HIGH BARS OR BEHIND BARS? THE EFFECT OF GRADUATION REQUIREMENTS ON ARREST RATES

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Abstract
This paper investigates the effect of high school graduation requirements on arrest rates with a specific focus on the number of required courses and the use of exit exams. Identifying variation comes from state-by-cohort changes in the laws governing high school graduation requirements from 1980 to 2010. Combining these law changes with arrest rates of young adults from the Federal Bureau of Investigation's Uniform Crime Reports, I find that the use of exit exams can reduce arrest rates by approximately 7 percent. Although it is difficult to parse out the exact mechanisms, additional exploration into heterogeneity by age and offense, as well as examination of labor market outcomes, suggest that these policies may have increased learning. Given the current debate around the use of exit exams, this paper provides evidence of beneficial effects on nonacademic outcomes. This paper also provides further evidence of the influence of education policy on crime.

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1. INTRODUCTION

High schools in the United States have often been criticized for not adequately preparing students for college and the workforce. Critics complain that high college attrition rates are due to students arriving unprepared, and employers often find that high school graduates lack the necessary skills to be effective workers immediately upon graduation.[1] To address these issues, state and local governments can raise high school graduation requirements, with the idea that raising the bar for graduation will better prepare students and ensure graduates are ready for life after high school. Since 1980, mostly in response to the Reagan administration's *A Nation at Risk* (NCEE 1983), nearly every state has raised the total number of courses required for graduation.[2] At the same time, many states have implemented exit exams in an attempt to guarantee that all high school graduates are proficient in the required curriculum. In 1980, only one state used an exit exam, whereas in 2012 twenty-five states used them, covering nearly 70 percent of all public school students in the United States (McIntosh and Kober 2012). More recently, controversy about withholding diplomas due to exam performance has led several states to delay their implementation or abandon them completely.

Prior research on the effects of raising graduation requirements has found mixed results. In some cases, the research shows positive effects on wages, employment, and college attendance, although some research has also shown adverse effects, causing more students to drop out of high school.[3] These papers focus on academic and labor market outcomes—less is known about the potential effect on other areas. This paper adds to the growing debate surrounding graduation requirements by estimating the effect they have on crime, in particular arrests for property and violent crimes. To date, only one other paper has investigated effects of these policies on crime. Baker and Lang (2013) examine the effects of exit exams on incarcerations among other labor market variables using census data and find no statistically significant effects. By focusing on arrests, this paper is able to more thoroughly investigate the effects and their potential mechanisms than did Baker and Lang.

Education has long been considered a key deterrent to crime. The correlations between education and crime are well known and large. For example, in 1997, 75 percent of state prisoners did not complete high school (Harlow 2003). Similarly, researchers using more sophisticated techniques estimate a causal effect, finding that more education reduces crime (Lochner and Moretti 2004; Machin, Marie, and Vujić 2011; Cook and Kang 2016). These effects appear to be driven by both human capital effects, which raise the opportunity cost of crime, and incapacitation effects, which keep students occupied (Jacob and Lefgren 2003; Luallen 2006; Anderson 2014; Fischer and Argyle

1. For first-time, full-time bachelor's-degree seeking students entering college in 2010, the six-year graduation rate was 60 percent (IES 2018).
2. Only five states did not change their mandated course requirements between the class of 1980 and 2010.
3. Goodman (2019) finds increases in wages and employment following math course increases. Bishop and Mane (2001) find an increase in earnings for states with exit exams. Dee and Jacob (2006) and Goodman (2019) see increases in college enrollment for certain populations. Dee and Jacob (2006), Warren, Jenkins, and Kulick (2006), Bishop and Mane (2001), and Lillard and DeCicca (2001) find that more difficult requirements lead to more high school dropouts. Additionally, using regression discontinuity designs, Martorell (2004), Papay, Murnane, and Willett (2010, 2014), and Ou (2010) find students who barely fail the exit exam are less likely to graduate than those who just pass the exam. However, Reardon et al. (2010) do not find these effects using a similar design.

2018). Given the high societal costs associated with crime, education policies that affect crime rates should be closely scrutinized for their potential large externalities.[4]

Given this evidence, the conventional belief is that more and higher-quality education reduces crime. However, the effect of increasing graduation requirements is not obvious a priori. As Betts and Costrell (2001, p. 2) state, “almost any change creates winners and losers.” If more rigorous requirements succeed in increasing the quality of education, one might expect crime rates to fall. This could happen if the exams incentivize students to learn the material more thoroughly or incentivize schools to improve teaching. The exams may focus the curriculum on the skills most important for productivity after graduation. Finally, the exam requirement may lead to increased attendance because students know they need to pass the exams to graduate. As such, crime may drop as more students choose to be in the classroom. Conversely, increasing requirements also makes it more difficult to graduate from high school. This means that students on the margin of graduating may drop out due to the policy change. If a diploma has signaling value,[5] the marginal dropouts will receive lower wages and will therefore have a lower opportunity cost of crime.[6] Furthermore, students who drop out of school early will have more free time with which to commit crimes. Thus, these policies may result in both “human capital” and “dropout” effects that work in opposite directions. For example, previous research has shown that students facing more difficult requirements have higher wages, larger college-going rates, and higher employment rates (Bishop and Mane 2001; Goodman 2019), which could lower crime. On the other hand, high school completion rates are lower, and dropout and GED rates are higher (Martorell 2004; Dee and Jacob 2006; Warren, Jenkins, and Kulick 2006; Ou 2010), which could increase crime. The net effect is an empirical question that depends on both of these effects. This paper will add a unique contribution to this literature by being one of the few papers to examine how education policy around curriculum can affect crime rates as opposed to simply more or “better” education.

To estimate the effects described above, I use arrest data from the Federal Bureau of Investigation’s (FBI’s) Uniform Crime Report (UCR)[7]. Using the repeated cross-section design of the UCR, I am able to create a panel dataset of cohorts within each police agency’s boundaries, where a police agency is typically the local precinct or district in charge of law enforcement. As states raise the bar for graduation, students face different requirements due to the plausibly exogenous “event” of when they entered high school. I control for agency-by-year and cohort-by-year differences in arrest rates

4. Externalities associated with crime can be quite large. High-crime neighborhoods are faced with lower property values, higher insurance costs, and often an overall lower standard of living (Anderson 1999). Taxpayers are also negatively affected by crime; in 2009, states spent $52.3 billion, or 3.4 percent of their total spending, on corrections alone (NASBO 2010). Lochner and Moretti (2004) estimate that raising high school completion rates by 1 percent could have social benefits of over $2 trillion. Belfield et al. (2006) estimate reductions in crime as a significant reason why the social benefits of the Perry Preschool Program exceed the costs.
5. Martorell and Clark (2014) find little evidence of signal effects from a high school diploma, whereas Jaeger and Page (1996) do find significant effects. Tyler, Murnane, and Willett (2000) also find a positive signal value of attaining a General Educational Development (GED) degree.
6. Machin and Meghir (2004) observe that wage decreases in the bottom 25th percentile are associated with significant increases in crime.
7. U.S. Department of Justice, FBI, *Uniform Crime Reports: Arrests by Age, Sex, and Race, Summarized Yearly* (Washington, DC: U.S. Department of Justice, FBI, and Ann Arbor, MI: Interuniversity Consortium for Political and Social Research, 1980–2010).

by implementing a fixed effects model. This specification focuses on variation within an agency and year, controlling for many of the potential confounding factors that could occur simultaneously with increases in graduation requirements. The assumption behind this identification strategy is that adjacent cohorts are essentially identical except that one cohort faces higher graduation requirements than the other.

Results show that requiring exit exams decreases arrest rates by approximately 7 percent. This finding is primarily due to a decrease in property crimes and its components, although there is also a decrease in overall violent crime. I do not find any significant effects of increasing course requirements. This finding supports the literature that finds beneficial effects of exit exam use, especially for post high school outcomes. It is also one of the few papers to show that specific education policies can have crime-reducing effects.

Although it is difficult to know the exact channels through which exit exams affect arrest rates, there is potential evidence of increased learning occurring. For example, reductions in arrests are largest in the poorest counties and there is evidence of increased hourly wages for students who faced an exit exam. Taken together, this is suggestive that the use of exit exams has, on average, increased the human capital of students. This could occur if the exams refocus the curriculum on skills that are important on the job market, if they incentivize students to study and learn the material better, or if they motivate schools to improve teaching.

The remainder of this paper proceeds as follows: I next describe the reforms and prior literature. Section 3 defines my empirical strategy and methods. Section 4 describes the data used in my estimation strategy, followed by estimation results in section 5. Section 6 concludes with a discussion of the implications of these results.

2. POLICY BACKGROUND AND PRIOR LITERATURE

Information on Graduation Requirements

Every high school requires students to meet certain requirements before they are allowed to graduate. Two of the most common requirements are course requirements and exit exams. A course graduation requirement (CGR) is the number of courses students are required to pass in order to graduate. The state typically mandates a minimum number of courses while allowing individual school districts flexibility to set higher requirements.[8] Under the assumption that students can only pass classes where they have learned the required material, this policy ensures that anyone receiving a diploma will have learned at least the minimum content the state deems necessary. In addition, CGRs require students to exert enough effort to finish all of their required classes, which may help to build valuable noncognitive skills such as persistence and motivation.

Prior to 1983, states' course requirements were quite static, but the Reagan Administration's release of *A Nation at Risk* (NCEE 1983) led to a series of state-level changes

8. Some states do not use state mandates and instead leave all the decisions to the local school districts. From 1980 to 2010, thirteen states have exercised this option at least once. These are California, Colorado, Connecticut, Florida, Iowa, Maine, Massachusetts, Michigan, Nebraska, New Jersey, Vermont, Washington, and Wisconsin. By 2010, only six states did not have statewide requirements. During the years that these states have no minimums, I assume the minimum courses are zero unless they require a full set of core courses, in which case I set the minimum number of courses to the sum of the number of core courses they must take.

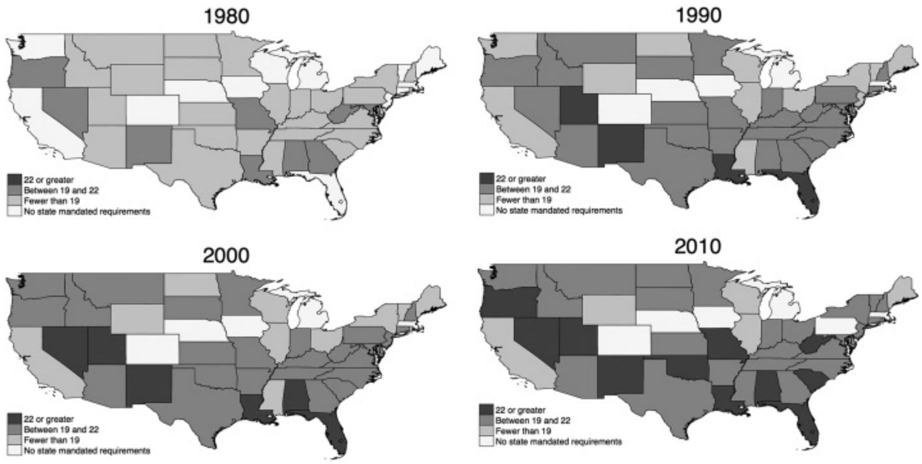


Figure 1. State Mandated Minimum Course Requirements

(Lillard and DeCicca 2001; Goodman 2019). The publication warned that the education system in the United States was failing to adequately prepare students for the future and provided several suggestions for improvements. The report recommended that all students should be required to take "(a) 4 years of English; (b) 3 years of mathematics; (c) 3 years of science; (d) 3 years of social studies; (e) one-half year of computer science" (pp. 123–124). In the years following this report, many states increased their requirements, resulting in a string of reforms from 1983 to 1986. These law changes typically applied to new ninth-grade cohorts, grandfathering prior students into their original requirements. Several states instituted further changes to their curricula, which mainly affected the graduating classes of the mid to late 1990s. Figure 1 provides a visual representation of these changes over time.

The second type of graduation requirement I examine is the presence of "exit exams"—standardized tests that students must pass in order to receive their diplomas. Details vary widely across states with respect to the subjects covered, difficulty, passing threshold, and number of attempts allowed. In most cases, the exams test material ranging from an eighth- to tenth-grade level.[9] In most cases, students who do not pass their first attempt are allowed to retake the exam during future testing periods. In theory, the presence of an exit exam focuses the curriculum on what the state deems important and guarantees each graduate leaves high school with at least that minimum knowledge. However, exit exams may also block marginal students from graduating, as well as alter incentives, leading to "teaching to the test" (Jacob 2005, 2007).

The use of exit exams was rare until the 1980s, when several states started to implement them, partially in response to *A Nation at Risk*.[10] Their popularity continued to rise throughout the next few decades such that eighteen states mandated an exit exam as part of their state curriculum in 2000. In addition, the No Child Left Behind Act

9. More recently, many states have moved toward "end-of-course" exams, which are focused on specific course content and are administered at the end of the semester in which the course was taken. The majority of these exams were implemented after the time frame studied in this paper.
10. Only New York used an exam in 1980.

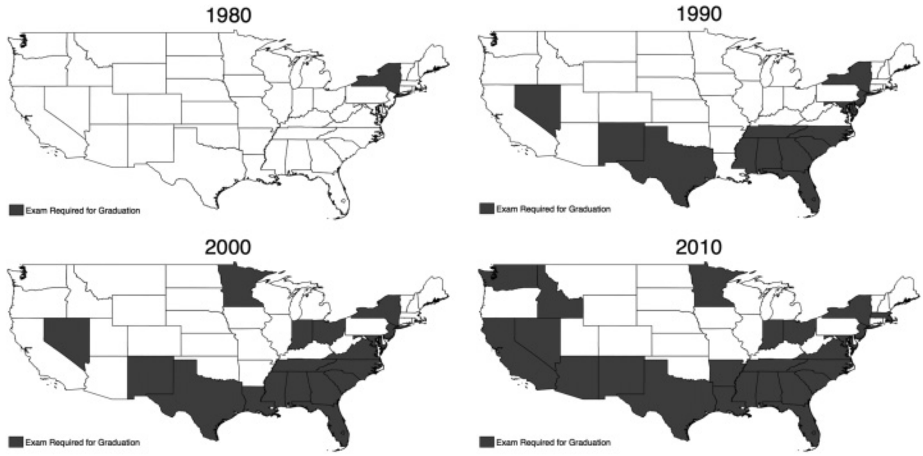


Figure 2. Exit Exam Requirements by State

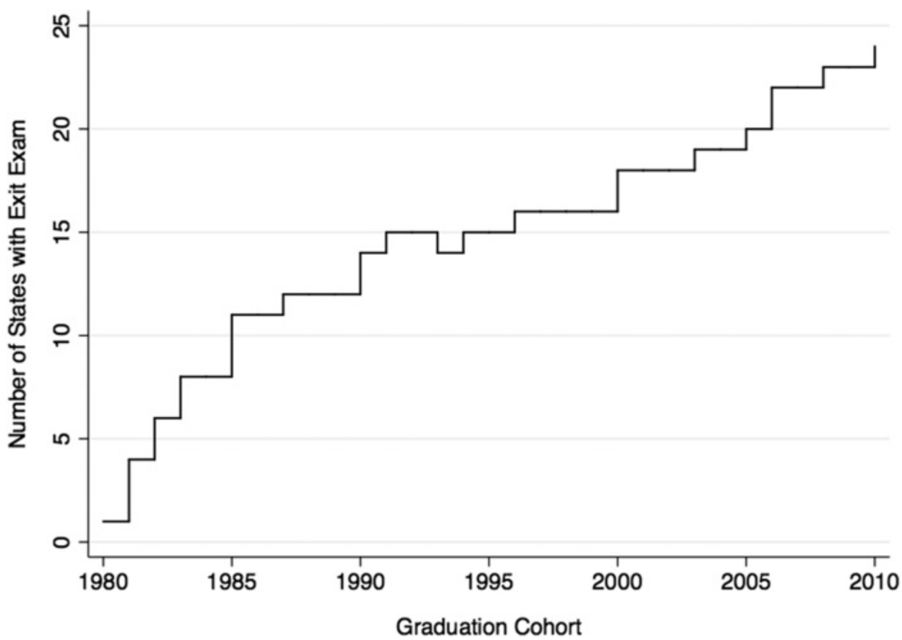


Figure 3. Changes in Exit Exams Over Time

of 2001 (NCLB) required states to give high school students at least one standardized exam. Many states chose to make this exam high stakes for students by requiring them to pass it in order to receive their diploma. The implementation of NCLB further expanded the number of states requiring exit exams throughout the 2000s, and by 2012 half of the states used some form of exit exam. Figures 2 and 3 show the growth in the use of exit exams from 1980 to 2010, with fairly steady growth over this period. The earliest exams tended to be in the South, before moving to the Northeast and eventually the Southwest.

Literature on Graduation Requirements

This paper bridges the gap between two different literatures: The first examines the effects of raising high school graduation requirements; the second estimates the effect of education policies on crime. In doing so, this paper adds to the sparse literature that seeks to find evidence of the effect of specific education policies on crime. Currently, only one known paper attempts to do this with exit exams. Baker and Lang (2013) use state and cohort variation in exit exam implementation to estimate their effects on a variety of outcomes, including incarceration rates. Using the 1990 and 2000 censuses they find positive but mostly insignificant effects on incarceration rates. In this paper I more thoroughly investigate the effect on crime by using yearly arrest rates that allow for investigation of immediate effects, rather than later-life incarceration rates. This is important as effects could fade over time or differ based on age. Additionally, not all arrests will lead to incarcerations, and those that do could have short sentences that are concluded before they are observed in the census. This may be especially true for juveniles, who are of particular interest when looking at the connection between education policy and crime. The arrest data are also more detailed than the incarceration data, allowing me to differentiate effects by offense type, and provide suggestive evidence of the mechanisms at work. Because Baker and Lang (2013) is the only paper that covers both literatures, I review both literatures separately below.

Several prior studies have evaluated the effect of raising high school graduation requirements on students' education and labor market outcomes.[11] Earlier work in the exit exam literature used rich individual datasets, such as the National Educational Longitudinal Survey or High School and Beyond, and generally find no effect on student dropout or graduation (Bishop and Mane 2001; Lillard and DeCicca 2001; Warren and Edwards 2005). However, Bishop and Mane (2001) do find positive effects on college attendance and later life earnings. Although these papers are able to control for robust observable characteristics, they are unable to control for any unobservable characteristics. In addition, these papers necessarily focus on a few select cohorts, which may limit generalizability.

To address the issue of unobservable characteristics, other researchers have used regression discontinuity designs. Results from these papers show that students who barely fail exit exams tend to graduate high school and attend college less often than those who barely pass (Ou 2010; Papay, Murnane, and Willett 2014), though in some cases effects were only evident for disadvantaged subgroups, certain subjects, or on the students' final attempt (Martorell 2004; Papay, Murnane, and Willett 2010). In addition, Reardon et al. (2010) find no effect on a variety of academic outcomes, potentially due to the fact that the exams they study have a lower pass rate than those studied in other settings. One limitation of these studies is that they can only evaluate effects for marginal students, potentially missing important positive or negative effects on students further away from the passing threshold.

A final set of papers uses the state and cohort variation in exam implementation mentioned above to estimate difference-in-differences models. This strategy is most similar to my own and has the benefit of controlling for some unobservable characteristics, as well as measuring effects for a wider range of students and cohorts than the

11. For a thorough review of the effects of exit exams, see Holme et al. (2010).

previous papers. These papers generally find that exit exams lead to a decrease in high school completion, although in some cases that finding depends on the difficulty of the exam administered (Greene and Winters 2004; Dee and Jacob 2006; Warren, Jenkins, and Kulick 2006; Baker and Lang 2013; Hemelt and Marcotte 2013).[12] Two of these papers also examine effects on labor market outcomes: Dee and Jacob find mixed results depending on the subgroup examined, and Baker and Lang find beneficial effects on employment and wages.[13]

In addition to the exit exam literature, several studies have examined the effects of curricular choice. Some of the papers above extended their exit exam analysis to curriculum requirements and found that more difficult course requirements typically have negative or null effects on high school graduation (Bishop and Mane 2001; Lillard and DeCicca 2001; Dee and Jacob 2006). Other papers have attempted to examine the effects of taking more (or higher level) math/science courses on future earnings and employment and found positive effects (Levine and Zimmerman 1995; Rose and Betts 2004; Goodman 2019).[14]

Literature on Education and Crime

This paper is also connected to the literature on education and crime. Education can work to increase human capital, which raises the benefit of legitimate work, thus making crime more costly. Additionally, there can be an incapacitation effect that keeps students busy when they could otherwise be committing crimes.[15] Using compulsory schooling laws as an instrument, several studies have found that additional years of education can lower crime (Lochner and Moretti 2004; Machin, Marie, and Vujić 2011; Cook and Kang 2016). A separate group of papers has evaluated the effect of school quality on crime using exogenous variation from school lotteries (Cullen, Jacob, and Levitt 2006; Deming 2011) and desegregation (Weiner, Lutz, and Ludwig 2009). These studies show that higher-quality schools can also reduce crime.

Finally, four papers have examined the incapacitation effect that education has on crime. Results show that property crime is reduced when students are required to be in school, and that violent crime can increase or decrease depending on whether the shock is temporary or permanent (Jacob and Lefgren 2003; Luallen 2006; Anderson 2014; Fischer and Argyle 2018).

This paper adds to this literature by moving beyond the effect of more or "better" education on crime toward seeing if curricular policy changes can affect crime. This may be especially useful for policy makers, as it can provide information about particular education policies rather than general increases in education. In addition, by exploring heterogeneous affects across ages, offenses, and county demographics, I am

12. Typically, these papers find the higher the grade level tested the bigger the dropout effects. Hemelt and Martcotte (2013) find that effects are mostly limited to states that do not allow alternative pathways to graduation.
13. In addition to effects on graduation and labor market outcomes, several papers have studied the effects on more immediate academic performance, such as test scores, with mixed results (Carnoy, Loeb, and Smith 2001; Jacob 2001; Clark and See 2011; Jürges et al. 2012; Ahn 2014). A related literature on curricular intensification also finds mixed effects on increasing the rigor of middle and high school curricula. As an example, Domina and Saldana (2012) find reduction in math skill gaps following math intensification, but Domina et al. (2015) find negative effects on student achievement.
14. Goodman (2019) only finds results for black students.
15. For a thorough review of the literature concerning the causal effects of education and crime, see Lochner (2012).

able to point to some possible mechanisms for the effect on crime that can be useful when thinking about shaping future policy.

3. DATA

The arrest data come from the FBI's UCR data on arrests.[16] The UCR is the primary source for crime statistics in the United States and is used to create numerous published statistics. The FBI compiles the data after they are collected by local police agencies. They are reported as aggregate counts of arrests by age, gender, and offense at the police agency-by-year level. Police agencies are typically contained within counties and generally represent city police jurisdictions.[17] I use the arrest counts for 15- to 24-year-olds[18] annually from 1980 to 2010. I assume any offender's state of high school attendance is the state in which they are arrested and their graduation year would have been the year in which they turned 18. It is possible that people are arrested in a different state than where they attended high school, but given the relatively young ages being examined it is unlikely this is a major concern.[19]

Even though the key variation occurs at the state-cohort level, I keep the data at the police agency-cohort level to help control for reporting differences that might exist at this level. The arrest data are reported voluntarily, which means that several agencies are missing from the sample for one or more years and reporting protocols may differ across agencies and over time.[20] In years in which agencies provide arrest counts, they are reported for all ages (and, therefore, all cohorts).

It is important to note these data only look at the number of arrests and not the total number of crimes or offenses. Because only arrest data contain the age of the offenders, it is the only measure of crime in which this analysis would be possible. Lochner and Moretti (2004) estimate correlations between the number of arrests and the number of crimes committed to be very high.[21] Furthermore, any changes in arrest rates not associated with changes in actual crime are unlikely to be correlated with graduation requirements.

Although the arrest data are given as the total number of arrests, a more informative measure is the arrest rate, which accounts for population size. Because UCR

16. I use the National Archive of Criminal Justice Data's "Arrests by Age, Sex, and Race, Summarized Yearly" files. These data are aggregated from monthly to yearly counts by the FBI, imputing or dropping police agencies when necessary due to missing monthly data.
17. Police agencies are also referred to as Originating Agency Identifier (or ORIs) in the data. In most cases these agencies are the local police precincts or districts (e.g., the Philadelphia, PA, police department). Other examples of agencies seen in the data are university police, port authorities, and airport police. These are often excluded in this analysis because population estimates are not available for these agencies.
18. Single year of age aggregates are only available for ages 15–24 years. Ages outside of this range are grouped into bins that makes separating cohorts impossible.
19. It is possible that the graduation date is measured incorrectly for some individuals, but this will likely lead to classical measurement error and bias estimates towards zero.
20. Of the 14,888 unique agencies during this period, the most unique in any given year is 11,689. The least in any given year is 6,825 and the average is 9,261. Data can be missing for several reasons. Some reporting agencies may have been created or disbanded in my sample window. Agencies may miss reporting deadlines or lose their records before the reporting deadline. In some years whole states did not report. Florida, Georgia, Iowa, Kansas, Kentucky, Montana, New Hampshire, South Carolina, Vermont, and Wisconsin all have at least one year where they did not report. However, coverage of the U.S. population during this period is approximately 80 percent (Snyder 2011).
21. They estimate values of: 0.97 for burglary, 0.96 for rape and robbery, 0.94 for murder, assault, and burglary, and 0.93 for motor vehicle theft.

agencies are not a commonly measured geographical area, the data contain an estimate of the total population within the jurisdiction of each police agency. Unfortunately, these population estimates are not age-specific. In order to estimate age-specific population estimates at the agency level, I use age, county, and gender population estimates from the Surveillance Epidemiology and End Results (SEER) Program. Specifically, I calculate the age-and-gender-specific population distribution of each county and assign that same distribution to each police agency within that county. Combining the distribution of ages from SEER with the total population counts from the UCR data, I obtain an estimated count of age-by-gender-specific populations in that agency's jurisdiction.

Data on state exit exam requirements come from a variety of sources.[22] CGR data are gathered from several editions of the *Education Commission of the States, Clearinghouse Notes,* and the *Digest of Education Statistics*. CGRs are defined as the state-mandated minimum number of total courses a particular cohort would have to pass in order to graduate from high school. These are standardized across states so that each unit is the equivalent to a "Carnegie unit" or year-long course.

Most school-level control variables are from the *Digest of Education Statistics*. These data include the average pupil–teacher ratio, teacher salary, and per-pupil expenditures. County level economic control variables are from regional data from the Bureau of Economic Analysis and include the employment-to-population ratio, average income, average transfer payments, and average unemployment payments.

After aggregating all years of data, my sample consists of 14,888 unique police agencies across forty-eight states for men and women aged 15 to 24 years.[23] This results in a total of 2,627,438 observations across an unbalanced panel of cohorts. Summary statistics are presented in table 1 and are weighted by police agency-age-year population cells in order to estimate population representative averages.

4. EMPIRICAL STRATEGY

To estimate the effect of graduation requirements I use variation in policies across states and cohorts using the following ordinary least squares model:

$$\frac{ARRESTS_{gpy}}{POP_{gpy}} = \alpha + \beta_1 CGR_{gs} + \beta_2 EE_{gs} + \beta_3 SCHOOL_{gs} + \beta_4 ECON_{gc} + \psi_{py} + \gamma_{yg} + \rho_s * g + \varepsilon_{gpy}, \tag{1}$$

where *ARRESTS* are the aggregate number of arrests for people in graduation cohort g in the boundaries of police agency p, in year y.[24] Population (*POP*) varies at the same level and is the estimated number of people of in cohort g residing in a given agency's jurisdiction each year.

22. The primary source of exam classification is Dee and Jacob (2006). To expand and verify their list I also used Warren, Jenkins, and Kulick (2006), various years of the *Digest of Education Statistics*, and independent research into various state education Web sites and law revisions.
23. Alaska and Hawaii are excluded from the analysis because not all control variables are available for these two states.
24. Graduation cohort is defined as year minus age plus 18. I can estimate this equation at the state level rather than police agency level, although some years have missing agencies that may cause bias when aggregating to higher levels. Results at the state level are similar and presented as a robustness check.

Table 1. Summary Statistics

	Mean	Standard Deviation	Minimum	Maximum
Population	332.24	1,476.30	0.09	168,585.60
Graduation requirements				
Total courses required	15.26	7.80	0	24
Total courses required (excluding zero)	18.63	3.37	10.5	24
Exit exam	0.22	0.41	0	1
Education variables (state level)				
Pupil–Teacher ratio	17.65	2.81	10.70	24.91
Teacher salary	33,998.55	12,087.29	10,695.25	65,813.25
Per-pupil expenditures	5,362.12	2,633.28	1,220.75	17,063.25
Minimum drop age	16.64	0.85	14	18
Economic variables (county level)				
Employment–Population ratio	0.58	0.22	0.11	3.70
Income (per capita)	23,077.44	11,304.22	2,798.50	147,073.80
Transfer payments ($1,000 per capita)	3.03	1.58	0.19	12.56
Unemployment payments ($1,000 per capita)	0.10	0.07	0.00	1.36
Arrest rates (per 1,000 people)				
Total	29.66	30.19	0	89,179.66
Violent	6.73	8.47	0	18,756.09
Property	22.93	26.65	0	89,179.66
Burglary	5.17	7.87	0	25,622.05
Larceny	15.25	21.18	0	89,179.66
Auto theft	2.35	4.57	0	9,377.15

Notes: Each variable presents the cell (police agency-by-age-by-year) average, standard deviation, and minimum and maximum values. Sample is restricted to the main analysis sample, which covers 18- to 24-year-olds from 1980 to 2010. This results in 2,627,305 observations for each variable. All variables (except population) are weighted by cell population size. Course requirements are the total number of courses required in a given state and year for graduation reported in Carnegie units. Education and economic variables are the state-by-cohort and county-by-cohort faced while expected to be enrolled in high school, respectively. All dollar values are in current year dollars.

CGR is the state-mandated minimum number of courses that graduation cohort *g* would have to pass in order to graduate in state *s*.[25] *EE* is an indicator variable for the presence of an exit exam.[26] Changes in any of these requirements will affect cohorts entering high school after the change, as continuing high school students are "grandfathered" into the prior requirements. Thus, each graduating class within a state faces the same requirements throughout their high school careers.

SCHOOL is a vector that includes mean school characteristics in each state during the time that the cohort is in high school (between age 15 and 18 years).[27] *ECON* are the

25. In some instances there is no state-mandated minimum requirement and instead course requirements are delegated to the local school district. In these states the requirement is set at zero, but an indicator variable is included that is equal to "1" when the state opts to delegate to the local level.
26. Unlike Dee and Jacob (2006) and Warren, Jenkins, and Kulick (2006), my primary results do not separate exams by difficulty. I found no significant differences when using this strategy, and therefore limit to a single indicator (results with separated difficulty available upon request).
27. These include pupil–teacher ratio, teacher salary, per-pupil expenditures, and dropout age. These data are from *Digest of Education Statistics* except for the minimum dropout age, which is from Oreopoulos (2009) and supplemented with the *Digest of Education Statistics*.

average economic characteristics each cohort faced in county c while in high school.[28] These control for the possibility that changes in graduation requirements could occur in a certain type of economic climate that may also affect crime rates. For example, states experiencing low average income may be the most in need of education reform but also tend to have the highest crime rates. These controls are complemented with a full set of police agency-by-year (ψ_{py}), and year-by-cohort (γ_{yg}) fixed effects as well as state-specific linear cohort effects.

This estimation strategy exploits the repeated cross-section nature of the data to control for many factors than would otherwise be possible. It is essentially a difference-in-differences strategy where the differences are across cohorts and states.[29] These models typically involve the inclusion of state and cohort fixed effects. However, because each cohort in a given state is observed in multiple years, the state and cohort fixed effects can be interacted with year fixed effects. These interactions are able to control for any factors common to all agency residents (across all cohorts) in a given year and any common shocks to each cohort in a given year, respectively. This strategy is similar to the one used by Hemelt and Marcotte (2013) and controls for more potential sources of omitted variable bias than those used by Baker and Lang (2013) and Dee and Jacob (2006). For example, the number of police officers employed in a given district and year, and the use of laws such as "three strikes" that gained popularity during this era, are controlled for with this model. Additionally, state-specific linear cohort effects ($\rho_s * g$) will control for differential arrest trends across states.

The necessary assumptions for estimating causal effects are that the requirement changes are exogenous to trends in crime and that there are no omitted variables or common shocks affecting crime and requirements simultaneously. Because many of the changes were in response to suggested national requirements and not part of an attempt to reduce crime, this assumption appears plausible, though it is possible that there are some differences in states that decided to adopt early versus late. Additionally, any simultaneous attempts to lower crime rates would likely affect all cohorts simultaneously and not just those select cohorts whose graduation year coincides with the change in requirements. Falsification tests provided later suggest that this is a reasonable assumption.

5. RESULTS

Before estimating equation 1, it is useful to examine the raw differences in trends and arrest rates across states with and without exit exams. This is somewhat difficult to do because states vary in the years in which they implement exams. Figure 4

28. Note that police agencies are matched to the counties in which they reside. Because there are no relevant control variables measured at the police agency level, the economic and education controls are instead measured at the county and state in which they are located.
29. Goodman-Bacon (2018) argues that the estimates from this type of model are a weighted average of all two period/two group (2x2) differences-in-differences models. The weights are determined by group sizes and the variance of treatment within each pair. The paper also states that when early adopters act as controls for later adopters and treatment effects change over time, their treatment effects are subtracted from the true effects, which typically biases the regression away from the sign of the true estimate. In the case of exit exams there are several states that are early adopters and therefore act as controls for later adopters. If the true treatment effect changes over time, then it is likely the case that these results understate the true results to some degree.

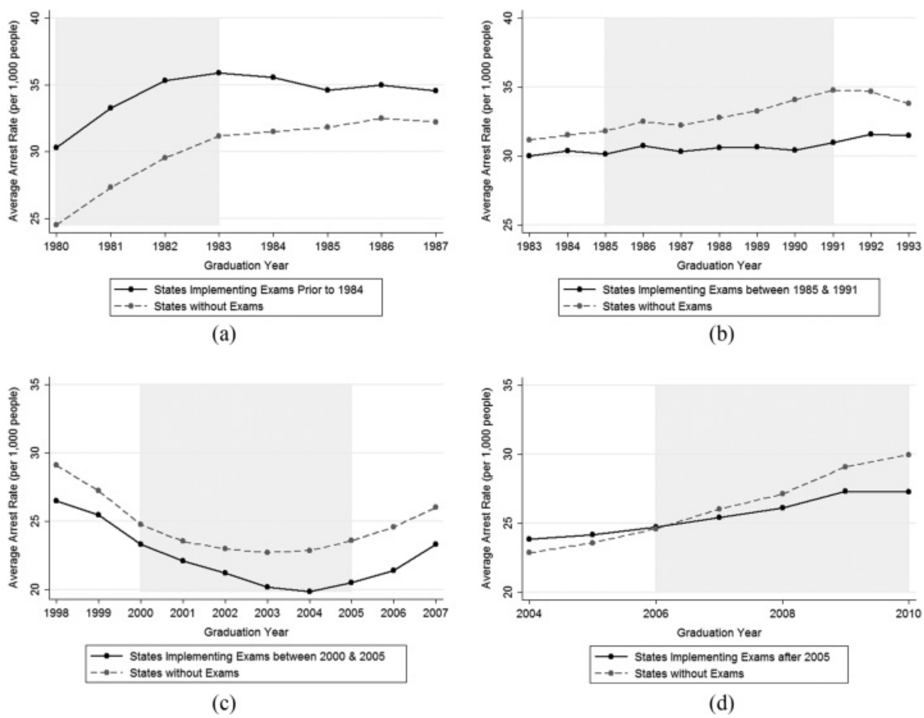


Figure 4. Trends in Arrest Rates for States with and without Exit Exams

examines the trends for states with exit exams versus those without exams, based on years in which the exams were first implemented (e.g., early, middle, and late adopters). These figures typically show a scenario where arrest-rate trends post-exam implementation favor the states that implement exams relative to those that do not. That is, in scenarios where exam states had an initially higher arrest rate, the gap was reduced, whereas in scenarios where exam states had an initially lower arrest rate, the gap widened.

Although the raw figures suggest crime-reducing benefits of exit exams, they do not control for any potential confounding factors, like differential linear trends or state and year effects. Table 2 shows a series of estimates based on equation 1 that control for these factors. All results are weighted by cell population size, contain full sets of state-by-year and cohort-by-year fixed effects, and use standard errors clustered at the state level. Columns 1 and 2 separately analyze the effects of the two key graduation requirements, and column 3 includes both in the same regression. These focus the key variation within state and year, while simultaneously controlling for any cohort and time effects. In each of these specifications the course requirements show no effect on arrest rates but exit exams reduce arrest rates by approximately 8 percent from the mean. Inclusion of additional state-by-cohort education and economic controls has little impact on any of the key estimates in column 4, likely because much of the confounding variation has been controlled for with the police agency-by-year fixed effects. Finally, in column 5, state-specific cohort trends are added in order to control for differential linear trends across states. Inclusion of these trends has little effect on the key coefficients.

Table 2. The Effect of Graduation Requirements on Arrest Rates

Specification	(1)	(2)	(3)	(4)	(5)
Courses required	−0.024 (0.294)	–	−0.002 (0.293)	0.122 (0.291)	−0.042 (0.114)
Percent change from the mean	−0.08%		−0.01%	0.41%	−0.14%
Exit exam	–	-2.483^{**} 0.958	-2.405^{**} (0.903)	-2.517^{**} (0.963)	-2.183^{***} (0.762)
Percent change from the mean		−8.37%	−8.11%	−8.49%	−7.36%
Observations	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305
Average arrest rate	29.66	29.66	29.66	29.66	29.66
State-by-year fixed effects	Yes	Yes	Yes	Yes	Yes
Graduation cohort-by-year fixed effects	Yes	Yes	Yes	Yes	Yes
Education and economic controls	No	No	No	Yes	Yes
State specific linear cohort trends	No	No	No	No	Yes

Notes: Each specification represents a different regression where the outcome is the total number of arrests per 1,000 individuals. The unit of observation is a police agency-by-graduation cohort-by-year cell. Each regression is weighted by police district-age-year population cell. Robust standard errors in parentheses are clustered at the state level. Education controls are state-cohort average pupil–teacher ratio, teacher salary, per-pupil expenditures, and dropout age. Economic controls are county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments. All specifications also include an indicator variable for cohort-state combinations that face no state mandated minimum course requirements.

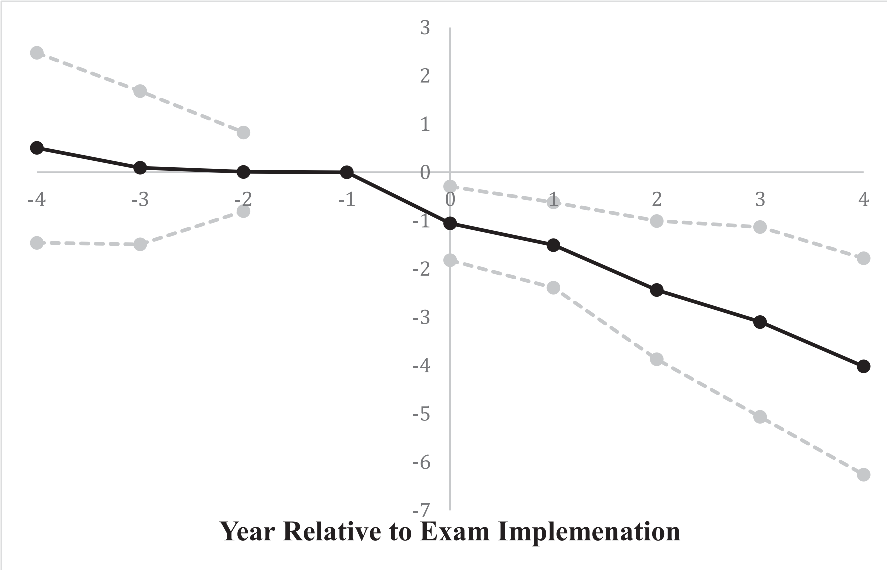
***Significant at 1%; **significant at 5%.

In this preferred specification, cohorts that face an exit exam have 2.2 fewer arrests per 1,000 individuals than those without any exit exams—an approximately 7-percent reduction from the average.

In order to explore potential dynamic effects and to examine whether there are violations of the “parallel trends” assumption, I estimate a modified version of equation 1 that replaces the single exit exam indicator variable with a set of interactions of the exit exam indicator with “event time” (year relative to exam introduction). The analysis is done within a nine-year window around the introduction of the exam. To ensure the estimated effects are not the result of sample composition differences, I use a balanced panel of observations across the window. This step necessarily limits the sample because many states and cohorts are not observed for each of those nine years.[30] Results are shown in figure 5 and are consistent with the raw trends in figure 4 and the estimated effects in table 2. Trends prior to the exam requirement are flat, whereas after the exams are implemented arrest rates decline immediately and continue declining with an average of approximately –2.4 arrests per 1,000 people over the five years post-implementation.

These results show moderate to large crime-reducing effects of exit exams, which may be initially surprising given the increased dropout rates seen in the prior literature. However, exit exams may have far-reaching effects beyond those for the marginal students who are induced to drop out. For example, they may help refocus the curriculum, build noncognitive skills, and potentially affect the labor market value of a diploma. In

30. Cohorts are limited to the classes of 1983–2004 and the states are limited to the seven that first implemented an exit exam between 1987 and 2000.



Notes: This figure plots coefficients form an even-study analysis. Coefficients are defined as year relative to exam implementation for the state. The regression is weighted by police district-age-population size and control variables are the same as those found in the main estimation model (see table 3). The dashed line represents a 95% confidence interval using state level cluster-robust standard errors.

Figure 5. Dynamic Effects of Exit Exams

addition, some of the prior literature finds that exams do lead to beneficial labor market outcomes.[31]

These effects are in contrast to the small, insignificant crime effects found in Baker and Lang (2013). This difference could arise for several reasons. Using their estimation strategy, Baker and Lang are unable to control for state-by-year fixed effects, which could add some potential omitted variables bias. For example, this would be the case if these requirement changes coincided with reductions in spending on direct crime reduction strategies, like policing and enforcement. In addition, they examine a wider age range and focus on incarcerations rather than arrests, both of which could lead to different estimates.

The magnitude of the effects is large but still smaller than those found by Lochner and Moretti (2004) for an additional year of education. Using the same dependent variables that I use in this paper, they find that an additional year of education reduces crime by approximately 11 to 18 percent, which is more than double the 7 percent seen through exit exams in this paper. This difference in effect size is reasonable given that an additional year of education would likely have a much stronger effect on crime than implementing an exit exam.

Although none of these specifications shows any statistically or economically significant effect of course requirements, there are a few important issues to keep in mind.

31. Baker and Lang (2013) and Dee and Jacob (2006) find evidence of beneficial labor market effects for some individuals after exit exam implementation.

Table 3. Effects of Graduation Requirements on Arrest Rates by Offense

			Property			
Offense Specification	Total (1)	All Violent (2)	All Property (3)	Burglary (4)	Larceny (5)	Auto Theft (6)
Courses required	−0.042	−0.042*	0.001	0.025	−0.023	−0.045*
	(0.114)	(0.022)	(0.107)	(0.026)	(0.082)	(0.024)
Percent change from the mean	−0.14%	−0.63%	−0.00%	0.48%	−0.15%	−1.91%
Exit exam	−2.183***	−0.351**	−1.832***	−.395*	−1.022**	−0.410***
	(0.762)	(0.162)	(0.647)	(0.211)	(0.439)	(0.136)
Percent change from the mean	−7.36%	−5.22%	−7.99%	−7.64%	−6.70%	−17.45%
Observations	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305
Average arrest rate	29.66	6.73	22.93	5.17	15.25	2.35

Notes: Each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. The unit of observation is a police agency-by-graduation cohort-by-year cell. Each regression is weighted by police agency-year-age population cell. Robust standard errors in parentheses are clustered at the state level. All specifications include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, all specifications control for state-cohort average pupil—teacher ratio, teacher salary, per-pupil expenditures, and dropout age, as well as county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments. All specifications also include an indicator variable for cohort-state combinations that face no state mandated minimum course requirements.

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

First, the estimation strategy uses the change in state-mandated minimum course requirements. It is possible that large numbers of students are unaffected by these changes if they voluntarily take more than the minimum courses or if their district or school sets a higher minimum requirement.[32] Additionally, what may matter more than total course requirements is the requirements of certain types of courses such as science, technology, engineering, and mathematics courses.[33]

Results by Offense Type

Results in table 3 use the preferred specification 5 from table 2 and examine the impact of graduation requirements on arrest rates by type of offense. Given the multiple mechanisms by which education may affect crime and the ways in which graduation requirements may affect education, it is difficult to predict which offenses should be most affected. However, if much of the effect is driven through an increase in income (due to higher wages or likelihood of employment), then it is reasonable to believe that crimes with monetary gains (such as burglary, larceny, robbery, and auto theft) would be most affected. If there are noncognitive mechanisms at work, then one might also expect changes in nonmonetary crimes (like murder, rape, or assault).

There are a few key results from this table. First, exit exams have negative coefficients across all offense categories.[34] Second, effects are smaller and less significant for

32. Work done by Goodman (2019) suggests that there are several areas that had requirements well above the state minimum and were therefore unaffected by the state-level changes.
33. It could also be the case that the effects of course requirements are nonlinear, but exploration of nonlinearities did not yield significant effects (available upon request).
34. The only component of violent crime with a statistically significant effect is assault. For brevity, the decomposition of violent crimes is excluded from the table. Arson is also excluded as it is inconsistently recorded across agencies over time. Results for all of these components are available upon request. There may be some concern about the large number of hypotheses being tested here. If one were to perform a Bonferroni correction for the eight offenses that make up the aggregate categories, then only auto theft would be significant at the 95

violent crime compared with property crime, with an approximately 5 percent reduction relative to the violent crime mean, and an 8 percent reduction in property crime. As was the case in the overall estimate, course requirements show no strong effects on any offense type, with some marginally significant but otherwise small effects on total violent crime and auto theft.

These results are in line with the theoretical effects and the previous literature. One likely mechanism for beneficial effects is through increased human capital and labor market outcomes. If cohorts facing exit exams have better labor market outcomes, then they would have less reason to obtain income through illegal means, suggesting bigger effects for property crimes like burglary, larceny, and auto theft, which is exactly what is observed in table 3. Although it may be surprising to see effects on violent crime, this is consistent with Lochner and Moretti (2004), who find statistically significant reductions in violent crime due to increased education.

Results by Income and Race

On average, school quality, crime rates, and employment opportunities are worse in poor neighborhoods. Additionally, crime rates tend to be higher among minorities and in more urban areas. Based on the prior research into graduation requirements on academic outcomes, it is difficult to predict which groups will be most negatively or positively affected in terms of arrests. Dee and Jacob (2006) and Hemelt and Marcotte (2013) find larger dropout effects for black students compared with white students. Papay, Murnane, and Willett (2010) find larger dropout effects for low-income urban students, while Hemelt and Marcotte (2013) find no discernable differences by urbanicity. Additionally, Goodman (2019) finds larger *wage* increases for black students following increased math requirements.

To test for differential effects on crime, I interact the graduation requirements with county-level demographic information from 1980. These include the quartiles for average income and percent white as well as the degree of urbanicity of the county.[35] Results of this exercise are presented in table 4. For income, I find that for both types of graduation requirements, counties with the lowest average incomes see the largest negative effects. Results for exit exams are large, negative, and statistically significant for the lowest quartile, while all other quartiles are not significantly different from zero or each other.

For race, course requirements have estimated effects that are similar across all four quartiles. Exit exams exhibit significant and large negative effects for the top two (mostly white) quartiles, while the bottom two quartiles are smaller and insignificant. When separating by urbanicity, it is the rural counties that exhibit the largest crime reduction due to these requirements, with no statistically significant effects being seen in the most metropolitan counties.

percent confidence level (Kling, Liebman, and Katz 2007). However, another common adjustment to multiple hypothesis testing is to combine effects into family categories, which is essentially what is done with the total, violent, and property crime specifications.

35. The urbanization variables are from the SEER Rural-Urban Continuum Code for 1983. I fix the degree of urbanization to the degree the county was in 1983, before the bulk of the change in requirements. (See http://seer.cancer.gov/seerstat/variables/countyattribs/ruralurban.html for more information.)

Table 4. Differential Effects of Graduation Requirements by Income, Race, and Urbanization

	Average County Income (1)	Fraction of County Population White (2)		Degree of Urbanicity (3)
Courses required × First quartile	−0.205 (0.124)	−0.012 (0.147)	Courses required × Metro counties	0.191 (0.173)
Courses required × Second quartile	0.026 (0.163)	0.045 (0.171)	Courses required × Urban counties	−0.263* (0.147)
Courses required × Third quartile	0.127 (0.160)	−0.087 (0.117)	Courses required × Rural counties	−1.070*** (0.394)
Courses required × Fourth quartile	0.046 (0.192)	−0.055 (0.125)		
Exit exam × First quartile	−6.265*** (0.957)	−1.783 (1.097)	Exit exam × Metro counties	−0.558 (0.698)
Exit exam × Second quartile	−0.591 (1.223)	−0.896 (1.292)	Exit exam × Urban counties	−6.794*** (1.058)
Exit exam × Third quartile	−0.420 (1.148)	−2.980*** (0.978)	Exit exam × Rural counties	−8.256*** (2.899)
Exit exam × Fourth quartile	−1.114 (0.945)	−3.725*** (1.077)		
Observations	2,625,693	2,586,329		2,305,377

Notes: Percentiles for income and fraction white are based on their 1980 status. Quartiles are increasing such that the lowest quartile contains the poorest or smallest fraction of the population white. Degree of urbanization is from Surveillance Epidemiology and End Results' (SEER) 1983 Rural-Urban Continuum codes. Each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. The unit of observation is a police agency-by-graduation cohort-by-year cell. Each regression is weighted by police agency-age-year population cell. Robust standard errors in parentheses are clustered at the state level. All specifications include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, all specifications control for state-cohort average pupil—teacher ratio, teacher salary, per-pupil expenditures, and dropout age, as well as county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments. All specifications also include an indicator variable for cohort-state combinations that face no state mandated minimum course requirements.

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

These results are interesting and provide some insight into the mechanism of these effects. If there is indeed a "human capital" effect that reduces crime and a "dropout" effect that increases crime, we would expect the less negative (or more positive) coefficients for the populations where the dropout effect is largest. Based on prior literature, that would be on nonwhite, low-income, and urban students. This is what is generally seen in table 4, with the possible exception of county income. One potential reason that the largest negative effects are in the poorest counties is that those counties have the most crime. Perhaps other counties do not have as much crime to reduce so it is more difficult to see large negative effects in those cases.

From a policy view, these results are favorable to increases in graduation requirements, as they show no significant *increases* in arrests for any of the subgroups examined. In addition, effects are largest in low-income counties, which are often those most in need of crime reduction. However, effects are also largest for the counties with the largest fraction of their population white and often insignificant for the counties with the largest minority presence. This suggests that these policies may widen already large minority–white arrest rate gaps, potentially leading to rising inequality in other dimensions.

Table 5. Differential Effects of Graduation Requirements by Age

	Total Crime (1)	Violent Crime (2)	Property Crime (3)
Courses required × (Ages 15–18)	−0.074 (0.108)	−0.057** (0.025)	−0.017 (0.103)
Courses required × (Ages 19–21)	−0.006 (0.111)	−0.033 (0.023)	0.027 (0.106)
Courses required × (Ages 22–24)	−0.005 (0.147)	−0.011 (0.027)	0.007 (0.139)
Exit exam × (Ages 15–18)	−1.630** (0.789)	−0.113 (0.202)	−1.517** (0.687)
Exit exam × (Ages 19–21)	−1.574** (0.742)	−0.396 (0.254)	−1.179* (0.633)
Exit exam × (Ages 22–24)	−3.577*** (1.209)	−0.712*** (0.264)	−2.865** (1.075)
Observations	2,627,305	2,627,305	2,627,305

Notes: Each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. The unit of observation is a police agency-by-graduation cohort-by-year cell. Each regression is weighted by police agency-age-gender population cell. Robust standard errors in parentheses are clustered at the state level. All specifications include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, all specifications control for state-cohort average pupil–teacher ratio, teacher salary, per-pupil expenditures, and dropout age, as well as county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments. All specifications also include an indicator variable for cohort-state combinations that face no state-mandated minimum course requirements.

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

Results by Age

To get an idea of how the effects might differ across age groups, I interact the key graduation requirement variables with age group indicators. The first age category is 15- to 18-year-olds (or those who are still of high school age), the second 19- to 21-year-olds, and the third is 22- to 24-year-olds. Results of these interactions are presented in table 5. Course graduation requirements generally have similar effects across age groups with one significant effect on violent crime for high-school-aged individuals.

For exit exams, offenses are largest and most statistically significant for the oldest age group (22- to 24-year-olds) across each crime category. Additionally, neither of the younger age groups exhibits any statistically significant effects for violent crime. Once again, this aligns well with the theory that exit exams may have beneficial human capital effects, while exhibiting some adverse dropout effects. If wages or other labor market outcomes increase due to the use of exit exams, the working age population (22- to 24-year-olds) will have the most direct benefits of the policy. Meanwhile, school-aged individuals will have the most direct stress and potential incapacitation changes due to the exams. Previous research has shown that failing the exam can make students depressed, embarrassed, and angry (Cornell, Krosnick, and Chang 2006; Jürges et al. 2012) and that receiving a negative “label” from a poor score on the exam can reduce college going (Papay, Murnane, and Willett 2016). Thus, one might expect the negative coefficients to be largest at the older ages, as observed here.

Table 6. Effect of Graduation Requirements on Educational Attainment and Labor Market Outcomes

	Ages 19–24, 1980–2010				Ages 25–48, 1987–2015			
Specification	High School Completion (1)	Unemployment Rate (2)	Log Weekly Earnings (3)	Log Hourly Wage (4)	High School Completion (5)	Unemployment Rate (6)	Log Weekly Earnings (7)	Log Hourly Wage (8)
Courses required	0.000	−0.001	0.000	0.000	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)	(0.000)	(0.001)	(0.001)
Exit exam	0.005	0.005	0.009	0.009**	−0.001	−0.002*	0.000	0.005
	(0.004)	(0.003)	(0.008)	(0.004)	(0.002)	(0.001)	(0.004)	(0.004)
Observations	7,869	7,869	7,869	7,869	20,537	20,537	20,537	20,537
Average value	0.87	0.10	290.64	7.51	0.91	0.05	719.56	14.04

Notes: Data are from the 1980-2015 Current Population Survey Merged Outgoing Rotation Groups (CPS-MORG). The sample is limited to the cohorts who turned 18 between 1980 and 2010. The unit of observation is the state-by-graduation cohort-by year cell. Robust standard errors in parentheses are clustered at the state level. All regressions include state-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, regressions include state-cohort average pupil–teacher ratio, teacher salary, per-pupil expenditures, dropout age, average employment-to-population ratio, income, transfer payments, and unemployment payments. All specifications also include an indicator variable for cohort-state combinations that face no state-mandated minimum course requirements.

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

Potential Mechanisms

It is difficult to identify the exact mechanisms that lead to the effects seen above. Results are consistent with the theory that both human capital and dropout effects are present, and the human capital effect dominates. However, this can be investigated further by examining effects on labor market and dropout directly. To do this, I turn to the Current Population Survey (CPS) and use a very similar strategy to the one described in equation 1, but with a focus on labor market outcomes instead of crime.

Table 6 presents these results for two different sets of cohorts. The first attempts to mirror the crime analysis as closely as possible by limiting to those who are beyond high school age but still within the ages available in the UCR data (i.e., 19- to 24-year-olds). With these results, none of the requirements has an effect on high school completion[36] or weekly earnings. However, there is evidence that exit exams increase hourly wages. This is generally consistent with the results seen for arrests. Positive wage effects may lead to a reduction in crime once students reach working age.

The second group of results expands the analysis to ages beyond those that can be observed in the UCR data and adds a few additional years in order to track these cohorts to older ages. This analysis shows no statistically significant effect on any of the outcomes, with the possible exception of exit exams marginally reducing unemployment. This may mean that any potential labor market effects are temporary, fading once workers gain sufficient experience. It is worth noting that the "temporary" nature of the wage effects may help explain some of the differences in effects seen across previous studies.

Robustness and Falsification

To ensure that the estimated effects are not a result of any methodological decisions made, table 7 provides robustness checks on the main specification for total arrests.

36. High school completion is defined as individuals who have stated they completed the twelfth grade. These averages are weighted by the weights given in the CPS files from the National Bureau of Economic Research.

Table 7. Robustness Checks

Specification	Unweighted (1)	Log (Arrest Rates) (2)	Log (Total Arrests) (3)	Exclude States without CGR (4)	Pre-NCLB (5)	State Level (6)
Courses required	−0.205	0.002	0.005*	0.325	−0.109	−0.090
	(0.142)	(0.003)	(0.003)	(0.238)	(0.096)	(0.100)
Exit exam	−2.697***	−0.049**	−0.020	−2.320***	−1.521**	−1.816**
	(0.946)	(0.019)	(0.019)	(0.835)	(0.621)	(0.732)
Observations	2,627,305	1,715,726	1,715,726	1,633,057	1,665,058	13,183

Notes: Unless otherwise noted each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. The unit of observation is a police agency-by-graduation cohort-by-year cell, except for specification 6, where it is the state-by-graduation cohort-by-year cell. Unless noted otherwise regressions are weighted by cell population. Robust standard errors in parentheses are clustered at the state level. Specifications 1–5 and 7 include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, they control for state-cohort average pupil–teacher ratio, teacher salary, per-pupil expenditures, and dropout age, as well as county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments. Specification 6 includes all the same controls except replaces agency and county controls with state controls where appropriate. All specifications also include an indicator variable for cohort-state combinations that face no state mandated minimum course requirements. Column 4 excludes the fourteen states that ever had a policy of no state-mandated course requirements. Column 5 limits the sample to 1980-2001 in terms of years and graduation cohorts. NCLB = No Child Left Behind Act; CGR = course graduation requirement.

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

Table 8. Falsification Tests: Alternative Outcomes

Specification	Pupil–Teacher Ratio (1)	Per-Pupil Expenditures (2)	Average Teacher Salary (3)	Minimum Dropout Age (4)	Employment/ Population Ratio (5)	Average Income (6)	Average Transfer Payments (7)	Average Unemployment Payments (8)
Courses required	0.014	16.88***	60.15*	−0.004	0.000	−8.102	0.001	0.000
	(0.017)	(4.053)	(30.140)	(0.006)	(0.000)	(14.300)	(0.001)	(0.000)
Exit exam	−0.221	−49.850	343.100	−0.177*	0.003	424.200	−0.006	0.006
	(0.151)	(56.360)	(431.700)	(0.093)	(0.005)	(297.500)	(0.025)	(0.008)
Observations	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305	2,627,305

Notes: Each cell represents a separate regression where the dependent variable is the column title and the unit of observation is a police agency-by-graduation cohort-by-year cell. Each regression is weighted by police agency-by-age-by year population. Robust standard errors in parentheses are clustered at the state level. All regressions include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends.

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

These include estimation without weighting, using the ln(arrest rates) and ln(total arrests) as dependent variables, excluding states without any state-level course requirements, limiting the sample to years before NCLB, and estimating at the state level. Exit exams are generally robust to these findings with the only insignificant effect being found when the dependent variable is ln(total arrests). Results for course requirements fluctuate from negative to positive but generally stay statistically insignificant, with the exception of using ln(total arrests) as the dependent variable.

Although the estimation strategy in this paper controls for many possible sources of bias, it is still possible that the model suffers from endogeneity. Tables 8 and 9 show the results of several falsification tests that suggest this is not the case. Estimates presented in table 8 are regressions that replace the dependent variable with the state-cohort average education variables and the county-cohort average economic indicators. Each cell in these columns is a separate regression. If the graduation requirements

Table 9. Falsification Tests: False Exit Exams

	Police Agency Level		State Level
Specification	**Four-Year Lead (1)**	**Two-Year Lead (2)**	**Simulated Random Exams (3)**
True exit exam	-2.0967^{***} (0.719)	-1.82^{***} (0.620)	–
False exit exam	-0.614 (0.592)	-0.914 (0.593)	-0.001 (0.744)
Observations	2,627,305	2,627,305	13,183

Notes: Each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. The unit of observation in columns 1–2 is a police agency-by-graduation cohort-by-year cell, while 3 is aggregated to the state-by-graduation cohort-by-year cell. Each regression is weighted by cell population size. Robust standard errors in parentheses are clustered at the state level. All specifications include police agency-by-year (or state-by-year) and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, all specifications control for state-cohort average pupil-teacher ratio, teacher salary, per-pupil expenditures, dropout age, the number of courses needed to graduate, and an indicator variable for cohort-state combinations that face no state mandated minimum course requirements. Columns 1–2 also control for county-cohort average employment-to-population ratio, income, transfer payments, and unemployment payments, while column 3 controls for these variables at the state level.

***Significant at 1%.

were completely exogenous (after inclusion of the fixed effects and trends) then one would expect all of the estimated coefficients in these regressions to be insignificant. This is primarily what I find. Exit exams never significantly predict any of the education or economic variables, with the possible exception of minimum dropout age being marginally significant. Course requirements predict an increase in per pupil expenditures and marginally an increase in teacher salary. However, given sixteen separate regressions one would expect to see at least one significant coefficient by chance.

Table 9 shows the results from two different types of falsification tests. The primary test in columns 1 and 2 includes "lead" exit exam indicators, which identify the periods immediately prior to the actual exam. These are included along with the correct exit exams so that in the absence of endogeneity one would expect the effect of the "lead" or "false" exams to be zero. This is done separately with two- and four-year lags. In these tests the true effect of exit exams is statistically significant and large while the effect of the false exam is smaller and insignificant. Although some specifications do still show a sizeable effect of the false exam, they are typically one-third to one-half as large as the true effect and always much less significant.

The second falsification test in table 9 estimates the effect of simulating random exit exams. This is accomplished by running 2,000 iterations of a simulation that randomly assigns exit exams to cohorts and states across the sample.[37] This is done at the

37. This iteration involves three main steps. First, the number of treated states is decided by drawing from a N(25, 3) distribution, where 24 is the actual number of treated states in the data. Then I randomly draw those treated states from the whole sample. Finally, the initial treated cohort for each state is randomly pulled from a N(1991, 10) distribution. This corresponds to the sample median and standard deviation. This simulation is estimated

state level because of computational limitations. The average effects of the randomly assigned exit exams are shown in column 3 and are very close to zero and highly insignificant. Taken together, these three different types of falsification checks provide strong evidence that the estimated effects are in fact true causal effects of the changes in graduation requirement policy.

6. DISCUSSION

In recent years there has been increasing pressure to improve the education system in the United States, especially for high schools, which have not been as competitive with other developed countries (IES 2018). One tool states have used to improve matters is to increase the standards that students must meet before they can graduate. These policy changes affect every public high school student so it is important to measure all of the potential benefits and costs that are associated with them.

One outcome of these policy changes that has been overlooked until this point is crime. There is a common belief that education can deter crime. Taxpayers and voters often tout slogans such as "build schools, not prisons," alluding to the effect that education can have on reducing crime. Whereas there is an abundance of research that attempts to find a causal link between education and crime, few papers have examined the effect of curricular changes and crime. This paper adds to the literature by demonstrating a causal link between raising graduation requirements and crime.

Using changes in state graduation requirements, I find that increasing graduation requirements decreases arrest rates, on average. This is likely due to the fact that students receive larger increases to their human capital under the tougher requirements, which can have direct effects on their propensity to commit crimes or an indirect effect through an increase in wages. It is not possible to fully disentangle the mechanisms that lead to this increase in human capital, but there are a few possibilities. The increased accountability could motivate schools and students to increase learning. The increased effort needed to pass the exams could also lead to better noncognitive skills and an increased signaling value of a high school diploma. Finally, there could be an incapacitation effect, where students choose to attend school more often knowing they will need to pass the exam in order to graduate.

Among recent controversies surrounding exit exams and the decision of many states to remove them, this paper provides evidence more in favor of their use. It is important to realize that these beneficial effects are an average effect and it is also likely that some students on the margin of graduating would be forced to drop out, and therefore be adversely affected. There is also evidence that effects may be more beneficial for white students than minority students, which may work to increase already large racial gaps in arrests. There is also evidence, however, that the effects benefit lowest income counties the most, which could help the areas most in need of crime reduction.

The goal of these reforms is likely not to reduce crime but it is a positive result. States should use caution when implementing such requirements and should be considerate of how different populations may be affected. Ideally, reforms of this nature would come with a support system that ensures that those students on the margin of passing are

assuming that once a state implements an exit exam it keeps it. This is mostly consistent with reality, with only two states removing their exit exams during this sample period, one of which eventually reinstated it.

able to continue to do so. As Betts and Costrell (2001, p. 15) recommend, “The way out of this dilemma is not necessarily to forgo the benefits of higher standards, but, if at all possible, to craft accompanying policies for those students whose efforts may flag, especially those who might drop out.”

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