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THE EFFECT OF HIGH SCHOOL EXIT EXAMS ON GRADUATION, EMPLOYMENT, WAGES AND INCARCERATION

Olesya Baker
Kevin Lang

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ABSTRACT

We evaluate the effects of high school exit exams on high school graduation, incarceration, employment and wages. We construct a state/graduation-cohort dataset using the Current Population Survey, Census and information on exit exams. We find relatively modest effects of high school exit exams except on incarceration. Exams assessing academic skills below the high school level have little effect. However, more challenging standards-based exams reduce graduation and increase incarceration rates. About half the reduction in graduation rates is offset by increased GED receipt. We find no consistent effects of exit exams on employment or the distribution of wages.

Olesya Baker
Division of Health Policy Translation
Brigham and Women's Hospital
NH 304
75 Francis St., Boston, MA 02215
obaker@bu.edu

Kevin Lang
Department of Economics
Boston University
270 Bay State Road
Boston, MA 02215
and NBER
lang@bu.edu

Twenty-five states, with nearly 70 percent of public school students, have high school exit exams (Center on Education Policy, 2012). Typically, students who do not pass all portions of the high school exit exam even after multiple attempts cannot obtain a high school diploma. This high-stakes nature of exit exams has made them highly controversial. Proponents of high-stakes testing assert that exit exams raise achievement by motivating students and assure employers and colleges of the quality of high school graduates. However, critics argue that exit exams increase the dropout rate, narrow the curriculum, and withdraw attention from low-performing students whose achievements fall far short of the established standards.

Most studies of high school exit exams find adverse effects on graduation rates, some positive effects on employment and no effect on mean wages. While replicating these findings except that we find no consistent employment effects, we broaden the range of outcomes to distinguish between GEDs and high school diplomas. We also examine different quantiles of the wage distribution and address incarceration.

We use Current Population Surveys (CPS) and Census data to create a state/cohort panel and exploit the staggered timing of the implementation of exit exams across states. We include state and graduation cohort fixed effects to control for time-invariant characteristics of states and characteristics that affect all states but vary across graduating cohorts. In some specifications, we also include state-specific cohort trends. We focus on results that are robust to the in/exclusion of these trends and the choice of sample.

Overall, we find modest effects of high school exit exams. “Minimum competency” exams that assess skills taught below the high school level have negligible effects on graduation rates. “Standards-based” exams that test high school level content are associated with a decline

in the graduation rate of about one percentage point, consistent with findings in the previous literature. In addition, this decline is only partially offset by an increase in the number of students obtaining a GED. We also find that in some specifications, standards-based exams that replace existing minimum competency exams have small and insignificant effects on graduation rates, while standards-based exams that were implemented from the ground up have a much larger negative effect. This suggests that the decline in the graduation rate might be a transitory adjustment cost. Our attempts to confirm this hypothesis directly give mixed results.

In addition, we find a robust adverse effect of standards-based exams on the institutionalization rate. The exams increase incarceration by about .2 percentage points or about 12.5 percent. As for graduation rates, we find that the effect of these exams is greater when they start from the ground up rather than replace a minimum competency exam. The point estimates of the effect of minimum competency exams on incarceration are also positive but fall well short of significance at conventional levels. We find no consistent effects on the remaining outcomes: percentiles of the wage distribution and employment.

The organization of this paper is as follows. The next section examines the history and current state of the high-school exit exam movement. Section 2 briefly describes the theory regarding the impact of exit exams on graduation rates and wages. Section 3 reviews previous studies. Sections 4 and 5 describe the data and our statistical approach. Section 6 presents the results. Finally, Section 7 concludes.

1 Background

The roots of the high school exit exam movement date to the 1970s. The initiators of this movement, mainly state policy-makers and grass-root reformers, were responding to concerns

that many young people were graduating from high school without a solid foundation of academic knowledge and skills. Their proposed solution was to set minimum standards for graduation. Whether high school students met this minimum quality standard was determined by a state high school exit exam. Figure 1 shows that early adopters were primarily border-states and East Coast states, while later adopters included the West Coast and a handful of centrally located states.

Initially, cutoffs for high school exit exams were low; exams tested basic academic skills typically taught between 6th and 8th grades. These minimum competency exams were prevalent in the 1970s and 1980s, but in more recent years they have been replaced with more rigorous standards-based exams that focus on academic skills learned in 9th grade or later. Both types of exams assess English and math skills; some of the more recent standards-based exams also assess other subjects such as science or social studies and several states implemented end-of-course exams that test content taught in particular courses. Exams are first administered in the 9th, 10th or 11th grades, depending on the type of exam and the state. Students have two to eleven retesting opportunities, depending on the state. In some states students who fail multiple attempts can petition to graduate based on course grades, letters of recommendation, school attendance and other academic factors (Center on Education Policy, 2002).

2. Theoretical Motivation

We describe the theory only briefly and informally. Fuller treatments can be found in Betts and Costrell (2001) and Lang (2007, chapter 8). We treat exit exams as raising the graduation requirement. Since typically performance on exit exams is not readily available to employers,

this is the primary mechanism through which they can affect education and labor market outcomes,

The easiest case arises when education is a pure sorting device. By definition, in this case, testing has no effect on average wages. By raising the cost of obtaining a diploma, exit exams induce some people who would otherwise graduate from high school to leave without a diploma. Assuming that individuals who are on the margin of obtaining their diploma are, on average, more skilled than are dropouts and less skilled than infra-marginal high school graduates, wages will rise both for individuals who would drop out anyway and for those or who get their diploma in both cases. The losers are those whom the exam induces to drop out.

If individuals have the option of earning a GED, the change is even more complicated. Since the minimum competency exams are at a lower level than the GED, it is difficult to see how the presence of the GED would influence their effect. However, when standards-based exams raise graduation requirements, we would expect many of the marginal diploma recipients to switch to the GED. This raises the value of the GED and should induce some additional low performing dropouts to also take the GED. Under this scenario, the increase in the number of GEDs would more than offset the reduction in the number of students getting diplomas.

Even in this very simple scenario, the effects of high-stakes exams on the wage distribution are complex, lowering wages for those who continue to have no certification, raising them for those who switch from no certification to GED or who hold GEDs in both scenarios, lowering wages for those who switch from a diploma to a GED and raising them for those who hold diplomas in both settings.

Adding the role of education in human capital production adds further complexity. Presumably, those individuals who are induced to work harder to pass the exam see somewhat larger wage increases while those who are induced to drop out lose even more.

It is possible to derive scenarios in which high-stakes exams increase graduation rates, but since we do not observe this in the data and since the focus of this paper is empirical, we do not discuss them here.

3. Literature Review

The existing literature reaches inconsistent conclusions about the consequences of exit exams. The early literature (David Grismer et al, 2000; Stephen Klein et al, 2000; Haney, 2000; Amrein and Berliner, 2002) often focused on reforms in a small number of states, most notably Texas, and on effects on test scores and, to a lesser extent, graduation rates often confounding accountability and exit exams. Later papers broadened the analysis to include more states and more outcomes.

Our paper is closest in spirit to Warren, Jenkins, and Kulick (2006), Dee and Jacob (2006), and Warren, Grodsky, and Lee (2008). All three papers are based on a time-series/cross-section analysis of states. Warren, Jenkins and Kulick use the October Current Population Surveys to measure dropout rates among 16 to 19 year olds, the Common Core of Data to measure high school completion rates and data from the American Council on Education to measure the proportion of 16 to 19 year olds taking the GED. They estimate that more difficult, but not less difficult, exams reduce the high school completion rate and may increase use of the GED. Moreover, they find larger effects in states with more ethnically and racially diverse populations and higher poverty rates.

Dee and Jacob use Census data to examine the effect of high school exit exams on school completion rates. Unlike Warren, Jenkins and Kulick, they can address ultimate graduation rates because they look at older respondents. However, the data do not distinguish between a GED and a traditional diploma, forcing them to examine the effects on high school graduation including the GED. They find small adverse effects of more difficult exams and even smaller adverse effects of less difficult exams. They also find some evidence of positive employment effects for women, positive wage effects for blacks (for more difficult exams) and adverse wage effects for whites and Hispanics.

Finally, Warren, Grodsky, and Lee use the U.S. Census 5% Public-Use Microdata Samples and the Outgoing Rotation Groups of the Current Population Surveys to assess whether high school exit exams have widened the gap in labor force status between those with and without high school diplomas. They find no evidence that high school exit exams affect gaps in labor force status or that these outcomes vary across race.[1]

Our analysis differs from the above studies in the following ways. First, we look at older cohorts for whom we can observe completed schooling. Second, using the older cohorts and CPS data we separately analyze GED and high school diploma rates and test whether a decrease in high school completion rates is offset by an increase in GED receipt. Third, we separately evaluate the short-term and long-term effects of exit exams on graduation and GED rates. Fourth, we use Census data to examine effects on rates of institutionalization. Finally, rather than focusing on mean wages, we analyze different quantiles of the wage distribution.

[1] There is a voluminous literature on the effects of school accountability systems. For an excellent review see Figlio and Loeb (2011). Deming et al (2013) includes a briefer but more recent review.

4. Data

We gathered information on the implementation date, type of exam and grade when first tested using archival research, direct communication with state departments of education and, primarily, from John Robert Warren at the University of Michigan.[2] These data allow us to distinguish between two types of high school exit exams: minimum competency (basic skills below the 9th grade) and standards-based (aligned with high school level standards).[3] We limit our sample period to cohorts who graduated between 1977 and 2001. Although new exit exam policies were implemented after 2001, No Child Left Behind (NCLB) policies also applied after then and graduation targets set under NCLB can confound the effects of exit exams.

By 2001 eighteen states had a high school exit exam: Alabama, Florida, Georgia, Indiana, Louisiana, Maryland, Minnesota, Mississippi, Nevada, New Jersey, New Mexico, New York, North Carolina, Ohio, South Carolina, Tennessee, Texas, and Virginia. The 33 remaining states (including the District of Columbia) either never introduced a high school exit exam or did so later.

Although not the primary focus of our study, it is important to account for the use of accountability systems because exit exams are often used for school and district accountability. There are two types of accountability systems: report card accountability that requires dissemination of school performance information to the public, and consequential accountability that goes a step further by attaching consequences (e.g., financial incentives, takeover threats) to school performance. Accountability systems might affect the graduation rate if, for example, schools at risk of sanctions improve student performance and increase high school completion.

[2] See http://www.hsee.umn.edu/.
[3] We code two states that have end-of-course exams as having standards-based exams.

We obtain data on the introduction of state accountability policies from Hanushek and Raymond's (2004) accountability study.[4]

We draw outcomes data from both the merged outgoing rotation groups (MORG) of the Current Population Survey (CPS) and the US Census. Both sources include information on educational attainment, earnings and basic demographics. After 1998 the CPS distinguishes between a high school diploma and GED while the Census combines these two categories into a single variable. The primary disadvantages of the CPS are that a) we must classify individuals by their current state of residence, not by where they lived when they attended high school, and b) it excludes the institutionalized population and therefore significantly under-represents high school dropouts (Sum and Harrington, 2003).[5] The Census, on the other hand, identifies and surveys the institutionalized population and provides information on the individual's state of birth. Therefore, the Census complements the CPS data, and we use both in our analysis.

When exit exam policies are first introduced, they apply to a given graduating class: the state first begins withholding diplomas from students who reach 12th grade in the "implementation year." Therefore, we focus on people who turned 18 (assumed high school graduation age) from 1977 to 2001 in the CPS and 1977 to 1998 in the Census data.[6] We refer to these yearly samples as "graduation cohorts," even though some individuals may have dropped out of high school before turning 18.

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[4] For the 9 states not in Hanushek and Raymond sample—Alaska, Idaho, Illinois, Iowa, New Hampshire, New Jersey, Ohio, Pennsylvania, and South Dakota—we conducted additional research using state education department websites. We assumed that once an accountability system is introduced, it remains in place for the following years.

[5] Exclusion of the non-civilian population likely produces a downward bias.

[6] Assuming all students graduate at 18 entails some measurement error and can create attenuation bias. We must limit the endpoint of Census data to 1998 because we focus on individuals who are 20 years or older in the Censuses.

Although we focus on individuals who turned 18, or "graduated", between 1977 and 2001, the outcomes data are drawn from the 1994 through 2011 CPS MORG files and from a 5 percent sample of the 1990 and 2000 Censuses. We begin the CPS analysis in 1994 because it is the first year in which we can eliminate immigrants who arrived after the age of sixteen and, hence, attended little or no high school in the United States. Furthermore, 1998 is the first year in which we can distinguish between having a GED and a high school diploma. We focus on the 1990 and 2000 Censuses because the time span of this sample is similar to that of the CPS sample. We restrict both samples to those individuals who are at least twenty (twenty-three for the wage analysis) at the time of the survey to allow for individuals who receive their diplomas when they are nineteen. The samples are restricted to those forty or younger in order to reduce educational attainment recall bias.[7]

Because we begin observing outcomes in the 1990s, individuals who turned 18 during the late seventies or early eighties will be older when we observe them in our sample. Assuming there is no recall bias about educational attainment, the effect of exit exams on the graduation rate should not vary by age at observation. Employment and wage outcomes for older cohorts, however, do reflect long-term effects and can help us learn whether effects of exit exams are short-lived or whether they persist.

The resulting CPS dataset consists of 1,500,396 individuals whose predicted graduation occurred between 1977 and 2001 and whose schooling and employment outcomes are observed between 1994 and 2011. The dataset of individuals for whom we can distinguish between holding a traditional diploma or a GED consists of 1,222,191 observations. The Census dataset

[7] Our initial examination of the data revealed a strong tendency for within cohort educational attainment to grow after age 40, which we deemed implausible.

consists of 5,811,769 individuals whose predicted graduation occurred between 1977 and 1998 and whose schooling, incarceration and employment outcomes are observed in 1990 or 2000.

Because exit exams apply uniformly to all students in a given state, within-state variation across students does not contribute to identification. Therefore, we aggregate the data so that our dependent variable is, for example, the proportion of individuals in cohort *c* in state *s* who received a high school diploma.[8] Wage and employment outcomes must be adjusted for the ages and years in which the cohort is observed. We discuss this adjustment in detail later. The aggregated CPS dataset contains 1275 state-graduation cohort observations. The Census dataset has 1122 observations, reflecting the absence of data on the 1999 to 2001 graduation cohorts.

Table 1 presents descriptive statistics for the key variables. High school diploma rates for the state-graduation cohort CPS sample are about 91 percent of which about 4 percent are GED.[9] The Census high school diploma rates are lower, at 87 percent. Roughly 22 percent of the state-graduation cohort observations have an exit exam, of which 15 percent are minimum competency exams and 7 percent are standards-based. Accountability systems are in place in 15 percent of the state-cohort sample.

5. Empirical Strategy

The contradictory conclusions presented in the literature review suggest that identifying the effect of exit exams is difficult. In particular, exit exam policies may be endogenous to the outcomes of interest. For example, states with a greater share of low-performing students might

[8] The individual-level observations are weighted using sampling weights provided by the CPS

[9] Our CPS estimates of graduation rates are higher than the national averages. This is because we exclude immigrants who arrived after the age of 16 and the institutionalized population. Both of these groups have high school dropout rates above the national average.

be more likely to pursue exit exam policies but also to have lower initial graduation rates relative to other states.

We use a fairly standard differences-in-differences approach with state and graduation cohort fixed effects to control for characteristics that are common across graduating cohorts within a state and characteristics that vary over graduation cohorts but are constant across all states (such as national economic or education policies). [10]

Using the state-graduation cohort dataset, we estimate the effects of high school exit exams on graduation/dropout, GED and non-employment rates and wage quantiles using the following equation:

$$y_{sc} = X_{sc}B + \alpha_s + \gamma_c + \epsilon_{sc} \quad (1)$$

where y_{sc} is the outcome variable of interest for state *s* and graduation cohort *c*; X_{sc} is a set of dummy variables capturing whether state *s* had a minimum competency exam, a standards-based exam, report card or consequential accountability that applied to graduating cohort *c*; α_s is a state fixed effect and γ_c is a cohort effect measuring the year of assumed graduation.

When we analyze short-term versus long-term effects of exit exams, we expand X_{sc} to include three types of exams: minimum competency, standards-based not preceded by a minimum competency exam and standards-based implemented after a minimum competency phase-out. In one specification, we then interact each of these three variables with years since exam implementation dummies.

[10] CPS data contain only the state of current residence; therefore, we must assume that the state where individuals are residing at the time of the survey is the state where they attended high school. Census data contain an individual's state of birth, and we use this variable for robustness checks.

A potential problem with Equation 1 is that not only may policies be correlated with unobserved state effects, but policy changes may also reflect underlying state trends. This could generate serial correlation in the state error terms and severely bias the standard errors. First, we address this by clustering the standard errors in Equation 1 by state. In addition, we estimate:

$$y_{sc} = X_{sc}B + \alpha_s + \gamma_c + \lambda_s c + Z_{sc}D + \epsilon_{sc} \quad (2)$$

where we do not cluster standard errors by state but have added a state-specific cohort trend $\lambda_s c$.[11] The state-specific cohort trend captures any systematic state-specific changes over the sample period; Z_{sc} is a set of additional state-specific time-varying regressors (unemployment rate, average hourly wages, poverty rate) measured at the cohort's graduation year. Those specifications that include these time-varying state-specific variables must drop the first three graduation cohorts.

As described in the theory section, exit exams can affect the distribution of wages. Therefore, we begin our wage analysis by using individual-level data and calculate the 10th, 30th, 50th, 70th, and 90th percentiles of the log hourly wage for each state-graduation cohort. We estimate the percentiles in two ways: our preferred Ordinary Least Squares (OLS) method is described below; the quantile regression method is described in the section on robustness checks. We then examine the effects of testing on each percentile separately.

To calculate wage quantiles for each graduation cohort in each state, we regress log wages on age and time (year of survey) dummies:

[11] For this specification, we do not cluster errors by state. Experimentation showed that such clustering did not have a consistent effect on the reported standard errors, and it is not clear that the problems that clustering is intended to address should remain in the presence of state-specific trends.

$$\ln w_{ics} = \sum \delta_{age} age_i + \sum \mu_t t_i + u_{ics} \qquad (3)$$

From this regression we obtain the residuals, u_{ics}, which have been purged of the effects of age (experience) and survey year (economy-wide factors). For each state and graduation cohort, we then compute the quantiles of these residuals. The resulting dataset is a collection of (residual) wage quantiles for each state and graduation cohort; therefore, it has the same observational structure as the aggregated data used in previous regression specifications. We then use these state-cohort quantiles as dependent variables. The explanatory variables are the same as in equations (1) and (2).

We estimate the employment effects in an analogous fashion. First, we regress a dummy variable for non-employment on year and age dummies as in (3). We then calculate the mean residual for each state-cohort group and use this corrected employment rate as the dependent variable in equations (1) and (2). Age, conditional on being 23-40, and year are in practice close to orthogonal to state-cohort especially once we condition on state and cohort fixed effects. Therefore, the residual means are very close to state-cohort fixed-effects estimates. In fact, in practice, our results using the corrected employment variable are virtually indistinguishable from estimates using the state-cohort mean employment rate across all surveys.

6. Results

Education Outcomes

Tables 2 and 3 show the effects of exit exams on high-school graduation rates when we do not distinguish between a high school diploma and GED. These tables consist of three panels. In the upper panel, we differentiate only between the presence and absence of an exit exam and

between the presence and absence of an accountability system. The middle panel distinguishes between minimum competency and standards-based exams and between report card and consequential accountability. Finally, the lower panel distinguishes among minimum competency, standards-based not preceded by minimum competency exams and standards-based that replaced minimum competency exams.

The coefficients in the first column of both tables are uniformly negative, which suggests that both exit exams and accountability systems are associated with lower graduation/GED rates. However, in the absence of state and cohort controls, there is little reason to believe that the estimated effects are causal. Indeed, once we control for state and cohort in column 2 and state-specific trends in column 3, the exam and accountability effects estimated using both CPS and Census data become smaller and some become statistically insignificant.

The coefficient on the exit exam dummy, displayed in the last columns of the first panel, is negative and significant when CPS data are used, and small and insignificant when Census data are used, making it difficult to draw strong conclusions. Panel two of both tables displays the coefficients for minimum competency and standards-based exams and for report card and consequential accountability separately. The CPS results suggest that more rigorous standards-based exams have an adverse effect on graduation rates of about 0.8 percentage points. The Census results are similar but generally somewhat smaller and not always statistically significant. The effects of minimum competency exams are smaller than those of standards-based exams using the CPS and non-existent using the Census. We find no consistent effect of either form of accountability.

The third panel addresses the possibility that implementing a challenging exit exam lowers graduation rates more than does replacing a less rigorous exit exam with a more rigorous one. As panel 3 shows, when we use the CPS data and do not control for state-specific trends, the effects of standards-based testing are estimated to be similar regardless of whether they replace a minimum competency exam. Using the Census data, column (2) suggests a stronger effect of standards-based exams when they follow a minimum competency exam. However, when we control for state-specific trends, standards-based exams implemented from scratch have a larger negative effect on graduation rates, 1.3 percentage points using the CPS and 1.2 percentage points using the Census. Standards-based exams that were preceded by minimum competency exams show no adverse effect—an insignificant decline in graduation rates of about 0.3 percentage points using CPS data and an insignificant increase of 0.4 percentage points using Census data.[12]

The results with state-specific trends suggest the hypothesis that schools can adjust to tougher requirements over time, by, for example, developing new teaching methods and remedial courses. This suggests interacting the three types of exit exam dummies (minimum competency, and standards-based with or without prior minimum competency) with year-since-implementation dummies. We distinguish among one, two, three and four or more years since exit exam implementation. The results, presented in table 4, are imprecise and inconsistent. Although the CPS estimates show that effects of standards-based exams that were not preceded by minimum competency exams are reduced in year two, Census estimates show the opposite – large and significant negative effects that are sustained even four or more years after exam

[12] The two standards-based dummies are mutually exclusive.

implementation. In neither case are the differences between the first and long-standing administrations of the test statistically significant. Thus while the adaptation hypothesis has some intuitive appeal, our data are inadequate to support or reject it.

Table 5 displays the results of CPS data analysis that distinguishes between a GED and a high school diploma.[13] With varying degrees of significance, all specifications tell the same story. Minimum competency exams do not affect GED receipt, but standards-based exams are associated with an increase in the GED that only partially offsets the decline in high school diplomas and that the effect is driven by standards-based exams that were not preceded by minimum competency exams.

The results presented in table 5 are partially consistent with theoretical models of exit exams. Theory predicts a decrease in high school graduation rates due to exit exams, and in some specifications we do observe a such a decrease in traditional diploma rates due to standards-based exams. Theory also predicts that this decline will be fully offset by individuals obtaining a GED. However, the increase in the proportion of individuals obtaining a GED is too small to offset the decline in graduation rates.

Extensions and Robustness Checks

We attempted to look at the effects of exit exams on graduation rates for three separate categories of race/ethnicity (non-Hispanic white, Hispanic, black) interacted with sex. However, our estimates were too imprecise to draw any conclusions. Some of the coefficients were not small, but only one of thirty-six coefficients was significant at the .05 level.

[13] Table 5 uses four fewer years of data than table 2. Therefore, we do not expect the sum of the high school diploma and the GED effect to be exactly the same as that reported in table 2. GED and high school diploma variables are not available in the Census.

In addition to the overall effect of exit exams on graduation, it would be helpful to understand how exit exams affect the timing of the dropout decision for those students who are adversely affected by exams. Martorell and Clark (2010) find little difference in the earnings of students who just pass and just fail the last administration of a high school exit exam. This suggests that if the students who are induced to drop out do so mostly late in high school, the cost of dropping out may be low. However, dropping out in earlier grades will lead to fewer years of schooling which might entail more substantial costs. Unfortunately, perhaps because we do not observe grade completion for individuals who earned a GED, we obtained no conclusive results.

In the analysis above, we treat individuals as currently residing in the state in which they attended high school. The Census permits us to limit the sample to individuals living in the state where they were born and who therefore attended high school there with high probability. Results of this robustness check are presented in table A3 of the Appendix. The estimates are similar to those in table 3 when we control for state-specific trends although the standard errors are higher. With only state fixed effects, the adverse effect of standards-based exams is attenuated.

We have followed Donald and Lang (2007) and Dickens (1990) in not weighting our estimates because the relatively large sample size should make most of the error term reflect a common state-cohort error rather than sampling error. As a robustness check, we replicate the bottom panel of table 2 using feasible GLS where we have obtained the weights by regression the squared residual on a constant and the inverse of sample size and used the inverse of the

predicted variance as the weights. As shown in appendix table A4, the results are similar to those with no weighting.

Other Outcomes

Before turning to labor market outcomes, we examine the effect of testing on institutionalization or, *de facto*, incarceration. We compute the percentage of individuals in each state/graduation cohort who are institutionalized and estimate equations similar to those we have used to examine high school graduation rates. As can be seen in the top panel of table 6, exit exams overall have no detectable effect on incarceration rates once we control for state and cohort fixed effects. However, the middle panel suggests that an important adverse effect of standard-based exams is obscured by a much smaller or zero effect of minimum competency exams. When we control for state and cohort fixed effects, we estimate that standards-based exams raise the incarceration rate by .23 percentage points, significant at the .02 level. Adding state-specific trends raises the standard errors but does not noticeably change the estimate. In this specification, standards-based exams raise the incarceration rate by .20 percentage points and the t-statistic is 1.64. Adding further time-varying state controls at the cost of three cohorts of data, produces an estimated effect of .22 percentage points but with a t-statistic of only 1.49. Strikingly, the estimated effect of minimum competency exams in all specifications is to raise the incarceration rate by .08 - .09 percentage points although the significance falls far short of conventional levels.

The implied effects on incarceration are large although somewhat imprecise. The average state has an institutionalization rate of about 1.6 percent during our sample period. An increase of even .2 percentage points is therefore substantial. Consistent with some of our earlier findings

regarding graduation, we also find larger effects of standards-based exams if they do not replace a minimum competency exam. Again, the absence of statistical significance when we add state/cohort trends reflects higher standard errors rather than different coefficients. The coefficient rises from .26 percentage points without such trends to .29 percentage points in the last two specifications. The point estimates also suggest that standards-based exams have adverse effects of .12 - .19 percentage points even when they follow minimum competency exams, but the coefficients are not significant at any conventional level.[14]

In the labor market analysis, we restrict our sample to those who are between the ages of 23 and 40 at the time of the CPS and Census surveys. In table 7, we show the effect of testing on the proportion of workers who do not report a wage, that is, on non-employment. The upper panel examines the effects of testing as a whole, while the lower panel examines the effects of minimum competency vs. the two types of standards-based exams separately. The results are very sensitive to whether or not we control for state-specific trends and somewhat sensitive to choice of sample. When we control for state trends, the results suggest a small but beneficial effect of exit exams on non-employment when CPS data are used, but no significant result when Census data are used. When we divide the sample into six groups based on race/ethnicity and sex, most of the estimated effects are small and none is significant at the .05 level.

[14] We attempted to examine the effect of testing on incarceration rates by race and gender, but the estimates were too imprecise to be useful. Coefficients often reversed sign between specifications and only one estimate out of thirty-six (white males in the specification with state-specific time-trends) was significant at even the .05 level.

Table 8 shows the effect of exit exams on quantiles of the log wage distribution. These quantiles are computed using the distribution of the log wage residuals that are purged of the effects of age and time. Overall, the CPS results show neither large nor significant effects of high school exit exams on the distribution of wages. It is difficult to say what would constitute a large effect, but certainly the results suggest neither a dramatic drop nor a dramatic increase in wages near the lower parts of the wage distribution which are most likely to be affected by exit exams. We performed a number of specification tests on these data, analyzing a finer partition of the lower part of the wage distribution, and again did not find any large or significant effects. The Census data do show significant effects on the 50^{th} and 70^{th} percentiles; however, these results are not robust to the choice of specification, nor would they survive a Bonferroni adjustment.

Note that the lack of an effect on the lower percentiles is consistent with the relatively small effect of exit exams on educational attainment, which is likely too modest to show up in the wage estimates. The lack of a wage effect also suggests that testing neither greatly improves nor greatly diminishes the quality of instruction.

Finally, as an alternative to our approach to calculating wage quantiles, we estimate quantile regressions separately for each state to get a normalized cohort wage for that state. Table A5 shows the effect of the exit exam policies on the 10th, 30th, 50th, 70th and 90th quantiles of the log wage distribution. The results are similar to those in table 8.

7. Conclusion

Perhaps the most important conclusion of our analysis is that, with the exception of incarceration, the effects of exit exams are modest and many are not robust. We find no effect on graduation rates of minimum competency exams. Standards-based exams reduce the graduation

rate by about one percentage point. This effect seems to be concentrated in states that had not previously administered a minimum competency exam although the evidence is somewhat mixed on this point. This suggests the hypothesis that the effect is short-lived while schools adjust to the new standards. However, the direct evidence for this hypothesis is at best weak.

We do not find consistent evidence of an effect of exit exams on non-employment or the distribution of wages. This lack of any measureable wage effect may reflect a lack of power. If we assume that the average additional dropout loses one and a half years of education valued at 10 percent per year, the effect on the average wage of a one percentage point increase in the dropout rate is too small to measure. However, we do not find any effect at lower percentiles, where there should be somewhat more power. At the same time, the absence of any positive effects suggests that the tests do not significantly increase student learning. Of course, it is possible that modest learning gains for those who stay in school just offset the large losses of those who do not, but the simplest explanation is that both effects are small.

The real concern raised by the results presented here is the effect on the incarceration rate where we find large effects of standards-based exams and smaller and statistically insignificant effects of minimum competency exams that are nevertheless also sufficiently large to be disturbing.

Overall, the results presented in this paper support neither the dreams of the strongest advocates of exit exams nor the nightmares of their severest critics. However, nothing in this paper suggests that exit exams have large positive effects on student learning and productivity growth while they do suggest an important adverse effect for one segment of the population.

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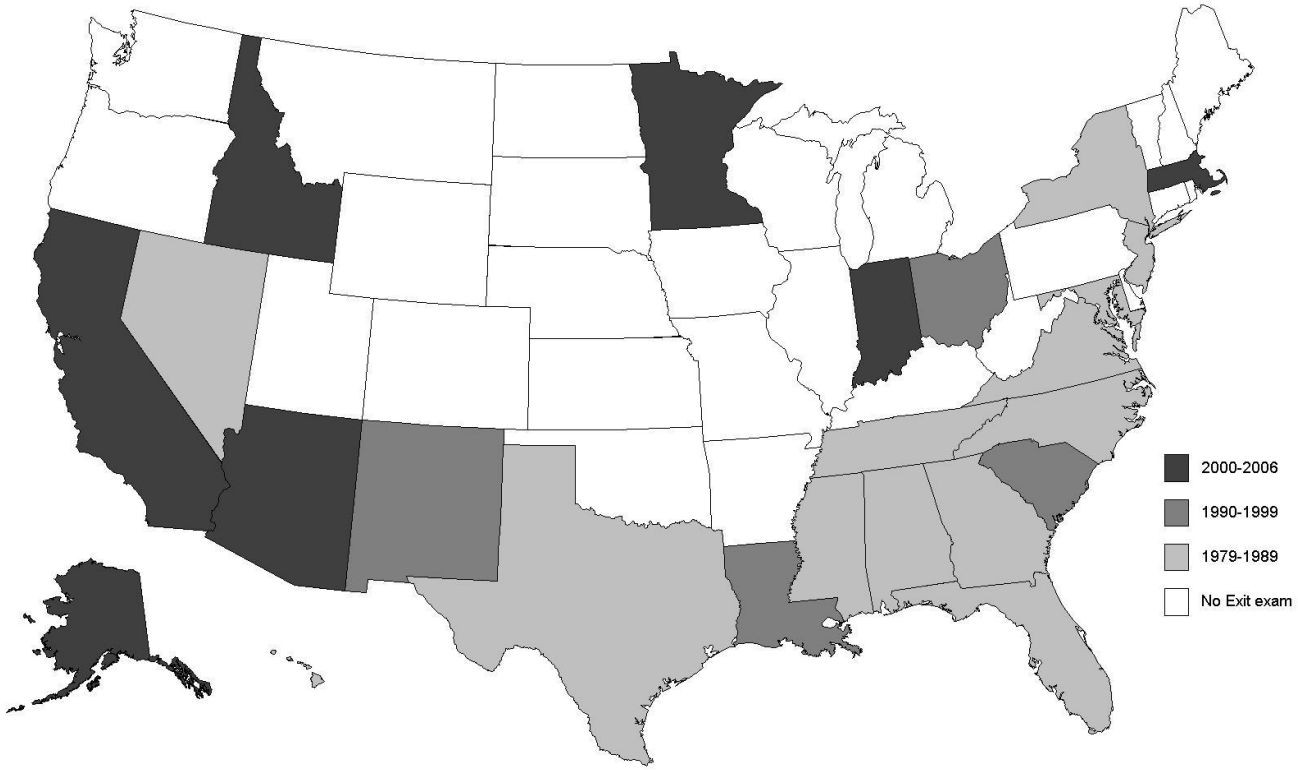
Figure 1: Geography and Timing of Exit Exam Implementation



Source: Robert Warren's archives, http://www.hsee.umn.edu/

Table 1: Descriptive Statistics

Variables	Obs.	Mean	Std. Dev.	Min	Max
Outcome Variables:					
CPS:					
High school graduate	1275	0.87	0.04	0.75	0.97
GED recipient	1275	0.04	0.01	0.00	0.10
Diploma, high school or GED	1275	0.91	0.03	0.80	0.98
Census:					
Diploma, high school or GED	1122	0.87	0.04	0.73	0.97
Incarceration Rates	1122	0.02	0.01	0	0.05
Policy Variables:					
Exit exam implemented	1275	0.22	0.42	0	1
Minimum competency	1275	0.15	0.36	0	1
Standards-based	1275	0.07	0.26	0	1
Accountability System	1275	0.15	0.35	0	1
Consequential accountability	1275	0.09	0.28	0	1
Report card accountability	1275	0.06	0.24	0	1
Other Controls:					
Unemployment rate	1275	0.06	0.02	0.02	0.17
Poverty rate	1122	0.13	0.04	0.03	0.27
Average hourly wage	1173	10.18	2.79	4.75	18.64

Notes: CPS data used in this analysis are obtained from the 1994-2011 CPS files (1998-2011 for GED), and are subset to individuals who turned 18 years old (expected year of high school graduation) between 1977 and 2001. Census data are obtained from the 1990 and 2000 US Census (5% sample) and are subset to individuals who graduated between 1977 and 1998, which results in fewer state-cohort observations than CPS data. Census variables do not separately distinguish between GED diploma and high school graduate status. Immigrants who arrived in the US after the age of 16 are excluded. Unit of observation is a graduation cohort c in state s. Poverty rate and average hourly wages are not available for the earlier graduation cohorts.

Table 2: Effects of Testing and Accountability on High School Graduation/GED Rates, 1994-2011 CPS MORG Data

HS Graduation/GED	No Controls	State FE, Cohort FE	State-Specific Cohort Trends	State-Cohort Controls
	(1)	(2)	(3)	(4)
Exit exam	-0.016***	-0.005	-0.005**	-0.006**
	(0.002)	(0.003)	(0.002)	(0.002)
Accountability	-0.015***	0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
F-test	69.12	1.328	3.004	3.343
Minimum competency	-0.015***	-0.004	-0.005**	-0.005**
	(0.002)	(0.003)	(0.002)	(0.002)
Standards-based	-0.019***	-0.007*	-0.008**	-0.008**
	(0.003)	(0.004)	(0.004)	(0.004)
Consequential	-0.014***	0.001	0.000	-0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Report card	-0.015***	0.000	-0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)
F-test	34.79	0.886	1.848	1.878
Minimum competency (MC)	-0.015***	-0.004	-0.003	-0.004
	(0.002)	(0.003)	(0.002)	(0.003)
Standards-based	-0.014***	-0.006	-0.013***	-0.013**
	(0.005)	(0.005)	(0.005)	(0.005)
Standards-based after MC	-0.023***	-0.008*	-0.002	-0.003
	(0.004)	(0.004)	(0.005)	(0.006)
Consequential	-0.014***	0.002	-0.000	-0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Report card	-0.015***	0.000	-0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)
F-test	27.16	0.689	2.406	2.273

Notes: The unit of observation is a state graduation year cohort and includes cohorts that turned 18 years old between 1977 and 2001 and were interviewed by the CPS between 1994 and 2011. The dependent variable is the proportion of people in that state and graduation cohort who received a high school diploma or a GED. The first panel combines all exit exams into one explanatory variable; the second panel distinguishes between a minimum competency and standards-based exams; the third panel further divides standards-based exams by whether or not they were preceded by a minimum competency exam (standards-based and standards-based after MC are mutually exclusive). Robust standard errors are reported in column (2). Reported F-tests are for explanatory variables displayed in each panel. State-cohort controls include: poverty and unemployment rates, average hourly wages.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Effects of Testing and Accountability on High School Graduation/GED Rates, 1990 and 2000 US Census Data

HS Graduation/GED	No Controls	State FE, Cohort FE	State-Specific Cohort Trends	State-Cohort Controls
	(1)	(2)	(3)	(4)
Exit exam	-0.027***	-0.003	-0.000	0.001
	(0.003)	(0.004)	(0.002)	(0.002)
Accountability	-0.026***	0.001	-0.004*	-0.003
	(0.004)	(0.004)	(0.002)	(0.002)
F-test	74.73	0.333	1.682	1.049
Minimum competency	-0.025***	-0.001	0.000	0.001
	(0.003)	(0.004)	(0.002)	(0.002)
Standards-based	-0.032***	-0.009*	-0.006*	-0.005
	(0.005)	(0.005)	(0.003)	(0.004)
Consequential	-0.028***	-0.002	-0.004*	-0.004
	(0.006)	(0.005)	(0.003)	(0.003)
Report card	-0.023***	0.004	-0.002	-0.002
	(0.006)	(0.005)	(0.003)	(0.003)
F-test	37.91	1.283	1.847	1.248
Minimum competency (MC)	-0.025***	-0.002	0.002	0.003
	(0.003)	(0.004)	(0.002)	(0.003)
Standards-based	-0.029***	-0.003	-0.012**	-0.012**
	(0.007)	(0.006)	(0.005)	(0.005)
Standards-based after MC	-0.036***	-0.015***	-0.000	0.004
	(0.007)	(0.004)	(0.005)	(0.006)
Consequential	-0.028***	-0.001	-0.005*	-0.004
	(0.006)	(0.005)	(0.003)	(0.003)
Report card	-0.024***	0.004	-0.003	-0.002
	(0.006)	(0.005)	(0.003)	(0.003)
F-test	31.96	0.197	2.356	2.239

Notes: The unit of observation is a state graduation year cohort and includes cohorts that turned 18 years old between 1977 and 1998 and were interviewed in the 1990 or the 2000 US Censuses. Both the 1990 and the 2000 Census datasets are 5 percent samples. The dependent variable is the proportion of people in that state and graduation cohort who received a high school diploma or a GED. The first panel combines all exit exams into one explanatory variable; the second panel distinguishes between a minimum competency and standards-based exams; the third panel further divides standards-based exams by whether or not they were preceded by a minimum competency exam (standards-based and standards-based after MC are mutually exclusive). Robust standard errors are reported in column (2). Reported F-tests are for explanatory variables displayed in each panel. State-cohort controls include: poverty and unemployment rates, average hourly wages. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Effects of Testing on High School Graduation/GED, by Year Since Implementation, 1994-2011 CPS and 1990, 2000 US Census Data

Graduation rate	CPS	CPS	Census	Census
MC, year 1	-0.002	-0.003	0.003	0.004
	(0.004)	(0.004)	(0.003)	(0.004)
MC, year 2	-0.001	-0.002	0.003	0.005
	(0.004)	(0.004)	(0.003)	(0.004)
MC, year 3	-0.006	-0.007	-0.002	-0.000
	(0.004)	(0.004)	(0.003)	(0.004)
MC, year 4+	-0.003	-0.004	0.001	0.006
	(0.003)	(0.003)	(0.003)	(0.003)
SB, year 1	-0.018**	-0.018**	-0.005	-0.005
	(0.007)	(0.007)	(0.007)	(0.007)
SB, year 2	-0.011	-0.010	-0.014**	-0.014*
	(0.007)	(0.007)	(0.007)	(0.007)
SB, year 3	-0.009	-0.008	-0.015**	-0.016**
	(0.009)	(0.008)	(0.007)	(0.008)
SB, year 4+	-0.012	-0.012	-0.014**	-0.017**
	(0.007)	(0.007)	(0.006)	(0.007)
SB after MC, year 1	0.001	-0.000	0.003	0.008
	(0.007)	(0.007)	(0.007)	(0.008)
SB after MC, year 2	-0.005	-0.006	-0.001	0.006
	(0.007)	(0.008)	(0.007)	(0.008)
SB after MC, year 3	-0.005	-0.005	-0.001	0.007
	(0.008)	(0.008)	(0.007)	(0.008)
SB after MC, year 4+	-0.002	-0.002	-0.003	0.005
	(0.007)	(0.008)	(0.006)	(0.008)
State-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort Trends	Yes	Yes	Yes	Yes
State-Specific Controls	No	Yes	No	Yes

Notes: The dependent variable is the proportion of people in that state and graduation cohort who received a high school diploma or a GED. Explanatory variables are interactions of test dummies with year since implementation dummies. Standards-based and standards-based after MC dummies are mutually exclusive. All regressions control for accountability; State-specific controls include poverty and unemployment rates, average hourly wages. *significant at 10%;** significant at 5%;*** significant at 1%.

Table 5: Effect of Testing on GED and High School Graduation, CPS Data

Dependent Variable	GED		Diploma	
	(1)	(2)	(1)	(2)
Minimum competency	0.000	0.001	-0.002	-0.004
	(0.002)	(0.002)	(0.004)	(0.003)
Standards-based	0.004	0.003	-0.009**	-0.011**
	(0.002)	(0.003)	(0.004)	(0.005)
Minimum competency (MC)	-0.000	0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.004)	(0.003)
Standards-based	0.006**	0.005	-0.010**	-0.015**
	(0.002)	(0.004)	(0.004)	(0.007)
Standards-based after MC	0.001	0.001	-0.007	-0.006
	(0.003)	(0.004)	(0.006)	(0.007)
State and Cohort FE	Yes	Yes	Yes	Yes
State-Cohort Trends	No	Yes	No	Yes

Notes: GED and diploma rate analysis uses observations from 1998-2011 CPS data (therefore, they do not add up to table 2 coefficients estimated using 1994-2011 data). GED and diploma rates are not available as separate variables in the US Census. The dependent variable is the proportion of people in that state and graduation cohort who received a GED or a traditional high school diploma. Standards-based and standards-based after MC dummies are mutually exclusive. All regressions control for accountability. Standard errors reported in column (1) are clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Effects of Testing and Accountability on Institutionalization Rates

	No Controls	State FE, Cohort FE	State-Specific Cohort Trends	State-Cohort Controls
	(1)	(2)	(3)	(4)
Exit exam	0.005***	0.001	0.001	0.001
	(0.000)	(0.001)	(0.001)	(0.001)
Accountability	0.003***	0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
F-test	73.33	2.283	0.835	0.696
Minimum competency	0.004***	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Standards-based	0.008***	0.002**	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Consequential	0.003***	0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Report card	0.003***	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
F-test	39.97	2.285	0.769	0.670
Minimum competency (MC)	0.004***	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Standards-based	0.008***	0.003***	0.003	0.003
	(0.001)	(0.001)	(0.002)	(0.002)
Standards-based after MC	0.007***	0.002	0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.002)
Consequential	0.003***	0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Report card	0.003***	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
F-test	32.46	2.997	0.846	0.727

Notes: The unit of observation is a state graduation year cohort and includes cohorts that turned 18 years old between 1977 and 1998 and were interviewed in the 1990 or the 2000 US Censuses. Both the 1990 and the 2000 Census datasets are 5 percent samples. The dependent variable is the proportion of people in that state and graduation cohort who are institutionalized. Robust standard errors are reported in column (2). Reported F-tests are for explanatory variables displayed in each panel. State-cohort controls include: poverty and unemployment rates, average hourly wages. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Effects of Testing on Non-Employment

No Wage	No Controls	State FE, Cohort FE	State-Specific Cohort Trends	State-Cohort Controls
	(1)	(2)	(3)	(4)
CPS				
Exit exam	0.023***	0.008*	-0.006**	-0.010***
	(0.003)	(0.004)	(0.003)	(0.003)
Minimum competency	0.019***	0.009**	-0.006*	-0.011***
	(0.003)	(0.004)	(0.003)	(0.004)
Standards-based	0.033***	0.007	-0.004	-0.005
	(0.006)	(0.014)	(0.007)	(0.007)
Standards-based after MC	0.001	0.004	-0.003	-0.009
	(0.008)	(0.015)	(0.010)	(0.010)
Census				
Exit exam	0.010***	0.008***	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Minimum competency	0.005*	0.007***	-0.004	-0.003
	(0.003)	(0.003)	(0.002)	(0.003)
Standards-based	0.017***	0.007	0.006	0.003
	(0.006)	(0.005)	(0.005)	(0.005)
Standards-based after MC	0.013	0.006	-0.011	-0.008
	(0.009)	(0.007)	(0.007)	(0.008)

Notes: The dependent variable is the proportion of individuals with no wage in each state/graduation year cohort. An individual is flagged as having no wage if their wages are missing or zero (not employed). All regressions control for accountability. Standard errors reported in column (2) are clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Estimated effects on the distribution of log wage residuals (conditional on working)

	10-percentile		30-percentile		50-percentile		70-percentile		90-percentile	
	CPS	Census	CPS	Census	CPS	Census	CPS	Census	CPS	Census
Exit exam	-0.006	0.005	-0.000	0.007	-0.003	0.007	-0.007	0.013**	-0.011	0.011
	(0.006)	(0.009)	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)
Minimum competency	-0.006	0.009	0.002	0.010	-0.001	0.012**	-0.004	0.016**	-0.006	0.012
	(0.007)	(0.010)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)
Standards-based	-0.011	0.000	-0.005	-0.003	-0.011	-0.009	-0.009	0.003	-0.009	0.004
	(0.014)	(0.021)	(0.012)	(0.014)	(0.012)	(0.012)	(0.013)	(0.013)	(0.017)	(0.017)
Standards-based after MC	-0.001	0.038	0.011	0.019	0.018	0.030	0.016	0.015	0.021	0.005
	(0.020)	(0.032)	(0.018)	(0.022)	(0.017)	(0.019)	(0.019)	(0.020)	(0.025)	0.012

Notes: The dependent variable is the quantile of a residual obtained from an individual-level regression of wages on age and time dummies. The quantiles are computed for each state-graduation year cohort. All regressions control for state and cohort fixed effects, state-specific cohort trends and accountability. * significant at 10%; ** significant at 5%; *** significant at 1% .

APPENDIX TABLES

Table A1: State and Year in which Specific Types of Exit Exams were First Introduced

Year Introduced	Minimum Competency	Standards-Based	End of Course
1979	NY		
1980			
1981	VA	NY	
1982	NC, MD, NV		NY
1983	HI (1983-1999), TN, FL		
1984			
1985	AL, GA	NJ	
1986			
1987	TX		
1988			
1989	MS		
1990	NM	SC	
1991		LA	
1992		TX	
1993		FL	
1994	OH		
1995		GA	
1996			
1997			
1998			
1999		NV	
2000	MN	NY, IN	
2001		AL	

Notes: Data on exit exams are obtained from John Robert Warren's website (http://www.hsee.umn.edu/). The "year introduced" refers to the first graduating class affected by the new requirements

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Table A2: State and Year in which Specific Types of Accountability Systems were First Introduced

	Consequential	Report Card
1987		ME
1988		LA
1989		
1990		
1991	IN	IL, MD, MS
1992		
1993	KY, WI	
1994		
1995	MD	
1996	GA	NJ
1997	CT, AL, MI, PA, TX	AZ, DC
1998	NV, NC, OK	MO, NH,
1999	CA, FL, VA	OH
2000	DE,RI, WV, LA, NY	
2001	AR, MA, OR, SD,TN, VT	MN, WA,

Table A3: Effects of Testing and Accountability on High School Graduation/GED Rates
State of Birth Equals State of Residence, US Census

HS Graduation/GED	No Controls	State FE, Cohort FE	State-Specific Cohort Trends	State-Cohort Controls
	(1)	(2)	(3)	(4)
Exit exam	-0.032***	-0.000	-0.002	-0.001
	(0.004)	(0.004)	(0.003)	(0.003)
Accountability	-0.020***	0.001	-0.002	-0.002
	(0.005)	(0.004)	(0.003)	(0.003)
F-test	53.61	0.0210	0.565	0.177
Minimum competency	-0.033***	0.001	-0.002	-0.000
	(0.004)	(0.004)	(0.003)	(0.003)
Standards-based	-0.029***	-0.004	-0.006	-0.003
	(0.006)	(0.005)	(0.005)	(0.006)
Consequential	-0.018**	-0.001	-0.002	-0.002
	(0.008)	(0.005)	(0.004)	(0.004)
Report card	-0.023***	0.003	-0.002	-0.002
	(0.007)	(0.005)	(0.004)	(0.004)
F-test	26.94	0.404	0.480	0.140
Minimum competency (MC)	-0.033***	0.001	-0.001	0.002
	(0.004)	(0.004)	(0.003)	(0.004)
Standards-based	-0.029***	-0.003	-0.010	-0.010
	(0.009)	(0.007)	(0.007)	(0.008)
Standards-based after MC	-0.029***	-0.006	-0.001	0.006
	(0.009)	(0.006)	(0.007)	(0.008)
Consequential	-0.018**	-0.001	-0.002	-0.002
	(0.008)	(0.005)	(0.004)	(0.004)
Report card	-0.023***	0.003	-0.002	-0.002
	(0.007)	(0.005)	(0.004)	(0.004)
F-test	24.95	0.163	0.671	0.588

Notes: The unit of observation is a state graduation year cohort and includes cohorts that turned 18 years old between 1977 and 1998 and were interviewed in the 1990 or the 2000 US Censuses. Both the 1990 and the 2000 Census datasets are 5 percent samples. The data is subset to individuals who are residing in the same state as their state of birth. The dependent variable is the proportion of people in that state and graduation cohort who received a high school diploma or a GED. The first panel combines all exit exams into one explanatory variable; the second panel distinguishes between a minimum competency and standards-based exams; the third panel further divides standards-based exams by whether or not they were preceded by a minimum competency exam (standards-based and standards-based after MC are mutually exclusive). Robust standard errors are reported in column (2). Reported F-tests are for explanatory variables displayed in each panel. State-cohort controls include: poverty and unemployment rates, average hourly wages. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A4: Effects of Testing on Graduation, Weighted Regressions

HS Graduation/GED	(1)	(2)	(3)	(4)
Mininimum Competency (MC)	-0.015***	-0.002	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.003)
Standards-based	-0.003	-0.007	-0.013***	-0.011**
	(0.005)	(0.005)	(0.005)	(0.005)
Standards-based after MC	-0.018***	-0.006*	-0.001	0.001
	(0.003)	(0.003)	(0.004)	(0.005)
Consequential	-0.016***	0.003	0.002	0.002
	(0.003)	(0.003)	(0.002)	(0.002)
Report card	-0.010***	-0.000	-0.003	-0.002
	(0.004)	(0.003)	(0.003)	(0.003)

Notes: The dependent variable is the proportion of people in that state and graduation cohort who received a high school diploma or a GED. Column (1) does not include any controls, column (2) controls for state and cohort FE and report robust standard errors. Column (3) controls for state-cohort time trends and column (4) adds state-specific controls. State-cohort controls include: poverty and unemployment rates, average hourly wages. Regression weights are inversely proportional to the variance of an observation. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A5: Estimated effects on the log(hourly wage) distribution (conditional on working)

	10-percentile		30-percentile		50-percentile		70-percentile		90-percentile	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Exit exam	0.007	-0.004	0.007	-0.002	0.017	-0.001	0.017	-0.007	0.005	-0.007
	(0.010)	(0.006)	(0.012)	(0.005)	(0.012)	(0.005)	(0.014)	(0.006)	(0.014)	(0.008)
Minimum competency	0.006	-0.003	0.006	-0.001	0.015	0.000	0.012	-0.007	0.006	-0.005
	(0.009)	(0.006)	(0.012)	(0.006)	(0.011)	(0.005)	(0.013)	(0.006)	(0.013)	(0.008)
Standards based	0.006	-0.008	0.009	-0.000	0.019	-0.004	0.028	0.003	0.004	-0.000
	(0.017)	(0.009)	(0.018)	(0.009)	(0.020)	(0.009)	(0.021)	(0.009)	(0.025)	(0.012)

Notes: The data used to produce this table comes from the CPS MORG files. Each cell is a regression model with two specifications. The dependent variable is a wage quantile for state *s* and cohort *c* obtained from a quantile regression, unit of observation is a state-graduation year cohort. Model 1 includes state and cohort dummies. Model 2 also includes state-specific cohort trends. Standard errors reported in column (2) are clustered by state. All regressions control for accountability.* significant at 10%; ** significant at 5%; *** significant at 1%.