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. Many states have altered their high school curriculum in an attempt to improve the human capital acquired by their high school graduates, but these changes may lead to externalities that have not been appreciated. I examine changes in the arrest rates of young people following state level increases to the minimum number of required courses and the implementation of exit exams. Identifying variation comes from changes in state laws governing high school graduation requirements from 1980 to 2000. By utilizing repeated cross section arrest data I estimate the effects of across state-cohort differences in graduation requirements on arrest rates. While pinpointing the exact mechanism is not possible, there are signs that point to both human capital effects that lower arrest rates and dropout effects that increase arrests due to the increased rigor of the requirements. I find evidence that using less difficult exit exams can reduce the arrest rate by 4.4% suggesting that the human capital effect may dominate the dropout effect on average. This effect is strongest on property crimes and in the lowest-income counties.

Keywords: Graduation Requirements; Crime; Exit Exams; Dropout Rate; Human Capital

JEL Classification: I20, I21, I28

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1. Introduction

High schools in the US 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.[1] At the same time, employers often find that graduates lack the necessary skills to be effective workers immediately upon graduation. One mechanism that state and local governments have used, and continue to use, in an attempt to improve these outcomes is raising their high school graduation requirements. The idea is that by increasing the rigor of high school, students will be better prepared for life after graduation. Since 1980, mostly in response to the Reagan administration's *A Nation at Risk*, 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 have mastered the required curriculum. In 1980, only two states utilized exit exams; currently 25 states use exit exams, covering nearly 70% of all public school students in the US (Center on Education Policy, 2012). As states and districts continue a push to increase their requirements, we still know little about their efficacy and potential detrimental effects. In this paper I estimate the effect that these policies have on one potential outcome: crime.

Previous 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; however, research has also shown adverse effects, causing more students to dropout

[1] In 2009, the 6 year graduation rate for Bachelor's students was 55.5%. (www.higheredinfo.org)
[2] 43 of 50 states increased their course requirements between the class of 1980 and the class of 2000.

of high school.[3] Only one other paper has investigated any potential effects of these policies on crime to date. Baker and Lang (2013) examine the effects of exit exams on incarcerations using census data and find no statistically significant effects.[4] My paper is able to more thoroughly investigate potential effects on crime by using yearly arrest rates, which allow me to observe immediate effects on crime rather than later life incarceration rates. The data are also more detailed allowing me to differentiate effects by offense types, providing suggestive evidence of the mechanisms at work.

Education has long been considered a key deterrent to crime. The correlations between education and crime are well known and large; in 1997, 75% 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 Vujic 2011). 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). Given the high costs associated with crime,[5] education

[3] Goodman (2012) finds increases in wages and employment following math course increases. Bishop and Mane (2001) finds an increase in earnings for states with exit exams. Dee and Jacob (2006) and Goodman (2012) find 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 evidence that more difficult requirements lead to more high school dropouts.

[4] Baker and Lang (2013) have recently added incarcerations to their working paper investigating the achievement and labor market effects of exit exams.

[5] 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% of their total spending, on corrections alone (State Expenditure Report 2009).

policies that affect crime rates should be closely scrutinized for their potential large externalities.[6]

The conventional belief is that more, or better, education will lower crime rates. However, the effect of increasing graduation requirements is not obvious a priori. If more rigorous requirements succeed in increasing the quality of education, one might expect crime rates to fall. However, increasing requirements also makes it more difficult to graduate from high school. This means that any student on the margin of graduating may drop out because of the change in policy. If a diploma has signaling value,[7] dropouts will receive lower wages and there will be a lower opportunity cost associated with crime.[8] Furthermore, students who drop out of school have more free time in which to commit crimes. Because the "human capital effect" and the "dropout effect" work in opposite directions, the net effect is an empirical question. Previous research has shown that students facing more difficult requirements have higher wages, larger college entrance rates, and higher employment rates (Goodman 2012; Bishop and Mane 2001). On the other hand, high school completion rates are lower, while dropout and GED rates are higher (Dee and Jacob 2006; Warren, Jenkins, and Kulick 2006; Martorell 2004; Ou 2010). The overall effect on arrest rates will depend on both of these effects.

To estimate the effects described above I use arrest data from the FBI's Uniform Crime Report (UCR). Utilizing the repeated cross section design of the UCR I am able to create a panel

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[6] For example, Lochner and Moretti (2004) estimate that raising high school completion rates by one 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.

[7] Martorell and Clark (2010) find little evidence of signal effects from a high school diploma, while Jaeger and Page (1996) do find significant effects. Tyler, Murnane, and Willet (2000) also find a positive signal value of attaining a General Educational Development (GED) degree.

[8] Machin and Meghir (2004) observe that wage decreases in the bottom 25th percentile are associated with significant increases in crime.

data set of cohorts within each police agency's boundaries. As states raised their requirements it led to students facing 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 by implementing a fixed effects model. My specification only compares cohorts within an agency and year, essentially controlling for many of the confounding law (such as three strikes laws) or spending changes that may be concurrent 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 less difficult exit exams, which test at an eighth grade or lower level, decreases the arrest rate by about 4.4%. I do not find a similar effect for more difficult exit exams, which suggests that while raising requirements can have beneficial effects, those effects may be mitigated if requirements are too difficult. In fact, I find that arrest rates *increase* for high school aged students that face these difficult exit exams by 8.4%. This finding is consistent with previous papers that find a dropout effect on more difficult exit exams but not on less difficult exams (Dee and Jacob 2006; Warren, Jenkins, and Kulick 2006; Baker and Lang 2013). I do not find any significant effects of increasing course requirements. The reduction in crime due to the less difficult exit exams is driven by property crimes and restricted to the counties with the lowest average incomes. The magnitude of these estimates are in line with the literature on education and crime which find an extra year of education reduces the arrest rate by approximately 10% (Lochner and Moretti 2004).

These results suggest that increased high school graduation requirements can have positive effects beyond increases in wages and employment. Lower arrest rates are not only beneficial for the students, but also for taxpayers and residents of neighborhoods around the

school districts. Raising the bar too high may actually work to undo these benefits and in some cases raise the arrest rate.

The remainder of this paper proceeds as follows: I first detail high school graduation requirements and their changes over time. Section 3 briefly describes the relevant literature. Section 4 describes my empirical strategy and methods. Section 5 describes the data used in my estimation strategy, followed by my main results in section 6. Section 7 provides robustness and falsification checks and section 8 concludes.

2. Graduation Requirements

Every high school has requirements that students must meet in order to graduate. In this paper I examine two of the more common requirements: course graduation requirements and exit exams. A course graduation requirement is the number of courses that students are required to pass in order to graduate. Most states mandate a minimum number of courses that each student must pass in order to receive a diploma.[9] Any given school district within that state can set its requirements above the state minimum but would not be allowed to set them any lower. Under the assumption that students can only pass their classes if they have learned the required material, this method ensures that anyone receiving a diploma will have learned at least the minimum content the state deems necessary. It also requires students to put in enough effort to

[9] Some states do not use state mandates and instead leave all the decisions to the local school districts. From 1980 to 2000 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 2000, only five 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.

finish all of the classes required, which may help to build valuable non-cognitive 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* led to a series of state level changes (Lillard and DeCicca 2001; Goodman 2012). The publication warned that the education system in the United States was failing to adequately prepare students for the workforce and college and made several suggestions for improving the education system. 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." In the years following this report, many states increased their requirements, resulting in a string of reforms from 1983 to 1986. Laws typically apply to new 9th grade cohorts, with continuing students covered under the previous requirements. Thus, a majority of these changes affected the graduating cohorts of 1987 to 1990. Several states instituted further changes to their curricula, which mainly affected the graduating classes of the mid to late 1990's. Table 1 shows the total courses required by state and graduation year.

The second type of graduation requirement I examine in this paper is the presence of "exit exams"—standardized tests that students must pass in order to receive their diploma. 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 grade to tenth grade level. The lower difficulty exams are often referred to as "minimum competency exams" and are first administered at the end of middle school or early high school. The more difficult exams, which test on some high school material, are traditionally administered at the end of the 10th grade. In many cases the students who do not pass the first

time receive extra opportunities. In theory, the presence of an exit exam guarantees that each graduate leaves high school with the minimum knowledge desired by the state. However, exit exams also could lead to "teaching to the test", in which teachers become more focused on getting students to pass the exam than learning the material (Jacob 2005; Jacob 2007).

Exit exams were relatively uncommon until the 1980's.[10] Since then, they have become much more popular, with many states adding exit exams throughout the last few decades. By the year 2000, eighteen states mandated an exit exam as part of the state curriculum and the number has continued to increase; in 2012 twenty-five states used an exit exam. Table 2 shows which graduating classes faced exit exams and whether the exams tested below the 9th grade level or at a 9th grade or higher level. As more states added exit exams, the grade level they tested at also increased. Most of the early exit exams only required knowledge up to an eighth grade level in order to be able to pass the exam. The curriculum standards began to rise in the 1990's such that today many of the exams test up to a tenth grade level. The use of exit exams has continued to rise, partially driven by the *No Child Left Behind Act of 2001* (NCLB). Since NCLB required schools to test their students in order to evaluate their progress, many states have made passing that exam a requirement for graduation.

3. Previous Literature

This paper bridges the gap between two different literatures. The first, examines the effect of raising high school graduation requirements on various outcomes; the second, estimates the effect of various education policies on crime. Currently, only one other paper overlaps with both of these literatures. Baker and Lang (2013) use state and cohort variation in exit exam

[10] Only New York and North Carolina used an exam in 1980.

implementation to estimate effects on incarceration rates using the 1990 and 2000 censuses. Once controlling for state and cohort fixed effects, they find positive but insignificant effects on incarceration rates, with smaller point estimates for less difficult exit exams. Since this is the only paper that covers both literatures, a brief independent review of each literature will follow separately.

3.1 Graduation Requirements

An existing literature has investigated the impacts of changing graduation requirements on students' education and labor market outcomes with mixed results and little consensus among them.[11] One set of papers examines these effects using rich individual data such as the National Educational Longitudinal Survey (NELS88) or High School and Beyond (HSB). Bishop and Mane (2001), Lillard and DeCicca (2001), and Warren and Edwards (2005) find that exit exams do not have a significant impact on dropout rates. Bishop and Mane also estimate that minimum competency exams are associated with increases in both college attendance and later life earnings, though the latter is likely due to an increase in hours worked and not a change in wage. In addition to exit exams, Bishop and Mane (2001) and Lillard and DeCicca (2001) study the effects of increasing course requirements and find some evidence that those requirements are associated with higher dropout rates.

Another set of papers use state-cohort variation in graduation requirement laws to estimate their effects on dropout rates and later life outcomes. Using this strategy with census data, Greene and Winters (2004) find no significant effects of exit exams on graduation rates. Dee and Jacob (2006) also use census data but separate exit exams into two difficulty measures. They find that while both types of exit exams lead to a decrease in high school completion, the

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

effect is larger for the more difficult exit exams. The effects of both types of exams are especially large for black students. Jacob and Dee also find mixed results on college going, employment, and earnings with no significant effects overall, but a combination of positive and negative effects across various gender and race subgroups for both types of exams. Warren, Jenkins, and Kulick (2006) use a very similar strategy to Dee and Jacob, but use the Common Core of Data's dropout measure as well as GED testing data. They find that more difficult exit exams lead to approximately a 3% decrease in high school completion and a 6% increase in GED test taking, but no significant effect on either outcome for the less difficult exams. Finally, Baker and Lang (2013) use both CPS and census data and find similar effects on high school completion and GED receipt. In addition to these outcomes and incarceration rates (as discussed earlier), they also examine non-employment and wages. They find that minimum competency exams significantly reduce non-employment when the CPS is used but that the results are insignificant using the census. At the same time, they find positive and significant effects of minimum competency exams on wages in the census, but insignificant effects using the CPS.[12]

Finally, several papers look beyond total graduation requirements and study the effects of course-specific requirements. Levine and Zimmerman (1995) and Rose and Betts (2004) find that math courses have positive and significant effects on future earnings. However, the number of math courses a student takes may be endogenous to other unobserved variables that affect earnings. To deal with this, Goodman (2012) uses a two-sample instrumental variables approach, using state changes in math requirements as an instrument for math courses taken.

[12] A different set of papers use a unique regression discontinuity approach to estimate the effect of exit exams on high school completion, comparing outcomes of students who barely pass an exit exam to those that barely fail (Martorell 2004; Ou 2010). These studies find that exit exams reduce graduation rates.

Using this strategy he finds that taking an additional math course results in approximately an 8% increase in wages for black students but an insignificant change for white students.

In summary, there is evidence suggesting that more graduation requirements can have both beneficial and adverse effects. This suggests that increasing requirements may increase human capital and therefore increasing labor market outcomes for high school graduates. However, there are also some students who are not able to finish high school because of the change. With findings in both directions it is important to examine all of the effects these policies may have. Throughout this paper I will refer to the beneficial impact of increased graduation requirements as a "human capital" effect and the adverse impacts as a "dropout" effect even though those terms simplify the actual mechanisms.

3.2 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. It also can have an incapacitation effect that keeps students busy when they could otherwise be getting into trouble.[13] Lochner and Moretti (2004) and Machin, Marie, and Vujic (2011) examine how additional years of education affect arrest rates using compulsory schooling laws as an instrument for education. Both studies find that more education results in large and significant reductions in arrest rates. Using the same arrest data source I use in this paper, Lochner and Moretti find an 11% decrease in arrest rates for each additional year of education. Cullen, Jacob, and Levitt (2006) and Deming (2011) find that access to better quality schools,

[13] For a thorough review of the literature concerning the causal effects of education and crime see Lochner (2012)

identified using school lotteries, can also reduce crime. Weiner, Lutz, and Ludwig (2009) find this effect as well, using access to better schools following desegregation.

Several papers have attempted to examine the incapacitation effect that education has on crime. Anderson (2012) uses variation in state compulsory education laws and finds that compared to students whose age is above the minimum dropout age, those below have arrest rates that are 7-12% lower. Jacob and Lefgren (2003) and Luallen (2006) compare crime rates in days when school is in session compared to those in which students have the day off. They find that on days when students are not in school there is an increase in property crime but a decrease in violent crimes. By examining the effects of changing graduation requirements on crime, my paper will provide yet another example of how education policy can affect crime rates.

4. Empirical Strategy

To estimate the effect of graduation requirements I use state policies and relate those to arrests for individuals facing different school requirements across states and graduating class cohorts. To estimate the difference in arrests I use the following ordinary least squares (OLS) model:

$$\frac{ARRESTS_{gpy}}{POP_{gpy}} = \alpha + \beta_1 CGR_{gs} + \beta_2 EE_{gs} + \beta_3 MCE_{gs} + \beta_4 SCHOOL_{gs} + \beta_5 ECON_{gc} + \psi_{py} + \gamma_{yg} + \rho_s * g + \varepsilon_{apy}$$

where *ARRESTS* are the aggregate number of arrests for people in graduation cohort *g* in the boundaries of police agency *p*, in year *y*.[14] Population (*POP*) varies at the same level and is the estimated number of people of in cohort *g* residing in a given agency's boundaries each year.

CGR is the state mandated minimum number of courses that graduation cohort *g* would have to pass in order to graduate in state *s*.[15] These are standardized across states so that each unit is the equivalent to a school year long course. In some instances there is no state mandated minimum requirement and instead course requirements are delegated to the local school district. The minimum requirements in these states are therefore set to zero, but an indicator variable is included which is set to "1" whenever the state opts to delegate to the local level. I do not use variation in the local policies for two reasons. First, historical data at this level are very difficult to find. Second, changes in the local policy are more likely to be endogenously implemented by local leaders and correlated with other school district characteristics.

EE and *MCE* are indicator variables for the presence of different types of exit exams where *MCE* refers to the "less difficult" minimum competency exams, those that test at material below the ninth grade level, and *EE* refers to exams that test at a ninth grade or higher curriculum.[16] These variables are defined as mutually exclusive so that no state has both at the same time. Changes in any of these requirements will only affect cohorts entering high school after the change has been implemented; any continuing high school students are "grandfathered"

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[14] Graduation cohort is defined as year minus age plus 18. Police agencies are also referred to as Originating Agency Identifier or "ORI's" in the data. In most cases these agencies are the local police precincts or districts. Other examples of agencies seen in the data are university police, port authorities, and airport police. I can estimate this equation at the state-age-year level rather than police agency-age-year level. However, some years have missing agencies that may cause bias when aggregating to higher levels. Results at the state level are available in Table 9 and show similar effects.

[15] Course requirement data are from *Education Commission of the States, Clearinghouse Notes* (1984, 1985, 1989, 1990, 1993, and 1996) and *Digest of Education Statistics* (2000, 2001).

[16] These variables and definitions are the same used in Dee and Jacob (2006).

and still face the previous 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 fifteen and eighteen).[17] These help control for any other changes that states may make to their education system at the same time as they change their graduation requirements. *ECON* are the average economic characteristics each cohort faced in county *c* while in high school.[18] These variables control for the possibility that changes in graduation requirements may tend to 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 would 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 a state-specific linear cohort effects.

This estimation strategy exploits the repeated cross section nature of the data and allows me to control for many more factors than would be possible otherwise. It is essentially a difference-in-differences strategy where the differences are across cohorts and states. However, each cohort in each state is observed in several different years (and therefore at different ages), which allows for multiple observations of the same group. In a traditional difference-in-differences strategy the estimate would be found by including cohort fixed effects and state fixed effects. In this situation, I am able to interact each of those sets of fixed effects with year fixed effects. The agency-by-year fixed effects (which also absorb state-by-year effects) are able to

[17] These include pupil-teacher ratio, teacher salary, per-pupil expenditures, and dropout age. These data are from *The Digest of Education Statistics* except for the minimum dropout age, which is from Oreopoulos (2009).
[18] These include employment-to-population ratio, income, transfer payments, and unemployment payments. These data are from the Bureau of Economic Analysis' Regional Economic Information System.

effectively control for any factors common to all agency residents (across all cohorts) in a given year. For example, the number of police officers and prisons or the use of laws such as "three strikes" that gained popularity during this era can all be controlled for—eliminating several possible sources of omitted variables bias. The year-by-cohort fixed effects control for any shocks common to each cohort within a year. They also simultaneously control for any year-by-age shocks, essentially controlling for the age profile of crime within each year.

The state-specific linear cohort effects ($\rho_s * g$) will control for the fact that different states have different arrest trends across cohorts. Figure 1 demonstrates this phenomenon for four states.[19] After demeaning by police agency and year there is still an increasing trend across cohorts, which can be quite different across states.[20] This could be due to the differing emphasis states put on rehabilitation programs, their prison system, the legal age of intent,[21] or various other factors.

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. Since many of the changes were in response to suggested national requirements and not part of an attempt to reduce crime, this assumption appears plausible. Also, any simultaneous attempts to lower crime rates would likely affect all cohorts and not only the select cohorts whose graduation year falls in line with the change in requirements. I will provide some falsification tests later in the paper that suggest this is a reasonable assumption.

[19] I choose four states for easier visualizations, but the same effect could be seen using all states.
[20] This is done by regressing arrest rates on state-by-year fixed effects and graphing the residuals by state and cohort.
[21] Legal age of intent is the minimum age at which an individual can be tried as an adult.

5. Data

The arrest data come from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) data on arrests.[22] The UCR is the primary source for crime statistics in the United States and is used to create numerous published crime statistics. The data are compiled by the FBI after being collected by local police agencies. These data are total counts of arrests at the age, gender, and offense level by agency jurisdiction and year. I use the arrest counts for 15-24 year olds[23] annually from 1980 to 2000. I assume that any offender's state of high school attendance is the state in which they are arrested and that 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 that this is a major concern.[24]

Even though the key variation occurs at the state-cohort level, I keep the data at the police agency-cohort level to reduce potential bias caused by missing data. The arrest data are reported voluntarily, which means that several agencies are missing from the sample for one or more years. Aggregating the data to larger geographical areas (e.g. county or state) could increase measurement error bias due to the missing observations.[25] Years in which agencies

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[22] I use the National Archive of Criminal Justice Data's (NACJD) "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.

[23] Single year of age aggregates are only available for ages 15-24. Ages outside of this range are grouped into bins which makes separating cohorts impossible.

[24] It is possible that the graduation date is measured incorrectly for some individuals, but this will likely lead to classical measurement error and bias my estimates towards zero.

[25] Of the 13,510 unique agencies during this period the most unique in any given year is 9,792. The least in any given year is 6,832 and the average is 8,567. 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. The District of Columbia, Florida, Georgia, Iowa, Kansas, Kentucky, Montana, New Hampshire, South Carolina, Vermont, and Wisconsin all have

provide arrest counts, they are reported for all ages (and, therefore, all cohorts) which allows the within agency-year across cohort estimation strategy to minimize potential bias.

It is important to note that these data only look at the number of arrests and not the total number of crimes or offenses. Since 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.[26] Furthermore, any changes in arrest rates that are not associated with changes in actual crime are unlikely to be correlated with graduation requirements, especially once time-varying agency effects are controlled for.

While the arrest data are given as the total number of arrests, a more informative measure is the arrest rate, which accounts for population size. Since UCR agencies are not a commonly measured geographical area, the data contain an estimate of the total population within each police agency. Unfortunately, these population estimates are not age-specific. In order to estimate age-specific population estimates at the agency level I utilize age, county, and gender population estimates from the Surveillance Epidemiology and End Results (SEER).[27] 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.

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at least one year where they did not report. However, coverage of the US population during this period is approximately 80% (http://bjs.ojp.usdoj.gov/content/pub/pdf/bgpcd.pdf).

[26] 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.

[27] SEER has, what is often thought of as, the closest approximation to age, race, and gender specific population counts at the county level.

My data on state exit exam requirements come from Dee and Jacob (2006). These are separated by difficulty where "less difficult" (or minimum competency) exams test below the ninth grade level and "more difficult" exams test at the ninth grade or higher level.[28] Course graduation requirement data (CGR's) are gathered from the *Education Commission of the States, Clearinghouse Notes* (1984, 1985, 1989, 1990, 1993, 1996) and the *Digest of Education Statistics* (2000, 2001). CGR's are defined as the state mandated minimum number of courses a particular cohort would have to pass in order to graduate from high school.

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 the Bureau of Economic Analysis' Regional Economic Information System (REIS) data 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 13,510 police agencies across 48 states and Washington DC for men and women age 15 to 24.[29] This results in a total of 1,574,295 observations across an unbalanced panel of cohorts. I drop years after 2000 so that any changes in requirements are not contaminated by the changes that came about due to *No Child Left Behind* which was passed in 2001. Summary statistics are presented in Table 3 and are weighted by police agency-age-year population cells in order to estimate population representative averages.

[28] As mentioned in Dee and Jacob (2006) this is just one dimension upon which "difficulty" could be measured, including: the passing score required, the number of attempts given, and the material covered in each grade.

[29] Alaska and Hawaii are excluded from the analysis because not all control variables are available for these two states.

6. Results

Table 4 shows a series of estimates based on equation (1). All results are weighted by cell population size and standard errors are clustered at the state level.[30] Columns 1–5 start with a simple model and progressively add more controls. The first column includes year, cohort, and police agency fixed effects along with the education and economic control variables. In this specification there is some evidence that course requirements may increase the arrest rate, but no effects are seen for either exit exam. Estimates in column 2 also include graduation cohort-by-year fixed effects and are very similar to the estimates in column 1. These specifications are subject to possible bias from changes in other state policies concurrent with graduation requirement changes. For example, if states that increased graduation requirements increased anti-drug campaigns at the same time results may be biased downward. Adding state-by-year fixed effects in column 3 removes this concern by controlling for all factors that are constant within a state and year. The inclusion of these effects has a large impact on the two exit exam coefficients, increasing both their point estimates and their standard errors. This demonstrates the importance of these fixed effects, which can be included in this paper due to repeated cross section nature of the data. Column 4 is very similar except that it controls for police agency-by-year fixed effects, which subsume the state-by-year fixed effects. This specification helps control for possible bias due to nonrandom missing police agency reporting. Results are very similar to column 3, suggesting that this may not be a big problem.

The estimates in column 5 (the preferred specification for the remainder of this paper) are based on a specification that also includes state specific linear cohort trends. Since the variation

[30] I weight in order to estimate effects representative of the population. Unweighted estimates are presented in Table 9. All results are estimated based on total arrest rates that include the number of arrests of both men and women. Results separated by gender are available in Appendix Table 1 and show similar effects across genders.

of the key graduation requirements is at the state-cohort level it is important to control for any state specific patterns in arrest rates that may exist across cohorts. Including these trends leads to a smaller but slightly more precise effect of the less difficult exit exams. Though not quite significant at the 5% level, this point estimate implies that students that were required to take this type of exit exam have 1.49 less arrests per 1,000 people. This corresponds to an approximate 4.4% decrease relative to the mean of 33.6 arrests per 1,000 people. The other graduation requirements do not have significant effects on arrest rates, though both of their point estimates are negative.

The magnitude of these estimates appear consistent with previous estimates of the effect of education on crime. Lochner and Moretti (2004) estimate an 11% decrease in crime due to compulsory schooling laws whereas my estimate for the less difficult exit exams is less than half of that. Given the larger, more established benefits of an additional year of education relative to the smaller, mixed effects of graduation requirements, the magnitude of the estimate seems reasonable. The fact that there are significant effects for the less difficult exit exams but not for the more difficult exams is consistent with the literature that finds little to no dropout effects of less difficult exams but that they do exist for more difficult exams (Dee and Jacob 2006; Warren, Jenkins, and Kulick 2006; Baker and Lang 2013). This dropout effect may work to increase crime and "undo" the crime reducing effects of implementing an exit exam.

6.1 Results by Offense Type

Results in Table 5 use the same controls in specification (5) of Table 4 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 (through 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 should be most affected. If there are noncognitive mechanisms at work, then perhaps one would expect to also see crimes like murder, rape, and assault being affected.

When arrest rates are disaggregated I lose some precision and many of the estimates are not statistically significant, nevertheless some of the patterns are interesting. The negative effect that the less difficult exit exams have on the overall arrest rates is driven by a significant decrease in property crimes, primarily through a decrease in larceny, though there are insignificant decreases in burglary and auto theft as well. The sign on violent crime is positive, though highly insignificant, with only rape showing a marginally significant decrease in arrest rates. Given that a majority of the reductions in these arrests are through crimes with monetary gains, the less difficult exams may reduce individuals desire to get money through illegal means. This may occur either through raising their own income (as some previous research has found) or perhaps through noncognitive skills, such as learning the importance of earning one's own money.

There is marginally significant evidence that requiring difficult exit exams may actually *increase* violent crime. This is mainly through an increase in robbery, though larceny and rape are the only offenses that demonstrate a decrease in arrests. One potential explanation for this is that the decrease in high school completion from difficult exit exams dominates the beneficial human capital effects. More students on the margin of graduating are pushed out because the requirements are too difficult to complete. Those students who dropout will suffer lower wages

and worse job opportunities and may turn to robbery as a source of income. There is also some evidence that failing exit exams can lead to depression, anxiety, despair, and anger (Cornell, Krosnick, and Chang 2006; Jurges et al 2009), which could lead to an increase in many of the different offense types.

Course requirements mainly show no significant effects on any type of arrest with the exception of rape, which is statistically significant at the 10% level, but not particularly economically significant. It is unclear why these requirements would lead to an increase in rape, but it is worth noting that Lochner and Moretti (2004) also find a positive effect on rape in their paper. There are several possible explanations for why course requirements may have no effect on the other offenses. It is possible that the course requirements are not very well enforced or perhaps are not binding in general if many districts were already above the new requirements before the change.[31] It is also possible that there is an effect in both directions but the human capital effect and dropout effect are similar in size and therefore the net effect is no effect.

Finally, none of these policies have a significant effect on white-collar or drug related offenses.[32] The former finding demonstrates that the effects of these policies are not simply a shift of crimes from "blue-collar" to "white-collar" due to the potential employment effects of the policies. The null findings on drug crimes may seem surprising given their association with young adults and, in particular, high school dropouts. However, this result is consistent with Anderson (2012), which finds no effect of minimum dropout age on drug related arrests for high school aged individuals.

[31] Work done by Goodman (2012) suggest that there are several areas that had requirements well above the state minimum and were therefore unaffected by the change.
[32] I have defined "white-collar" crimes as embezzlement, fraud, and forgery. Drug related crimes included both possession and distribution/sales offenses.

The information in this table can also help reconcile my estimates of the effects of exit exams on arrest rates with the positive, insignificant effects on incarceration rates seen in Baker and Lang (2013). Since nearly 70% of the incarcerated population in the US are sentenced for either violent or drug crimes,[33] the estimates in Baker and Lang are weighted more towards these offenses. Looking at these two categories in Table 5 shows a very similar story to Baker and Lang; less difficult exit exams show smaller positive and insignificant effects while more difficult exit exams show larger positive and (mostly) insignificant effects. If my estimates could be reweighted to reflect the probability of incarceration, it is likely that the results would look very similar to those of Baker and Lang.

Overall, the estimates in this table show that the effects of these policies differ by offense type. Most of the significant effects appear for crimes that yield monetary rewards, such as larceny and robbery, suggesting that these policies likely affect the relative opportunity cost of obtaining income through illegal means. There is also some evidence that the more difficult exit exams are more likely than less difficult exams to *increase* arrest rates for certain offenses. This fits well with the literature that finds larger dropout effects of the more difficult exams.

6.2 Results by Income and Race

On average, school quality, crime rates, and employment opportunities are worse in poor neighborhoods. Thus, it is likely that crime rates would be most affected in the lowest income neighborhoods. To test this, I separate counties into quartiles based on their 1980 average income and interact those quartiles with the key graduation requirements. Results of this exercise are shown in the first column of Table 6. I find that for each type of graduation

[33] Beck and Page (2001), *Prisoners in 2000*, Bureau of Justice Statistics

requirement, counties with the lowest average incomes see the largest, most significant impacts. Results for the other quartiles are not significantly different from zero, though in some cases nor are they significantly different from the first quartile estimate.

These results suggest that it is the poorest counties that see the largest benefits from the increase in graduation requirements. The students in these counties may have the lowest human capital and therefore the most room for improvement following these changes. Similarly, the use of exit exams may have very little impacts in highest income counties. Students in these counties may be essentially unaffected by the implementation of the exams, because they can easily pass them. Overall, this appears to be strong evidence for these requirements having beneficial effects on the areas most in need of improvement.

It is also possible that these policies have differential impacts across race. In general, arrest rates are larger for minorities relative to whites, so one may expect to see larger marginal effects for minority students. However, to complicate matters, Dee and Jacob (2006) report larger dropout rates for black students, while Goodman (2012) finds larger wage increases for black students following requirement changes. This makes it difficult to hypothesize how total effects might differ across races.

Ideally, I would estimate the effect of graduation requirements on arrest rates for each race separately. Unfortunately, age-specific arrest data are not available by race so this is not possible using my estimation strategy. As a proxy for this, I separate counties into quartiles based on the fraction of their county that is white in 1980 and interact these quartile dummies with the graduation requirements. These estimates are presented in Table 6.

Estimated effects are strongest and negative in counties that are predominantly white and mostly insignificant in counties with a small fraction of the county being white. This suggests

that the human capital effect dominates the dropout effect for white students but not for minority students. This would be the case if minority students were more in danger of dropping out due to having fewer resources available to them on average. This explanation is supported by Dee and Jacob's (2006) finding of a larger decrease for black students' high school completion rates following exit exam implementation. Overall, these results suggest that white arrests would decrease but minority arrests would remain the same after the implementation of an exit exam. From a policy standpoint this could be troubling because it suggests that these policies may increase an already large minority/white arrest gap. While the differential effects are not ideal from an equity standpoint, other than for the more difficult exit exams, I can rule out large *increases* in arrest rates for minorities. With other studies finding an increase in dropout rates after raising requirements, it is useful to find that the dropouts are not followed by large increases in crime on average.

One other possible explanation for these findings is that the fraction of population white and the income quartiles are a proxy for some other differences, such as degree of urbanization of the county. Some of the poorest counties with the highest concentration white residents are in rural areas. To investigate this, I interact the graduation requirements with indicator variables for metro, urban, and rural counties.[34] Results of this interaction are in column 3 of Table 6. The effects are definitely strongest in rural areas giving credence to this explanation. However, there are also strong negative effects in urban areas. Therefore, while some of the effect of race and income could be due to a proxy for urbanization, it is unlikely that this is the entire story.

[34] 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. (http://seer.cancer.gov/seerstat/variables/countyattribs/ruralurban.html)

6.3 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 17 year olds, which are usually thought of as juveniles when it comes to prosecution in most states. The two other categories are 18 to 20 year olds and 21 to 24 year olds. Results of this exercise are available in Table 7. There are no significant impacts for course requirements in any age group.

Looking at less difficult exit exams I find that 21 to 24 year olds are more responsive than younger age groups, especially for property crimes. There is also weak evidence that the more difficult exit exams may reduce the property crime arrest rate at older ages as well. It is important to note that the effect is not statistically different from the effect at the other ages; however, it is consistent with a human capital explanation. As wages increase due to the use of exit exams, the working age population (21 to 24 year olds) will have less incentive and a higher opportunity cost of committing crimes. Thus, one may expect the effect to be strongest at the older ages, as I observe here. One potential reason that the effects are not as large for the 18-20 year old group is that they may not have finished their schooling yet. There is some evidence that these exams can increase both college going and delay graduation (Bishop and Mane 2001). In either case, the individuals may have not have started working yet, and therefore do not experience any of the employment effects which will distort the opportunity cost of crime.

Interestingly, more difficult exit exams show a positive effect for the youngest age groups with a significant coefficient on violent crimes. The effect is no longer significant by the older age groups. Since students will typically take these exams at the end of 10th grade, they will know that they have not passed the exam around age 16. While they are almost certainly

allowed another attempt, the news that they have failed the exam can make them depressed, embarrassed, and angry (Cornell, Krosnick, and Chang 2006; Jurges et al 2009) which could directly affect their propensity to commit crime or indirectly affect it by inducing them to dropout of school.

This larger, more significant effect of more difficult exit exams relative to the less difficult exams is consistent with the dropout effects of these policies seen by other authors. Dee and Jacob (2006); Warren, Jenkins, and Kulick (2006); and Baker and Lang (2013) all find much larger dropout effects of more difficult exams with Dee and Jacob the only paper finding any dropout effects for the less difficult exams. The students that are dropping out of high school early due to difficulties in passing the exit exam may have more time (and incentive) to commit crimes. This effect would be largest while school aged because the "untreated" cohorts would still be incapacitated at school. The fact that this is most significant for violent crimes suggests that perhaps high school dropouts are more likely to commit violent crimes, though recall that robbery was the main offense driving the overall violent crime effects. This result is counter to Jacob and Lefgren (2003) and Luallen (2006) who find *decreases* in violent crime when students are not in school. However, their results are based on day-to-day variation that may be different than the more permanent dropout effect that my estimate may be capturing. Anderson (2012) uses a much more similar strategy to mine (yearly changes in school attendance through minimum dropout ages), and finds results much more consistent with my own.

Table 8 investigates this phenomenon a little bit further. Here I interact an indicator variable that signals whether a cohort was legally able to dropout of school with the requirement

variables.[35] The effects on less difficult exit exams fit the hypothesis that there is both a dropout and human capital effect. When students are unable to legally dropout, the crime increasing effect is mitigated resulting in a stronger overall negative effect. However, while noisy, the same evidence is not seen on the more difficult exit exams. This may be due to the fact that there may be difficulty enforcing minimum dropout age laws (Oreopoulos 2006).

7. Robustness

Estimates in Table 9 show my results are robust to changes in the model's specification. In column 1, I present the results of an unweighted regression. I prefer the weighted results because they produce an estimate that is representative of the population, but it is possible that weighted estimates are driven by a few large states. Results here are similar to the main specification and show, if anything, a stronger effect of less difficult exit exams.

Column 2 shows the estimated effect of graduation requirements on the log arrest rate. The results are similar here to my preferred specification. The presence of a less difficult exit exams leads to a 3.3% decrease in the arrest rate, which is similar to the 4.6% difference from the mean found using my main specification. There is evidence that even the more difficult exit exams lower arrest rates on average under this specification. I chose the arrest rate as my main specification over the log arrest rate because it allows for the use of observations with zero arrests reported, which provide useful information that would be ignored using the log arrest rate.

Since the level of variation of interest is at the state-cohort level, one could argue the data should be aggregated up from the police agency level to the state level. However, one reason for keeping the data at a lower level of aggregation is that this allows me to explore richer

[35] This is constructed depending on the age of the cell combined with the minimum dropout age by state-and-year.

differential effects by interacting the graduation requirements with county characteristics as seen in Table 6. Results of this aggregation are shown in column 3. As one would expect, the results here are similar to the effects seen using the police agency level. The effect on less difficult exit exams is slightly smaller, though more statistically significant using this specification.

Finally, column 4 disregards the difficulty of the exit exams and simply looks at the effect of having *any* exit exam. The 9th grade curriculum cutoff is somewhat arbitrary and I want to ensure that this distinction is not driving the results. Having any exit exam decreases the arrest rate by 1.51 arrests per 1,000 people which is very similar to the -1.49 coefficient associated with less difficult exit exams in my preferred specification. Thus, the grade cutoff of exit exam difficulty does not seem to be driving the beneficial effects. I take this as strong evidence that the observed effects are not simply an artifact of my treatment of the regressors of interest.

My estimation strategy controls for many possible sources of bias, however, it is possible that some omitted variable is affecting the same cohorts and states as the increase in graduation requirements. Table 10 shows the results of several falsification tests that suggest that this is not the case. Estimates presented in columns 1–8 are regressions that replace the dependent variable with the state-cohort average of various education variables and the county-cohort average of economic indicators. Each cell in these columns is a separate regression. If the graduation requirements were completely exogenous (after inclusion of the fixed effects) then one would expect all of the estimated coefficients in these regressions to be insignificant. This is what I find. The less difficult exit exams never significantly predict any of the education or economic variables. Course requirements may predict an increase in teacher salary, and there is some evidence that the difficult exit exams are coupled with a decrease in per-pupil expenditures and

unemployment payments. However, there are 24 separate regressions estimated and one would expect to see at least one significant coefficient only by random chance. This seems to suggest that identification strategy is capturing an exogenous effect.

Columns 9–11 show the results of a different type of falsification test. Here I include a "lead" exit exam that occurs four years earlier than the actual exam. This is included along with the correct exit exam timing. Column 9 shows that the point estimates on the actual exit exams are similar to the effects on the false exams. This would be troubling since ideally the estimate on the false exit exam would be zero, but upon closer inspection the estimated effects on the false exit exam are not robust. In column 10, I focus on states that switch to an exit exam in my sample period. These are the only states that are identifying the effects of both the actual and false exit exams. For this sample the estimated effect on the actual exam is much larger and more significant than the false exam. Column 11 uses the state level aggregation and, in this specification as well, only the true exam shows a significant effect. In all three specifications the true exam coefficients are consistent, while the false exam is sensitive to the choices made.

8. Conclusion and Discussion

With the passage of *No Child Left Behind* in 2001 and recent suggested reforms by President Obama, there has been a lot of pressure to improve the education system in the United States. This seems especially true for high schools, which have not been as competitive with other developed countries in recent years (*The Condition of Education* (2011)). One tool states have used to improve matters is to increase the standards that their students must meet before they can graduate from high school. 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 these changes.

One outcome 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. However, very few papers have estimated the causal effect of various education policies on crime.

I specifically estimate the impact of one particular policy: increasing graduation requirements. Raising minimum requirements likely increases the human capital of many, if not all, graduates. However, it is also possible that fewer students are actually graduating, which could in turn lead to more crime.

This paper adds to the growing economic literature that demonstrates a link between education and crime. Utilizing 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.

Results also suggest that administrators should be wary about how much they increase requirements. On average there is no significant crime reducing effects of course requirements or difficult exit exams. One possible reason would be that the requirements are too difficult for some students and those students commit more crimes. That increase in crime may offset the beneficial effects. There appears to be some evidence for this as the more difficult exit exams actually raise the average arrest rate in violent crimes for high school aged individuals.

It is important to realize that this is the average effect of these changes. It is likely that only students on the margin of graduating would be forced to dropout due to the increased requirements, and therefore adversely affected. However, many more students may receive the benefits that come from increasing these requirements. Even students who would have no problem graduating may receive the benefits of extra courses or exit exams. Thus, while arrest rates on average appear to decrease, it does not necessarily mean that this is the case for every student. Select students may be more likely to commit crimes, which should be understood before policy decisions are made.

Though the goal of these reforms is likely not to reduce crime, it is a positive outcome. School districts should use caution when implementing such requirements as a tool to improve outcomes. Ideally reforms of this nature would come with a support system that ensures that those students on the margin of passing are able to continue to do so. This would help eliminate some of the negative mechanisms that work to increase crime. More research on the mechanisms in place would go a long way to helping policy makers choose the best graduation requirements for their schools.

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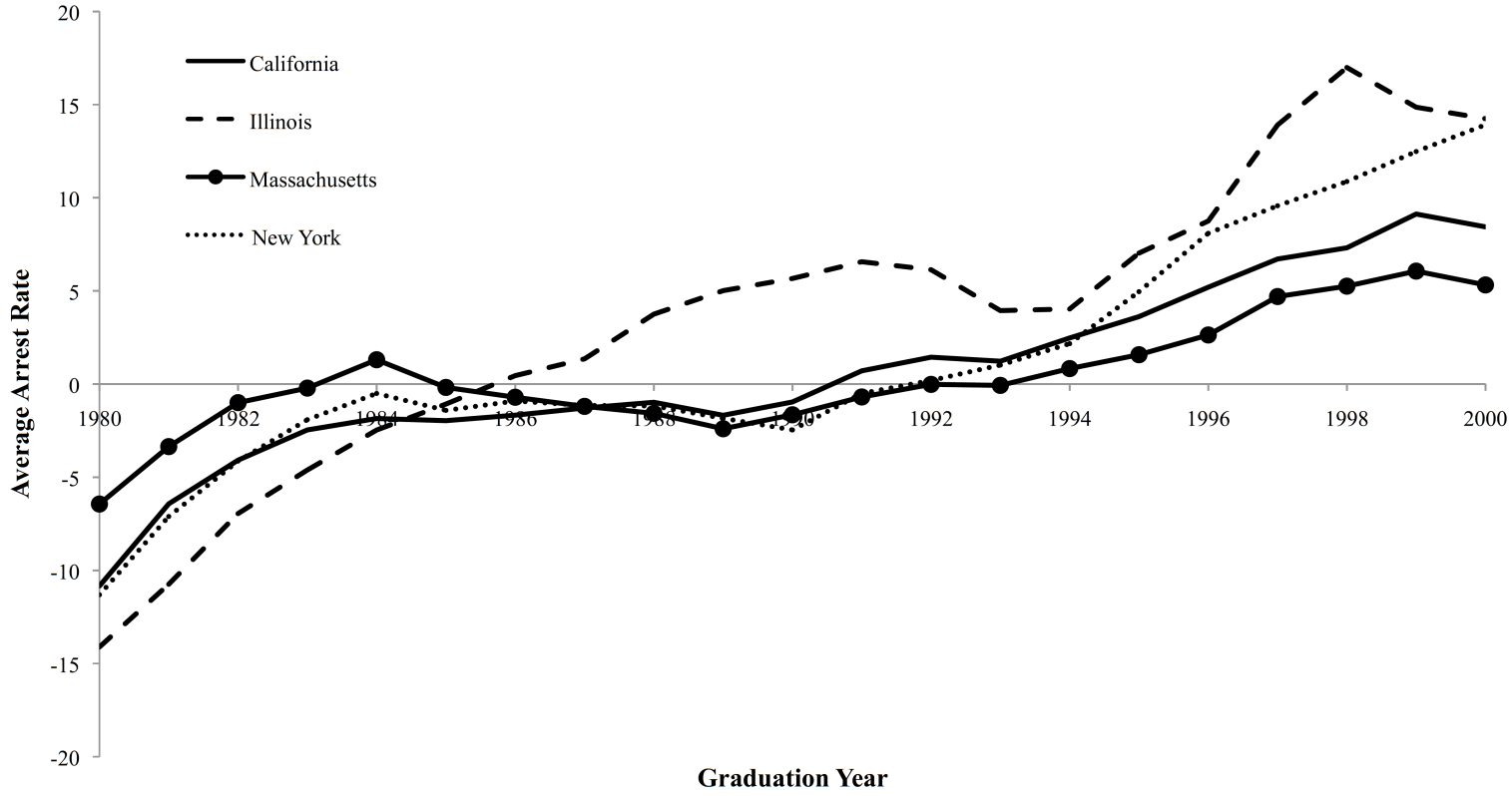
Figure 1: Arrest Rate by State and Graduation Year



Notes: Average arrest rates its calculated as the graduation year average residual of a regression of arrest rate on police district-by-year fixed effects.

Table 1: Total Course Graduation Requirements by State and Graduation Year

Graduation Year	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Alabama	20	20	20	20	20	20	20	20	20	22	22	22	22	22	22	22	22	22	22	22	24
Arizona	16	16	16	16	16	16	16	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Arkansas	16	16	16	16	16	16	16	16	20	20	20	20	20	20	20	20	20	21	21	21	21
California	0	0	0	0	0	0	0	13	13	13	13	13	13	13	13	13	13	13	13	13	13
Colorado	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Connecticut	0	0	0	0	0	0	0	0	20	20	20	20	20	20	20	20	20	20	20	20	20
Delaware	18	18	18	18	18	18	18	19	19	19	19	19	19	19	19	19	19	19	19	20	22
DC	18	18	18	18	18	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	23.5	23.5	23.5	23.5	23.5
Florida	0	0	0	0	0	0	0	24	24	24	24	24	24	24	24	24	24	24	24	24	24
Georgia	20	20	20	20	20	20	20	20	21	21	21	21	21	21	21	21	21	21	21	21	21
Idaho	18	18	18	18	18	18	18	18	18	21	21	21	21	21	21	21	21	21	21	21	21
Illinois	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16
Indiana	16	16	16	16	16	16	16	16	16	19.5	19.5	19.5	19.5	19.5	19.5	19.5	19.5	19.5	19.5	19.5	19.5
Iowa	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Kansas	17	17	17	17	17	17	17	17	17	21	21	21	21	21	21	21	21	21	21	21	21
Kentucky	18	18	18	18	18	18	18	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Louisiana	20	20	20	20	20	20	20	20	20	23	23	23	23	23	23	23	23	23	23	23	23
Maine	0	0	0	0	0	0	0	0	0	16	16	16	16	16	16	16	16	16	16	16	16
Maryland	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	21	21	21	21
Massachusetts	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Michigan	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Minnesota	15	15	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Mississippi	16	16	16	16	16	16	16	16	16	18	18	18	18	18	18	18	18	18	18	18	18
Missouri	20	20	20	20	20	20	20	20	22	22	22	22	22	22	22	22	22	22	22	22	22
Montana	16	16	16	16	16	19	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Nebraska	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Nevada	19	19	19	19	19	19	20	20	20	20	20	20	22.5	22.5	22.5	22.5	22.5	22.5	22.5	22.5	22.5
New Hampshire	16	16	16	16	16	16	16	16	16	19.8	19.8	19.8	19.8	19.8	19.8	19.8	19.8	19.8	19.8	19.8	19.8
New Jersey	0	0	0	0	0	0	0	0	0	18.5	18.5	18.5	18.5	18.5	21.5	21.5	21.5	21.5	21.5	21.5	21.5
New Mexico	20	20	20	20	20	20	20	21	21	21	23	23	23	23	23	23	23	23	23	23	23
New York	16	16	16	16	16	16	16	16	16	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5
North Carolina	16	16	16	18	18	18	18	20	20	20	20	20	20	20	20	20	20	20	20	20	20
North Dakota	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17	17
Ohio	17	17	17	17	17	17	17	17	18	18	18	18	18	18	18	18	18	18	18	18	18
Oklahoma	10.5	10.5	10.5	10.5	10.5	10.5	10.5	20	20	20	20	20	20	20	20	20	20	20	20	20	21
Oregon	21	21	21	21	21	21	21	21	22	22	22	22	22	22	22	22	22	22	22	22	22
Pennsylvania	13	13	13	13	13	13	13	13	13	21	21	21	21	21	21	21	21	21	21	21	21
Rhode Island	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16
South Carolina	18	18	18	18	18	18	18	20	20	20	20	20	20	20	20	20	20	20	20	20	20
South Dakota	16	16	16	16	16	16	16	16	19	20	20	20	20	20	20	20	20	20	20	20	20
Tennessee	18	18	18	18	18	18	18	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Texas	18	18	18	18	18	18	18	18	21	21	21	21	21	21	21	21	21	22	22	22	22
Utah	15	15	15	15	15	15	15	15	24	24	24	24	24	24	24	24	24	24	24	24	24
Vermont	0	0	0	0	0	0	0	0	0	14.5	14.5	14.5	14.5	14.5	14.5	14.5	14.5	14.5	14.5	14.5	14.5
Virginia	18	18	18	18	18	18	18	18	20	21	21	21	21	21	21	21	21	21	21	21	21
Washington	0	0	0	0	0	0	0	0	0	18	18	19	19	19	19	19	19	19	19	19	19
West Virginia	19	19	19	19	19	19	19	19	19	21	21	21	21	21	21	21	21	21	21	21	21
Wisconsin	0	0	0	0	0	0	0	0	0	13	13	13	13	13	13	13	13	13	13	13	13
Wyoming	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18	18

Source: *Digest of Education Statistics* (2000, 2001) and *The Education Commission of the States, Clearinghouse Notes* (1984, 1985, 1989, 1990, 1993, 1996)

Notes: Values are in Carnegie units and represent the equivalent of a year's worth of total courses. Requirements are based on the total number of courses that each student faces given their graduating class. A value of "0" means that the state did not require any minimum requirements and instead left the decision entirely up to the local school districts.

Table 2: Exit Exams by State and Graduation Year

Graduation Year	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Alabama	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2
Delaware	0	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
Florida	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2
Georgia	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2
Hawaii	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0
Indiana	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2
Louisiana	0	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1
Maryland	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Minnesota	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Mississippi	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1
Nevada	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2	2
New Jersey	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2
New Mexico	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1
New York	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2
North Carolina	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Ohio	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2	2	2	2	2	2
South Carolina	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1
Tennessee	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Texas	0	0	0	0	0	0	0	1	1	1	1	1	2	2	2	2	2	2	2	2	2
Vermont	0	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	1	1	1	1	1

Source: Jacob and Dee (2006)

Notes: Values of "0" refer to no exit exams, "1" refer to exit exams that test at below a ninth grade curriculum, and "2" refer to exit exams that test at a ninth grade or higher curriculum. States not listed did not have exit exams for the graduating classes listed.

Table 3: Summary Statistics

	Observations	Mean	Std Dev	Min	Max
Population	1,574,295	342.04	1,600.27	0.12	168,585.60
Graduation Requirements					
Total Courses Required	1,574,295	14.84	7.51	0	24
Total Courses Required (Excluding zero)	1,487,605	18.11	3.11	10.5	24
"Less Difficult" Exit Exams	1,574,295	0.26	0.44	0	1
"More Difficult" Exit Exams	1,574,295	0.06	0.23	0	1
Education Variables					
Pupil-Teacher Ratio	1,574,295	18.33	2.61	13.06	24.91
Teacher Salary	1,574,295	23,925.86	3,784.00	15,397.56	34,288.61
Per Pupil Expenditures	1,574,295	3,466.19	969.88	1,853.22	6,769.85
Minimum Drop Age	1,574,295	16.43	0.74	14.00	18
Economic Variables					
Employment-Population Ratio	1,574,295	0.57	0.25	0.11	3.77
Income	1,574,295	14,460.75	4,675.89	3,856.54	46,392.83
Transfer Payments	1,574,295	1,781.35	540.26	258.66	4,324.69
Unemployment Payments	1,574,295	71.96	42.78	0	423.39
Arrest Rates (per 1000 People)					
Total	1,574,295	33.76	32.76	0	89,179.66
Violent	1,574,295	7.50	9.32	0	18,756.09
Murder	1,574,295	0.21	0.62	0	280.32
Rape	1,574,295	0.37	1.10	0	1,207.80
Robbery	1,574,295	2.51	4.40	0	4,068.04
Assault	1,574,295	4.34	6.13	0	18,756.09
Property	1,574,295	26.27	28.89	0	89,179.66
Burglary	1,574,295	6.22	8.63	0	25,622.05
Larceny	1,574,295	17.00	22.59	0	89,179.66
Auto Theft	1,574,295	2.88	5.23	0	9,377.15
Arson	1,574,295	0.17	0.97	0	6,646.17
Drug Related	1,574,295	13.52	18.91	0	10,501.54
White Collar	1,574,295	4.84	11.31	0	8,358.33

Notes: All variables (except population) are weighted by police agency-by-age population size. All dollar values are calculated in real 1982-1984 base year dollars.

Table 4: Effect of Graduation Requirements on Arrest Rates

Specification	(1)	(2)	(3)	(4)	(5)
Number of Courses Required	0.204*	0.208*	0.166	0.314	-0.061
	(0.118)	(0.119)	(0.274)	(0.292)	(0.108)
Percent Change from the Mean	0.60%	0.62%	0.49%	0.93%	-0.18%
"Less Difficult" Exit Exam	-0.437	-0.427	-3.014*	-2.901*	-1.49*
	(0.569)	(0.580)	(1.568)	(1.637)	(0.795)
Percent Change from the Mean	-1.29%	-1.26%	-8.93%	-8.59%	-4.41%
"More Difficult" Exit Exam	-0.295	-0.231	-2.207	-1.935	-0.090
	(1.002)	(1.036)	(2.376)	(2.446)	(0.751)
Percent Change from the Mean	-0.87%	-0.68%	-6.54%	-5.73%	-0.27%
Observations	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295
Average Arrest Rate	33.76	33.76	33.76	33.76	33.76
Education Controls	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Police Agency Fixed Effects	Yes	Yes	No	-	-
Year Fixed Effects	Yes	-	-	-	-
Graduation Cohort Fixed Effects	Yes	-	-	-	-
Graduation Cohort-by-Year Fixed Effects	No	Yes	Yes	Yes	Yes
State-by-Year Fixed Effects	No	No	Yes	-	-
Police Agency-by-Year Fixed Effects	No	No	No	Yes	Yes
State-by-Linear Cohort Controls	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 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%, * Significant at 10%

Table 5: Effects of Graduation Requirements on Arrest Rates by Offense

		Violent					Other
Offense	Total	All Violent	Murder	Rape	Robbery	Assault	Drug Related
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Courses Required	-0.061	-0.041	-0.001	0.003*	-0.026	-0.017	0.039
	(0.108)	(0.034)	(0.002)	(0.002)	(0.022)	(0.014)	(0.062)
Percent Change from the Mean	-0.18%	-0.55%	-0.59%	0.71%	-1.04%	-0.39%	0.29%
"Less Difficult" Exit Exam	-1.49*	0.028	0.001	-0.018*	0.166	-0.113	0.116
	(0.795)	(0.181)	(0.011)	(0.010)	(0.167)	(0.095)	(0.400)
Percent Change from the Mean	-4.41%	0.38%	0.69%	-4.77%	6.62%	-2.60%	0.86%
"More Difficult" Exit Exam	-0.090	0.268*	0.018	-0.010	0.234*	0.034	0.331
	(0.751)	(0.152)	(0.012)	(0.013)	(0.127)	(0.141)	(0.607)
Percent Change from the Mean	-0.27%	3.57%	8.86%	-2.63%	9.33%	0.78%	2.45%
Observations	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295
Average Arrest Rate	33.76	7.50	0.21	0.37	2.51	4.34	13.52
		Property					Other
Offense		All Property	Burglary	Larceny	Auto Theft	Arson	White Collar
Specification		(9)	(9)	(10)	(11)	(12)	(13)
Course Requirements		-0.020	0.025	-0.030	-0.014	-0.001	-0.026
		(0.087)	(0.022)	(0.072)	(0.025)	(0.001)	(0.038)
Percent Change from the Mean		-0.07%	0.41%	-0.18%	-0.47%	-0.66%	-0.54%
"Less Difficult" Exit Exam		-1.518**	-0.098	-1.233***	-0.194	0.007	-0.288
		(0.654)	(0.187)	(0.437)	(0.202)	(0.008)	(0.326)
Percent Change from the Mean		-5.78%	-1.57%	-7.25%	-6.75%	3.87%	-5.95%
"More Difficult" Exit Exam		-0.357	0.114	-0.688	0.199	0.018	0.037
		(0.654)	(0.174)	(0.570)	(0.178)	(0.012)	(0.215)
Percent Change from the Mean		-1.36%	1.83%	-4.05%	6.92%	10.35%	0.75%
Observations		1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295
Average Arrest Rate		26.27	6.22	17.00	2.88	0.17	4.84

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 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%

Table 6: Differential Effects of Graduation Requirements by Income, Race, and Urbanization

	Average County Income (1)	Fraction of County Population White (2)		Degree of Urbanization (3)
Number of Courses Required x First Quartile	-0.375*	0.097	Number of Courses Required x Metro Counties	0.184
	(0.204)	(0.145)		(0.131)
Number of Courses Required x Second Quartile	0.0997	0.0456	Number of Courses Required x Urban Counties	-0.613***
	(0.173)	(0.175)		(0.112)
Number of Courses Required x Third Quartile	0.081	-0.107	Number of Courses Required x Rural Counties	-1.592***
	(0.162)	(0.105)		(0.285)
Number of Courses Required x Fourth Quartile	0.042	-0.237		
	(0.193)	(0.150)		
"Less Difficult" Exit Exam x First Quartile	-4.317***	-0.981	"Less Difficult" Exit Exam x Metro Counties	-0.468
	(0.768)	(1.121)		(0.748)
"Less Difficult" Exit Exam x Second Quartile	-1.188	-0.250	"Less Difficult" Exit Exam x Urban Counties	-6.837***
	(1.846)	(1.274)		(0.769)
"Less Difficult" Exit Exam x Third Quartile	0.635	-2.358***	"Less Difficult" Exit Exam x Rural Counties	-11.92***
	(1.363)	(0.862)		(1.623)
"Less Difficult" Exit Exam x Fourth Quartile	-0.172	-3.402***		
	(0.843)	(1.007)		
"More Difficult" Exit Exam x First Quartile	-4.566*	0.970	"More Difficult" Exit Exam x Metro Counties	2.987**
	(2.616)	(2.857)		(1.477)
"More Difficult" Exit Exam x Second Quartile	2.237	4.639**	"More Difficult" Exit Exam x Urban Counties	-11.7***
	(4.064)	(2.232)		(3.197)
"More Difficult" Exit Exam x Third Quartile	3.067	-1.905	"More Difficult" Exit Exam x Rural Counties	-26.98***
	(2.042)	(1.543)		(5.736)
"More Difficult" Exit Exam x Fourth Quartile	0.672	-8.205***		
	(2.036)	(1.910)		
Observations	1,366,035	1,552,146		1,388,211

Notes: Percentiles for income and fraction black 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 SEER's 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-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%, * Significant at 10%

Table 7: Differential Effect of Graduation Requirements by Age

	Total Crime (1)	Violent Crime (2)	Property Crime (3)
Number of Courses Required x (Ages 15-17)	-0.059	-0.041	-0.018
	(0.122)	(0.034)	(0.097)
Number of Courses Required x (Ages 18-20)	-0.025	-0.051	0.027
	(0.111)	(0.036)	(0.086)
Number of Courses Required x (Ages 21-24)	-0.065	-0.030	-0.034
	(0.115)	(0.038)	(0.097)
"Less Difficult" Exit Exam x (Ages 15-17)	-1.030	0.292	-1.321
	(1.309)	(0.474)	(1.058)
"Less Difficult" Exit Exam x (Ages 18-20)	-0.295	0.250	-0.545
	(1.054)	(0.213)	(0.873)
"Less Difficult" Exit Exam x (Ages 21-24)	-2.383**	-0.262	-2.121***
	(0.899)	(0.178)	(0.787)
"More Difficult" Exit Exam x (Ages 15-17)	0.721	0.623**	0.098
	(1.256)	(0.298)	(1.095)
"More Difficult" Exit Exam x (Ages 18-20)	-0.154	0.001	-0.154
	(1.135)	(0.254)	(0.971)
"More Difficult" Exit Exam x (Ages 21-24)	-0.828	0.037	-0.865*
	(0.607)	(0.243)	(0.465)
Observations	1,574,295	1,574,295	1,574,295

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%, * Significant at 10%

Table 8: Differential Effect of Graduation Requirements by Ability to Dropout

	Total Crime (1)	Violent Crime (2)	Property Crime (3)
Number of Courses Required	-0.049	-0.044	-0.005
x Legally Able to Dropout	(0.101)	(0.035)	(0.081)
Number of Courses Required	-0.079	0.001	-0.080
x Unable to Dropout	(0.145)	(0.036)	(0.126)
"Less Difficult" Exit Exam	-0.980	0.241	-1.221*
x Legally Able to Dropout	(0.825)	(0.201)	(0.692)
"Less Difficult" Exit Exam	-4.748*	-1.181***	-3.567
x Unable to Dropout	(2.391)	(0.413)	(2.151)
"More Difficult" Exit Exam	-0.292	0.144	-0.436
x Legally Able to Dropout	(0.810)	(0.174)	(0.724)
"More Difficult" Exit Exam	0.526	0.586	-0.059
x Unable to Dropout	(1.904)	(0.545)	(1.501)
Observations	1,574,295	1,574,295	1,574,295

Notes: Each specification represents a different regression where the outcome is the number of arrests per 1,000 individuals. Ability to drop is determined by the minimum dropout age laws in a given state and year and the age of the offender. 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%, * Significant at 10%

Table 9: Robustness Checks

Specification	Non-Weighted (1)	Log(Arrest Rates) (2)	State Level (3)	Any Exit Exam (4)
Number of Courses Required	-0.313	-0.001	-0.097	-0.077
	(0.258)	(0.002)	(0.073)	(0.110)
"Less Difficult" Exit Exam	-2.803***	-0.0327*	-1.3276**	-
	(0.759)	(0.017)	(0.553)	
"More Difficult" Exit Exam	-1.365	-0.0365*	0.142	-
	(1.076)	(0.019)	(0.595)	
Any Exit Exam	-	-	-	-1.509*
				(0.761)
Observations	1,574,295	1,084,429	8,625	1,574,295

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 3 where it is the state-by-graduation cohort-by-year cell. Unless noted all regressions are weighted by police agency-age population cell. Robust standard errors in parentheses are clustered at the state level. Specifications 1,2, and 4 include police agency-by-year and graduation cohort-by-year fixed effects, as well as state-specific linear cohort trends. In addition, specifications 1,2, and 4 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 3, 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.

*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Table 10: Falsification Tests

	Independent Variable Test								Lead Treatment Test		
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)	Full Sample (9)	Switchers Only (10)	State Level (11)
Number of Courses Required	0.006	1.406	51.178**	-0.016	0.000	14.820	-0.511	0.399	-0.065	0.316	-0.101
	(0.018)	(6.217)	(0.019)	(0.010)	(0.000)	(7.350)	(0.682)	(0.451)	(0.109)	(0.109)	(0.109)
"Less Difficult" Exit Exam	-0.084	18.630	138.298	-0.165	-0.002	-41.320	-6.334	5.566	-1.612**	-1.797**	-1.3802**
	(0.308)	(41.670)	(0.310)	(0.181)	(0.008)	(197.200)	(10.670)	(8.593)	(0.797)	(0.797)	(0.797)
"More Difficult" Exit Exam	-0.028	-136.7*	-643.405	0.308	0.007	-24.980	-10.860	-15.19*	-0.356	-1.136*	-0.046
	(0.352)	(70.120)	(0.572)	(0.290)	(0.008)	(162.100)	(20.200)	(8.776)	(0.704)	(0.704)	(0.704)
"Less Difficult" Exit Exam - 4 Year Lead	-	-	-	-	-	-	-	-	-1.634**	-0.812	-0.955
									(0.679)	(0.679)	(0.679)
"More Difficult" Exit Exam 4-Year Lead	-	-	-	-	-	-	-	-	-0.704	-0.900	-0.999
									(0.687)	(0.687)	(0.687)
Observations	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	1,574,295	570,036	8,625

Notes: In columns (1)-(8) the unit of observation is a police agency-by-graduation cohort-by-year cell and each cell is a separate regression where the outcome variable is listed at the top of each column. In columns (9)-(11) the outcome is the number of arrests per 1,000 individuals. Each regression is weighted by police agency population cell, except for column (11) which is weighted by the state-age population cell. 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. In addition, specifications (9)-(11) controls for state-cohort average pupil-teacher ratio, teacher salary, per-pupil expenditures, and dropout age, as well as county-cohort (or state-cohort for column (11)) average employment-to-population ratio, income, transfer payments, and unemployment payments.

*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Appendix Table 1: The Effect of Graduation Requirements on Arrest Rates by Gender

	Men			Women		
Specification	Total (1)	Violent (2)	Property (3)	Total (4)	Violent (5)	Property (6)
Number of Courses Required	-0.037	-0.037	0.000	-0.023	-0.004	-0.019
	(0.086)	(0.030)	(0.067)	(0.031)	(0.005)	(0.028)
Percent Change from the Mean	-0.14%	-0.56%	0.00%	-0.34%	-0.42%	-0.33%
"Less Difficult" Exit Exam	-1.075*	0.010	-1.085**	-0.421**	0.015	-0.436**
	(0.638)	(0.171)	(0.500)	(0.177)	(0.027)	(0.173)
Percent Change from the Mean	-3.99%	0.15%	-5.34%	-6.19%	1.72%	-7.33%
"More Difficult" Exit Exam	0.130	0.216	-0.086	-0.204	0.0574**	-0.262
	(0.585)	(0.154)	(0.491)	(0.234)	(0.024)	(0.229)
Percent Change from the Mean	0.48%	3.25%	-0.42%	-3.00%	6.73%	-4.40%
Observations	1,574,119	1,574,119	1,574,119	1,574,109	1,574,109	1,574,109
Average Arrest Rate	26.96	6.64	20.32	6.80	0.85	5.95

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%, * Significant at 10%