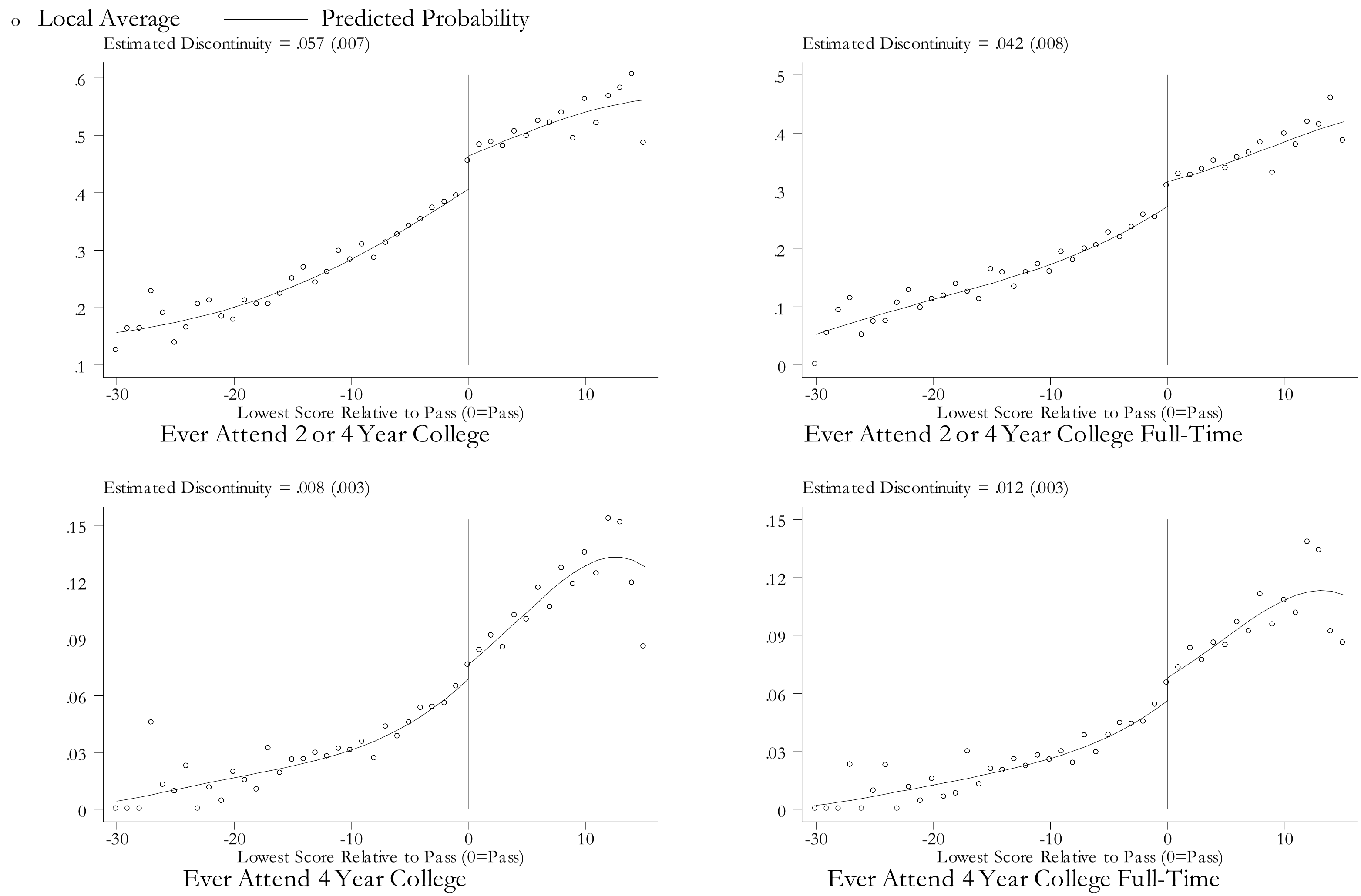
Notes: 21,705 observations. Only students passing all sections of the TAAS within 2 years of the "last chance" test and students with valid scores on all sections unpassed at the time of the "last chance" test included. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The predicted probabilities are from a probit regression of not graduating on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in the test score.

Figure 8: Estimated Fraction of Students Not Graduating Due to Failing Exit Exam by Score on Initial Attempt



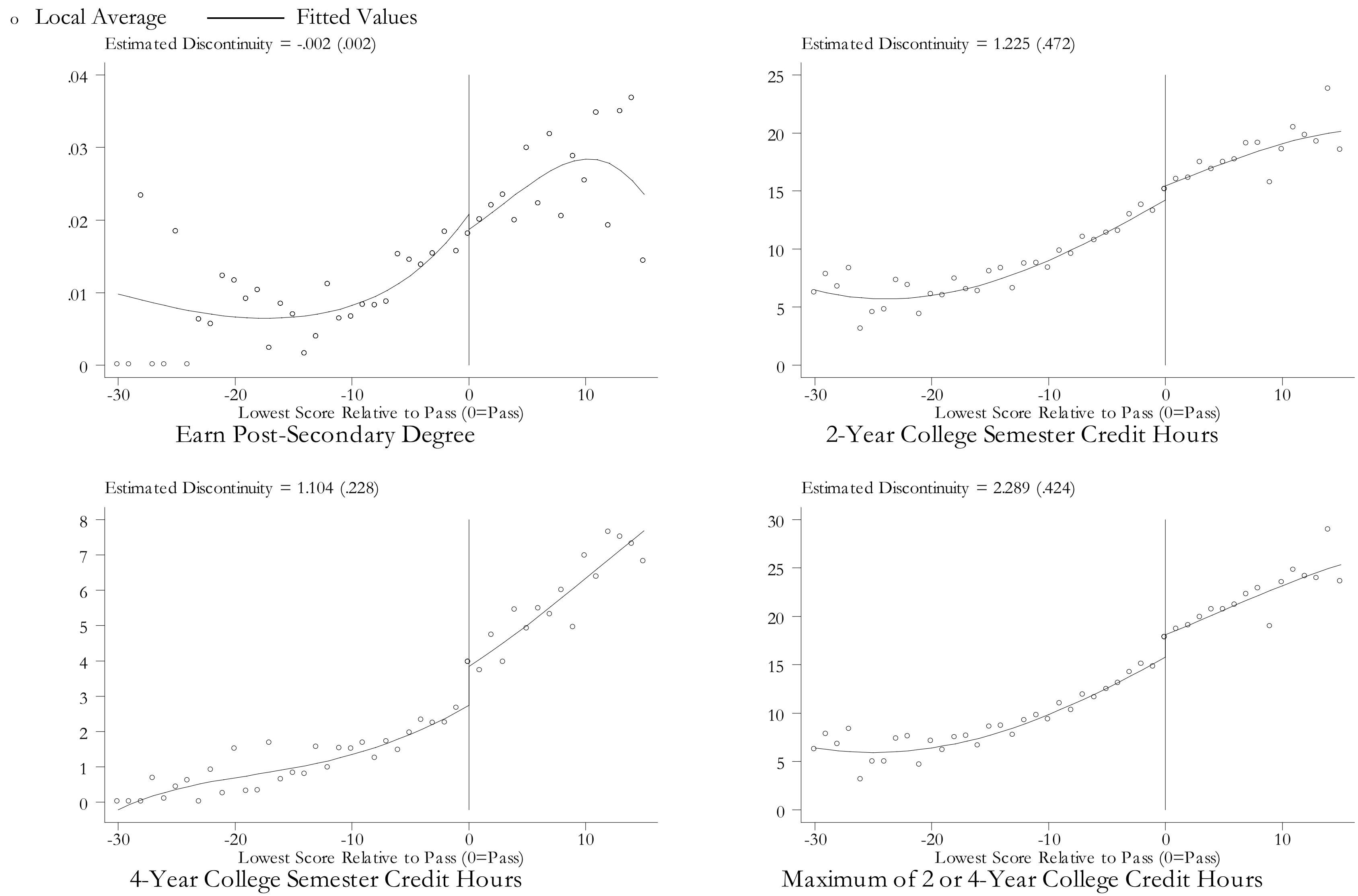
Note: Adjustments refer to correction made for misclassification of graduation status.

Figure 9: Enrollment in Post-Secondary Schooling by Score on "Last Chance" Test



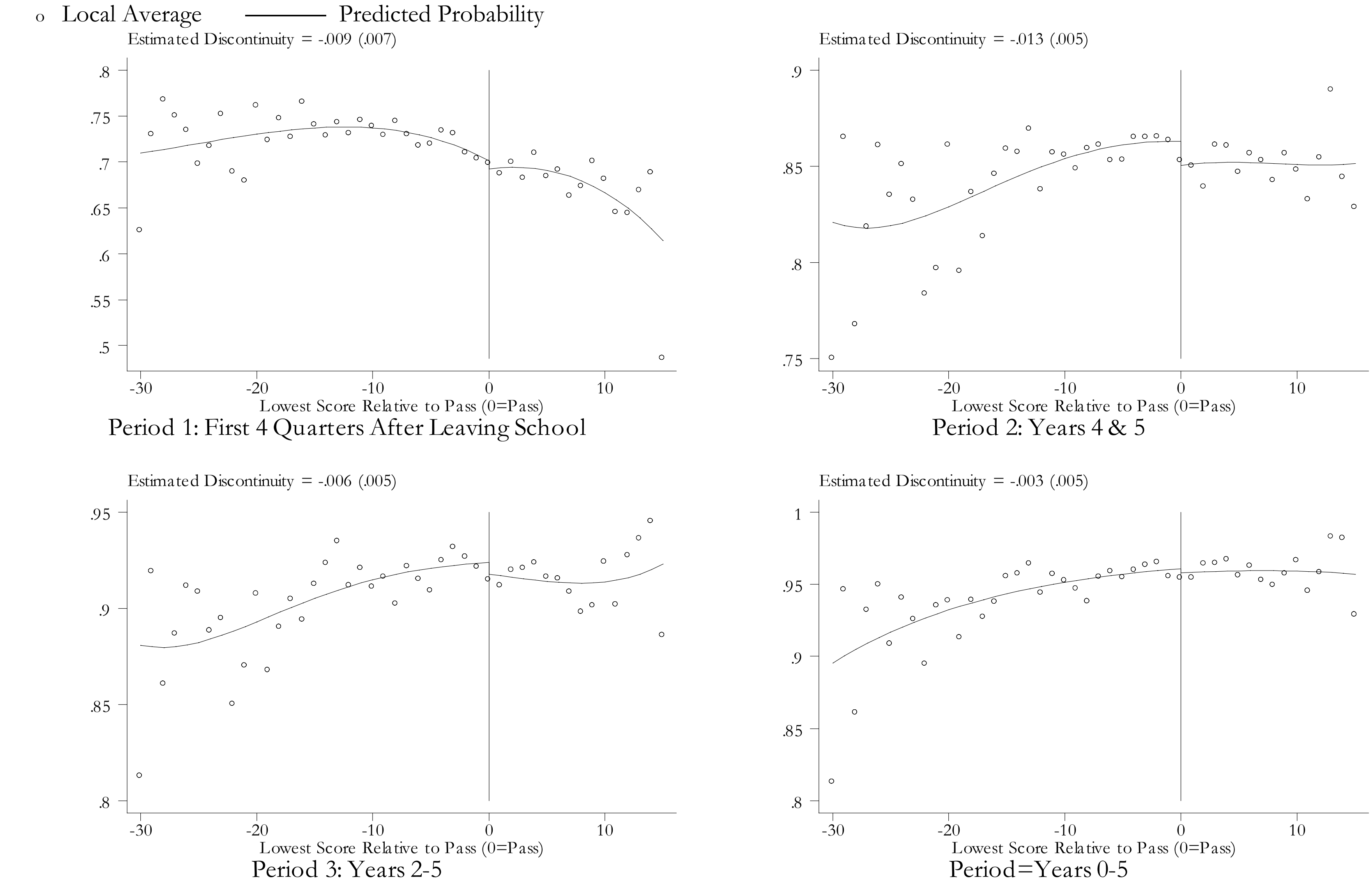
Notes: 37,114 observations. Only includes students with valid SSN's and valid scores on all sections unpassed at the time of the "last chance" test. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The predicted probabilities are from a probit regression of the dependent variable on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in the test score.

Figure 10: Post-Secondary School Attainment by Score on "Last Chance" Test



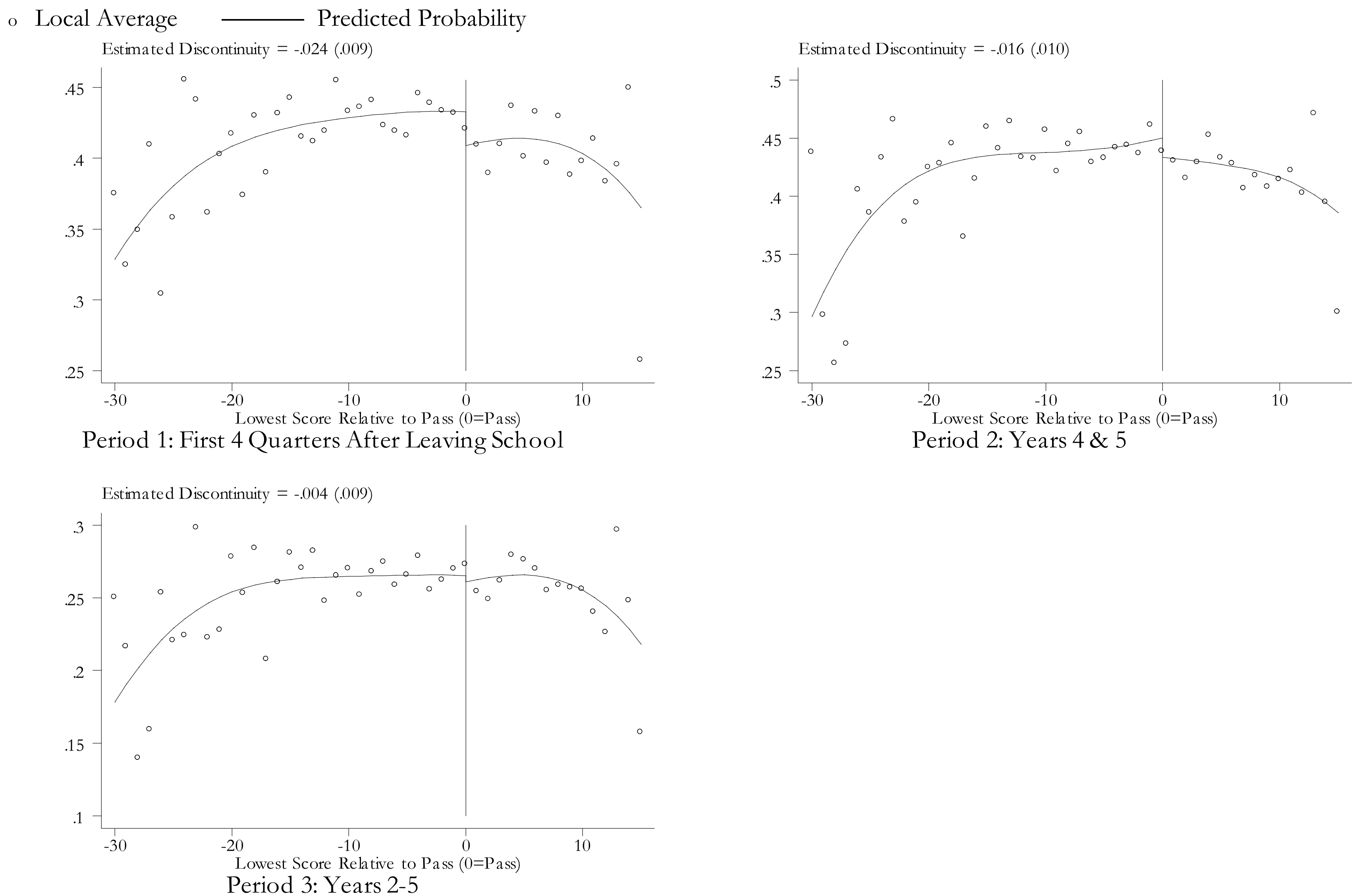
Notes: 37,114 observations. See notes to Figure 10 for additional details. Fitted values for post-secondary degree are predicted probabilities from a probit regression.

Figure 11: Fraction With Positive Earnings in At Least One Quarter in Period by Score on "Last Chance" Test



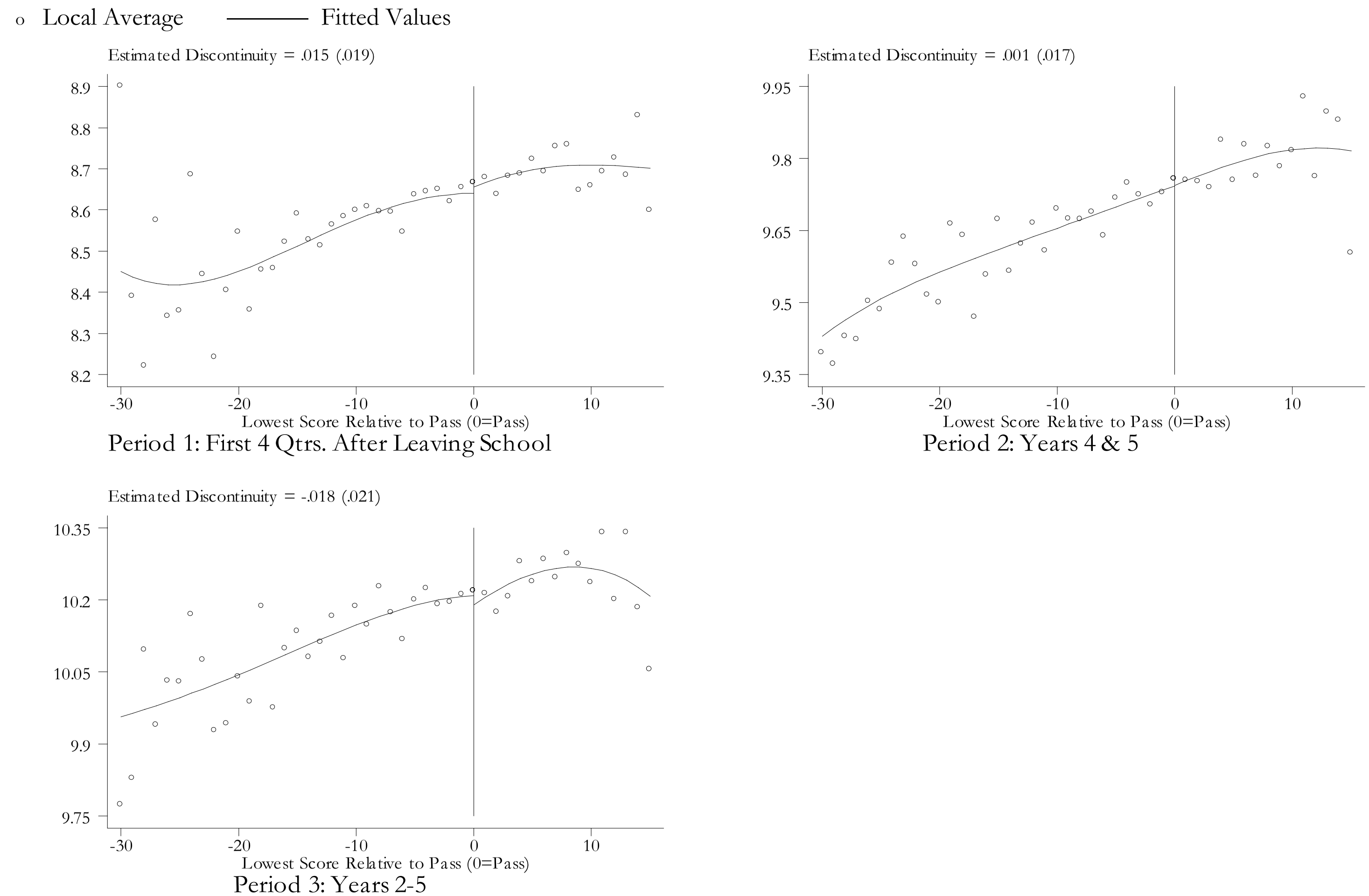
Notes: 37,114 observations. See notes to Figure 10 for additional details. Years in period designation are relative to the the expected end of high school.

Figure 12: Fraction With Positive Earnings in All Quarters in Period by Score on "Last Chance" Test



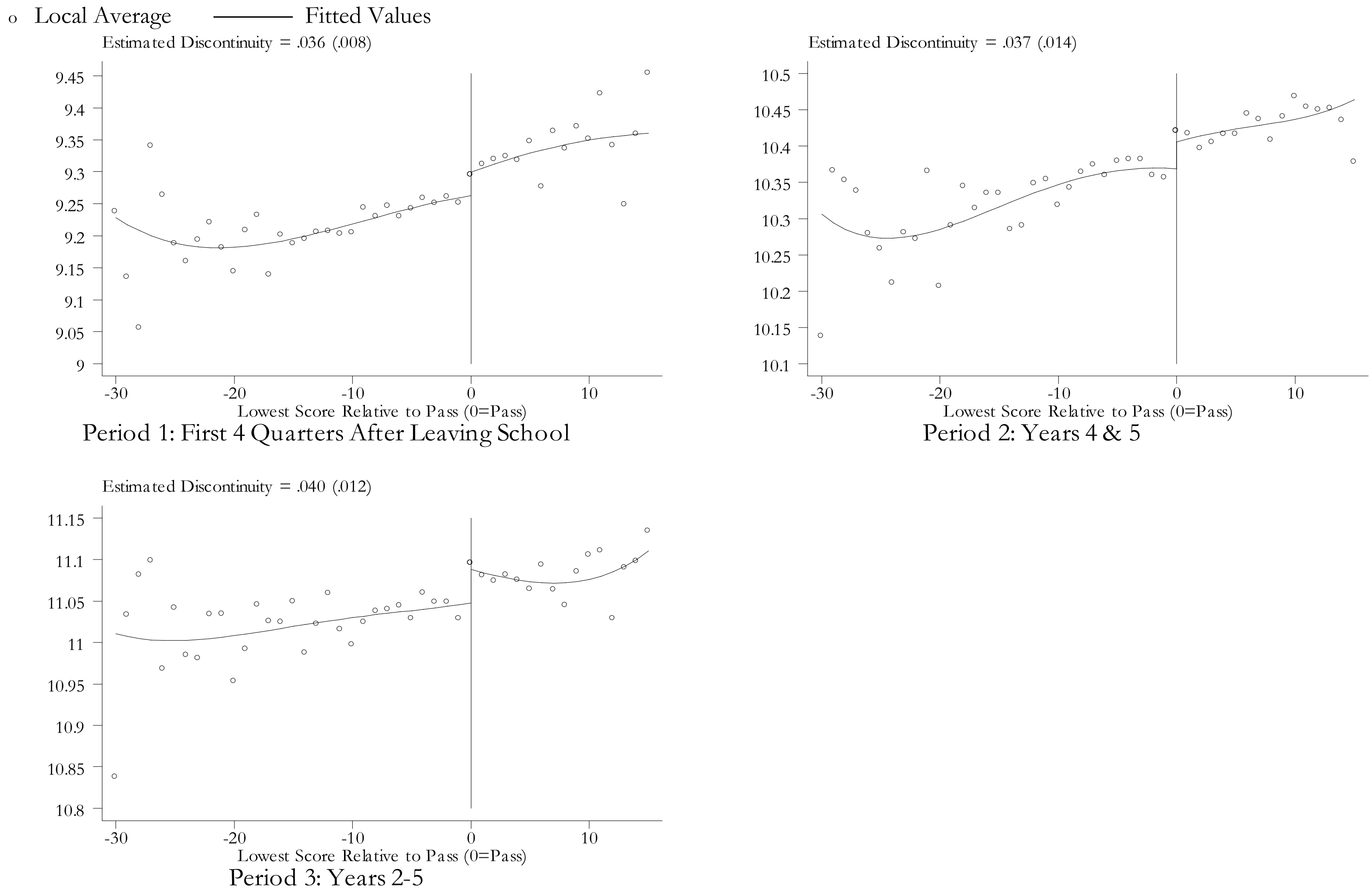
Notes: 37,114 observations. See notes to Figure 10 for additional details. Years in period designation are relative to the the expected end of high school.

Figure 13: Log Earnings in Period by Score on "Last Chance" Test, Includes Observations with Zero Earnings in Some Quarters in Period



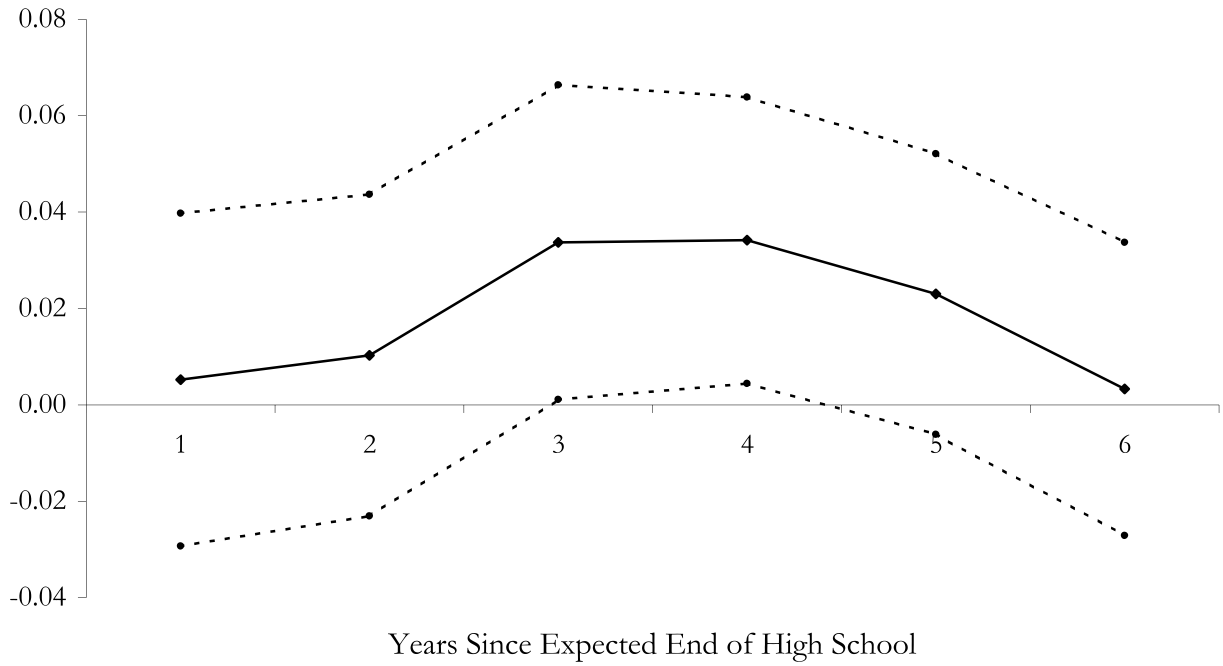
Notes: Sample sizes for Periods 1, 2, and 3 are 26418, 31670, 33970, respectively. Years in period designation are relative to the the expected end of high school. Only includes students with valid SSN's and valid scores on all sections unpassed at the time of the "last chance" test. For each period, only observations with positive earnings in some quarter in period used. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The fitted values are from a regression of the dependent variable on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in the test score.

Figure 14: Log Total Earnings in Period by Score on "Last Chance" Test, Restricted to Observations with Positive Earnings in All Quarters in Period



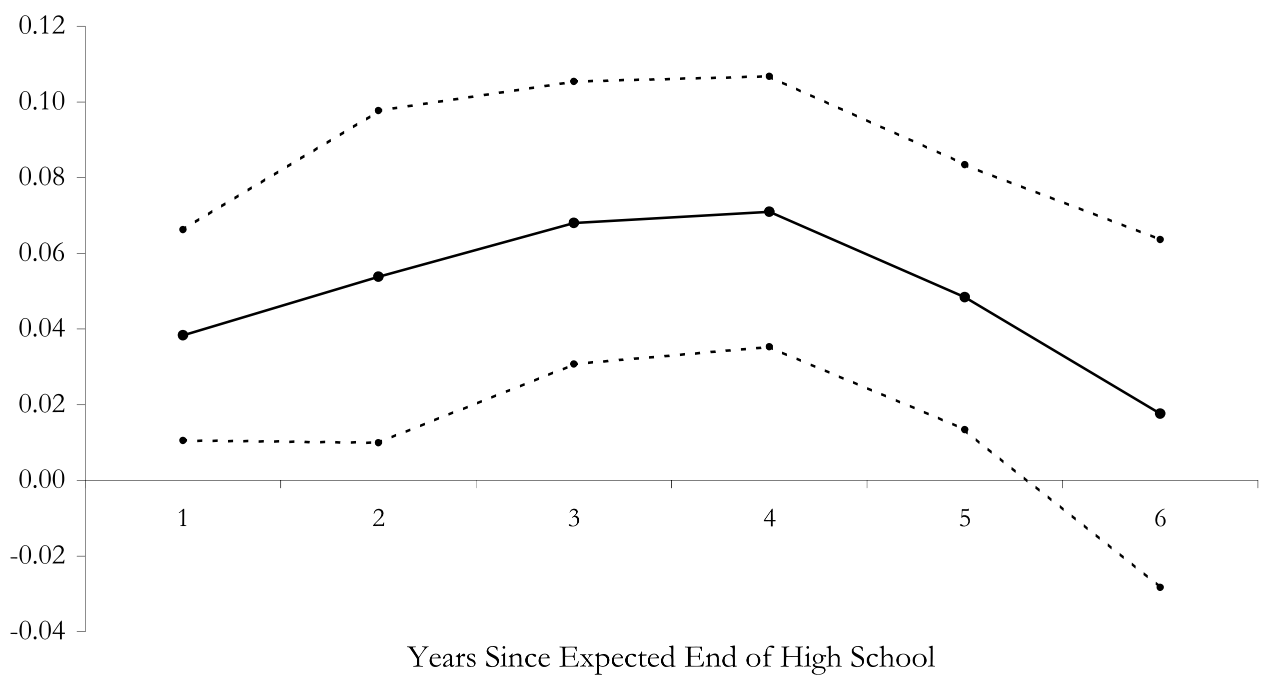
Notes: Sample sizes for Periods 1, 2, and 3 are 15619, 16098, 9751, respectively. Years in period designation are relative to the the expected end of high school. Only includes students with valid SSN's and valid scores on all sections unpassed at the time of the "last chance" test. For each period, only observations with positive earnings in all quarters in period used. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The fitted values are from a regression of the dependent variable on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in

Figure 15: RD Estimate of Test Failing Impact on Log Earnings, Includes College Attendees



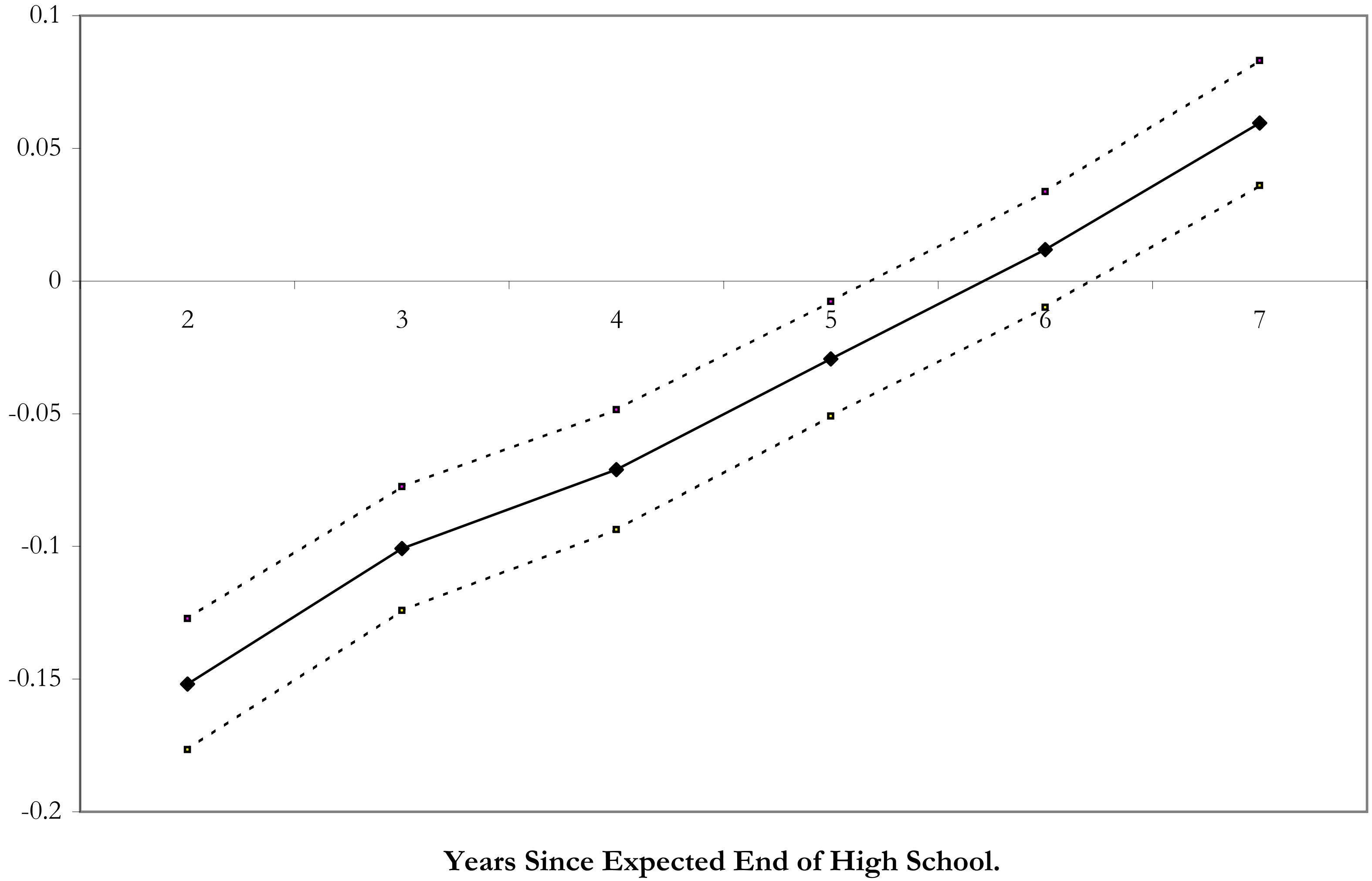
Notes: Each point on the solid line is the estimated coefficient of a dummy for passing the test in a regression of log earnings in a given year on the passing dummy and a fourth-order polynomail in the test score. Dashed lines are +/- 2 SE bands. Estimates for each year use only observations with positive earnings in each quarter of the year. Only 1991-1994 cohorts used. Test score polynomial is specified as a fourth-order polynomial with an interaction between the passing dummy and the linear test score term. See Table 11 for sample sizes.

Figure 16: RD Estimate of Test Failing Impact on Log Earnings, Excludes College Attendees



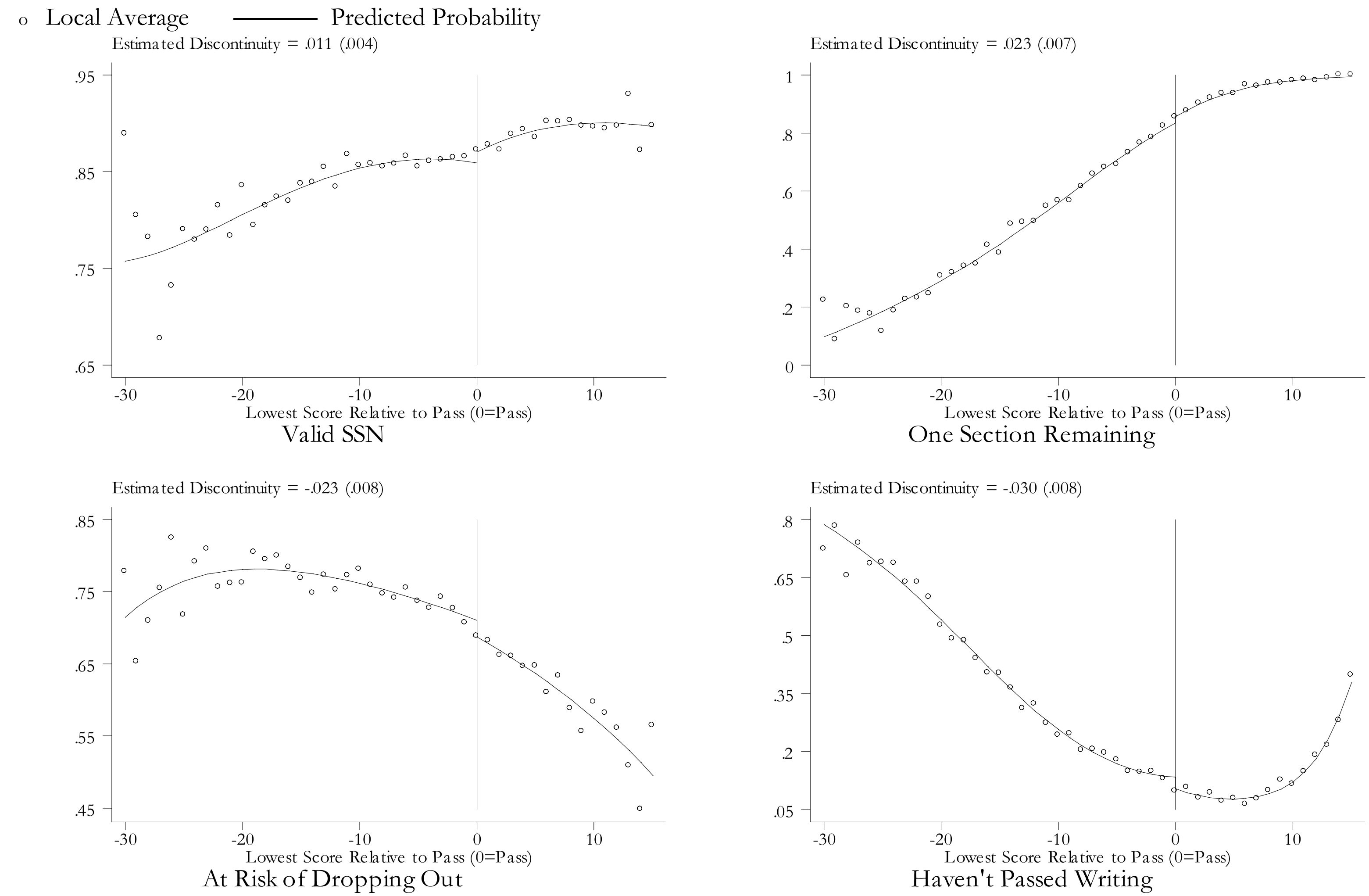
Notes: See notes to Figure 15. For each year, sample excludes students ever attending college in Texas within six years of the expected completion of high school.

Figure 17: College Attendance Log Earnings Gap by Year Since "On Time" Graduation



Notes: A point on the solid line is the estimated coefficient from a least squares regression of log average quarterly earnings in a given year on a dummy for attending a 2 or 4 year college within 5 years of the year of expected high school completion. Dashed lines are +/- 2 robust standard error bands. Average earnings calculated using only quarters with positive earnings. Sample limited to observations with positive earnings in each year (N=15834).

Appendix Figure 1: Average Pre-Determined Characteristics by Score, "Last Chance" Sample



Notes: 42,957 observations. Only includes students with valid scores on all sections unpassed at the time of the "last chance" test. The test score variable is the lowest score of the unpassed sections rescaled to be zero at the passing cutoff. The circles are the mean of the dependent variable for students with a given test score. The predicted probabilities are from a probit regression of the characteristic on a dummy for passing, a fourth-order polynomial in the test score, and an interaction between the passing dummy and a linear term in the test score.

Table 1: Exit Exam Administrations by Academic Cohort

Cohort	Spring 10^{th} Grade	Summer 10^{th} Grade	Fall 11^{th} Grade	Spring 11^{th} Grade	Summer 11^{th} Grade	Fall 12^{th} Grade	Spring 12^{th} Grade	May/April Retest for Seniors*
1991			X	X	X	X	X	
1992			X	X	X	X	X	X
1993	X	X	X	X	X	X	X	X
1994	X	X	X	X	X	X	X	X
1995	X	X	X	X	X	X	X	X

Notes: The adminstrations listed for each cohort are for students not held back in grade after initially taking the exam. Cohort refers to the calendar year in which a student was in 10^{th} grade during the spring.
* - Test administered in May for 1992-1994 cohorts and in April for 1995 cohort.

Table 2: Passing and Exemption Rates, 1991-1994 Test-Taking Cohorts

	Full Sample			Whites			Non-Whites		
	All Students	Non-Graduates	Graduates	All Students	Non-Graduates	Graduates	All Students	Non-Graduates	Graduates
All Test Takers									
Eventually Pass	0.848	0.438	0.957	0.902	0.581	0.969	0.781	0.320	0.941
Eventually Pass or Exempt	0.872	0.452	0.983	0.924	0.596	0.992	0.807	0.332	0.971
Number of Students	635,770	133,076	502,694	352,896	60,139	292,757	282,874	72,937	209,937
Students Who Fail At Least Once*									
Eventually Pass	0.694	0.263	0.899	0.736	0.334	0.900	0.665	0.221	0.898
Eventually Pass or Exempt	0.742	0.281	0.961	0.795	0.359	0.974	0.704	0.235	0.951
Ever Exempt (Any Section)	0.050	0.020	0.064	0.061	0.027	0.075	0.041	0.016	0.055
Number of Students	315,418	101,544	213,874	130,419	37,877	92,542	184,999	63,667	121,332

Notes: To pass the exam, all 3 sections must be passed. A student is considered to have passed a section if a matching record is found with either a passing score for that section or with a score code of "P" indicating the section was passed at an earlier date. Only exams taken within 4 years of initially attempting the test "count" towards eventually passing (althougl no limit exists after which a student cannot return, pass the exam, and receive a diploma).

* - Students who did not take all 3 sections initially are included among the students failing at least once .

Table 3: Probit Regression Discontinuity Estimates of the Impact of Passing Status on Graduating and GED Acquisition by Test Administration, 1993-1995 Test Taking Cohorts

(a) Quartic Polynomial in Test Score*

	10th Grade				11th Grade						12th Grade					
Outcome	Initial Attempt		Summer		Fall		Spring		Summer		Fall		Spring		"Last Chance"	
Graduate	0.001	0.001	0.001	0.000	0.002	0.002	0.006	0.006	0.020	0.019	0.026	0.024	0.125	0.131	0.444	0.447
	(0.004)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)	(0.004)	(0.007)	(0.007)	(0.012)	(0.013)
Take GED	0.004	0.003	0.004	0.004	-0.000	-0.002	-0.005	-0.004	-0.008	-0.008	-0.007	-0.005	-0.023	-0.024	-0.074	-0.072
Exam	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)	(0.004)
Receive GED	0.004	0.003	0.004	0.004	0.002	-0.000	-0.006	-0.005	-0.008	-0.008	-0.004	-0.002	-0.014	-0.013	-0.044	-0.040
Certificate	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Receive GED or	0.003	0.003	0.004	0.003	0.001	-0.000	-0.000	-0.000	0.013	0.011	0.023	0.020	0.116	0.118	0.410	0.410
HS Diploma	(0.002)	(0.002)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.004)	(0.007)	(0.006)	(0.012)	(0.014)
Controls?	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
N	505291		73420		187004		126878		42337		74926		45153		20711	

(b) Quadratic Polynomial in Test Score*

	10th Grade				11th Grade						12th Grade					
Outcome	Initial Attempt		Summer		Fall		Spring		Summer		Fall		Spring		"Last Chance"	
Graduate	0.001	0.001	-0.009	-0.009	-0.018	-0.014	-0.010	-0.007	0.012	0.011	0.017	0.016	0.117	0.126	0.438	0.442
	(0.004)	(0.004)	(0.006)	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.009)	(0.008)	(0.012)	(0.013)
Take GED	0.004	0.003	0.008	0.008	0.005	0.002	0.003	0.002	-0.004	-0.004	-0.002	-0.002	-0.023	-0.024	-0.078	-0.077
Exam	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
Receive GED	0.004	0.003	0.007	0.007	0.005	0.003	0.000	0.000	-0.006	-0.005	-0.002	-0.001	-0.014	-0.013	-0.050	-0.045
Certificate	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Receive GED or	0.003	0.003	-0.004	-0.004	-0.014	-0.012	-0.010	-0.008	0.008	0.007	0.016	0.015	0.110	0.114	0.398	0.399
HS Diploma	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)	(0.008)	(0.008)	(0.013)	(0.014)
Controls?	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
N	505291		73420		187004		126878		42337		74926		45153		20711	

Notes: Each cell represents the estimated discontinuity in the outcome at the passing score for the test administration indicated in the column heading. It is the marginal effect of passing the exam obtained from a probit regression of not graduating on a dummy for passing, a polynomial in the test score, and where indicated, controls. Marginal effects calculated at the sample mean for observations at the passing score. Robust standard errors adjusted for clustering at the test score level in parentheses. For each test administration, students with invalid scores for any unpassed section, or whose reported grade level is higher than that implied by normal grade progression are excluded. Students with extreme scores (less than -30 or greater than 15) are dropped. The covariates in the regressions with controls include dummies for black, white, hispanic (other=excluded race category), at grade level at time of inital test attempt, limited english proficient, economically disadvantaged, at risk of dropping out, male, special education, initially taking the test in spring of 10th grade, and cohort dummies. For the initial test, controls for worst section is math and worst section is reading (writing=excluded category) are included, and for the retests, controls for grade retention since initial attempt, still haven't passed math, still haven't passed reading, still haven't passed writing, and one section remaining are included.

* - The quadratic and quartic specifications both include an interaction between the passing dummy and a linear test score. The quadratic also interacts the dummy with the test score squared.

Table 4: Probit Regression Discontinuity Estimates of the Impact of Passing Status on Attendance and Dropping Out by Test Administration, 1993-1995 Test Taking Cohorts

(a) Quartic Polynomial in Test Score

	10^{th} Grade				11th Grade						12^{th} Grade					
Outcome	Initial Attempt		Summer		Fall		Spring		Summer		Fall		Spring		"Last Chance"	
Attend End of Grade 12	-0.004	-0.004	0.003	0.003	-0.003	-0.001	0.002	0.002	0.012	0.012	0.001	-0.005	-0.008	-0.004	-0.002	-0.001
	(0.003)	(0.003)	(0.005)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)	(0.008)	(0.008)	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)	(0.006)
Stop Attending After Exam	0.001	0.001	-0.000	-0.000	-0.002	-0.002	-0.000	-0.000	-0.006	-0.004	0.009	0.009	-0.001	-0.001	0.063	0.059
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.007)	(0.006)
Controls?	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
N	505291		73420		187004		126878		42337		74926		45153		20711	

(b) Quadratic Polynomial in Test Score

	10^{th} Grade				11th Grade						12^{th} Grade					
Outcome	Initial Attempt		Summer		Fall		Spring		Summer		Fall		Spring		"Last Chance"	
Attend End of Grade 12	-0.004	-0.004	0.001	0.002	-0.007	-0.003	0.003	0.005	0.014	0.014	0.000	-0.003	-0.008	-0.002	0.000	0.000
	(0.003)	(0.003)	(0.005)	(0.006)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)	(0.006)
Stop Attending After Exam	0.001	0.001	0.000	0.000	-0.002	-0.002	0.000	-0.000	-0.004	-0.003	0.010	0.010	-0.001	-0.001	0.067	0.060
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.009)	(0.008)
Controls?	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
N	505291		73420		187004		126878		42337		74926		45153		20711	

Notes: Each cell represents the estimated discontinuity in the outcome at the passing score for the test administration indicated in the column heading. Robust standard errors adjusted for clustering at the test score level in parentheses. See notes to Table 3 for additional details.

Table 5: Probit Regression Discontinuity Estimates for Graduation and GED Acquisition by Eventual Passing Status, "Last Chance" Sample

	Only Students Eventually Passing				All Students			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Graduate	0.162	0.167	0.155	0.161	0.406	0.412	0.401	0.410
	(0.009)	(0.008)	(0.010)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Take GED Exam	-0.006	-0.006	-0.001	-0.002	-0.072	-0.070	-0.070	-0.068
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Receive GED Certificate	-0.003	-0.003	0.001	0.000	-0.053	-0.048	-0.052	-0.047
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Controls?	N	Y	N	Y	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic	Quartic	Quartic	Quadratic	Quadratic

Notes: 21,705 observations in columns 1-4 and 42,957 observations in columns 5-8. Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. Estimates in columns 1-4 only include students passing all sections of the TAAS within 2 years of the "last chance" test. Only students with valid scores for all unpassed sections included. Additional sample restrictions same as those described in notes to Table 3.

Table 6: Overall Effect of Failing Exit Exam on Non Graduation Rates

	Full Sample	Non-White	Economically Disadvantaged
<u>No Adjustment for Misclassification of Graduation Status</u>			
% Not Graduate Due to Failing Test	0.014	0.025	0.026
% Non Graduates Not Graduating Due to Failing	0.069	0.101	0.094
<u>Conservative Adjustment for Misclassification of Graduation Status</u>			
% Not Graduate Due to Failing Test	0.011	0.019	0.021
% Non Graduates Not Graduating Due to Failing	0.054	0.080	0.075
<u>Direct Adjustment for Misclassification of Graduation Status</u>			
% Not Graduate Due to Failing Test	0.012	0.023	0.024
% Non Graduates Not Graduating Due to Failing	0.063	0.093	0.086
Number of Students Ever Taking the Exit Exam	810260	361936	199567

Notes: Cells report estimates of the fraction of students induced to not graduate because of failing the "last chance" test. Estimates for non-whites and economically disadvantaged students use regression discontinuity estimates obtained using only members of that subgroup. See text for description of the adjustments for misclassification of graduation status.

Table 7a: Probit Regression Discontinuity Estimates for Post-Secondary School Attendance

Outcome	(1)	(2)	(3)	(4)
Ever Attend 2 or 4 Year College	0.057	0.058	0.053	0.056
	(0.007)	(0.008)	(0.007)	(0.008)
Ever Attend 2 or 4 Year College Full Time	0.042	0.044	0.046	0.048
	(0.008)	(0.010)	(0.008)	(0.009)
Ever Attend 4 Year College	0.008	0.009	0.008	0.010
	(0.003)	(0.003)	(0.003)	(0.003)
Ever Attend 4 Year College Full Time	0.012	0.013	0.013	0.014
	(0.003)	(0.003)	(0.003)	(0.003)
Controls?	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic

Note: 37,114 observations. Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. College attendance measures refer to the first five years after the "last chance" test. Only students with valid scores for all unpassed sections and valid SSN's included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

Table 7b: Comparison of Effects on Post-Secondary Schooling by Number of Years Since Expected End of High School

	College Attendance Up to 5 Years After Expected End of High School				College Attendance Up to 7 Years After Expected End of High School			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ever Attend 2 or 4 Year College	0.057	0.058	0.056	0.060	0.050	0.051	0.047	0.051
	(0.011)	(0.011)	(0.012)	(0.011)	(0.010)	(0.009)	(0.010)	(0.009)
Ever Attend 2 or 4 Year College Full Time	0.046	0.048	0.046	0.051	0.050	0.052	0.050	0.054
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Ever Attend 4 Year College	0.009	0.011	0.013	0.015	0.010	0.011	0.013	0.015
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
Ever Attend 4 Year College Full Time	0.014	0.015	0.016	0.018	0.010	0.012	0.012	0.014
	(0.004)	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.005)
Controls?	N	Y	N	Y	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic	Quartic	Quartic	Quadratic	Quadratic

Note: 37,114 observations in columns 1-4 and 26,045 in columns 5-8. Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. College attendance measures refer to the first five years after the expected end of high school in columns 1-4 and the first seven years in columns 5-8. Estimates in columns 5-8 include only 1991-1993 cohorts. Only students with valid scores for all unpassed sections and valid SSN's included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

* - Estimates in columns 5-8 refer to degrees earned within six years of "on time" graduation

Table 8: Regression Discontinuity Estimates for Post-Secondary School Attainment

Outcome	(1)	(2)	(3)	(4)
Earn Post-Secondary Degree	-0.002	-0.002	-0.003	-0.002
Within 4 Years	(0.002)	(0.002)	(0.002)	(0.002)
Earn Post-Secondary Degree	-0.005	-0.004	-0.004	-0.003
Within 6 Years*	(0.005)	(0.005)	(0.004)	(0.004)
Total 2-Year College Semester	1.225	1.269	0.923	0.992
Credit Hours	(0.472)	(0.549)	(0.436)	(0.501)
Total 4-Year College Semester	1.104	1.129	1.164	1.194
Credit Hours	(0.228)	(0.214)	(0.213)	(0.195)
Maximum of 2 or 4-Year College	2.289	2.347	2.036	2.128
Semester Credit Hours	(0.424)	(0.523)	(0.379)	(0.464)
Controls?	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic

Note: 37,114 observations. Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. College attendance measures refer to the first five years after the "last chance" test. Only students with valid scores for all unpassed sections and valid SSN's included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

* - Only uses 1991-1993 cohorts (26,045 observations).

Table 9: Probit Regression Discontinuity Estimates for Having Positive Earnings

(a) Positive Earnings in Any Quarter in Period

Period	(1)	(2)	(3)	(4)
(1) First 4 Qtrs. After Leaving School	-0.009	-0.010	-0.012	-0.013
	(0.007)	(0.007)	(0.008)	(0.007)
(2) Years 4 and 5	-0.013	-0.012	-0.015	-0.014
	(0.005)	(0.005)	(0.004)	(0.004)
(3) Years 2-5	-0.006	-0.006	-0.008	-0.008
	(0.005)	(0.005)	(0.005)	(0.004)
Years 0-5	-0.003	-0.003	-0.004	-0.003
	(0.005)	(0.005)	(0.004)	(0.004)
Controls?	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic

(b) Positive Earnings in All Quarters in Period

Period	(1)	(2)	(3)	(4)
(1) First 4 Qtrs. After Leaving School	-0.024	-0.025	-0.022	-0.022
	(0.009)	(0.009)	(0.010)	(0.010)
(2) Years 4 and 5	-0.016	-0.017	-0.009	-0.009
	(0.010)	(0.010)	(0.011)	(0.011)
(3) Years 2-5	-0.004	-0.005	-0.001	-0.003
	(0.009)	(0.009)	(0.010)	(0.011)
Controls?	N	Y	N	Y
Specification of Test Score Polynomial	Quartic	Quartic	Quadratic	Quadratic

Note: 37,114 observations. Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. Years in period designation are relative to the expected year of high school completion. Only students with valid scores for all unpassed sections and valid SSN's included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

Table 10: Least Squares Regression Discontinuity Estimates for Log Total Earnings in Period

(a) Only Observations with Positive Earnings in At Least 1 Quarter in Period

Period	(1)	(2)	(3)	(4)	(5)	(6)	N
(1) First 4 Qtrs. After Leaving School	0.015	0.015	<0.001	-0.003	-0.002	-0.015	26418
	(0.019)	(0.019)	(0.023)	(0.018)	(0.019)	(0.024)	
(2) Years 4 and 5	0.001	0.004	0.018	0.005	0.009	0.021	31670
	(0.017)	(0.020)	(0.020)	(0.017)	(0.019)	(0.019)	
(3) Years 2-5	-0.018	-0.018	-0.005	-0.026	-0.024	-0.017	33970
	(0.021)	(0.022)	(0.023)	(0.022)	(0.023)	(0.024)	
Baseline Controls?	N	Y	Y	N	Y	Y	
Experience Controls?	N	N	Y	N	N	Y	
Specification of Test Score Polynomial	Quartic	Quartic	Quartic	Quadratic	Quadratic	Quadratic	

(b) Only Observations with Positive Earnings in All Quarters in Period

Period	(1)	(2)	(3)	(4)	(5)	(6)	N
(1) First 4 Qtrs. After Leaving School	0.036	0.035	0.030	0.030	0.029	0.026	15619
	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)	(0.009)	
(2) Years 4 and 5	0.037	0.035	0.038	0.033	0.035	0.036	16098
	(0.014)	(0.016)	(0.016)	(0.014)	(0.015)	(0.015)	
(3) Years 2-5	0.040	0.035	0.041	0.043	0.041	0.043	9751
	(0.012)	(0.013)	(0.014)	(0.011)	(0.011)	(0.013)	
Baseline Controls?	N	Y	Y	N	Y	Y	
Experience Controls?	N	N	Y	N	N	Y	
Specification of Test Score Polynomial	Quartic	Quartic	Quartic	Quadratic	Quadratic	Quadratic	

Note: Each cell represents the estimated discontinuity in the outcome at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. In panel (a), estimates for each row only use observations with at least one quarter of positive earnings in the indicated period while panel (b) requires positive earnings in all quarters in the period. Years in period designation are relative to the expected year of high school completion. Only students with valid scores for all unpassed sections and valid SSN's included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

Table 11: Correlation Between Average Quarterly Earnings in 7th Year After Expected Date of High School Completion and Earlier Years

Year Since Expected Date of HS Completion		Number of Quarters with Positive Earnings in Early Year: 4	Number of Quarters with Positive Earnings in Early Year: Fewer Than 4
2	0.460 [17704]	0.482 [11119]	0.313 [6585]
3	0.524 [17964]	0.550 [12210]	0.333 [5754]
4	0.599 [18305]	0.621 [12231]	0.515 [6074]
5	0.681 [18749]	0.697 [14353]	0.480 [4396]

Notes: Entries are the estimated correlation between the average quarterly earnings in the 7th year after the expected date of high school completion and the average earnings in the year indicated in the first column. Only quarters with positive earnings are used to calculate the averages so only individuals with some positive earnings in year 7 and the early year are used. Only 1991-1993 cohorts used because data beyond the 7th year is not available for subsequent cohorts. Sample sizes in brackets.

Table 12: Regression Discontinuity Estimates for Log Earnings by Year Since Expected End of High School

Year Since Expected	Includes College Attendees			Excludes College Attendees		
End of High School	(1)	(2)	(3)	(4)	(5)	(6)
1	0.005	<-0.001	0.003	0.038	0.032	0.034
	(0.017)	(0.016)	(0.015)	(0.014)	(0.014)	(0.015)
2	0.010	0.013	0.020	0.054	0.047	0.051
	(0.017)	(0.016)	(0.016)	(0.022)	(0.019)	(0.019)
3	0.034	0.037	0.045	0.068	0.060	0.072
	(0.016)	(0.018)	(0.015)	(0.019)	(0.022)	(0.017)
4	0.034	0.037	0.045	0.071	0.068	0.076
	(0.015)	(0.018)	(0.016)	(0.018)	(0.020)	(0.019)
5	0.023	0.019	0.023	0.048	0.035	0.040
	(0.015)	(0.017)	(0.017)	(0.018)	(0.019)	(0.018)
6	0.003	0.005	0.015	0.018	0.007	0.018
	(0.015)	(0.016)	(0.014)	(0.023)	(0.020)	(0.019)
Baseline Controls?	N	Y	Y	N	Y	Y
Experience Controls?	N	N	Y	N	N	Y

Note: Each cell represents the estimated discontinuity in log earnings in the indicated year at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. For each year, only individuals with earnings in each quarter included. Sample sizes in columns 1-3 (starting in year 1): 16896, 19012, 20222, 20141, 21932, 19052 and in columns 4-6 (starting in year 1): 10099, 11193, 11713, 11767, 12666, 10823. Years in period designation are relative to the expected year of high school completion. Only students with valid scores for all unpassed sections, valid SSN's, and earnings in each quarter of the relevant year included. Estimates in columns 4-6 exclude individuals attending college within six years of completing high. Test score polynomial is specified as a fourth-order polynomial with an interaction between the passing dummy and the linear test score term. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

Appendix Table 1: Sample Means for the 1993-1995 Sample by Retest

	Initial Attempt	Summer 10th Grade	Fall 11th Grade	Spring 11th Grade	Summer 11th Grade	Fall 12th Grade	Spring 12th Grade	"Last Chance"
Score on Initial Test	-1.463	-11.267	-13.635	-15.204	-15.909	-17.342	-18.849	-20.005
Pass	0.538	0.263	0.295	0.285	0.236	0.326	0.370	0.322
Graduate	0.798	0.834	0.741	0.732	0.802	0.697	0.642	0.530
Take GED Exam	0.070	0.056	0.091	0.084	0.051	0.086	0.085	0.092
Receive GED Certificate	0.059	0.044	0.072	0.062	0.034	0.059	0.052	0.049
Attend End of Grade 12	0.813	0.861	0.787	0.804	0.894	0.833	0.865	0.938
Stop Attending After Exam	0.010	0.007	0.013	0.007	0.015	0.044	0.039	0.845
Designated as a Dropout	0.041	0.032	0.058	0.058	0.030	0.057	0.058	0.055
Black	0.122	0.192	0.183	0.204	0.240	0.223	0.234	0.256
White	0.550	0.379	0.388	0.334	0.275	0.278	0.231	0.193
Hispanic	0.297	0.403	0.408	0.441	0.459	0.476	0.511	0.522
At Grade Level (Initially)	0.776	0.714	0.666	0.633	0.657	0.594	0.564	0.527
Limited English Proficient	0.052	0.086	0.101	0.123	0.135	0.150	0.179	0.206
Economically Disadvantaged	0.260	0.357	0.373	0.405	0.416	0.436	0.470	0.495
At Risk of Dropping Out	0.412	0.643	0.651	0.696	0.735	0.722	0.741	0.764
Male	0.488	0.408	0.472	0.461	0.390	0.460	0.448	0.411
Special Education	0.043	0.046	0.068	0.075	0.056	0.076	0.066	0.049
Retained In Grade			0.089	0.103	0.065	0.138	0.153	0.044
1993 Cohort	0.315	0.339	0.351	0.344	0.342	0.341	0.339	0.375
1994 Cohort	0.331	0.325	0.326	0.329	0.350	0.337	0.330	0.325
Sections Yet to be Passed	3	1.618	1.695	1.550	1.353	1.448	1.386	1.292
Total Number of Retests	2.747	2.952	2.931	3.737	4.930	4.678	5.409	6.279
Number of Earlier Attempts	0	1	1.295	2.242	3.388	3.591	4.530	5.665
Eventually Pass	0.895	0.851	0.769	0.732	0.772	0.675	0.599	0.460
Eventually Pass or Exempt	0.912	0.881	0.812	0.783	0.819	0.731	0.643	0.479
Only Students who Fail								
Eventually Pass	0.710	0.800	0.674	0.632	0.699	0.514	0.391	0.205
Eventually Pass or Exempt	0.757	0.841	0.734	0.701	0.760	0.598	0.457	0.232
Exempt (Any Section)	0.049	0.042	0.063	0.072	0.064	0.087	0.069	0.030
Retake Test	0.921	0.975	0.928	0.895	0.956	0.857	0.835	0.693
Number of Sections Failed	1.752	1.517	1.554	1.465	1.346	1.407	1.359	1.298
N	505291	73420	187004	126878	42337	74926	45153	20711

Notes: See notes to Table 2 for description of sample for each test administration.

Appendix Table 2: Sample Means for "Last Chance" Sample by Passing Status

	Full Sample				Only Records with Valid SSN's			
	All	Pass	Fail	T-Ratio for Fail - Pass	All	Pass	Fail	T-Ratio for Fail - Pass
Pass "Last Chance" Attempt	0.37	1.00	0.00		0.38	1.00	0.00	
Score (0=Pass)	-3.65	4.29	-8.31	-240.54	-3.43	4.32	-8.18	-226.99
	(8.03)	(3.52)	(6.03)		(7.96)	(3.52)	(5.93)	
Score on Initial Attempt*	-16.45	-13.80	-18.01	-64.03	-16.21	-13.67	-17.76	-58.77
	(6.76)	(6.31)	(6.52)		(6.68)	(6.27)	(6.44)	
Eventually Pass	0.51	1.00	0.21		0.51	1.00	0.22	
Eventually Pass or Exempt	0.52	1.00	0.24		0.53	1.00	0.25	
Retest if Fail "Last Chance" Test			0.72				0.72	
Exempt (Any Section)			0.03				0.03	
Graduate	0.57	0.92	0.36	-134.18	0.58	0.92	0.37	-124.93
Take GED Test	0.08	0.01	0.13	42.09	0.09	0.01	0.14	42.65
Receive GED Certificate	0.05	0.01	0.08	30.37	0.06	0.01	0.09	30.81
Receive GED or HS Diploma	0.61	0.93	0.43	-118.19	0.63	0.93	0.44	-108.21
Attend 2 or 4 Year College					0.38	0.50	0.31	-36.80
Attend 2 or 4 Yr. College Full Time					0.25	0.34	0.19	-32.71
Attend 4 Yr. College					0.06	0.10	0.04	-23.28
Attend 4 Yr. College Full Time					0.05	0.09	0.03	-22.22
Earn Bachelor or Associate Degree					0.02	0.02	0.01	-9.00
Any Earnings, Period 1					0.71	0.69	0.73	8.39
Earnings in all Quarters, Period1					0.42	0.41	0.43	3.31
Any Earnings, Period 2					0.85	0.85	0.85	0.79
Earnings in all Quarters, Period2					0.43	0.43	0.44	2.05
Any Earnings, Period 3					0.92	0.92	0.92	-0.03
Earnings in all Quarters, Period3					0.26	0.26	0.26	0.31
Any Earnings, Years 1-6					0.95	0.96	0.95	-3.07
ln(Earnings in Period 1)					8.62	8.69	8.59	-6.59
					(1.20)	(1.20)	(1.20)	
ln(Earnings in Period 1)					9.26	9.32	9.23	-10.37
Earnings in all Quarters					(0.55)	(0.55)	(0.54)	
ln(Earnings in Period 2)					9.71	9.78	9.67	-7.94
					(1.18)	(1.15)	(1.20)	
ln(Earnings in Period 2)					10.38	10.42	10.35	-9.49
Earnings in all Quarters					(0.46)	(0.46)	(0.46)	
ln(Earnings in Period 3)					10.19	10.23	10.16	-5.69
					(1.22)	(1.21)	(1.22)	
ln(Earnings in Period 3)					11.05	11.08	11.03	-5.50
Earnings in all Quarters					(0.40)	(0.40)	(0.40)	
Retained in Grade (Since Initial Test)	0.06	0.04	0.07	13.34	0.06	0.04	0.07	12.43
Economically Disadvantaged	0.45	0.39	0.48	19.99	0.44	0.38	0.48	18.03
Black	0.24	0.23	0.25	3.28	0.25	0.23	0.25	5.43
White	0.23	0.30	0.19	-26.43	0.25	0.32	0.21	-23.71
Hispanic	0.50	0.45	0.53	16.68	0.48	0.43	0.50	13.22
At Grade Level (Time of Initial Test)	0.53	0.61	0.48	-25.71	0.54	0.62	0.50	-23.02
Limited English Proficient	0.18	0.12	0.22	26.50	0.15	0.10	0.18	20.85
At Risk of Dropping Out	0.71	0.64	0.75	23.73	0.70	0.63	0.75	23.86
Male	0.43	0.44	0.43	-2.37	0.42	0.43	0.42	-2.18
Special Education	0.04	0.03	0.05	10.07	0.04	0.03	0.05	9.91
1991 Cohort	0.36	0.46	0.31	-31.03	0.37	0.46	0.31	-28.65
1992 Cohort	0.15	0.12	0.17	15.44	0.15	0.12	0.17	14.44
1993 Cohort	0.18	0.18	0.18	1.75	0.18	0.18	0.19	2.07
1994 Cohort	0.16	0.12	0.18	16.57	0.15	0.12	0.18	14.96
Haven't Passed Math	0.78	0.76	0.80	9.71	0.79	0.77	0.81	9.90
Haven't Passed Writing	0.19	0.10	0.25	39.63	0.17	0.09	0.23	33.94
Haven't Passed Reading	0.37	0.23	0.45	47.92	0.36	0.22	0.44	42.93
One Section Remaining	0.73	0.92	0.61	-74.26	0.74	0.93	0.63	-67.12

Notes: 42,957 observations in the full sample and 37,114 with valid SSN's (see Table 8 for the sample sizes relevant for log earnings). Standard deviations of non dichotomous variables in parentheses. See text for definition of earnings periods.
* - Only includes students taking all 3 sections on initial attempt.

Appendix Table 3: Probit Estimates of Discontinuity of Observable Characteristics at Passing Score

Characteristic	Initial Attempt		Summer 10th Grade		Fall 11th Grade		Spring 11th Grade		Summer 11th Grade		Fall 12th Grade		Spring 12th Grade		"Last Chance"	
Black	-0.001	-0.004	-0.003	-0.002	-0.006	-0.003	-0.002	0.004	0.012	0.014	-0.005	-0.001	0.007	0.014	-0.005	0.017
	(0.002)	(0.003)	(0.006)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.008)	(0.007)	(0.005)	(0.005)	(0.011)	(0.009)	(0.011)	(0.011)
White	-0.001	0.003	0.009	-0.001	0.008	0.001	-0.004	-0.007	0.007	0.003	0.015	0.013	-0.005	-0.007	0.009	0.005
	(0.003)	(0.003)	(0.009)	(0.009)	(0.004)	(0.003)	(0.005)	(0.004)	(0.009)	(0.008)	(0.006)	(0.006)	(0.008)	(0.008)	(0.010)	(0.011)
Hispanic	0.001	-0.007	-0.005	0.003	-0.001	-0.001	0.003	-0.000	-0.022	-0.023	-0.008	-0.011	-0.004	-0.007	-0.002	-0.025
	(0.003)	(0.003)	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)	(0.011)	(0.010)	(0.008)	(0.007)	(0.014)	(0.013)	(0.019)	(0.022)
At Grade Level (at Time	0.001	0.005	0.007	0.009	0.000	0.002	-0.006	-0.010	0.020	0.013	0.005	0.002	-0.009	-0.011	-0.018	-0.032
of Initial Attempt)	(0.004)	(0.004)	(0.008)	(0.007)	(0.004)	(0.004)	(0.006)	(0.005)	(0.008)	(0.007)	(0.005)	(0.005)	(0.011)	(0.010)	(0.008)	(0.009)
Limited English	-0.003	-0.004	-0.003	-0.001	-0.012	-0.009	-0.003	-0.001	-0.006	-0.005	-0.005	-0.005	0.011	0.008	-0.014	-0.022
Proficient	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.001)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.008)	(0.008)	(0.012)	(0.013)
Economically	0.005	-0.003	-0.008	0.001	-0.012	-0.012	-0.013	-0.006	0.006	-0.000	0.004	0.009	0.012	0.016	-0.016	-0.024
Disadvantaged	(0.003)	(0.003)	(0.009)	(0.007)	(0.004)	(0.004)	(0.006)	(0.004)	(0.009)	(0.008)	(0.008)	(0.006)	(0.008)	(0.007)	(0.021)	(0.020)
At Risk of Dropping	0.002	-0.009	-0.011	-0.006	-0.007	-0.005	-0.005	-0.003	0.021	0.014	0.003	0.004	0.003	0.002	-0.015	-0.017
Out	(0.006)	(0.006)	(0.007)	(0.007)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)	(0.008)	(0.011)	(0.011)
Male	0.007	0.002	0.002	-0.003	-0.001	-0.001	-0.002	0.002	0.014	0.016	0.002	0.005	0.018	0.008	-0.029	-0.041
	(0.007)	(0.006)	(0.005)	(0.005)	(0.002)	(0.002)	(0.006)	(0.005)	(0.010)	(0.008)	(0.011)	(0.012)	(0.008)	(0.007)	(0.010)	(0.009)
Special Education	-0.001	-0.003	-0.001	0.000	-0.000	-0.003	-0.006	-0.004	0.003	0.005	-0.001	0.000	-0.009	-0.011	0.002	0.005
	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.006)	(0.007)
1993 Cohort	-0.007	-0.009	0.013	-0.001	-0.012	0.007	0.011	0.000	-0.010	-0.013	-0.014	-0.019	0.007	-0.011	-0.013	-0.019
	(0.005)	(0.008)	(0.005)	(0.008)	(0.004)	(0.006)	(0.004)	(0.004)	(0.012)	(0.011)	(0.005)	(0.005)	(0.009)	(0.013)	(0.015)	(0.015)
1994 Cohort	0.006	0.004	0.003	0.008	0.004	-0.013	-0.006	-0.014	0.001	-0.004	0.003	0.012	-0.006	0.013	0.016	0.011
	(0.003)	(0.003)	(0.008)	(0.008)	(0.003)	(0.006)	(0.004)	(0.006)	(0.007)	(0.007)	(0.004)	(0.005)	(0.012)	(0.013)	(0.012)	(0.011)
Math Still Not Passed			-0.023	-0.023	-0.018	-0.014	-0.011	-0.011	0.001	-0.002	0.008	0.010	-0.014	-0.010	0.034	0.036
			(0.013)	(0.014)	(0.006)	(0.006)	(0.008)	(0.010)	(0.007)	(0.008)	(0.011)	(0.012)	(0.010)	(0.011)	(0.020)	(0.021)
Writing Still Not Passed			-0.004	0.018	-0.002	0.014	-0.006	0.015	-0.004	0.009	-0.010	0.004	-0.004	0.006	-0.016	0.000
			(0.009)	(0.014)	(0.006)	(0.010)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.006)	(0.008)	(0.007)	(0.011)	(0.007)
Reading Still Not Passed			-0.004	0.001	-0.017	-0.012	-0.004	-0.004	-0.003	-0.010	-0.010	-0.018	0.007	-0.002	-0.023	-0.024
			(0.005)	(0.007)	(0.006)	(0.008)	(0.007)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)	(0.008)	(0.007)	(0.017)	(0.016)
Only One Section Not			0.025	0.016	0.026	0.022	0.016	0.009	0.014	0.011	0.020	0.021	0.001	0.005	0.017	0.012
Passed			(0.006)	(0.007)	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)	(0.010)	(0.011)	(0.010)
Retained in Grade After					0.005	0.007	-0.001	0.001	0.005	0.005	-0.008	-0.006	0.005	0.007	0.001	-0.001
Initial Attempt					(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)	(0.005)
Specification of Test Score Polynomial	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic	Quartic	Quadratic

Notes: Each cell represents the estimated discontinuity in the outcome at the passing score for the test administration indicated in the column heading. Robust standard error: adjusted for clustering at the test score level in parentheses. See notes to Table 3for details.

Appendix Table 4: Estimated Discontinuities of Observable Characteristics - "Last Chance" Sample

Characteristic	Quartic Polynomial	Quadratic Polynomial
Valid SSN	0.011	0.006
	(0.004)	(0.004)
Black	-0.006	-0.002
	(0.008)	(0.009)
White	0.011	0.008
	(0.007)	(0.008)
Hispanic	-0.003	-0.009
	(0.011)	(0.012)
At Grade Level (Initially)	-0.012	-0.023
	(0.007)	(0.006)
Limited English Proficient	-0.018	-0.016
	(0.007)	(0.007)
Retained in Grade (Since Initial	0.002	-0.004
Attempt)	(0.004)	(0.003)
Economically Disadvantaged	-0.009	-0.009
	(0.008)	(0.007)
At Risk of Dropping Out	-0.023	-0.017
	(0.008)	(0.007)
Male	-0.013	-0.018
	(0.009)	(0.010)
Special Education	-0.002	-0.004
	(0.004)	(0.004)
1991 Cohort	-0.005	-0.006
	(0.008)	(0.007)
1992 Cohort	0.005	0.007
	(0.006)	(0.006)
1993 Cohort	-0.006	-0.012
	(0.009)	(0.008)
1994 Cohort	0.008	0.005
	(0.007)	(0.005)
Math Still Not Passed	0.005	0.008
	(0.018)	(0.021)
Writing Still Not Passed	-0.030	-0.010
	(0.008)	(0.009)
Reading Still Not Passed	-0.005	-0.017
	(0.011)	(0.012)
Only One Section Not Passed	0.023	0.021
	(0.007)	(0.007)
Initial Math z-Score*	0.002	-0.016
	(0.016)	(0.016)
Propensity Score (of Not Graduating)**	0.005	-0.004
	(0.003)	(0.002)

Notes: 42,957 observations. Each cell represents the estimated discontinuity in the characteristic at the passing score for the "last chance" test. Robust standard errors adjusted for clustering at the test score level in parentheses. Only students with valid scores for all unpassed sections included. Details on additional sample restrictions and regression specifications same as those described in notes to Table 3.

* - Only includes students with valid math scores on initial attempt (N=42,182)

** - Estimated from a probit of not graduating on a dummies for valid SSN, black, white, hispanic, at grade level at time of initial test, limited english proficient, economically disadvantaged, at risk of dropping out, male, special education, cohort dummies, dummies for sections unpassed at time of "last chance" test, and initial math z-score (N=42,182).