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Gregory A. Gilpin and Luke A. Pennig

“We also know that when students aren’t allowed to walk away from their education, more of them walk the stage to get their diploma. So tonight, I call on every State to require that all students stay in high school until they graduate or turn eighteen.”

President Obama

State of the Union Address 2012

Minimum dropout age (MDA) laws have been touted as effective policies to bring dropouts off streets and into classrooms. One question to better understand the costs and benefits of these laws is: to what extent do MDA laws displace crime from streets to schools? This research expands the compulsory schooling literature and extends the sparse research on in-school crime by studying how MDA laws affect crimes committed in U.S. public high schools.

The analysis is conducted using a difference-in-difference estimator exploiting variation in state-level MDA laws over time. The results indicate that an increase in the MDA to 18 significantly increases in-school crime by 0.434 incidences per 1,000 students or a 6.2% increase. Analyzing specific crime types, the results find that attacks without a weapon, threats without a weapon, and illegal drug incidences increase by 0.627, 0.588 and 0.437 incidences (or 12.2%, 36.3%, and 43.4% increase), respectively. An increase in the MDA to 17 is found to have no effect on in-school crime. The results are robust across different socioeconomic student bodies and control groups. Lastly, we find that in-school crime prevention resources do not increase with an increase in the MDA, but that utilization rates of suspensions and expulsions change in the direction of fortifying state policymakers efforts to keep juveniles in schools.

KEYWORDS: Compulsory Schooling Laws; Minimum Dropout Ages; In-School Crime; Crime; Juvenile Crime; Delinquents; Education; Difference-in-Difference

JEL classification codes: I21; K42

1. Introduction

Minimum Dropout Age (MDA) laws have been touted as effective public policies to bring delinquents off the streets and into classrooms.¹ Studying educational outcomes, the literature has shown that increases in compulsory schooling increase educational attainment rates and decrease dropout rates. For example, Schmidt (1996) and Goldin and Katz (2003) analyze the efficacy of MDA laws on the growth of high school attendance for the early 20th century. They both find that requiring students to attend an additional year of school increases overall educational attainment by approximately 5 percent (see Lleras-Muney (2002) for similar results). Oreopoulos (2009) has shown that more restrictive laws reduce dropout rates, increase college enrollment, and improve career outcomes. Research by Denison (1985) finds that improvements in human capital explain 28 percent of the per capita growth residual between 1925 and 1982. The bulk of this increase in human capital is attributed to the growth of high school attendance.

The compulsory schooling literature has also shown that compelling juveniles to remain in school have long-term effects. Angrist and Krueger (1991) and Acemoglu and Angrist (2001) both find that for students compelled to stay in school one year longer, U.S. adults earnings rise by approximately 10 percent. Oreopoulos (2007) estimates average lifetime wealth increases by 15 percent for an additional year of schooling caused by MDA laws. He also finds that students who are forced into additional schooling are less likely to report poor health, being depressed, looking for work, being in a low-skilled manual occupation, and being unemployed. Similarly, Acemoglu and Angrist (2001) find that education has a causal effect of lowering mortality using compulsory schooling laws as instruments. Interestingly, Black, Devereux, and Salvanes (2008)

¹In this paper we focus on one statute of compulsory schooling laws (CSLs), the minimum dropout age (MDA). CSLs are set at the state level and cover a variety of aspects of education. For example, there are legal provisions specifying the maximum age at which a child is legally compelled to begin school, the minimum age at which a child may leave school, the average length of the school term (in days), and the circumstances under which a child is exempt from attending school. In addition, state level child labor legislation may influence school enrollment and attendance of teenagers by setting a floor on the age they are legally employable (Edwards (1978)).

find that increases in compulsory schooling decreases the rate of teenage childbearing.

In addition, the compulsory schooling literature has also shown that crime rates and criminal activity decrease. Lochner and Moretti (2004) find that increasing compulsory schooling significantly reduces the probability of incarceration. They attribute these reductions to a decrease in the rates of murders, assaults, and motor vehicle thefts. Using a different empirical methodology, Anderson (2011) finds similar decreases in juvenile crime rates. He suggests that his results are consistent with an incapacitation effect, school attendance decreases the time available for criminal activity.

While the literature has shown compulsory schooling laws have positive effects, surprisingly, there are no studies on whether there are negative effects, such as increases in in-school crime.² Indirectly, Jacob and Lefgren (2003) and Luallen (2006) study whether rates of crime change when school is in-session. Using teacher in-service days, Jacob and Lefgren (2003) find that the level of property crime committed by juveniles decreases by 14 percent on days when school is in-session, but that the level of violent crime increases by 28 percent on such days. They attribute the increase in crime to a concentration effect. Using teacher strike days, Luallen (2006) finds similar results. While these papers do shed some light on whether juvenile crime rates change when school is in-session, the composition of students is held fixed in both studies. That is, these papers study a fixed population of students that have all chosen to remain in school voluntarily, and are now forced out of school due to school administrators' decisions. To the best of our knowledge, no study has investigated changes in in-school crime rates due to changes in the composition of students from compulsory schooling mandates forcing dropouts back into the public school system.

Aside from the fixed student composition, the juvenile crime rates explored in the existing

²One partial exception is Anderson (2011) who shows that females in states with a MDA of 18 are more likely to self-report missing school for fear of safety than those attending schools in states with MDA laws of 16 or 17.

literature suffer from measurement error. The error is caused by school administrators not reporting to law enforcement all crimes committed in-school. Overall, 41.3% of crimes are not reported to law enforcement, and there is substantial variation in reporting by the type of crimes being committed (author's calculations). For example, 32.1% of in-school drug crimes, including distribution, possession, and usage, are not reported to law enforcement; 40.1% of in-school violent crimes are not reported to law enforcement, while 59.1% of all attacks and threats without a weapon are not reported to law enforcement (author's calculations). This underreporting is due to school administrators choosing to not involve law enforcement when a crime is committed, rather than the crime going unnoticed by school administrators. This suggests that juvenile crime rates using youth arrest rates contain substantial measurement error when measuring total crime. Thus, the effects identified in the literature may partially be due to school administrators choosing to report some crimes more than others to law enforcement.

The purpose of this paper is to investigate the effects of MDA laws on in-school crime rates. This study focuses on the rate of crime incidences reported to school administrators, and not just those reported to law enforcement.³ We secure identification through exogenous state-level changes in MDA laws from 2003 through 2010, a statute of compulsory schooling laws. Using rates of in-school crime reported to school administrators, a difference-in-difference (DD) estimator uncovers the effects of changes in MDAs. Robustness checks test against potential endogeneity between crime and in-school crime resources. The effects are further studied across demographic groups. Lastly, we study whether changes in MDAs effect schools' allocations of in-school crime resources and rates of utilization of in-school punishment.

An overview of the results are as follows. First, an increase in the MDA of 18 significantly

³This measure of crime resolves issues regarding underreporting of crime. While the issue of why school administrators do not fully report all in-school crimes to authorities is important, it is beyond the scope of this paper (see Gilpin (2012)).

increases in-school crime. Specifically, attacks without a weapon, threats without a weapon, and drug incidences. An increase in the MDA of 17 does not to have any effect on in-school crime. We also find that the results are robust across various alternative specifications and demographic groups.⁴ Lastly, we find that changes in MDA do not have an effect on measures of in-school crime prevention resources, a positive effect on the utilization of student suspensions, and a negative effect on the utilization of expulsions.

The paper is organized as follows. Section 2 introduces the data and describes variables used in the study. Section 3 describes the empirical strategy. Section 4 presents the main results of the effect of MDA laws on in-school crime and presents results from alternative specifications and control groups. Section 4 also provides results from robustness across demographics. Section 5 presents results from in-school crime prevention resources and punishment administered by school administrators. Section 6 concludes.

2. Data

To examine the effect of MDA laws on in-school crime, we exploit variation in crime rates with school-level data. The school crime data and characteristics are provided by the restricted-access versions (2003-04, 2005-06, 2007-08, and 2009-10) of the *School Survey on Crime and Safety* (SSOCS) survey conducted by the *National Center for Education Statistics* (NCES). The SSOCS is a school-level, bi-annual survey regarding school violence and pays particular attention to the frequency of varying types of in-school crime and delinquency. Collectively, these rounds of surveys construct a repeated cross-section of schools. The final data set consists of roughly 3,130 school-level observations from U.S. public high schools.⁵ The dataset includes the number of

⁴The use of alternative control groups are also explored and found to be consistent with the main results.

⁵ The number of observations are rounded to the nearest 10's place per restricted-use data license.

in-school crime incidences as well as school and student body characteristics.⁶ Since schools are selected with unequal probabilities, sampling weights provided by NCES are used in all descriptive statistics and regressions.⁷

The state-level MDA data are provided by the NCES' *Digest of Education Statistics*, as well as various state published reports and policy briefings. The NCES provides yearly MDA laws for all 50 states during the sample period. However, comparing the NCES's MDA data with state-level legislation reports and policy briefing identified three discrepancies. First, NCES reports Minnesota as having a MDA of 18 for 2003 - 2005 and 2007 - 2010 and a decrease to 16 in 2006. However, Minnesota has had a MDA of 16 for the entire period. Second, NCES reports that Rhode Island increased their MDA from 16 to 18 in 2005, then decreased it to 16 in 2006. However, Rhode Island had a MDA of 16 for the entire period. Lastly, NCES reports that New York has a MDA of 17. However, New York's MDA depends on the size of the city's population, 17 for cities greater than 4,500 in population and 16 for cities less than 4,500 in population. The MDA data for this analysis has been verified for every state for the entire sampling period. Where discrepancies occurred, the MDA mandate by state legislation supersedes NCES's MDA data.

Table 1 reports the MDA law for each state during the sample period. Between 2002 and 2009, there are 5 states that change their MDA: Colorado, Illinois, Indiana, Nebraska, and Nevada. No state in the sample decreases their MDA. Illinois and Colorado increase their MDA from 16 to 17. Indiana and Nebraska increase their MDA from 16 to 18, and Nevada increases its MDA from 17 to 18. Table 2 shows that a MDA of 16 is most prevalent in the sample period (41% of the schools), followed by a MDA of 18 (39% of all schools), and MDA of 17 (20% of

⁶The in-school crime data are fully described in Table A1.

⁷The full sample weights provided with the restricted-access data files take into account three components. First, a *base weight* defined as the reciprocal of the probability of selecting a school for the sample is constructed. Second, the weight is adjusted to correct for non-responses. Finally, a post-stratification adjustment is applied based on the Public School Universe File of the 1998-1999 Common Core of Data.

all schools). A MDA of 16 becomes less prevalent throughout the sample, which indicates that the national MDA average is increasing. Schools from the five states that change their MDA total 330 school-level observations.⁸ This makes the treatment groups approximately 10.5% of the total sample.

There are six types of crimes examined: attacks without a weapon, threats without a weapon, illegal drugs, property, violent, and total in-school crimes. Illegal drugs include usage, distribution, and possession. Property crimes are an aggregate of thefts and vandalism. Violent crimes are an aggregate of rape, sexual battery, attack with a weapon, threat with a weapon, possession of a firearm, robbery, robbery with a weapon, and possession of a knife. For each crime aggregate, we construct incidence rates by dividing the total number of incidences rates in a particular aggregate by total enrollment per 1,000, i.e., $\text{crime rate} = \text{in-school crime} / (\text{total enrollment} / 1000)$. We then adjust the incidence rates for varying survey dates and length of school year. Thus, differences in incidence rates do not reflect difference in survey response date or the length of school year.

Summary statistics for the six crime aggregates are provided on Table 2. On average, 51.0 incidences per 1,000 students occurred each year over the sample period. The three most prevalent in-school incidences are attacks without a weapon and property crimes, approximately 19 incidences per 1,000 students each year, and threats without a weapon, 13.9 incidences per 1,000 students per school year. The least prevalent incidences are illegal drugs and violent crimes, with 6.2 and 4.3 incidences per 1,000 students per year, respectively. Table 2 also presents the rate of change over the sample period in column ‘Change’ defined as the difference in the two year averages of 2003 & 2005 and 2007 & 2009. Across all crime aggregates, downward trends are evidenced, and in particular, for attacks and threats without weapons. In

⁸ The number of observations are rounded to the nearest 10's place per restricted-use data license.

contrast, there are relatively large upward trends in the number of in-school drug crimes, and small upward trends in property and violent crimes.

The SSOCS also provides a rich set of student body and school characteristics. The descriptive statistics of all characteristics used in regressions are listed in Table 3 for the sample period by whether the school is in a state that changes its MDA. The average student enrollment is 1,072 students and remains stable across the sample period. The most populated school in the sample contains 5,090 students and the lowest populated school contains 100 students.^{9,10} The average student-to-teacher ratio is 13.9 students per teacher. The maximum student-to-teacher ratio is 39 students per teacher, and the minimum is 4 students per teacher. The SSOCS data also contains several in-school crime prevention resources, e.g., random dog sniffs, random sweeps, mandatory uniforms, and whether security cameras are present.¹¹ On average, 63% of schools practice random dog sniffs and 27% of schools practice random sweeps. The average number of schools that have random sweeps decreases over the sample. Only 0.5% of schools require students to wear uniforms.

The numbers of security guards and in-school police officers are also included in the SSOCS data. As displayed in Table 3, security guards are the most prevalent in high schools with an average of 0.83 guards per 1,000 students. Police officers are divided into two subcategories: regular officers and resources officers.¹² Resource officers are the least prevalent security in public high schools with an average rate of 0.6 officers per 1,000 students compared to 0.11 regular officers per 1,000 students.

Lastly, the SSOCS provides student body characteristics. The analysis includes the percent

⁹ An enrollment floor of 100 students per school is implemented.

¹⁰ All maximums are truncated to the fourth largest as per data license restriction.

¹¹ Random dog sniffs are used to check for drugs. Random sweeps are used to check for contraband including drugs and weapons.

¹² Resource officers include all career law enforcement officers with arrest authority who have specialized training and are assigned to work in collaboration with school organizations.

of student body that is eligible for free lunch (to control for the affluence of the school), that is male, that is below the 15th percentile on standardized tests, that are of a minority race.¹³ The average percentage of students eligible for free lunch across the sample is 37.8% (see Table 3). In the sample, 41 schools or 1.3% of the sample have 100% of students eligible for free lunch, and 19 schools or 0.6% of the sample have 0% eligible. The average percent of student body that is male is 49.1%, and as expected, does not change across the sample period. The average percentage of students below the 15th percentile on standardized tests is 13.9%. The percent of students below the 15th percentile on standardized tests has a slight decreasing trend between 2003 and 2009. 27% of the student body is a minority race.

We augment the SSOCS by including neighborhood characteristics surrounding each school. Using the FBI's *Uniform Crime Reports*, we estimate the street crime rate as the number of offense per 1,000 persons reported at the county level for those that are over 18 years of age. The variable is linked to the SSOCS data by county-level identifiers for each school. The street crime rates provide some understanding of the amount of crime surrounding each school, but not necessarily where students live. The average street crime surrounding each school is 53.5 arrests per 1,000 persons per year. The study also contains urbanization indicators for whether each school is located in a city, rural or, urban area. 19% of schools in the sample are located in cities, 32% are located in urban areas, and 49% are located in rural areas or towns.

3. Empirical Specification

Given that MDA laws bring dropouts off the streets and into schools, it is expected that in-school

¹³ The percent of student body of a minority race is calculated as: 100 - percent of student body that is of the majority race.

crime may increase if the juveniles are delinquent and criminal activity is not incapacitated. The effect of changes in MDA on various school outcomes is studied using a difference-in-difference (DD) empirical model. The DD estimator measures the difference in an outcome variable between public high school students in states with a MDA of 16 to high school students in states with a MDA of 17 or 18, and the difference between the two over time.

The distribution of in-school crime incidences have a heavy right skew with large outliers and many schools reporting zero crimes. This is typical of count data. Preliminary regressions show that using the number of crime incidences per 1,000 students per school year as the dependent variable violate the normality assumption. Q -normal plots indicate that the normality assumption is not satisfied (available from authors upon request). When the error term is not normally distributed, the t distributions can be a poor approximation of the distribution of the t statistic, causing biased t -tests. The normality assumption holds in the limit if and only if there is no heteroskedasticity present in the model. A Breusch-Pagan test for heteroskedasticity rejects the null hypothesis of homoskedasticity for all crime rates. We resolve this issue by transforming the crime rates using a two-parameter Box-Cox transformation.

The two-parameter Box-Cox transformation contains an intercept shifter and a power shifter. The Box-Cox transformation specification is:

$$\widetilde{Crime}_{ist}^j = \frac{((Crime_{ist}^j + C^j)^{\lambda^j} - 1)}{\lambda^j} \quad (1)$$

where $\widetilde{Crime}_{ist}^j$ is the Box-Cox transformed number of crime incidences per 1,000 students per school year for school i , in state s , at time t , $Crime_{ist}^j$ is the number of crime incidences per 1,000 students per school year being transformed, C^j is a scale shift parameter, and λ^j is the power shift parameter for the j^{th} crime rate. A benefit of the Box-Cox transformation with a scale shift is that it accommodates zero valued observations, whereas a log transformation

does not. We provide the parameter values in Table 4 for all six crime types. All of the parameter estimates are significantly different from zero at 1%. Thus, a log transformation is not suitable for this particular data. One notable difference between the Box-Cox transformed crime rates and the non-transformed crime rates is that the average number of incidences per 1,000 students is significantly lower. The Box-Cox transformed incidences rates approximate a normal distribution and the Brusch-Pagan test for heteroskedasticity fails to reject homoskedasticity for all crime types.

Using the Box-Cox transformed crime rates, the DD specification is:

$$\widetilde{Crime}_{ist}^j = \alpha + \beta_1 MDA17_{st} + \beta_2 MDA18_{st} + \beta_3 \mathbf{X}_{ist} + \beta_4 \mathbf{S}_s + \beta_5 \mathbf{T}_t + \varepsilon_{ist} \quad (2)$$

where $MDA17_{st}$ and $MDA18_{st}$ are indicators for the state-level MDA laws, $\mathbf{X}_{ist}$ is a vector of school characteristics discussed in the previous section (see Table 3), $\mathbf{S}_s$ are state fixed effects, $\mathbf{T}_t$ are time fixed effects, and ε_{ist} is a random error term. The coefficients of interest are β_1 and β_2 and these represent the marginal effect on schools in states with MDA laws of 17 and 18 relative to schools in states with a MDA law of 16, respectively. The coefficients β_1 and β_2 are expected to be positive and significant if constrained dropouts increase in-school crime. The state fixed effects pick up the static MDA laws throughout the entire period. Thus, coefficients MDA 17 and MDA 18 quantify the effect for those states that have changed their MDA to either 17 or 18, respectively.

Given that the Box-Cox transformed crime rates are significantly smaller, this implies that the coefficient estimates are lower bounds, and the magnitudes of effect may be larger than reported in this paper. However, the relative magnitudes of effect can be computed precisely if the coefficient estimates are compared to Box-Cox transformed average incidence rates.

One weakness of the DD model is the potential for the standard errors to be biased due to

serial correlation of the policy variables over time within a state. To account for this, the standard errors are clustered at the state level (see Bertrand, Duflo, and Mullainathan (2004)). Robustness checks are also conducted using multiple control groups and subsamples of states by MDA laws. The model is also used to estimate the effect of MDA laws across various socioeconomic groups, in-school crime prevention resources, and utilization rates of school punishment policies.

4. The Effect of MDA laws on In-School Crime

Prior to conducting regression analysis, a visual analysis of crimes rates over time between states that change their MDA and those that do not is provided in Figure 1. This graphic separately displays the demeaned changes in crime incidences across the sample period for the states that change their MDA and for all other states. The subgraphs for the states that change their MDA also contain a vertical line to represent the year the state changed their MDA. As shown in the figure, crime incidences increase immediately when MDA increases in four out of five states. The exception, Nevada, displays a downward trend throughout the entire sample period. It is also interesting to note that the increase in crime is short-lived in the two states (Illinois and Colorado) that increase their MDA to 17, while crime is more persistent in states that increase their MDA to 18 (Indiana and Nebraska). The non-change states display an overall decline in the crime rate across the sample period, with a slight uptick during the 2005-06 school year.

To understand the effect of changes in MDA laws on in-school crime, the empirical results are built-up using four regressions with increasing controls. The results of these four estimations are presented in Table 5. The first estimation controls for state and time fixed effects, the second adds time invariant controls, and the third estimation adds controls for school characteristics. The last estimation controls for state and time fixed effects, school

characteristics, and crime prevention resources. Estimated coefficients associated with school and neighborhood characteristics indicate relationships consistent with economic intuition. As the percentage of students eligible for free lunch increases, the rate of crime incidences increases. The presence of a security officer does not significantly reduce crime incidences. No other control is statistically significant at 10%. The MDA 18 coefficient on all specifications remains positive and significant while MDA of 17 is only significant without school controls or crime resource controls. The magnitude of the MDA of 18 coefficient decreases when school controls are included into the estimation, and only slightly when crime prevention resources are added. This suggests that omitted variable bias occurs without school and student body characteristics, and that additional school characteristics beyond the baseline model, such as school crime resources, do not significantly change the MDA of 18 coefficient magnitude of effect. The full specification including crime prevention resources shows that an increase in MDA to 18 increases in-school crime by 0.434 incidences per 1,000 students per year. This equates to 6.2% percent increase in crime in states that increase their MDA to 18.

We conduct sensitivity analysis to the main results using alternative control groups. First, we exclude low sampled states from the regressions (Delaware, North Dakota, Vermont). Second, we exclude states typically omitted in the MDA literature (Alaska, Hawaii, Mississippi, and Washington D.C.). Third, we exclude states with multiple MDA laws (New York). Fourth, we exclude states that meet any of the above criteria. Four separate regressions are conducted using the exclusion criteria, and the results from these robustness checks are displayed in Table 6. *Control Group A* shows the results excluding the low sample states, *Control Group B* shows the results from the exclusion of the typically excluded states, *Control Group C* show the results from excluding New York, and *Control Group D* presents the results from excluding all of the relevantly excluded states. As shown in the Table, the coefficients on the MDA variables

have similar coefficients to the baseline regressions and only vary by .086 incidences per 1,000 students per year.

A second sensitivity analysis is performed to examine if isolated changes in MDA laws have similar magnitudes of effect. There are three changes in MDA laws that occur during the sample: increases from 16 to 18, from 16 to 17, and from 17 to 18. To examine these changes, states that do not have the MDA being examined are dropped. For example, to measure the effect of an increase from 16 to 18, any states that have or had a MDA of 17 during the sample are excluded. Table 6 shows the results from the three alternate specifications. The estimation results from dropping schools in states with a MDA of 16 are reported in column *Control Group E*. Likewise, estimation results from dropping schools in states with a MDA of 17 and 18 are reported in columns *Control Group F* and *Control Group G*, respectively. The results from this sensitivity analysis indicate that increasing from 16 to 18 provide a larger magnitude of effect than an increase in MDA from 16 to 17 or 17 to 18. This is expected as the change forces more students into schools than the other two changes in MDA laws. No significant change in MDA is reported from 16 to 17 or 17 to 18. The magnitude of effect of an increase in MDA from 16 to 18 is similar to that of the fully-specified model, a difference of only 0.077 incidences per year.

The empirical analysis also studies the effect of changes in MDA laws on various crime aggregates as well. Table 7 displays the regression results for the crime aggregates: attacks without a weapon, threats without a weapon, illegal drugs, property, and violent. Given that the main effects are only significant for an increase in MDA to 18, we only focus our analysis on this particular group (but do report an increase to 17 in the table). The results indicate that as the percent of students eligible for free lunch increase, attacks without a weapon, threats without a weapon, and violent crime increase. As student enrollment increases, the number of threats without a weapon increase, illegal drugs, and violent crimes increase. These results are

consistent with a concentration effect studied in the literature (see Jacob and Lefgren (2003) and Luallen (2006)). The analysis also finds that the presence of a security guard is not correlated with crime while resource officers are positively correlated with higher crime. Studying drug crime prevention resources on illegal drug crimes, the results find a positive and significant coefficient on random drug sniffs, and no effect of random sweeps. All of the above results are consistent with the literature and economic intuition.

The results in Table 7 also suggest that an increase in MDA to 18 has a significant effect on attacks without a weapon, threats without a weapon, and illegal drug crimes, and that an increase in MDA to 17 does not significantly increase crime incidences. Specifically, an increase in MDA to 18 increases attacks without a weapon by 0.627 per 1,000 students per year, or a 12.2% increase in states changing their MDA to 18, increases threats without a weapon by 0.588 per 1,000 students per year, or a 36.3% increase in states changing their MDA to 18, and increases drug incidences by 0.437 per 1,000 students per year, or 43.4% increase in states changing their MDA to 18. An increase in MDA laws to 17 or 18 do not have significant effects on property or violent crime.

The consistently significant effect of an increase in MDA to 18 on crime incidences and the lack of significance when the MDA is increased to 17 may be due to the difference in sentencing lengths once juveniles are considered adults at age 18, i.e., individual crime near the age of criminal majority may change for those on the margin of committing a crime. Thus, one interpretation of the increase in crime incidences from an increase to MDA of 18 is that dropouts are now committing their *last hurrah* crimes in schools instead of on streets before turning 18 and being tried as adults. This interpretation is consistent with Lee and McCrary (2009) who exploit the discontinuous increase in the punitiveness of criminal sanctions at 18 to estimate the deterrence effect of incarceration and empirically estimate a 2 percent decline in the

log-odds of offending at 18.

4.1. Robustness of Results Across Demographics

The results presented in the previous section provide inferences for the average marginal effect of MDA laws on in-school crime for public high schools across the United States over the last decade. These effects may be heterogeneous and depend on demographics of the schools in question. Thus, it is useful to estimate the effects of MDA laws across various subgroups. We focus on two consistently significant demographics: the socioeconomic status of the students as measured by the percent of students eligible for school lunch and the size of the school as measured by enrollment levels. Socioeconomic status is an important demograph and is typically analyzed in the majority of economics of education literature. School enrollment is also an important consideration when analyzing in-school crime for a few reasons. First, more students increase the likelihood of interactions that lead to conflict. Second, schools with high enrollment may be harder to monitor. Third, high enrollment lead to more opportunities for delinquents to negatively influence *good* students. Finally, schools with high enrollment are generally associated with more diversity among students and staff.

Table 8 provides descriptive statistics for rates of in-school crime across the four quartiles for the two demographics. Note that schools with the lowest percentage of students eligible for free lunch (high incomes) are quartile 1. The table makes evident that as the percentage of low-income students increase, crime rates increase. This relationship is observed in states that changed their MDA during the sample and in states that do not change their MDA during the sample. Interestingly, the descriptive statistics indicate that crime decreases as the size of the school increases. We also find that states that change their MDA have higher rates of crime than

schools that do not change their MDA law across the sample.

To study these groups empirically, we extend the baseline DD estimator with interaction terms for quartiles of either the percent eligible for free lunch or school enrollment. The general specifications is as follows:

$$\begin{aligned}
\widetilde{Crime}_{ist}^j = & \alpha + \beta_1 MDA17_{st} + \beta_2 MDA18_{st} + \beta_3 R_{ist} \\
& + \beta_4 (MDA17_{st} * Q_{ist}^1) + \beta_5 (MDA18_{st} * Q_{ist}^1) \\
& + \beta_6 (MDA17_{st} * Q_{ist}^2) + \beta_7 (MDA18_{st} * Q_{ist}^2) \\
& + \beta_8 (MDA17_{st} * Q_{ist}^3) + \beta_9 (MDA18_{st} * Q_{ist}^3) \\
& + \beta_{10} X_{ist} + \beta_{11} S_s + \beta_{12} T_t + \varepsilon_{ist}
\end{aligned} \tag{3}$$

where R_{ist} is either the percent eligible for free lunch or school enrollment for school i , in state s , at time t . Q_{ist}^m is an indicator to whether R_{ist} is in quartile m of R for $m = \{1, 2, 3\}$. The coefficients of interest are β_1 , β_2 , and β_4 through β_9 . These capture the effect of the treatment on the treated. The interaction terms measure the marginal effect of the MDA laws of 17 and 18 on the specific quartile of the group, relative to the control group, i.e., against high school in states with no change in MDA in the subgroup quartile. The coefficients of interest are expected to be either positive or negative and significant if MDA laws affect the relevant subgroups differentially from the 4th quartile of the group (the baseline comparison subgroup).

Table 9 presents the estimation results and indicate that an increase in the MDA to 18 significantly increases in-school crime in all four income quartiles identically. The table also provides the results of the enrollment quartile estimates. These estimates show that an increase in the MDA to 18 significantly increases in-school crime across the enrollment quartiles, and in particular, the second, third, and fourth quartiles of enrollment. There is no significant

effect of a MDA of 18 on the first enrollment quartile schools (the smallest 25% of schools). The descriptive statistics show downward trends in crime with increasing enrollment. The DD model controlling for school demographics, street crime, and crime prevention resources indicate that crime increases with higher enrollment levels. This is consistent with economic intuition, as enrollment increases, there are more interactions between students, and this increases the likelihood that altercations will occur (see Gaviria and Raphael (2001)).

5. In-school Crime Resources and Utilization of In-School Punishment

In this section we study whether schools foresee crime increasing in their schools from an increase in the MDA and allocate additional in-school resources to crime prevention.¹⁴ To conduct this analysis, we estimate Eq. (2) with rates of in-school crime prevention resources on the left hand side and empirically test whether changes in MDA laws have an effect. The results of this estimation are presented in Table 10. The results show that an increase in the MDA to 18 does not increase any of the crime prevention resources. In fact, the results indicate a reduction in the number of resource officers per 1,000 students. We suspect that crime prevention resources do not increase significantly due to the lumpiness of these resources. That is, a few additional students per 1,000 students per school is not sufficient to add an additional police officer in every school (nor are these additional students large enough in number to reduce most crime prevention resource per 1,000 students). Thus, when dropout students are compelled to attend public school, it is too costly for schools to increase these resources above and beyond what they

¹⁴If increases in MDA cause both more crime and increases in the amount of crime prevention, then the expected value of the error term in the crime regression is not zero, i.e., if $E[\varepsilon|X_1...X_n] \neq 0$, then estimates are biased and inconsistent. We find that this is not the case.

would normally require on a per 1,000 students basis.

Even though school administrators do not increase in-school crime prevention resources when the MDA increases, they may change their utilization rates of in-school punishment. There are two potential effects on utilization rates. First, school administrators may respond to an increase in the number of delinquents by increasing the rate of expulsions and transfers, and decreasing the rate of suspensions to safeguard education production. That is, they may switch to more zero-tolerance policies and punish students to the fullest extent possible. Second, school administrators may respond by proactively retaining more dropouts by decreasing the rates of expulsions and transfers, and increasing the rate of suspensions to punish offenders. This response would demonstrate more tolerance towards dropouts as schools attempt to rehabilitate delinquents back into the school system. To understand whether changes in MDA laws have an effect on in-school punishment utilization rates and the direction, we estimate Eq. (2) with rates of suspensions, expulsions, and transfers to specialized schools per 1,000 students per school year on the left hand side, respectively. Table 2 show descriptive statistics for these three in-school punishments. The average number of student suspensions is 17.6 per 1,000 students per year, the average number of expulsions is 2.5 per 1,000 students per year, and the average number of student transfers is 10.3 per 1,000 students per year. Suspensions, expulsions, and transfers all have decreasing trends over the sample period.

The results of the effect of MDA laws on suspensions, expulsions, and transfers are presented in Table 11. Interestingly, the results indicate that an increase in the MDA to 18 increases suspensions by 4.8 per 1,000 students per year, decreases expulsions by 1.9 per 1,000 students per year, and has no effect on transfers. These correspond with a 34.4% increase in suspensions and a 35.8% decrease in expulsions in states that increase their MDA to 18. These findings suggest that school administrators reinforce lawmakers efforts by retaining more students in-

school and keeping them off the streets.¹⁵

6. Conclusion

The empirical findings estimated in this paper add valuable information to the debate about the effect of MDA laws. The previous literature has estimated the positive effects of changes in MDA laws and have failed to account for negative effects, such as changes in rates of in-school crime. This paper investigates the implications of changes in state-level MDA laws on in-school crime rates, in-school crime prevention resources, and school administered punishment utilization rates. The first investigation estimates the effect of an increase in MDA laws on in-school crime and the results suggest that an increase in the MDA to 18 increases in-school crime by 6.2% in states that change their MDA relative to those that do not. The results are robust across alternative control groups. Studying crime incidences by type, the results indicate that attacks without a weapon, threats without a weapon, and drug incidences significantly increase by 12.2%, 36.3%, and 43.4%, respectively, by a increase in MDA to 18, but property and violent crimes do not increase. However, the results do not find systematic evidence that an increase in MDA to 17 effects any rate of in-school crime. These results are consistent with a *last hurrah* effect when juveniles increase crimes prior to turning age 18. We also find that an increase in MDA does not change in-school crime prevention resources. Last, the results provide evidence that schools change their utilization of school administered in-school punishment versus out-of-school punishment with a decrease in expulsions and an increase in suspensions. Thus, school administrators fortify state policymakers efforts to keep juveniles in-school and off of streets.

¹⁵Robustness checks are estimated for the effect of the MDA laws on suspension and expulsions rates across income levels and enrollment levels. No significantly different results are identified across quartiles.

The literature has shown that in-school crime profoundly affects education production. For example, Grogger (1997) finds that moderate levels of violence reduce the likelihood of high school graduation by 5.1 percentage points and lower the likelihood that a student will attend college by 6.9 percentage points. Similarly, Bowen and Bowen (1999) find that neighborhood and school danger both contributed significantly to the prediction of each school outcome, especially attendance and behavior.¹⁶ Walker and McGarvey (2010) investigate the effect of in-school crime on student achievement in Georgia and their preliminary results show that in-school crime is associated with lower pass rates. Studying why teachers exit the teaching profession, Gilpin (2011) finds that new teachers threatened by a student increases their probability of exiting the teaching profession by 8 percentage points. Studying peer influence, Gaviria and Raphael (2001) use a sample of tenth-graders and finds that the propensity to engage in drug use, alcohol drinking, cigarette smoking, church going, and dropping out of high school increase due to peer-group effects at the school-level. These papers make clear that increasing the MDA will have negative affects on students, teachers, and schools.

While there are additional costs born by educators and students alike, as discussed in the introduction, there are also additional societal benefits of forcing delinquent students back into schools. One benefit not discussed in the MDA literature is the compounding effects of reducing juvenile crime. Cohen (1998) estimates the potential benefits of saving high-risk youths based on the lifetime costs associated with the typical career criminal, drug abuser, and high-school dropout. Using a 2 percent discount rate, external costs of a typical career criminal are estimated to be \$1.8 - \$2.0 million, a heavy drug user to be \$500,000 - \$1.3 million, and a high school dropout to be \$330,000 - \$530,000. Disregarding individuals who are both heavy drug users

¹⁶Outside of economics, there is a large literature studying the effects of crime on student achievement. See Chen (2007); Burdick-Will et al. (2010); Henrich et al. (2004); Nash (2002); and Macmillan and Hagan (2004).

and career criminals, the monetary value of saving a high-risk youth is \$2.3 to \$3.2 million.¹⁷ Furthermore, Bernburg and Krohn (2003) using labeling theory empirically find that official intervention in youth has a significant, positive effect on crime in early adulthood, and this effect is partly mediated by educational achievement and employment. Thus, the type of penalty delinquents receive, such as in-school suspension versus entering the juvenile justice system, may also have a profound long-term impacts on the delinquent youth.

While we know that when students aren't allowed to walk away from their education more of them walk the stage to get their diploma, they also cause negative effects on students and educators alike. Thus, state policymakers must consider both the positive and negative effects when choosing to increase their state's MDA to eighteen.

¹⁷ Costs are inflated to today's dollars.

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Table 1: Minimum Dropout Ages by State and Year

State ^a	2002	2003	2004	2005	2006	2007	2008	State	2002	2003	2004	2005	2006	2007	2008
Alabama	16	16	16	16	16	16	16	Montana	16	16	16	16	16	16	16
Alaska	16	16	16	16	16	16	16	Nebraska	16	16	16	18	18	18	18
Arizona	16	16	16	16	16	16	16	Nevada	17	17	17	17	17	18	18
Arkansas	17	17	17	17	17	17	17	New Hampshire	16	16	16	16	16	16	16
California	18	18	18	18	18	18	18	New Jersey	16	16	16	16	16	16	16
Colorado	16	16	16	16	17	17	17	New Mexico	18	18	18	18	18	18	18
Connecticut	18	18	18	18	18	18	18	New York ^b	17	17	17	17	17	17	17
District of Columbia	18	18	18	18	18	18	18	New York ^c	16	16	16	16	16	16	16
Delaware	16	16	16	16	16	16	16	North Carolina	16	16	16	16	16	16	16
Florida	16	16	16	16	16	16	16	North Dakota	16	16	16	16	16	16	16
Georgia	16	16	16	16	16	16	16	Ohio	18	18	18	18	18	18	18
Hawaii	18	18	18	18	18	18	18	Oklahoma	18	18	18	18	18	18	18
Idaho	16	16	16	16	16	16	16	Oregon	18	18	18	18	18	18	18
Illinois	16	16	17	17	17	17	17	Pennsylvania	17	17	17	17	17	17	17
Indiana	16	16	16	18	18	18	18	Rhode Island	16	16	16	16	16	16	16
Iowa	16	16	16	16	16	16	16	South Carolina	17	17	17	17	17	17	17
Kansas	18	18	18	18	18	18	18	South Dakota	16	16	16	16	16	16	16
Kentucky	16	16	16	16	16	16	16	Tennessee	17	17	17	17	17	17	17
Louisiana	18	18	18	18	18	18	18	Texas	18	18	18	18	18	18	18
Maine	17	17	17	17	17	17	17	Utah	18	18	18	18	18	18	18
Maryland	16	16	16	16	16	16	16	Vermont	16	16	16	16	16	16	16
Massachusetts	16	16	16	16	16	16	16	Virginia	18	18	18	18	18	18	18
Michigan	16	16	16	16	16	16	16	Washington	18	18	18	18	18	18	18
Minnesota	16	16	16	16	16	16	16	West Virginia	16	16	16	16	16	16	16
Mississippi	17	17	17	17	17	17	17	Wisconsin	18	18	18	18	18	18	18
Missouri	16	16	16	16	16	16	16	Wyoming	16	16	16	16	16	16	16

^a States that change their MDA during the sample period are boldface.

^b Cities in the state of New York with a population greater than 4,500.

^c Cities in the state of New York with a population less than 4,500.

Figure 1: Crime Incidences per 1,000 Students in MDA Change States Versus Non-change States, 2003 – 2009^a

^a: Crime incidence are in deviations from their respective means.

Table 2: Summary Statistics for Key Variables Variables Across Time

Variables	2003 ^a	2005 ^a	2007 ^a	2009 ^a	All-Years ^a	Change ^b
<i>Compulsory Schooling Laws</i>						
MDA 16	0.46 (0.50)	0.43 (0.50)	0.36 (0.48)	0.38 (0.48)	0.41 (0.49)	-17%
MDA 17	0.15 (0.35)	0.20 (0.40)	0.25 (0.43)	0.20 (0.40)	0.20 (0.40)	30%
MDA 18	0.39 (0.49)	0.37 (0.48)	0.39 (0.49)	0.42 (0.49)	0.39 (0.49)	7%
<i>In-School Crime Rates</i>						
All Crime	52.2 (47.2)	56.9 (48.4)	46.9 (32.5)	48.3 (44.5)	51.0 (43.8)	-12.7%
Attack w/o Weapon	20.9 (27.5)	19.2 (20.0)	19.1 (35.6)	17.5 (25.5)	19.1 (27.7)	-8.8%
Threat w/o Weapon	15.8 (32.3)	14.4 (25.2)	12.7 (29.0)	12.7 (25.1)	13.9 (28.0)	-15.6%
Illegal Drug	2.6 (7.9)	8.0 (11.2)	6.7 (7.3)	7.4 (10.0)	6.2 (9.5)	33.7%
Property	18.2 (20.1)	19.7 (20.4)	20.8 (21.1)	18.3 (20.7)	19.3 (20.6)	3.3%
Violent	3.0 (7.0)	5.3 (8.3)	4.2 (5.8)	4.8 (11.2)	4.3 (8.4)	7.8%
<i>Disciplinary Policies</i>						
Suspensions	(4.0) 21.4 (35.8)	(3.8) 18.3 (25.5)	(4.8) 15.6 (20.5)	(4.4) 15.5 (23.6)	(4.3) 17.6 (26.9)	-21.8%
Expulsions	2.8 (7.8)	2.5 (6.0)	2.1 (5.5)	2.6 (7.8)	2.5 (6.9)	-11.1%
Transfers	11.6 (21.0)	11.4 (20.3)	10.7 (22.6)	7.7 (16.2)	10.3 (20.2)	-20.0%
Observations ^c	840	760	750	780	3,130	
Weighted Observations ^c	8,840	9,410	9,270	9,770	37,300	

^a Means with standard deviations in parentheses.

^b Percent change between the averages of 2003-2005 and 2007-2009. Two year averages are used to account for unanticipated or large deviations.

^c The number of observations are rounded to the nearest 10s place as per restricted-use data license.

Table 3: Descriptive Statistics by Whether a State that Changes Its MDA Law

	Change States		No Change States	
	Mean	Std. Dev	Mean	Std. Dev
<i>Compulsory Schooling Laws</i>				
MDA 16	0.29	0.45	0.42	0.49
MDA 17	0.44	0.50	0.17	0.37
MDA 18	0.28	0.45	0.41	0.49
<i>In-School Crime Rates</i>				
All Crime	60.73	53.99	49.66	41.92
Attacked w/o Weapon	27.03	58.72	18.01	19.21
Threat w/o Weapon	18.39	33.89	13.21	26.95
Illegal Drug	6.78	11.27	6.13	9.24
Property	23.15	23.69	18.70	20.11
Violent	4.86	8.48	4.27	8.41
<i>Disciplinary Policies</i>				
Suspensions	19.19	30.67	17.42	26.28
Expulsions	3.55	8.21	2.32	6.65
Transfers	6.67	13.89	10.80	20.89
<i>Student Body Characteristics</i>				
Percent Eligible for Free Lunch	33.07	24.70	38.57	24.23
Percent Male	49.50	9.76	49.04	9.12
Percent Below 15 th % on Std. Tests	15.30	17.11	13.77	14.08
Percent Minority	21.58	26.07	27.80	27.42
<i>School Characteristics</i>				
Enrollment	1019	782	1079	742
Student-to-Teacher Ratio	13.78	4.14	13.95	4.12
Random Dog Sniffs	0.63	0.48	0.63	0.48
Random Sweeps	0.22	0.41	0.28	0.45
Uniform	0.06	0.25	0.05	0.22
Security Cameras	0.78	0.42	0.75	0.43
Number of Security Guards	1.06	1.80	0.80	1.44
Number of Resource Officers	0.55	1.03	0.61	0.92
Number of Officers	0.18	0.84	0.10	0.44
<i>Neighborhood Characteristics</i>				
Adult Street Crime	45.77	23.66	54.28	28.90
City	0.17	0.38	0.19	0.39
Urban	0.32	0.47	0.31	0.46
Observations ^a	380		2,760	
Weighted Observations ^a	4730		32683	

^a The number of observations are rounded to the nearest 10s place as per restricted-use data license.

Table 4: Box-Cox Transformation Parameters

In-school Crime	C	λ	Mean	Std. Dev.
All Crime	1.731	0.264	6.460	1.917
Attack w/o Weapon	0.027	0.271	4.157	2.508
Threat w/o Weapon	0.038	0.084	1.227	3.205
Illegal Drug	0.006	0.335	0.832	2.701
Property	0.001	0.213	3.582	2.388
Violent	0.004	0.168	0.113	2.174

^a See equation 1 for further details.

C is the scale shifter.

λ is the power shifter

Table 5: In-School Crime Regression Results

Variables	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
MDA 17	0.447***	4.93	0.425***	5.22	-0.096	-0.51	0.027	0.22
MDA 18	0.713***	6.09	0.730***	6.66	0.486***	4.12	0.434***	3.60
City			0.220	1.53	-0.031	-0.20	-0.049	-0.34
Urban			-0.240***	-2.85	-0.100	-1.01	-0.123	-1.24
Enrollment					-0.010	-0.12	-0.008	-0.10
Student-to-Teacher Ratio					0.026	1.58	0.024	1.39
Adult Street Crime					0.000	0.16	0.000	0.02
Percent Eligible for Free Lunch					0.016***	5.13	0.015***	4.73
Percent Male					0.000	0.09	0.002	0.43
Percent Below 15 th % on Std. Tests					0.005	1.00	0.004	1.02
Percent Minority					0.001	0.40	0.002	0.66
Number of Security Guards							0.064	1.27
Number of Resource Officers							0.080	0.84
Number of Police Officers							-0.207	-1.40
Uniform							-0.523*	-1.71
Security Cameras							-0.009	-0.05
Time Fixed Effects	Yes		Yes		Yes		Yes	
States Fixed Effects	Yes		Yes		Yes		Yes	
School Controls	No		No		Yes		Yes	
In-School Security	No		No		No		Yes	
Resource Controls	No		No		No		Yes	
Drug Prevention	No		No		No		No	
R-Squared	0.082		0.088		0.122		0.126	

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table 6: Robustness Regression Analysis

	Control Group A		Control Group B		Control Group C		Control Group D		Control Group E		Control Group F		Control Group G	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
MDA 17	0.036	0.29	0.040	0.32	0.030	0.23	0.054	0.42					0.035	0.24
MDA 18	0.444***	3.69	0.450***	3.77	0.425***	3.36	0.452***	3.61	-0.087	-0.41	0.511***	5.76		
Time Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
States Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
School Controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
In-School Security	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Resource Controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Drug Prevention	No		No		No		No		No		No		No	
R-Squared	0.126		0.126		0.125		0.126		0.143		0.122		0.135	
Observations	2,520		2,500		2,430		2,370		1,490		2,120		1,360	

Control Group A: Excludes low sample states (VT, ND, DE)

Control Group B: Excludes states that generally excluded from MDA models (AK, DC, HI, MS)

Control Group C: Excludes states with Multiple MDA laws (NY)

Control Group D: Excludes all excludable states (VT, ND, DE, AK, DC, HI, MS, NY)

Control Group E: Excludes states that have a MDA of 16 at any time in the sample period

Control Group F: Excludes states that have a MDA of 17 at any time in the sample period

Control Group G: Excludes states that have a MDA of 18 at any time in the sample period

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table 7: Various In-School Crime Regression Results

	Attacks w/o Weapon		Threats w/o Weapon		Drug Crimes		Property Crimes ^a		Violent Crimes ^a	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
MDA 17	-0.358*	-1.76	-0.458	-0.88	0.385	0.57	0.364	0.79	-0.360	-1.49
MDA 18	0.627***	2.76	0.588**	2.53	0.437**	2.27	-0.055	-0.10	-0.110	-0.45
City	-0.347*	-1.90	-0.062	-0.26	0.099	0.63	0.209	0.93	0.232	1.49
Urban	-0.332**	-2.13	0.148	0.83	-0.196*	-1.77	0.395***	2.77	-0.169	-1.58
Enrollment	0.104	0.92	0.701***	3.91	1.000***	7.74	-0.055	-0.60	0.528***	3.81
Student-to-Teacher Ratio	0.031	1.33	0.015	0.59	0.035*	1.75	0.030	1.46	0.008	0.45
Adult Street Crime	0.000	-0.05	0.005*	1.70	0.001	0.43	-0.003	-1.09	0.000	0.11
Percent Eligible for Free Lunch	0.024***	5.38	0.025***	4.33	0.001	0.42	0.004	0.94	0.009**	2.28
Percent Male	-0.003	-0.50	0.002	0.18	0.005	0.72	0.003	0.51	0.004	0.93
Percent Below 15 th % on Std. Tests	0.010*	1.96	0.001	0.17	0.005	0.94	-0.004	-0.86	0.003	0.73
Percent Minority	0.009**	2.19	-0.010	-1.49	0.001	0.20	0.001	0.23	0.001	0.23
Number of Security Guards	0.026	0.36	0.081	1.09	0.032	0.54	0.026	0.54	0.084*	1.86
Number of Resource Officers	-0.062	-0.56	0.175	1.55	0.248***	2.98	0.153	1.66	0.036	0.42
Number of Police Officers	-0.197*	-1.75	-0.179	-1.38	0.197	0.92	-0.092	-0.54	-0.173	-1.26
Uniform	-1.091***	-2.70	-0.912**	-2.35	-0.341	-0.94	-0.214	-0.58	0.192	0.66
Security Cameras	0.321*	1.82	0.343*	1.75	0.538***	3.17	-0.172	-1.00	0.269**	2.13
Random Dog Sniffs					0.395***	2.97				
Random Sweeps					0.124	0.94				
Time Fixed Effects	Yes		Yes		Yes		Yes		Yes	
States Fixed Effects	Yes		Yes		Yes		Yes		Yes	
School Controls	Yes		Yes		Yes		Yes		Yes	
In-School Security	Yes		Yes		Yes		Yes		Yes	
Resource Controls	Yes		Yes		Yes		Yes		Yes	
Drug Prevention	No		No		Yes		No		No	
R-Squared	0.137		0.086		0.287		0.103		0.085	

^a Crime aggregates are addressed in Table 3.

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table 8: Means of In-School Crime Rates Across Income and Enrollment Quartiles

Demographics	Quartiles	No Change States	Change States	All States
Income ^a	1	35.9	44.5	37.2
	2	49.0	56.9	50.3
	3	53.8	62.2	54.6
	4	58.3	92.9	61.4
Enrollment	1	52.0	69.0	54.7
	2	51.3	53.8	51.6
	3	49.2	64.2	51.0
	4	46.1	52.1	46.8

^a The lowest percentages of students eligible for free lunch are in quartile 1.

Table 9: In-School Crime Across Percent Eligible for Free Lunch and Enrollment Quartiles
Regression Results

Variable	Coeff.	t-stat	Coeff.	t-stat
MDA 17	0.116	0.51	0.327**	2.28
MDA 18	0.448*	1.75	0.586***	4.41
Percent Eligible for Free Lunch	0.013***	2.70		
Enrollment			-0.156*	-1.69
MDA 17*Free Lunch ₁	-0.317	-1.11		
MDA 17*Free Lunch ₂	0.234	0.94		
MDA 17*Free Lunch ₃	-0.082	-0.28		
MDA 18*Free Lunch ₁	-0.383	-1.36		
MDA 18*Free Lunch ₂	0.226	0.94		
MDA 18*Free Lunch ₃	0.106	0.45		
MDA 17*Enrollment ₁			-1.105**	-2.54
MDA 17*Enrollment ₂			-0.260	-1.36
MDA 17*Enrollment ₃			-0.050	-0.31
MDA 18*Enrollment ₁			-0.560**	-2.65
MDA 18*Enrollment ₂			0.045	0.21
MDA 18*Enrollment ₃			-0.047	-0.38
Time Fixed Effects	Yes		Yes	
States Fixed Effects	Yes		Yes	
School Controls	Yes		Yes	
In-School Security	Yes		Yes	
Resource Controls	Yes		Yes	
Drug Prevention	No		No	
R-Squared	0.135		0.089	

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table 10: In-School Crime Prevention Resources Regression Results

Variables	Sec. Guard		Resource Officer		Police Officer		Drug Sniff ^a		Drug Sweep ^a		Sec. Camera ^a	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Minimum Dropout Age of 17	0.361***	2.84	-0.063	-0.33	-0.072	-0.23	-0.019	-0.19	-0.463	-2.48	0.638**	2.09
Minimum Dropout Age of 18	-0.562	-1.28	-0.312**	-2.59	0.024	0.41	-0.164	-1.29	-0.096	-0.20	0.330	1.01
City	0.759***	4.93	0.115	1.44	0.005	0.12	-0.801***	-6.38	0.085	0.81	0.040	0.32
Urban	0.377***	3.51	0.031	0.57	-0.022	-1.05	-0.329***	-2.83	-0.052	-0.73	0.100	0.90
Enrollment	0.091	1.19	-0.106**	-2.28	-0.010	-0.60	0.113*	1.70	0.088	1.45	0.287**	2.50
Student-to-Teacher Ratio	-0.003	-0.14	-0.007	-0.62	-0.006	-0.90	0.007	0.83	-0.012	-1.29	0.006	0.41
Adult Street Crime	0.001	0.61	-0.001	-0.39	0.000	0.87	0.003**	2.03	0.003*	1.87	0.000	0.21
Percent Eligible for Free Lunch	0.005	1.67	0.004**	2.21	0.000	-0.01	0.002	0.69	0.009***	5.46	0.005**	1.98
Percent Male	-0.003	-0.79	-0.004	-1.17	0.001	0.59	-0.003	-0.74	-0.002	-0.83	-0.004	-0.70
Percent Below 15 th % on Std. Tests	0.011**	2.44	0.007**	2.14	0.003	1.50	0.000	-0.16	0.001	0.35	0.008***	2.91
Percent Minority	0.010**	2.50	-0.002	-1.07	0.002**	2.06	-0.007**	-2.50	0.002	1.55	-0.002	-1.17
Time Trends	Yes		Yes		Yes		Yes		Yes		Yes	
States Trends	Yes		Yes		Yes		Yes		Yes		Yes	
School Controls	Yes		Yes		Yes		Yes		Yes		Yes	
R-Squared	0.293		0.136		0.081		0.2185		0.0996		0.1499	

^a Coefficients are marginal effects estimated using a probit limited probability model.

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table 11: Utilization of School Attendance Regression Results

Variable	Suspensions		Expulsions		Transfers	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Minimum Dropout Age of 17	-4.506***	-2.90	-1.498***	-2.82	1.051	0.49
Minimum Dropout Age of 18	4.840***	4.01	-1.943***	-3.38	0.968	0.57
City	1.569	0.91	-0.493	-0.90	-1.972*	-1.70
Urban	0.024	0.02	-0.242	-0.55	-0.985	-0.86
Enrollment	1.550	1.68	-0.004	-0.02	0.433	0.72
Student-to-Teacher Ratio	0.207	1.29	0.126*	1.84	0.199**	2.06
Adult Street Crime	0.036*	1.88	0.008	1.10	0.006	0.25
Percent Eligible for Free Lunch	0.111***	2.90	0.015	1.41	0.090***	3.64
Percent Male	-0.076	-1.34	-0.060*	-1.92	0.011	0.24
Percent Below 15 th % on Std. Tests	0.181***	2.81	0.027	1.44	0.036	1.32
Percent Minority	0.047	1.03	0.006	0.63	0.064	1.66
Number of Security Guards	0.286	0.46	0.086	0.67	0.002	0.01
Number of Resource Officers	0.100	0.17	0.043	0.21	0.403	0.59
Number of Police Officers	-0.902	-0.75	0.006	0.04	0.435	0.30
Time Fixed Effects	Yes		Yes		Yes	
States Fixed Effects	Yes		Yes		Yes	
In-School Security	Yes		Yes		Yes	
School Controls	Yes		Yes		Yes	
Resource Controls	Yes		Yes		Yes	
R-Squared	0.149		0.127		0.240	

Asterisks *, **, *** indicate significance at the 10%, 5%, and 1% statistical levels, respectively.

Table A1: Variable Definitions and Sources

Variable	Definition	Source
<i>Compulsory Schooling Laws</i>		
Minimum Dropout Age of 16	1: if state has a MDA of 16	NCES and policy briefs
Minimum Dropout Age of 17	1: if state has a MDA of 17	NCES and policy briefs
Minimum Dropout Age of 18	1: if state has a MDA of 18	NCES and policy briefs
<i>Crime and In-school Punishment</i>		
In-School Crime ^{a,b}	The number of attacks without a weapon, threats without a weapon, drug crimes, property crimes, and violent crimes	SSOCS
Attacks w/o Weapon ^{a,b,c}	An actual and intentional touching or striking of another person against his or her will, or the intentional causing of bodily harm to an individual without the use of a weapon	SSOCS
Threats w/o Weapon ^{a,b,c}	The threat of an actual and intentional touching or striking of another person against his or her will, or the intentional causing of bodily harm to an individual without the use of a weapon	SSOCS
Drug ^{a,b}	Distribution, possession, or use of illegal drugs	SSOCS
Property ^{a,b}	Aggregate of thefts and vandalisms	SSOCS
Violent ^{a,b}	Aggregate of rapes, sexual batteries, attacks with a weapon, threats with a weapon, possessions of a firearm, robberies, robberies with a weapon, and possessions of a knife	SSOCS
Suspensions ^a	The number of out-of-school suspensions lasting 5 or more days, but less than the remainder of the school year	SSOCS
Expulsions ^a	The number of student removals with no continuing school services for at least the remainder of the school year	SSOCS
Transfers ^{a,d}	The number of student transfers to specialized schools for disciplinary reasons ^c	SSOCS

^a Per 1,000 students.

^b Total number of crime incidents brought to the attention of school administrators.

^c Weapon - any instrument or object used with the intent to threaten, injure, or kill. This includes look-alikes if they are used to threaten others.

^d Specialized school - a school that is specifically designed for students who were referred for disciplinary reasons.

Table A1 Con.: Variable Definitions and Sources

Variable	Definition	Source
<i>Student Body Characteristics</i>		
Eligible for Free Lunch	Percent of student body that is eligible for free or reduced-price lunch	SSOCS
Male	Percent of student body that is male	SSOCS
Below 15 th Percentile	Percent of student body below 15 th percentile on standardized tests	SSOCS
Minority	Percent of student body that is of minority race	SSOCS
<i>School Characteristics</i>		
Enrollment	Total student enrollment (in thousands)	SSOCS
Student to Teacher Ratio	Students per full-time-equivalent teacher	SSOCS
Random Dog Sniffs	1: if school practices random dog sniffs for drugs	SSOCS
Random Sweeps	1: if school practices random sweeps for contraband not including dog sniffs	SSOCS
Uniform	1: of school requires students to wear uniforms	SSOCS
Security Cameras	1: if security camera(s) monitor the school	SSOCS
Number of Security Guards ^a	The number of full-time security guards	SSOCS
Number of Resource Officers ^a	The number of full-time school resource officers (SROs)	SSOCS
Number of Officers ^a	The number of full-time sworn law enforcement officers—not SROs	SSOCS
<i>Community Characteristics</i>		
Adult Street Crime	County-level crime incidences per 1,000 persons over 18 years of age	UCR
City	1: if school is located in a city	SSOCS
Urban	1: if school is located in an urban area	SSOCS

^a Per 1,000 students.