How Important is Labor Market Signaling? New Evidence from High School Exit Exams

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Abstract

Credentials-based tests of the signaling hypothesis assess whether, conditional on years of completed education, workers with an educational credential earn more than workers without one. While several studies have shown that they do, and while this has been interpreted as evidence in support of the signaling hypothesis, this interpretation will be misleading if workers with credentials are observably different (to firms) than workers without them. In this paper we exploit the introduction of high school exit exams to provide new credentials-based tests of the signaling hypothesis. Specifically, we use a regression discontinuity approach to compare the earnings of workers who narrowly fail the exam and do not obtain a diploma and workers who narrowly pass the exam and obtain one, an approach that should identify the diploma earnings premium irrespective of any other productivity information observed by firms. Using linked administrative data on education and earnings for large samples of individuals that attended Florida and Texas high schools, we show that while estimates based on the previous approach point to large diploma effects consistent with the signaling hypothesis, discontinuity-based estimates suggest much smaller effects, much less supportive of signaling.

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1 Introduction

When estimates of the return to education are used to justify public investment in education, two objections are commonly raised. First, it is said, these estimates will overstate the private return to education because education is likely positively correlated with unobserved determinants of earnings (“ability biases”). Second, it is said, even if these estimates do reflect the true private return to education, they will overstate the social return to education because education is, in part, a signal of underlying productivity (Arrow (1973), Spence (1973), Stiglitz (1975)).

In response to the first objection, many studies have tried to generate bias-free estimates of the private return to education. Surveying these, Card (1999) concluded that the true private return could be even larger than the partial correlations suggest. Fewer studies have tested the signaling hypothesis and different approaches have led to different conclusions. Approaches that find support for signaling include tests of whether education credentials increase earnings (as they would if they acted as signals) and tests of whether policy changes impact the educational decisions of students they do not directly affect (as they would if these students wished to differentiate themselves from the directly-affected students). Approaches that find less support include tests of whether the returns to education decline with labor market experience (as they would if firms gradually learned about ability) and tests of whether the private returns exceed the social returns to education (as they would if signaling dominated any positive externalities).

This paper focuses on the credentials approach to signaling. The first generation of these studies, beginning with Layard and Psacharopoulos (1974), tested whether a year of education that culminated in a credential generated a larger return than a year of education that did not. If human capital accumulates at the same rate across these two years and if the credential year is harder to complete, it should also generate a stronger signal and a higher return. The second generation of these studies, beginning with Jaeger and Page (1996), tested whether, conditional on

1 Alternatively, the ability to complete high school could be a signal of persistence.
years of completed education, workers with credentials earned more than workers without them. If workers with the same years of education are observably equivalent (to firms), any credential wage premium must reflect the signaling value of the credential. While both generations of studies have found evidence consistent with signaling both rely heavily on strong assumptions. If some years of education generate more human capital than others and if some determinants of diploma receipt conditional on years of schooling (such as passing courses) are positively correlated with other productivity signals (such as interview performance), estimates from both sets of studies could be biased upwards. The third generation of studies, beginning with Tyler et al (2000), estimate the wage premium associated with the General Educational Development (GED) degree by exploiting different GED passing standards across states. Tyler et al (2000) find that the GED sends a strong productivity signal, although a more recent study that exploits within-state variation in GED passing standards finds less support for signaling (Lofstrom and Tyler, 2007).

In this paper we exploit the introduction of high school exit exams to estimate the signaling value of a high school diploma. High school exit exams, statewide exams that students must pass before receiving a high school diploma, introduce uncertainty into the diploma acquisition process; this uncertainty ensures that some students narrowly pass these exams and receive a diploma while some students narrowly fail these exams and do not. Because we observe the exit exam score that determines whether or not students pass, we can use a regression discontinuity design to implement this threshold comparison and estimate the wage premium associated with earning a high school diploma. Under reasonable assumptions, the students on either side of the passing threshold will, on average, be observably equivalent to firms. As such, the diploma wage premium will reflect the extent to which firms use the diploma as a signal of underlying productivity.

Our study uses linked administrative data on education and earnings for students that were enrolled in Florida and Texas high schools. These offer two further advantages over the preceding literature. First, since sample sizes are large, we can estimate the earnings discontinuities precisely, document how they differ across groups and assess whether they decrease with time in
the labor market, as suggested by the employer learning approach to signaling. Second, since our data include information on any further education pursued, including GED attempts and college enrollment, we can assess whether further education accounts for any of the estimated earnings discontinuities. Earlier credentials studies avoided this issue by conditioning on terminal years of education. Since this could be endogenous to whether students obtain a diploma, this type of conditioning is likely to generate biased estimates of the diploma wage premium. For example, if college costs are lower for students with a high school diploma (because it is hard to attend college without one), among the students who choose not to enroll in college, those with a diploma may be observably (to firms) different to those without one. We tackle this problem by combining estimates of diploma effects on various types of further education with assumptions about the effects of this further education on earnings. This allows us to assess the contribution of further education to our estimated earnings discontinuities.

We begin the paper with a brief description of high school graduation requirements in the absence and the presence of high school exit exams. We argue that exit exams introduce uncertainty into the high school graduation process and ensures that high school graduation is determined by a variable that the econometrician (but not firms) can observe (the exit exam score). We then present a simple theoretical framework designed to clarify why the exit exam facilitates credible estimation of the credential wage premium and clarify how the resulting estimates should be interpreted. In regards to identification, the framework makes clear that if there is no uncertainty in the credential acquisition process, then the credential wage premium can only be identified under a strong assumption: that all productivity information held by firms is also held by the econometrician. With uncertainty, such as that introduced by exit exam requirements, we can base estimates on a threshold comparison that should identify the diploma premium irrespective of the productivity information held by firms. In regards to interpretation, the framework shows that with exit exams, the diploma earnings premium likely includes a component that reflects the productive effort expended in pursuit of the diploma. This implies that the premium observed in
an exit exam environment is likely an upper bound on the premium that would be observed in an environment that did not feature exit exams. It also complicates across-group comparisons of the diploma premium, since these might reflect across-group differences in the costs of productive effort (e.g., driven by differential school quality).

We then provide a detailed description of the data and present our estimates of the diploma earnings premium. When we use the approach taken in the previous credentials studies (i.e., earnings comparisons of those with and without the diploma conditional on completed years of schooling) we obtain estimates consistent with those studies: diploma premia between 10 and 20 percent. When we use our preferred approach and focus on the earnings discontinuity at the exit exam passing threshold we obtain estimates an order of magnitude smaller. Since we estimate small diploma effects on the probability of pursuing different types of further education, our estimates are robust to a wide range of assumptions regarding the earnings effects of these courses.

We then discuss how our findings relate to the wider signalling literature and discuss two explanations for why high school diplomas might send relatively weak productivity signals. First, it may be that high school graduation criteria do not predict productivity. This could be because job performance depends on one set of skills (e.g., non-cognitive skills) while the high school diploma distinguishes students according to another (e.g., cognitive skills). We reject this explanation on account of the strong correlation that we find between earnings and exit exam scores (even conditional on completed years of schooling). Second, it may be that a high school diploma predicts productivity, but does not predict productivity conditional on the other information available to firms. This could be because firms observe productivity directly (e.g., via entry tests), because firms observe high school performance (assuming this is correlated with productivity) or because firms learn about productivity quickly, even if they cannot observe it upon labor market entry. Whatever the explanation for why the diploma does not add significantly to the productivity information held by firms, our findings undermine a key pillar supporting the signaling
interpretation of the returns to school, at least in so far as this relates to the high school years completed by the workers now in their twenties and thirties. It is possible that a high school diploma would send a stronger signal if the market for non-college bound labor was structured differently (e.g., if the jobs performed by these workers revealed less productivity information). It follows that college degrees might send stronger productivity signals, as might high school diplomas in other countries.

2 High School Graduation Requirements

In the absence of exit exams, high school graduation is usually conditional on the accumulation of a certain number of credits, where these credits depend on passing courses, and where the passing criteria for these courses are course- and school-specific. Some courses might, for example, require only that students attend class. Others might require that students complete course papers and submit homework.

These requirements ensure there is little uncertainty in the diploma acquisition process. Students are generally aware of the graduation requirements and the course requirements on which they are based. Even if a student inadvertently fails to meet one of these course requirements (e.g., forgets to complete a course paper), she can typically rectify this by, for example, completing a course paper later in the course. Ultimately, this lack of uncertainty stems from high schools’ desire not to deny diplomas to students prepared to stay in school until the end of twelfth grade. Others allege that the same motivation causes schools to "dumbed down" the graduation requirements until they represent little more than time serving (Bishop). This was the original motivation for the introduction of high school exit exams, which also require students to demonstrate a minimum level of academic competence.
2.1 High School Exit Exams

Since we present only the Texas results in this draft, we describe only the Texas exit exam system. In future drafts we will describe the exit exams operating in Florida and add estimates based on the Florida data. The exit exam system is similar across the two states.

The class scheduled to graduate high school in 1987 was the first to be subject to a Texas exit exam requirement. In addition to the types of course-passing requirements described above, this meant these students had to pass a state-designed test of “higher-order thinking and problem-solving” skills (TEA, 2003). Since the exam was reformed in 1990 (all of our sample were subject to the post-1990 requirements), the exit exam has consisted of three sections – reading, math and writing – each of which is multiple-choice (the writing test also contains an open-ended essay component) and each of which must be passed before a diploma is received. State law sets a passing standard of 70 percent for the reading and math sections; the passing criterion for the writing section combines the multiple-choice and essay components.

The exit exam requirement introduces some uncertainty into the high school graduation process, since students do not know whether they have passed the exam (and are eligible for a diploma) until after they are informed of the exit exam results. The exam also ensures that we can, in principle, observe the determinants of whether a student earns a high school diploma (the exit exam score). As discussed below, these features mean that we can use the exam to estimate the signaling value of the diploma, by comparing the earnings of those that narrowly failed the exit exam (and did not receive a diploma) and the earnings of those that narrowly passed the exit exam (and did receive a diploma).

There are, however, two features of these exams that complicate the analysis somewhat. First, students that fail one or more sections of this first administration can retake the failed sections during a subsequent administration. These occur in the fall and spring of eleventh and twelfth grades and in a special administration for seniors given in April or May. We refer to this as
the “last-chance exam”. Second, even at this last chance exam there exists some slippage in
the relationship between whether students pass and whether they receive a diploma. There are
several sources of "imperfect compliance". First, some students are exempt from the exit exam
requirement. In particular, special education students may apply for exemptions from the entire
exam or certain sections of it. 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. Second, students can pass the exam at an administration that occurs after the last-chance
exam. In other words, the last-chance exam is not a student’s last chance to pass. We refer to
it as the last-chance exam because it is a student’s last chance to pass in order to graduate “on
time” and because many of the students that do not pass this exam do not attempt to pass again.
Despite these complications, we argue in the theoretical framework below that a modified version
of the basic threshold comparison can still be used to estimate the signalling value of the diploma.
In the section describing the empirical strategy, we outline exactly how this is done.

3 Theoretical Framework

In this section we outline a simple theoretical framework. We use this framework to clarify why
exit exams facilitate more credible identification of the credential wage premium and to clarify
the interpretation of this estimated wage premium.

3.1 Credential Acquisition Under Certainty

As already noted, the key feature of exit exams is that they introduce uncertainty into the high
school graduation process. To show why this is important, we begin with a simple baseline model
of credential acquisition under certainty. The model assumes that individuals live for two periods.
In the first period they attend school, in the second period they work. We assume individuals
are perfect substitutes in production and we assume there are a large number of risk-neutral
firms competing for their services. This implies that the second-period wage equals expected productivity. We characterize individuals in terms of a one-dimensional index of ability denoted $a$ drawn from a distribution with density $f(a)$. We assume that ability is known to individuals at the start of the first period but is never observed by firms.

We start with a general model of high school graduation requirements that encompasses the requirements that exist in the presence and absence of an exit exam. Specifically, we assume that students graduate and earn a diploma when a one-dimensional measure of high school performance $p$ exceeds some threshold level $P$. The one-dimensional nature of $p$ is unrestricitive. In a setting without exit exams this could refer to the minimum of the number of credits accumulated and the number of days attended (assuming these two criteria determined whether a student graduated); in the setting with exit exams it could refer to the minimum of the math, reading and writing scores. We assume that $p$ is determined by the following equation:

$$p = \gamma_0 a + \gamma_1 s$$

where $s$ is the amount of studying done in the first period. We think of studying as encompassing various behaviors and activities that make high school graduation more likely given ability. In the absence of exit exams, studying could correspond to attendance; with exit exams, it could correspond to test preparation. We assume that studying is costly for all individuals and is, potentially, a function of ability. We can parameterize this dependence as:

$$C(s) = (c + \gamma_2 a)g(s); g(0) = 0, g'(s) > 0$$

The costs of obtaining a diploma can be written as a function of ability $C(\bar{s}(a))$, where $\bar{s} = \frac{P - \gamma_0 a}{\gamma_1}$ if $a \leq \frac{P}{\gamma_0}$ and $\bar{s} = 0$ if $a > \frac{P}{\gamma_0}$. Let the indicator $D$ denote whether a worker obtained the diploma:

$$D = 1 \text{ if } p \geq P$$
We assume that second-period productivity (denoted $\pi$) is determined by a simple but general function of ability and studying:

$$
\pi = a + \gamma_3 s
$$

This captures the idea that productivity is related to underlying ability (since $\frac{d\pi}{da} = 1$). It also implies that the studying maybe valued by firms (if $\gamma_3 > 0$).

The framework described by these equations encompass a number of special cases. For example, if $\gamma_0 = 0, \gamma_1 = 1, \gamma_2 < 0$ and $\gamma_3 = 0$ we have the classic signaling idea that students obtain a diploma by engaging in unproductive studying, the costs of which are decreasing in ability. If $\gamma_0 = \gamma_1 = 1, \gamma_2 = 0, \gamma_3 = 1$, then second-period productivity equals high school performance and diploma status is a direct signal of productivity. This captures the idea that exit exams can induce students to engage in productivity-enhancing studying that they would not exert in their absence (c.f., Costrell (1994), Betts (1998)). Indeed, this special case can be seen as a greatly simplified version of the Betts (1998) model.

It turns out that under certainty, the credential wage premium cannot be identified if firms observe more productivity information than the econometrician. To illustrate this result, we consider this basic model under two assumptions regarding firm information.

**Firms Observe Only Diploma Status ($D$)**

Assuming that firms only observe $D$ (i.e., whether a worker obtained a diploma), it follows that there can be at most two wages paid on the labor market: a wage for those with the diploma and a wage for those without it. This implies that in this simple baseline model, the diploma wage premium is a constant equal to $\Delta W = W(D = 1) - W(D = 0)$.

Consider the study responses to a given diploma wage premium $\Delta W_0 > 0$. Assuming $\gamma_0 > 0$, the study required to attain the diploma $\pi(a)$ is decreasing in ability (over the ability range $[0, \frac{W}{\gamma_0}]$). Assuming $\gamma_2 \leq 0$, this implies that the cost of attaining the diploma is decreasing in ability. This implies that there will be an ability cutoff $a_0$ such that workers with ability $a < a_0$ will not study.
and will not obtain the diploma while workers with ability \( a \geq a_0 \) will engage in study \( s(a) \) and obtain the diploma. Study levels other than \( s = 0 \) and \( s = \bar{s} \) will never be observed, since the wage depends solely on whether workers have a high school diploma.

Given these effort strategies, a Nash equilibrium in this game is an equilibrium credential wage premium \( \Delta W_\ast \) that induces a cutoff \( a_\ast(\Delta W_\ast) \) that ensures that this premium equals the expected productivity difference between those with and without the credential. In equilibrium, such a wage premium must satisfy:

\[
\Delta W_\ast = E[\pi|D = 1] - E[\pi|D = 0]
\]

\[
= \{E[a|a \geq a_\ast(\Delta W_\ast)] - E[a|a < a_\ast(\Delta W_\ast)]\} + \gamma_3 E[\bar{s}(a)|a \geq a_\ast(\Delta W_\ast)]
\]

where the first term captures the difference in average ability between those with and without the diploma and the second term captures the average additional effort exerted by those that obtained the diploma. These equilibrium effort and wage levels are illustrated in Appendix Figure 1.

**Firms Observe Diploma Status and Other Productivity Signals**

In practice, firms are likely to observe other signals of productivity. Suppose in particular that firms observe a noisy signal of productivity \( \pi^s = \pi + \epsilon \), where \( \epsilon \) is a random variable assumed to be independent of \( \pi \) with mean zero and variance \( \sigma^2_\epsilon \). Since firms will predict productivity using both \( \hat{\pi} \) and \( D \), there will be two wage functions offered on the labor market: \( W(D = 1, \pi^s) \) and \( W(D = 0, \pi^s) \). Provided \( \sigma^2_\epsilon \) is finite, \( \pi^s \) will provide some information about \( \pi \), hence \( \frac{d}{d\pi^s} W(D, \pi^s) > 0 \). Provided \( \gamma_3 > 0 \), this implies that there will exist incentives to exert effort even for students for whom high school performance is too low to obtain a diploma and for students that can obtain a diploma with zero effort. Provided \( \sigma^2_\epsilon > 0 \), \( \pi^s \) will not be completely predictive of \( \pi \) and \( D \) will provide additional productivity information. In this case, the credential wage premium - the premium paid to workers with \( D = 1 \) conditional on
\( \pi^* \) - will be positive. That is, \( W(D = 1, \pi^*) > W(D = 0, \pi^*) = \Delta W(\pi^*) > 0 \). This implies that the expected credential wage premium for students with performance close to \( P \) is positive: 
\[
E[\Delta W(P)] = E[W(D = 1, p = P)] - E[W(D = 0, p = P)] > 0.
\]
In turn, this implies that for students with high school performance close to \( P \), there is an extra return to obtaining the diploma, such that some students exert the additional effort required to obtain it. This implies that for a given wage structure as characterized by \( E[W(D, p)] \), study levels will jump up to \( \overline{\pi} \) at some ability level \( \alpha_2 \) below \( \frac{P}{\gamma_1} \). Equilibrium study responses must imply productivity differences that ensure the wage functions generate zero profits. The study and wage functions that characterize this type of equilibrium are illustrated in Appendix Figure 2.

### 3.2 Credential Acquisition under Uncertainty

We now wish to contrast these cases with the case in which there is some uncertainty in the credential acquisition process. There are at least three ways to introduce uncertainty into this process. First, students may be imperfectly informed about ability (\( a \)). Then, even if high school performance is a deterministic function of ability and study, students cannot forecast performance exactly. Second, students might not know how productive will be their study effort (i.e., \( \gamma_1 \) could be a random variable whose distribution varied across students). Third, performance itself could be stochastic - a function of ability, study and a random component, where the random component could be driven by unanticipated factors such as the quality of a student’s teacher.

To take the simplest of these cases (the other cases have the same implications), suppose that high school performance is stochastic, such that students obtain a diploma if \( p = \hat{p} + \eta > P \), where \( \hat{p} = \gamma_0 a + \gamma_1 s \) and \( \eta \) is a random variable assumed to be independent of \( \hat{p} \) with mean zero and variance \( \sigma^2_\eta \). Provided the variance of \( \eta \) is sufficiently high, all students will obtain a diploma with probability between zero and one. To see how this affects study choices, return to the case in which education credentials are a firm’s only productivity signal and assume the credential wage premium is \( \Delta W_0 \). The expected return to the non-stochastic component of performance is
\( B(\hat{p}) = P(D = 1|\hat{p})\Delta W_0 \). This implies that the marginal return to study effort is \( \gamma_1 \frac{dP}{d\hat{p}} \Delta W_0 = -\gamma_1 f_\eta(P - \hat{p})\Delta W_0 \), where \(-f_\eta()\) is the impact of an additional unit of effort on the probability of obtaining the diploma. If \( \eta \) is normally distributed, then for an individual with ability \( a \), \(-f_\eta()\) will be an inverted U shape with a maximum at \( s = \frac{P - \gamma_0 a}{\gamma_1} \) (i.e., \( \hat{p} = P \) and \( P(D = 1) = 0.5 \)).

Intuitively, since small shocks are more frequent than large shocks, additional effort will have the biggest impact on the probability of obtaining a diploma when performance is most sensitive to small shocks (i.e., when \( \hat{p} = P \)). This implies that if study costs are unrelated to ability (\( \gamma_2 = 0 \)), the individual with ability \( a_3 \) will exert most effort, where \( C'(\hat{p}(a_3)) = C'(P) = \gamma_1 f(0)\Delta W_0 \). All others exert less effort, but effort will be a smooth function of ability. Because uncertainty affects study choices, it will also affect firms’ productivity inferences. Again, an equilibrium premium is one that induces effort choices such that the premium equals the difference in expected productivity between workers with and without the credential. This type of equilibrium will feature effort choices and wage levels similar to those depicted in Appendix Figure 3.

### 3.3 Identification and Exit Exams

In both cases of the model under certainty (Appendix Figures 1 and 2), we have a "no overlap" result. That is, there is no pair of workers that have different values of \( D \) and the same value of \( E[\pi^s] \). In general (e.g., in the second case), this implies that we cannot identify the credential wage premium when there is no uncertainty in the credential acquisition process. In fact, in the special case in which firms only observe \( D \) then we can identify the premium, by comparing the average wages of those with and without the diploma. This is strategy pursued in the generation II studies of credential wage premia. The assumption that firms only observe \( D \) (more generally, do not observe more information than the econometrician), is a very strong one. When this is violated, this approach will overstate the credential wage premium by attributing some of the returns to \( \pi^s \) to \( D \).

In the model under uncertainty (Appendix Figure 3) there exist two type of overlap. First, we
can find workers with the same level of ability hence the same level of study and the same expected productivity who differ in terms of their diploma status (because this is to some extent random). Second, we can find workers with high school performance on either side of the performance threshold $P$ who differ in diploma status but have (approximately) the same level of expected productivity. This overlap exists regardless of the productivity information held by firms.

Since high school exit exams introduce uncertainty into the diploma acquisition process, they can, potentially, be used to identify the credential wage premium. Since they also allow us to observe the variable determining diploma status (i.e., high school performance $p$), they facilitate identification of the credential wage premium via a threshold comparison of workers with performance slightly above $P$ (who obtained a diploma) and workers with performance slightly below $P$ (who do not). In the section below, we describe how a regression discontinuity approach can exploit this threshold comparison.

### 3.4 Interpretation and Exit Exams

With regards to the interpretation of the credential wage premium, notice that when $\gamma_3 > 0$ the premium will include a component that reflects the additional studying of those that receive the diploma. This is driven by the "sorting-cum-learning" nature of the model with $\gamma_3 > 0$, which is in the spirit of the model analyzed by Weiss (1983). In other words, while the model allows individuals to signal their underlying abilities (as in the classic signaling models of Arrow (1973), Spence (1974) and Stiglitz (1975)), signalling can be done by productivity-enhancing study.

The introduction of this study component has two implications. First, it implies that the credential wage premium will depend on the structure of study costs. This implies, for example, that if different groups (e.g., black and white students) have different study costs (e.g., because of differences in school quality), these can generate differences in the group-specific premia irrespective of the other factors typically thought to determine these (c.f., Belman and Heywood (1991)).

The second implication of this study component is that it implies that the credential wage
premium will vary with correlation between high school performance and productivity. When performance is redefined (as it is when the graduation regime is changed to include an exit exam), then the credential wage premium will change. It seems reasonable to assume that high school performance as required with an exit exam (i.e., math and reading scores) is more strongly correlated with productivity than high school performance as required without an exit exam (i.e., high school course credits). Subject to some caveats, we might expect the wage premium estimated in a setting with exit exams to be an upper bound on the wage premium that would be estimated in the setting without exit exams. One caveat is that improvements in the passing score might be temporary (i.e., not driven by cramming and/or test preparation), such that $\gamma_3 = 0$. Another caveat is that the introduction of an exit exam will likely change the composition of those who obtain the diploma (i.e, change the ability cutoff in the above models). Depending on the ability distribution, this could outweigh any effects of changing the basis on which high school performance is measured.

### 3.5 Exit Exam Complications

We have shown that when high school requirements include an exit exam, a threshold approach can identify the credential wage premium irrespective of the information held by firms. We now consider whether this threshold approach is robust to how exit exams operate in practice.

*Multiple chances to pass*

In the framework outlined above, student performance is measured at the end of the high school. With exit exams, this implies these are taken once, at the end of a student’s high school career. In practice, high school students have multiple opportunities to take the exit exam. To consider the implications of these multiple opportunities, suppose the exam is offered $L$ times over the course of a student’s high school career, where the $L’th$ test is the "last-chance" test. Suppose a student that passes on the $i’th$ administration of the test receives a diploma, while a student
that fails on the $i^{th}$ administration can retake on the $(i + 1)^{th}$ administration (provided $i \leq L - 1$). These retake opportunities have implications for both the interpretation and identification of the credential wage premium.

With regard to interpretation, multiple testing will change the equilibrium wage premium because it will change the incentives to exert effort and, therefore, the inferences made by firms given the productivity signals they observe. For example, suppose educational credentials are the only productivity signal that firms observe. Then, for a given credential wage premium, an individual’s effort strategy will consist of an optimal effort level $e_1$ for the first exam, an optimal effort level $e_2$ for the second exam (assuming the first exam was failed), up to $e_L$ for the last exam. If the test outcome is certain, students do not gain from additional exams, since they only pass if they exert effort $e \geq T - a$ on at least one exam. With uncertain exam outcomes, students can gain from the possibility that they may, by chance, pass an early exam with minimal effort. As such, we would expect effort choices to satisfy $e_1 < e_2 < \ldots e_L$. While these effort strategies complicate the calculation of expected productivity given credential information, the equilibrium with multiple testing will be qualitatively similar to the equilibrium with a single test. In particular, it must consist of a credential premium that induces effort choices that ensure this premium equals the expected productivity difference between those with and without the credential. The intuition extends to the cases in which firms observe additional productivity signals.

With regard to identification, assuming firms cannot observe the number of times that the test was taken, then a comparison based on the last-chance sample would still identify the credential wage premium. Without this assumption, a threshold comparison can still be used, but the estimated premium will be specific to this (observable) group of students. Since this group would, presumably, be less heterogeneous than the wider group of high school students, we might expect this credential wage premium to be small. We are reasonably confident that firms cannot observe

\footnote{Depending on the type of uncertainty (e.g., over ability), workers’ effort decisions may depend on the score obtained on the previous tests.}
this information and, in the discussion and interpretation section, we provide evidence in support of this claim.\(^3\)

\textit{Imperfect compliance}

In practice, as noted above, there is some slippage between passing the exam and obtaining a diploma, even among students in the last-chance sample. Some students that fail the last-chance exam are awarded a diploma by virtue of being exempt from the passing requirement. Others re-take and pass the exam later, typically by returning to high school for a "thirteenth" year. Some students that pass the last-chance exam are denied a diploma because they have not fulfilled the other requirements for high school graduation. Again, this type of imperfect compliance has implications for the interpretation and identification of the credential wage premium.

With regard to interpretation, the existence of future retake opportunities has no implications beyond those discussed in the context of multiple testing. The existence of high school exemptions has slightly different implications. To see these, recall the equilibrium in the model in which high school performance is certain and firms observe only diploma status (Appendix Figure 1) and assume that a fraction of those that fail the test are re-classified as exempt and given a credential. This will reduce the difference in expected productivity between those with and without a credential which must reduce the credential wage premium. This will reduce optimal study levels which will further reduce expected productivity differences and the credential wage premium. This process stops when the study choices induced by the credential premium ensure this premium equals the expected productivity difference between those with and without the credential. This suggests that the impact of exemptions will depend on the overall fraction of students exempted. In both of the states analyzed in this paper, this fraction is small. Other types of slippage, such

\(^3\) In particular, we estimate the wage impact of narrowly failing the first test. We show that this has no impact on the probability of graduating high school but does impact the number of times the test is taken. If firms could observe this, they would use it as a signal of productivity and pay workers that passed at the first attempt more than workers that took two or more attempts. Since we find no wage effect of passing the first test, we find no evidence of this type of behavior.
as students that pass the exam being denied the diploma for not completing other requirements will have similar implications.

With regard to identification, imperfect compliance can be handled via a modified version of the threshold comparison on the last chance sample. In particular, we can scale up the earnings difference at the last chance passing threshold with the difference in the probability of obtaining a diploma as observed at the last chance passing threshold. For example, if the probability of obtaining a diploma is one for those that pass the last-chance exam and one half for those that fail (because some pass on a later attempt and some are exempt), we simply double the estimated last-chance wage premium to get a consistent estimate of obtaining the diploma. The intuition is that the threshold comparison is still a good one (the individuals either side should have equal levels of expected productivity), but the imperfect compliance, unless corrected, causes us to misclassify students and underestimate the credential wage premium.

*High school dropout and further education*

All of the above analysis assumed that students complete high school but do not pursue further education. In practice, regardless of high school graduation requirements, students can drop out of education before the end of high school or continue their education beyond it.

The assumption of no dropout is easy to relax. If firms can observe years of schooling, hence can observe which students drop out, the main consequence will be to reduce the credential wage premium by effectively truncating the left tail of the productivity distribution. The threshold approach will continue to provide consistent estimates of this premium, since any other productivity signals observed by firms will be the same (on average) for the group that narrowly passes and the group that narrowly fails. If firms cannot observe years of schooling, hence cannot observe which students drop out, the high school diploma signals test scores and high school completion. Proper consideration of this case would require an extended analysis that modeled student dropout behavior. In turn, this would require assumptions about the costs and productivity effects of ad-
ditional time spent in school. Without specifying such a model, it seems reasonable to expect that these considerations would generate larger credential wage premia and that these will be consistently estimated by threshold comparisons. The intuition for larger premia is that students that did not complete high school would choose to drop out at the earliest opportunity. Assuming human capital increases with time in high school (even if only slightly), this will increase the expected productivity difference between those with and without the credential. The intuition for the consistency of the threshold comparison is the same as before: any other productivity signals observed by firms will be the same (on average) for the groups on either side of the threshold. We assume that firms can observe years of schooling hence can observe which students drop out. This assumption was also made in the previous credentials literature.

The assumption of no further education is more problematic. To see why, suppose that only students that obtained a high school diploma were able to attend college. Further, suppose that one half of the students that obtained a high school diploma attend college and suppose that college attendance is observed by firms. This would have implications for the credential wage premium and for the strategies used to identify it. We would expect it to reduce the credential wage premium to the extent that it lowers average productivity among the group that earn a high school diploma. That will reduce the expected productivity difference between those with and without the diploma, reduce the wage premium and reduce effort until a new equilibrium premium is reached.

With regard to identification, strategies that ignore college enrollment and strategies that deal with it by excluding students that enroll in college could both generate upwards-biased estimates of the premium. The intuition for the first case is that ignoring college enrollment will lead us to load some of the returns to college onto the credential wage premium. The intuition for the second result is more subtle. To see it, suppose that in addition to observing worker’s educational credentials, firms receive a second productivity signal that is either good or bad with probability related to productivity. The groups on either side of the threshold have similar productivity hence
will contain similar proportions of good-signal workers. If, however, students with diplomas are more likely to attend college if they receive a bad signal, then among the subsample of threshold students that do not go to college, the students that receive the diploma will contain a larger fraction of good-signal workers. This will cause us to over-estimate the credential wage premium, although it is easy to tell other types of selection stories that lead it to be under-estimated.

A similar argument can be made in regards to GED certification for those that do not receive a diploma. Since the workers that obtain GED certification may be more productive than the average members of this group, GED certification may increase the credential wage premium. With regard to identification, ignoring GED certification will cause us to under-state the premium. Restricting the sample to students that do not pursue GED certification could result in downwards-biased estimates of the premium if GED certification is pursued by students that obtain worse productivity signals.

3.6 Summary

To summarize, we have shown that when there is no uncertainty in the high school graduation process, the credential wage premium can only be identified under very strong assumptions: that firms do not observe more productivity information than the econometrician. Under uncertainty, the credential wage premium can be identified under weaker assumptions; when the uncertainty is generated via high school exit exams, the premium can be identified via a threshold comparison of those that narrowly pass and narrowly fail the exit exam.

The practical operation of these exams requires us to adopt a modified version of this threshold comparison, but this modified version still identifies the credential wage premium of interest. In particular, the modified comparison involves three steps. First, make a threshold earnings comparison on the "last chance" sample of students. Second, scale up the threshold earnings effect using a threshold comparison of the effect of passing on the probability of obtaining a diploma. Third, use a threshold comparison of the effect on the probability of pursuing various
types of further education to assess the contribution of further education to the estimated diploma premium. To preview our results, we find that among the last chance sample of students, passing the exam has relatively small effects on the probability of pursuing further education. As such, we conclude that any further education impacts are likely to be small.

4 Empirical Framework

In this section we apply the insights of these theoretical framework to a more general model of wage determination. This forms the basis of our empirical strategy. Without loss of generality, we assume that firms observe two productivity signals – whether workers hold a high school diploma ($D$) and a noisy measure of productivity ($\pi^*$). As discussed above, the weights placed on these signals will depend on the accuracy of the productivity signal. We also assume that firms observe years of completed schooling, such that the following equation can be considered as a linear approximation to the wages of those with at least 12 years of schooling, where $C$ denotes whether those workers attended college:

$$W_i = \beta_0 + \beta_1 D_i + \beta_2 C_i + \beta_3 \pi^*_i \quad (1)$$

The approach taken in the previous literature is to regress wages on diploma status and completed years of education ($C$). To see what this identifies, write the linear projection of $\pi^*$ on $D$ and $C$ and substitute this into the above equation:

$$W_i = \beta'_0 + (\beta_1 + \beta_3 r_1)D_i + (\beta_2 + \beta_3 r_2)C_i + \varepsilon_i$$

where the $r$'s are the projection coefficients and $\varepsilon$ is the projection error. If $\beta_3 > 0$ (i.e., firms observe other productivity signals), this procedure could generate biased estimates of $\beta_1$. The extent of the bias depends on $r_1$, the partial correlation of $\pi^*$ and $D$ conditional on $C$. There
are two reasons why \( r_1 \) may be non-zero. First, \( D \) may be predictive of \( \pi^s \), whether or not \( C \) is conditioned on. That is, workers with a high school diploma may have characteristics predictive of productivity observed by firms but not the econometrician. In the theoretical framework discussed above, that will be the case if \( p \) and \( \pi \) are positively correlated, which they will be unless \( \gamma_0 = \gamma_2 = \gamma_3 = 0 \). Second, \( D \) may be uncorrelated with \( \pi^s \), but predictive of \( \pi^s \) conditional on \( C \). This represents the endogeneity bias introduced by conditioning on years of education. For an example of how this could arise, suppose that both \( D \) and \( \pi \) have positive causal effects on \( C \). In that case, those observed with \( C = 1 \) and \( D = 0 \) must have especially high values of \( \pi \). In that case, \( D \) will be predictive of \( \pi^s \) conditional on \( C \).

In contrast to this previous approach, we seek to identify the causal effect of \( D \) on \( W \) conditional on \( \pi^s \) but not \( C \). In other words, we seek to estimate the parameter \( \beta_1'' \) in the following equation, arrived at by substituting the linear projection of \( C \) on \( D \) and \( \pi^s \) into equation (1):

\[
W_i = \beta_0'' + (\beta_1'' + \beta_2'p_1)D_i + (\beta_3'' + \beta_2'p_2)\pi_i^s + v_i
\]

\[
= \beta_0'' + \beta_1''D_i + \beta_3''\pi_i^s + v_i \quad (2)
\]

where the \( p' \)‘s are the projection coefficients and \( v \) is the projection error. A regression of \( W \) on \( D \) would still generate biased estimates of \( \beta_1'' \), because it would not account for \( \pi^s \). As discussed above however, the threshold approach can solve this problem. The reason is that the ability of those with scores slightly above and slightly below the passing threshold should, on average, be the same.

Rather than compare the earnings of two (small) groups around the passing threshold, we will identify \( \beta_1'' \) using a regression discontinuity approach that uses data away from the passing threshold. If compliance was perfect, this would involve a regression of \( W \) on \( D \) and a flexible function of \( p \):

\[
W_i = \beta_0''' + \beta_1''D_i + g(p_i) + v_i' \quad (3)
\]
This equation can be derived from equation (2) via substitution of the linear projection of $\pi^*$ onto $D$ and $g(p)$. This equation would identify $\beta_1''$ provided the projection coefficient on $D$ is zero. With perfect compiance, this would correspond to the condition that $\pi^*$ is a smooth function of $p$. Although $\pi^*$ is not observed (to the econometrician), as discussed above, we expect $\pi^*$ will be smooth function of $p$.

Since we have imperfect compliance, the condition that $\pi^*$ is a smooth function of $p$ does not ensure the projection coefficient on $D$ is zero. Intuitively, among those that narrowly fail the exam, there may be differences in the productivity signals associated with those that subsequently obtain (and do not obtain) a diploma. Rather than estimate equation (3), we will instead use passing the exam as the excluded instrument in a two-stage least squares procedure. That is, we will instrument for $D$ using the following "first-stage" equation:

$$D_i = \alpha_0 + \alpha_1 \text{PASS}_i + g(p_i) + \omega_i \quad (4)$$

This two-stage least squares procedure will identify $\beta_1''$ provided $\pi^*$ is a smooth function of $p$.

Note that this regression discontinuity approach will identify $\beta_1''$ in (2) but will not identify $\beta_1$ in (1). The difference between $\beta_1''$ and $\beta_1$ is $\beta_2 p_1$. The parameter $p_1$ is the coefficient on $D$ in the linear projection of $C$ on $D$ and $\pi^*$. This is the causal effect of $D$ on $C$ and can be identified via a procedure similar to the one used to estimate $\beta_1''$ in equation (3). The parameter $\beta_2$ is the causal effect of $C$ on $W$. Although this is unknown, we can use estimates of $p_1$ and assumptions about $\beta_2$ to bound the contribution of $C$ to $\beta_1''$. To preview our findings, we estimate $p_1$ to be small. This implies that for a wide range of values of $\beta_2$, $\beta_1$ and $\beta_1''$ will be similar.

4.1 Estimation Issues

As shown above, the regression discontinuity approach can identify the diploma wage premium with minimal assumptions regarding the other productivity signals observed by firms. In imple-
menting this regression discontinuity approach however, various choices must be made. In this
version of the paper we take a fairly standard approach to these issues (see for example Imbens
and Lemieux (2008)). In particular, we use a "global polynomial" approach that uses a wide range
of data and allows \( g(.) \) to be a high-order polynomial (e.g., a fourth-order polynomial). We check
the validity of our functional form assumptions by visual comparison of the fitted functional form
and the raw data. We also experiment with adding covariates to equations (3)-(4) in order to
improve this fit.

5 Data

In this version of the paper we present analyses based on Texas data. In future versions we will
also present analyses based on Florida data. The Texas 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. 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.

The datasets used in this analysis are part of the the Texas Education Agency’s (TEA) records.
These files have information on enrollment, attendance, dropout status, GED acquisition, and
performance on the exit-level TAAS. We examine five cohorts of test takers, students in 10th
grade in the spring of 1991 through 1995. Linking multiple test records on the basis of a unique
student identifier, we 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.

The main 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. We refer to this administration as the “last chance” test. For the 1992-1995 cohorts, last
chance test was a special test given in May or April to seniors who had 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. Despite our labelling these as "last chance" tests, students could have re-taken the tests at a later date, typically by returning to high school for a thirteenth year of study. We label the tests "last chance" since they were students’ last chance to pass in order to graduate on time and because many of the students that failed these tests did not retake them later. The final step in creating the datasets involves merging the test data to the TSMP files that have information on student outcomes. In Appendix A we discuss possible errors in the matching process.

The main outcomes used in this paper are graduation status, further education acquisition and earnings.

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. We refer to this year as the expected date of high school completion.

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. We 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.
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. In addition to looking at attendance, we also examine the amount of post-secondary schooling acquired. Using the THECB’s data on degrees conferred by Texas post-secondary institutions, we 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, we look at the number of semester credit hours a student enrolled in up to five years after the expected high school completion date.

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. 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 their accuracy, which constrasts will with survey data (Jacobson, Lalonde, and Sullivan, 1993). The main limitation is our inability to construct an hourly wage measure. Instead, we must estimate effects on total earnings. This will conflate diploma effects on wages with diploma effects on labor supply.

Although this may at first glance seem like a disadvantage, there are four reasons why it need not be. First, since we are interested in how much of the labor market return to education is attributable to its role as a productivity signal, we can compare diploma earnings effects to
estimates of the earnings effects of a year of school. Second, since we are interested in whether previous estimates of the credential wage premium were driven by ability biases, we can compare discontinuity-based estimates of the earnings effects of the diploma with estimates obtained using the approach taken in the previous literature. The generation III studies, especially Tyler et al (2000), also estimate effects on total earnings. Third, as noted by Altonji & Pierret (2001), since firms may base hiring decisions on productivity signals such as a high school diploma, the labor supply channel is one of independent interest. Fourth, a cost-benefit analysis of the impact of high school exit exams should include as an input the earnings impact of passing the exam and obtaining the diploma. Intuitively, the larger is the diploma earnings premium, the larger are the effort incentives facing high school students.

5.1 The Last Chance Sample

Since our analysis is based on the last chance sample it is important to understand how this is constructed. Figure 1 is an important first step to that end. This shows the probability of graduating high school as a function of the test obtained at each administration of the high school exit exam, starting with the first administration in grade 10 and finishing with the last chance sample at the end of grade twelve.

The first graph is based on data for all five cohorts and for all students that took the initial test (nearly all of them). The x-axis is defined as the minimum score on the three subtests, such that students with a minimum greater than zero passed at the first attempt and students with a minimum less than zero failed at least one component of the exam. Not surprisingly, there is a positive relationship between the score on this exam and the probability of graduating. Students that passed the exam graduate with probability at least 0.8: those that do not graduate will include those that dropped out before the end of grade twelve and those that completed grade twelve but failed to meet other graduation requirements. Those that narrowly failed the first test graduate with probability around 0.8. This reflects the fact that nearly all of these students retook the
exam and passed on a subsequent attempt.

The next graph is based on data for the students in all five cohorts that took the exam at least twice. This will consist of a large fraction of those students that scored less than zero on the first test. The x-axis in this case is the minimum of the score obtained the subtests that had still not been passed. Again, a minimum greater than zero implies the exam was passed on this second attempt, a minimum less than zero implies that at least one subtest was failed. Again, there is a positive relationship between this minimum and the probability of ever graduating. Again, there is no discontinuity at the passing threshold, a reflection of the fact that students that fail still have multiple opportunities to retake and pass.

Only when we get to the final "last chance" test is there a large discontinuity in the probability of obtaining a diploma. This last chance sample will, for the most part, contain students that have already failed the test seven times. Again, the score represented on the x-axis is the minimum of the (rescaled) score obtained on the unpassed sections. Around 90% of the students with positive scores (i.e., who passed the last chance exam) graduate. Just over 40% of the students who fail the last chance test graduate, generating a discontinuity of around 45% in the probability of graduation as a function of the last chance test score.

We return to this discontinuity later. First, we present some stylized facts associated with the last chance sample. Since the students in the last chance sample have failed the exam seven times, we should not be surprised to see that their initial scores are towards the bottom of the initial test score distribution. This is illustrated in Figure 2a, which plots the density of first scores obtained by the last chance sample against the density of first scores obtained by the full sample. The mean of the first score among the last chance sample is at the 11th percentile of the full sample distribution. Note that the full sample distribution is shaped by ceiling effects. That is, a lot of students score close to the maximum when they take the test for the first time.

In Figure 2b we focus on the last chance sample and plot the distribution of last chance test scores. In our theoretical framework we argued that exit exams ensure that high school
performance is uncertain. In turn, this ensures that there will be students with scores just below and just above the last chance passing cutoff. The distribution seen in Figure 2b is consistent with this prediction. A formal test establishes that there is no discontinuity in the density of the last chance scores around the passing threshold (McCrary (2008)).

Table 1 presents some stylized facts for the last chance sample and presents more tests of the validity of the identification assumptions that we use to identify the credential wage premium. Recall that the key assumption is that other productivity signals observed by firms are smooth through the last chance passing threshold. We do not observe all of the productivity signals observed by firms, but we can assess whether the characteristics that we observe are smooth through this passing threshold.

The characteristics that we observe are listed in the rows of Table 1. The columns present the means of these variables among the last chance sample, the number of observations associated with the last chance sample (around 37,500) and the estimated discontinuity in these variables through the passing threshold. These estimated discontinuities are obtained by regressing these characteristics on a dummy variable for passing the last chance exam and a fourth-order polynomial in the last chance test score. The estimates are small and for the most part statistically insignificant. Where they are statistically significant, the associated graphs suggest that these findings are unlikely to be robust to minor changes in the regression discontinuity specification.

5.2 Matching and Misclassification

Because the datasets used here link 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 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 the earnings outcomes, since it is difficult to envision a scenario in which a student’s TAAS-passing status would affect the likelihood of being merged to the UI data.

6 Results

In this section we present four sets of results. First, we estimate the impact of passing the last chance exam on the probability of graduation. Next, we use these estimates to generate estimates of the impact of obtaining a diploma on earnings. To compare our results with those estimated in the previous literature, we then estimating the credential wage premium using the approach taking by this literature. Finally, we return to the diploma earnings effects as estimated using the regression discontinuity approach and estimate the impact of passing the last chance exam on the probability of pursuing various types of further education. This allows us to assess the extent to which the discontinuity-based diploma earnings effects are driven by further education impacts.

6.1 Estimates of the Diploma Effects of Passing the Last-Chance Exam

We have already seen that passing the last chance exam is associated with a roughly 45% increase in the probability of obtaining a high school diploma (Figure 1). We now investigate this relationship in more detail. To that end, Table 2 reports estimates of the diploma effects of passing the last-
chance exam at various points in time relative to the last chance test. One semester after the last chance test the effect (i.e., discontinuity) is around 0.485. At longer intervals the effect gets smaller, reaching a minimum of 0.418 two years after the last chance exam.

It is not surprising that the estimated effect decreases with time. This reflects the increased opportunities that students that fail the last chance test have to retake and pass. Effectively, as more time elapses after the last chance exam, the probability of earning a diploma conditional on failing increases.

Passing at future retakes is not the only phenomenon at work here. As suggested by the relative flatness of the diploma-score relationship to the left of the threshold, many students obtain a diploma without ever passing the test, presumably because they are exempted from the graduation requirements. This can be seen in Figure 3, which plots separately the two routes that students can take to a diploma: exemption and passing. Note that while the fraction of students that obtain a diploma via exemption is large among the last chance sample, it is small among the overall sample of students that pass the exam. Unless firms can observe at which point students passed the exam (which we assume they cannot), these exemptions should have little effect on the size of the credential wage premium.

6.2 Estimates of the Earnings Effects of Obtaining a Diploma

With these "first stage" estimates in hand, we can use the last-chance sample to estimate the earnings effects of obtaining a diploma. Table 3 presents the estimated earnings effects of obtaining the diploma between one and seven years after the last chance exam. As seen in the first row, mean earnings among this sample is increasing over this period. This will be reflect both an increase in average hourly wages and an increase in labor supply. The increase in labor supply will be driven in part by the addition of some college-educated workers to the sample; average hourly wages would be expected to increase as all workers acquire more experience.

In the third and fourth columns we present the "reduced-form" regression discontinuity esti-
mates. Both columns present discontinuity estimates based on a fourth-order polynomial in last chance scores. Unlike the estimates in the third column, the estimates in the fourth column are based on models that also include pre-determined covariates (the variables listed in Table 1). These turn out to have almost no impact on the estimates.

The reduced-form estimates are relatively small and statistically insignificant, ranging from around -$200 to $200. Further, they show no obvious pattern across the seven year window. When these are scaled up by the diploma effects of passing the exam (the instrumental variables estimated in columns (5)-(6)), the range of point estimates roughly doubles (from -$500 to $500) while the effects remain statistically insignificant and show no obvious pattern. To put these effect sizes into perspective, we report underneath them the effect as a percentage of mean earnings in the relevant year. These percentages are small: in a range of around 3 percent either side of zero.

6.3 Estimates Based on the Previous Approach

In comparison to the third generation of credentials studies, which also estimated effects on total earnings, these estimates are tiny. For example, Tyler et al (2000) estimated GED credentials effects of around 20% of mean earnings. To see how our estimates compare with those that would be estimated using the methods used in the second generation of credentials studies, we used those methods on these data. In particular, we started with the full sample of workers, restricted to those that completed twelve years of education but did not complete college, then regressed total earnings on whether or not these workers held a diploma. Of course this regression boils down to a comparison of the average wages of those with a diploma and the average wages of those without a diploma.

The results are presented in the final column of Table 3. In contrast with the estimates reported in the left-hand columns of the Table, these are positive, statistically significant and increasing with years since the last chance exam, although broadly stable from year six onwards. The obvious interpretation of this pattern of increasing effects (also found by Tyler et al (2000) is that it takes
students some time to settle into the labor market. Expressed as percentages of mean earnings, the estimates are between 10 and 20 percent, consistent with the previous literature (e.g., Jaeger and Page, 1994).

6.4 Diploma Earnings Effects and Further Education

Our earnings estimates are much smaller than those estimated previously. In the next section we discuss possible explanations for these differences. First we assess whether they could be driven by the effects of passing the exam on the probability of pursuing various types of further education. These estimates are reported in Table 4. They are again discontinuity estimates based on a fourth-order polynomial in the last chance test scores. In this Table, to save space, only the estimates based on models that include covariates are reported.

The columns to the left of the vertical line refer to outcomes for which we expect to find negative effects of passing the last chance test. For example, in the first column we look at the probability of being enrolled in high school one year after the last chance exam. Since students will, typically, only do this if they need to retake the exit exam, we would expect to find a negative effect. That is, we would expect to find that students that pass the exam will be less likely to enroll in high school. Similarly, we would expect to find negative effects of passing the exam on the the probability of attempting the GED and earning the GED credential. This is exactly what we find. The high school effect is negative, but small and short-lived. The effect is only observed for one year, consistent with the pattern of first stage estimates presented in Table 2. The GED effect is negative and growing, as more students that failed the exam decide to pursue the GED credential. We return to the possible impacts of these GED effects below.

The columns to the right of the vertical line refer to outcomes for which we expect to find positive effects of passing the exam. That is in part because these college-based outcomes sometimes require a high school diploma as a condition of enrollment; more broadly, because these options may be open to those without a diploma but may be more costly to pursue, for example because
these students have to satisfy other substitute criteria (e.g., enroll in a remedial class in college). The estimates take the expected sign but are small. It seems that a lot of students that narrowly pass the last-chance exam enroll in college for one semester but then drop out shortly afterwards. While this may at first glance seem surprising, it is worth recalling from Figure 2 that the last chance sample is one with low academic ability, at least as measured by the initial exit exam score. Ultimately, this difference in college attendance translates into only a tiny effect on college course credits and no effect on whether students earn a college degree.

To summarize, the main effect of passing the last chance sample is to reduce the probability that students pursue a GED. If pursuing - and earning - a GED is associated with a large increase in earnings, this could explain why our diploma earnings effects are smaller than those found in the preceding literature. Yet a quick back-of-the-envelope calculation suggests that these GED effects are too small to explain more than a tiny part of the difference. Suppose for example that the GED earnings effect is around 20% (consistent with Tyler et al (2000) and far in excess of the estimates obtained by Heckman et al). Then a 5% GED effect of passing multiplied by a 20% GED effect on earnings corresponds to a 1 percentage point effect of passing on earnings. Our estimates are between ten and twenty percentage points smaller than those obtained in the previous literature.

### 7 Discussion and Interpretation

Our estimates suggest that the credential value of a high school diploma is small if not zero. This compares with the large positive effects estimated in the previous literature and the large positive effects obtained here when we use an approach taken in the previous literature. Before concluding that our results cast doubt on the signaling hypothesis however, we wish to consider whether our results are robust to three types of biases not yet considered in our analysis. We will then reconsider our results in relation to the previous credentials literature and the broader signaling
literature. Finally, we will discuss why a high school diploma might not send a strong productivity signal.

7.1 Robustness

In future drafts we will assess whether our results are robust to three types of bias not yet considered.

Information biases

Our working assumption has been that firms do not observe the number of times that a student takes the exit exam and do not observe workers’ exact scores. As noted already, if firms can observe the number of times a worker took the test, they can in effect observe a strong productivity signal which will reduce the power of the additional signal sent by the diploma. We do not believe that firms observe this information and intend to test this assumption by assessing whether there is an earnings discontinuity associated with passing the first test. The workers on either side of that threshold should be equally productive and earn a diploma with equal probability. If firms observe that one set of workers took the exam more often, there should be a positive earnings effect associated with passing the exam first time.

We have also assumed that firms cannot observe the exact test scores. This is consistent with studies of what firms observe about high school performance and anecdotal evidence from Texas. This assumption cannot be tested using our data, but we intend to test it using the information contained on AFQT scores in the NLSY. Specifically, if firms can observe this information, and if the exit exam score is correlated with the AFQT score (as we would expect), then the AFQT-wage correlation will be higher in states with exit exams.

Zero earnings biases

One disadvantage of our earnings data is that we observe zero earnings for some groups that have positive earnings. These include federal employees (e.g., those in the military), the self-
employed, those working in the black economy and those working outside the state of Texas. The issue in each case is whether the earning the diploma will affect the probability of belonging to any of these groups. We will again use the AFQT and the NLSY to assess the plausibility of each problem. For example, we might be able to rule out large army effects by showing that the military typically recruits from a higher point in the ability distribution (i.e., the thirteenth percentile upward). This would suggest that passing the last chance test could have only a small effect on the probability of being recruited into the military. Similarly, we can assess what fraction of this low-achieving population moves state, is self-employed and may be working in the black economy.

7.2 Relation to the previous signaling literature

We have argued that the second generation of credentials studies likely generated estimates that were biased upwards by a correlation between diploma status and productivity signals observed by firms but not the econometrician. Our own estimates of the high school diploma effects as estimated using the second generation method further support this interpretation.

It is harder to explain why our results differ from the third generation of credential studies. One possibility is that the market for high school dropouts is fundamentally different to that for those that complete twelve years of school. For example, it could be that employers have much less information about dropouts, so that the GED carries a stronger signal. In future drafts we will give more consideration to this issue.

7.3 Why might the diploma send a weak signal?

Our results beg the question of why the high school diploma does not send a stronger productivity signal. Equivalently, why do firms not use high school diploma information to predict productivity? One possibility is that the variable underlying diploma status (in this case the exit exam score) does not predict productivity. That is not the case here, since there is a strong correlation between
this variable and earnings even conditional on the high school diploma information.

Instead, it must be the case that the diploma does not predict productivity conditional on the other information observed by firms. This could be productivity tests that workers take upon entry to the job, other information about high school performance (to the extent that this is correlated with productivity on the job) or fast learning on the part of firms. We intend to investigate the other information held by firms using data from the Bay Area Labor Study (BALS). This contains detailed information on the hiring and pay practices of firms employing workers in the types of jobs likely to be performed by those in the last chance sample.

8 Conclusion

The signaling hypothesis finds some of its strongest support in the literature that estimates the signaling value of education credentials. Previous estimates of credential wage premia are however likely biased upwards. As we showed above, the previous estimates were based on settings in which there was no uncertainty in the process by which a credential was acquired. As such, the credential wage premium can only be identified under very strong assumptions: that firms do not observe more productivity information than the econometrician. Under uncertainty, such as that introduced by high school exit exams, the credential wage premium can be identified under much weaker assumptions.

Our estimates suggest much smaller credential wage effects, which provide much less support for the signaling hypothesis. That we also obtain large positive estimates when we use the approach taken in the previous literature adds further weight to the argument that these estimates are biased upwards. It is less clear why these results differ from those obtained in the third generation of credential studies (Tyler et al, 2000) and it is not clear why the high school diploma sends such a weak signal. These are among the issues that we plan to investigate in future versions of this paper.
References


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: Density of first score for full sample and last-chance sample

Figure 2b: Density of last-chance score
Figure 3: How does last-chance sample obtain diplomas?
Table 1: Validity of regression discontinuity design on last chance sample

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Mean (full)</th>
<th>Mean (LCS)</th>
<th>N (LCS)</th>
<th>4th order poly</th>
<th>2nd order poly</th>
</tr>
</thead>
<tbody>
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<td>se</td>
<td>Coeff</td>
<td>se</td>
<td>Coeff</td>
</tr>
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<td>0.013</td>
<td>0.011</td>
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<td>0.002</td>
<td>0.001</td>
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<td>0.002</td>
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<td>0.008</td>
<td>-0.014</td>
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<td>1992 cohort</td>
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<td>-0.010</td>
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<td>1993 Cohort</td>
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<td>0.006</td>
<td>0.006</td>
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<td>1994 Cohort</td>
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Table 2: “First stage” estimates of passing on P(diploma)

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<th>2nd order poly</th>
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<td>2 semesters</td>
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<td>1 year</td>
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<tr>
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<td>0.417</td>
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Table 3: Earnings estimates

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<th>Years since LCE</th>
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<th>Reduced-form estimates</th>
<th>IV estimates</th>
<th>Difference in avg wages</th>
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<td>315.42</td>
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Note: RF estimates scaled up by 1/RD in grad within 2 years

Table 4: Effects of passing last-chance exam on further education

<table>
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<tr>
<th>Yrs since LCE</th>
<th>P(attend HS)</th>
<th>(attempt GED)</th>
<th>P(earn GED)</th>
<th>P(enrolled in college</th>
<th>Credits Earned</th>
<th>Earn BA/AA</th>
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</thead>
<tbody>
<tr>
<td>1 year</td>
<td>-0.053</td>
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<td>0.000</td>
<td>0.000</td>
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<td>2 years</td>
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<td>-0.014</td>
<td>-0.012</td>
<td>0.000</td>
<td>0.007</td>
<td>(0.01)</td>
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<tr>
<td>3 years</td>
<td>-0.002</td>
<td>-0.049</td>
<td>-0.036</td>
<td>0.000</td>
<td>0.009</td>
<td>(0.01)</td>
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<tr>
<td>4 years</td>
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<td>-0.048</td>
<td>-0.002</td>
<td>0.000</td>
<td>0.006</td>
<td>(0.01)</td>
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<tr>
<td>5 years</td>
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<td>-0.054</td>
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<tr>
<td>6 years</td>
<td>-0.075</td>
<td>-0.057</td>
<td>-0.011</td>
<td>0.000</td>
<td>0.008</td>
<td>(0.01)</td>
</tr>
<tr>
<td>7 years</td>
<td>-0.080</td>
<td>-0.061</td>
<td>-0.008</td>
<td>0.542</td>
<td>-0.002</td>
<td>(0.00)</td>
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Table 5a: Earnings estimates: Female

<table>
<thead>
<tr>
<th>Years since LCE</th>
<th>Mean earnings</th>
<th>Reduced-form estimates</th>
<th>IV estimates</th>
<th>Difference in avg wages</th>
</tr>
</thead>
<tbody>
<tr>
<td>7</td>
<td>12392.79</td>
<td>-282.28</td>
<td>-630.20</td>
<td>-231.39</td>
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<td></td>
<td>(271.80)</td>
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<td>(631.49)</td>
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<td>(93.45)</td>
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Table 5b: Earnings estimates: Male

<table>
<thead>
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<th>Years since LCE</th>
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<th>Reduced-form estimates</th>
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<th>Difference in avg wages</th>
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Appendix Figure 1: Baseline case (assuming $\gamma_1>0$, $\gamma_3>0$)
Appendix Figure 2: Firms observe second productivity signal (assuming $\gamma_1 > 0$, $\gamma_3 > 0$)

\[\pi = E[\pi^s] \]

\[\tau = E[\tau^s] \]
Appendix Figure 3: High school performance uncertain (assuming $\gamma_1 > 0$, $\gamma_3 > 0$)