Child Access Prevention Laws, Youth Gun Carrying, and School Shootings

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Despite public interest in keeping guns out of schools, little is known about the effects of gun control on youth gun carrying or school violence. Using data from the Youth Risk Behavior Surveys (YRBS) for the period 1993-2013, we examine the relationship between child access prevention (CAP) laws and gun carrying among high school students. Our results suggest that CAP laws lead to an 18.5 percent decrease in the rate of gun carrying and a 19 percent decrease in the rate at which students reported being threatened or injured with a weapon on school property. These results are concentrated among minors, for whom CAP laws are most likely to bind. To supplement our YRBS analysis, we assemble a data set on school shooting deaths for the period 1991-2013. We find little evidence that CAP laws deter school-associated shooting deaths, but these estimates are insufficiently precise to reach a policy conclusion.

JEL Codes: K4, I2, H7

Key Words: Gun Control, Child Access Prevention Laws, Youth Risky Behavior, School Violence

1. Introduction

School shootings, such as the recent high-profile events in Chardon, Ohio; Sparks, Nevada; Troutdale, Oregon; Benton, Kentucky; and Santa Fe, Texas, are usually committed by students under the age of 18.[1] In fact, between 2012 and 2015, approximately 70 percent of shootings at K-12 schools in the United States were committed by minors (Everytown.org 2015a). More often than not, the shooters obtained their guns from their own home or the home of a relative (Violence Policy Center 2001; Wood 2001; Copeland 2014). Based on survey data, roughly one-third of households with children reported that firearms were kept in or around their home (Okoro et al. 2005). Among homes with children and firearms, 43 percent reported having at least one gun in an unlocked place (Schuster et al. 2000). According to the Brady Campaign to Prevent Gun Violence (2016), 1.7 million children in the United States live in a home with an unlocked and loaded gun.

Advocates for stricter gun control laws cite the wide availability of firearms to youths as an important risk factor for school violence (Wood 2001; Christakis and Christakis 2012; Vartabedian 2014). In an effort to restrict youth firearm access, a number of states have passed child access prevention (CAP) laws, which impose criminal liability on gun owners who allow children unsupervised access to firearms (Law Center to Prevent Gun Violence 2013).

[1] On February 27, 2012, Thomas Lane, a 17-year-old student, brought a .22 caliber handgun he had taken from his grandfather's barn to the Chardon High School cafeteria. He fired 10 rounds, killing three students and injuring two others (Crimesider staff 2012; Dolak, Ng, and Lowe 2012). On October 21, 2013, 12-year-old seventh-grade student Jose Reyes brought a 9-mm semi-automatic handgun from home to the Sparks Middle School playground. He killed one teacher and injured two students before turning the gun on himself. He obtained the weapon from an unlocked case on a shelf above his kitchen refrigerator (Associated Press 2013). On June 10, 2014, 15-year-old freshman Jared Padgett carried an AR-15 assault rifle in a guitar case on the school bus to Reynolds High School. He used it to kill one student and wound a teacher in the boys' locker room before committing suicide (Bernstein 2014). He took the gun from his brother, a member of the U.S. Army Reserve program (Slauson 2014). On January 23, 2018, 15-year-old student Gabe Parker used a handgun to kill two students at Marshall County High School. The gun was taken from a closet at the shooter's home (Markgraf 2018; Yan, Stapleton, and Murphy 2018). On May 18, 2018, 17-year-old Dimitrios Pagourtzis smuggled a shotgun and .38 revolver under his coat into Santa Fe High School and killed 10 people. The guns were taken from the student's father (Fernandez, Fausset, and Bidgood 2018).

Proponents of CAP laws argue that they not only limit intentional shootings but also accidental ones (Shaffer 1999). On the other hand, opponents of gun control contend that CAP laws impede on a gun owner's constitutional right to bear arms. In emergency situations, they argue that safe storage requirements encumber a potential victim's ability to use a firearm for self-defense (Shaffer 1999; Lott and Whitley 2001).

While a small literature on CAP laws exists (Cummings et al. 1997; Webster and Starnes 2000; Lott and Whitley 2001; Webster et al. 2004; DeSimone, Markowitz, and Xu 2013), including recent evidence that CAP laws reduce the probability that minors reside in families with unsafely stored weapons (Prickett, Martin-Storey, and Crosnoe 2014), no prior studies have estimated the effect of CAP laws on youth gun carrying or gun-related school violence. In general, we know very little about the effects of gun control policies on these outcomes.[2] Studying the impact of CAP laws is appealing because (1) there is substantial state-level variation in the timing of policy adoption, (2) the laws create predictions as to which age groups should be most affected, and (3) heterogeneity in standards for criminal liability (negligent storage vs. reckless endangerment) generates predictions as to where effects should be most strongly felt.

This study begins by examining the relationship between CAP laws and gun carrying among U.S. high school students using data from the Youth Risk Behavior Surveys (YRBS) for the period 1993-2013 when 17 states and D.C. enacted CAP laws.[3] Our estimates show that CAP laws are associated with an 18.5 percent decrease in the rate at which high school students

[2] Economists have studied the crime effects of concealed-carry laws (Ludwig 1998), juvenile gun bans (Marvell 2001), right-to-carry laws (Mustard 2001; Aneja, Donohue, and Zhang 2012), and stand-your-ground laws (McClellan and Tekin 2017; Cheng and Hoekstra 2013), but none have examined the effects of gun control on school violence.

[3] Data are available for before and after a CAP law went into effect for D.C. and 13 of these 17 states.

under the age of 18 reported carrying a gun in the past month. Based on the 90 percent confidence interval around this estimate, we can rule out CAP law effects smaller than 1.9 percent in absolute value. A causal interpretation of this finding is supported by the fact that we find no evidence to suggest that CAP laws are associated with gun carrying among high school students 18 years of age and older, those students for whom the laws are less likely to bind.

Turning to outcomes related to a student's own safety, we find that CAP laws are associated with a 19 percent decrease in the rate at which students reported being threatened or injured with a weapon on school property. We also find some evidence that CAP laws are associated with decreases in the rate at which students reported having missed school in the past month due to feeling unsafe.

Finally, we assemble the first comprehensive data set of school-associated shooting deaths in the United States and examine the relationship between CAP laws and these events.[4] Our results provide little evidence that CAP laws are associated with school shooting deaths committed by minors. However, it should be noted that these estimates are insufficiently precise to rule out substantially sized effects.

2. Background

2.1. Youth Gun Violence

Highly publicized school shootings have increased awareness about youth firearm access and use, and a large literature exists on the individual-level correlates of youth gun carrying. Researchers have found that mental health (Saukkonen et al. 2016), victimization (Ruggles and Rajan 2014; Saukkonen et al. 2016), parental involvement (Vaughn et al. 2012), substance use

[4] Using data on Chicago high school students, Chandler, Levitt, and List (2011) studied the determinants of getting shot.

(Hemenway 1996; Ruggles and Rajan 2014), academic performance (Hemenway et al. 1996), and drug dealing (Vaughn et al. 2012) are all strong predictors of youth gun carrying.[5]

Teens who own guns for sport typically have parents who socialize them into gun use and are unlikely to be involved in criminal activity. On the other hand, teens who obtain guns illegally are often socialized into gun use by their peers, more likely to be criminally active, and more likely to bring guns to school (Lizotte et al. 1994; Lizotte et al. 1997). These teens may impose substantial costs on others. For example, beyond the direct costs incurred by victims and their families, gun-related school violence may have far-reaching consequences for educational attainment. Grogger (1997) found that increased levels of violence in and around schools led to lower graduation rates in the United States. Beland and Kim (2016) found that fatal shootings in U.S. high schools were associated with increased dropout rates and reduced test scores, while Abouk and Adams (2013) found evidence to suggest that school shootings induced private school enrollment. Poutvaara and Ropponen (2017) concluded that a highly publicized school shooting in Finland decreased the academic performance of students in other schools.[6]

Student gun carrying alone may create an environment that hinders academic performance. Researchers have established that school safety is correlated with test scores (Arum 2003; Lacoe 2013), classroom engagement (Ripski and Gregory 2009), absenteeism (Bryk and Thum 1989), and dropout rates (Rumberger 1995). Studies have also found that students who fear that their classmates may be carrying guns are more likely to carry themselves (Bergstein et al. 1996; Hemenway et al. 1996; Hemenway et al. 2011).

[5] See Emmert and Lizotte (2015) for a thorough discussion of the research on the demographics of youth weapon carriers and the risk factors for juvenile weapon carrying.

[6] Relatedly, Gershenson and Tekin (2017) found that the 2002 "Beltway Sniper" attacks in Washington, D.C. reduced school-level proficiency rates in schools within five miles of an attack. They concluded that traumatic community events such as mass shootings have the potential to disrupt student learning.

2.2. CAP Laws

While there are no CAP laws at the federal level, state CAP laws have been around for over 30 years. In 1981, Missouri passed the first law aimed at punishing adults who give children unsupervised access to firearms. Since 1981, 26 states and D.C. have passed a CAP law. As of 2014, 13 states were considering some form of CAP legislation (Patel 2014).

The strongest CAP laws impose criminal liability when a minor gains access to a firearm that has been stored negligently. If a child uses a firearm that was not properly locked up or stored to injure or kill a person, CAP laws penalize the gun owner with fines, imprisonment, or a combination of both. For instance, based on California's recently signed Firearm Safe and Responsible Access Act, violators risk a potential $1,000 fine and/or six months in jail (Peters 2013a). To take another example, Massachusetts imposes a minimum $5,000 fine and/or 2.5 years in jail for those who allow children unsupervised access to handguns. Within the law, owners are not required to use specific locks or storage methods and may choose from a variety of options, so long as their guns are inaccessible to children (Shaffer 1999). On the other hand, some states impose a weaker standard for criminal liability and forbid persons from "intentionally, knowingly, and/or recklessly providing some or all firearms to children" (Law Center to Prevent Gun Violence 2013). CAP laws have also been used to penalize manufacturers and dealers who fail to include safety devices with the sale of their firearms (Shaffer 1999).[7]

[7] It should also be noted that CAP laws vary along other margins. For instance, some states impose criminal liability only if a child uses or carries the firearm. CAP laws may apply to all firearms, loaded firearms, or handguns only. Some states require that firearms not only be stored, but must be done so with a locking device in place. The age at which a state defines a "minor" also varies. While most states define a minor as anyone under the age of 18, some states operate under a lower age threshold (Law Center to Prevent Gun Violence 2013). In our YRBS sample, three states that changed their CAP laws defined a "minor" more narrowly than individuals under the age of 18. Illinois, New Hampshire, and Texas classified minors at ages 13 and younger, 15 and younger, and 16 and younger, respectively. Given this information and the fact that our approach defines the treated group as students in CAP law states who are under the age of 18, we potentially capture a lower bound policy effect. However, (i) dropping these states from our estimation sample, or (ii) restricting each state's sample of students based on their own definition of a "minor", yielded a similar pattern of results.

CAP law prosecutions are most common when police are investigating other crimes, including gun violations, but also occur when police are given tips on unsafely stored firearms. For confirmed reports on individuals being charged with unsafe gun storage in a number of CAP law states, see Amaral (2014), Bell (2016), Boren (2017), Cutts and Majchrowicz (2016), "Father Charged" (2017), Harmacinski (2013), James (1996), Lopez and Goff (2014), Ly (2013), "Parents Charged" (2009, 2017), and Young (2012).[8]

The absence of state panel data on adult gun storage behavior has prevented researchers from studying the impact of gun control on the safe storage of weapons. However, recent evidence suggests that CAP laws are associated with a decrease in unsafe firearm storage behavior in the cross section. Using data from the Early Childhood Longitudinal Study for the year 2005, Prickett, Martin-Storey, and Crosnoe (2014) found that unsafe firearm storage was least likely to occur among families in states with both CAP laws and stronger firearm legislation.[9] Yet, because this evidence is cross-sectional in nature, it should be viewed as largely descriptive.

While no previous studies have focused on school-related outcomes, several have used state-level data to examine the relationship between CAP laws and gun-related deaths. Using data for the period 1979-1994, Cummings et al. (1997) found that CAP laws were associated with a decrease in accidental shooting deaths by roughly 23 percent among children 14 years of age and younger. Using data for the period 1979-1997, Webster and Starnes (2000) also examined the relationship between CAP laws and accidental shooting deaths among children 14

[8] Perhaps understandably, there is some reticence on the part of prosecutors to charge parents with violations of CAP laws if their child completed suicide or an accidental death occurred (Peters 2013b; Lithwick 2015).

[9] Relatedly, Cook and Ludwig (2004) found that the prevalence of gun ownership in a community predicted gun carrying among adolescent males.

years of age and younger. They found a negative association between CAP laws and accidental firearm deaths, but this association was entirely driven by one state (Florida). Webster et al. (2004) found that CAP laws were associated with an 11 percent decrease in the gun-related suicide rate among 14- to 17-year-olds for the period 1976-2001. These authors also found that CAP laws were associated with a similar decrease in the gun-related suicide rate among 18- to 20-year-olds, raising the possibility that their findings for the younger age group were spurious. Using data for the period 1979-1996, Lott and Whitley (2001) found little evidence to suggest that CAP laws were associated with accidental gun deaths or suicides among teens. On the other hand, they found that CAP laws were consistently associated with more rapes, robberies, and burglaries. Lott (2003) analyzed data for the period 1977-1998 and found results similar to those reported in Lott and Whitley (2001). However, Pepper (2005) showed that the estimates in Lott (2003) are sensitive to model specification.[10]

Most recently, DeSimone, Markowitz, and Xu (2013) used annual hospital discharge data for the period 1988-2003 to estimate the relationship between CAP laws and nonfatal gun injuries. They found that CAP laws were associated with 26 and 5 percent decreases in self-inflicted and non-self-inflicted gun injuries, respectively, among individuals under the age of 18. Supporting a causal interpretation, they found no effects on self-inflicted gun injuries among adults or on self-inflicted injuries without a gun.

Our study contributes to the above literature by being the first to examine the effect of CAP laws on youth gun carrying and school violence. We also compile the first census of school-associated shooting deaths in the United States over the last two decades and explore the relationship between CAP laws and these events.

[10] For a critical review of the research on state gun laws, see National Research Council (2005).

3. YRBS Analysis

3.1. YRBS Data

The data for our primary analysis come from the national and state YRBS and cover the period 1993-2013. Government agencies rely on these data to track trends in teen behaviors including physical activity, unhealthy eating, tobacco use, alcohol and other drug use, sexual activity, and violence. Previous studies such as Simon, Crosby, and Dahlberg (1999), Dinkes et al. (2009), and Sabia and Bass (2017) have used these data to examine determinants of weapon carrying and student victimization on school grounds.[11]

The national YRBS is carried out biennially by the Centers for Disease Control and Prevention (CDC) and is representative of high school students in the United States.[12] We obtained the restricted-use versions of the national YRBS so respondents could be linked to their state of residence. The state surveys, which are also biennial and school-based, are coordinated by the CDC, administered by state education and health agencies, and mirror the national surveys in terms of content.[13] Roughly half of the states have granted the CDC permission to release their data, while the remaining states require direct requests. For both the national and state YRBS, the final stage of sampling selects classes within schools, where all students in sampled classes are eligible to participate (Centers for Disease Control and Prevention 2013).

[11] Researchers have used these data to study a wide of a range of state policies. For examples, see Cawley, Meyerhoefer, and Newhouse (2007), Carpenter and Stehr (2008), Anderson (2010), Hansen, Rees, and Sabia (2013), and Anderson, Hansen, and Rees (2015).

[12] Although designed to be nationally representative, not all 50 states contributed data to the national YRBS in any given survey wave.

[13] With a few exceptions, most states conducted their own YRBS at some point between 1993 and 2013.

For the national and majority of state surveys, trained data collectors travel to each participating school to administer the questionnaires.[14] The data-collection procedures are designed to protect respondent privacy by preserving anonymity and allowing voluntary participation. Students complete the self-administered questionnaire during one class period and record their answers in a computer-scannable booklet. When possible, students' desks are spread throughout the classroom to decrease the likelihood they can see each other's responses. Students are also encouraged to cover their answers with an extra sheet of paper provided by the survey administrator as they complete the questionnaire. When finished with the survey, students seal their answer sheet in an envelope and place it in a box. For further details on the YRBS data-collection protocols, see Centers for Disease Control and Prevention (2013).[15]

Following previous studies, we combine the national and state YRBS data so that identification comes from as many law changes as possible (Sabia, Pitts, and Argys 2014; Anderson and Elsea 2015; Anderson, Hansen, and Rees 2015; Hansen, Sabia, and Rees 2017). Between 1993 and 2013, 12 states (CO, DE, GA, IL, MA, MN, MS, NC, NV, TN, TX, and UT) and D.C. contributed data to the national YRBS before and after the adoption of a CAP law and 12 states (CO, DE, GA, IL, MA, MS, NC, NH, NV, TN, TX, and UT) contributed data to the state YRBS before and after the adoption of a CAP law. When the state and national YRBS are combined, we observe pre- and post-treatment data for 13 states and D.C (see Table 1).[16] Six of these states and D.C. impose criminal liability for negligent storage, while seven states impose

[14] In some states, the questionnaires are sent directly to schools and teachers administer the survey by following a standardized script (Centers for Disease Control and Prevention 2013).

[15] Research on survey setting conducted by the CDC found that students are more likely to report health-risk behaviors when answering questions at school rather than at home (Brener et al. 2006).

[16] Online Appendix Table AO1 shows the number of observations in our sample for each state-year cell.

criminal liability for reckless endangerment. The YRBS sample contains over one million observations and includes individuals from all 50 states and D.C.

We measure youth gun carrying using responses to the following questionnaire item:

> *"During the past 30 days, on how many days did you carry a gun?"*

The variable *Carry Gun* is set equal to 1 if the respondent reported carrying a gun at least once in the past 30 days, and equal to 0 otherwise.[17]

Respondents are then asked about weapons carrying, both overall and on school property, which we use to generate two separate indicators:

> *"During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club?"*
>
> *"During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club on school property?"*

Carry Any Weapon (*at School*) is equal to 1 if the respondent reported carrying a weapon (on school property) at least once in the past 30 days, and equal to 0 otherwise. The obvious disadvantage of these two measures is that we cannot separate out gun carrying effects of CAP laws from knife or club carrying effects. Thus, we can only observe the *total* effect of CAP laws on weapon carrying and are unable to examine whether knives or other weapons are complements to or substitutes for guns. However, a comparison of the estimated effect of CAP laws on *Carry Gun* and *Carry Any Weapon* will provide at least some evidence as to whether substitution across weapons exists.

Students are also asked if they faced a weapon-related threat or injury on school property:

> *"During the past 12 months, how many times has someone threatened or injured you with a weapon such as a gun, knife, or club on school property?"*

[17] We also examined the intensive margin of gun carrying, as well as the intensive margin for outcomes for which we have measures of frequency. These results, which are available upon request, suggest that CAP law effects tend to be largest on the extensive margin.

Weapon Threat at School is equal to 1 if the respondent reported being threatened or injured at least once in the past 12 months, and equal to 0 otherwise. Finally, respondents are asked:

> *"During the past 30 days, on how many days did you not go to school because you felt you would be unsafe at school or on your way to or from school?"*

Missed School Due to Safety is equal to 1 if respondents reported missing school at least once in the last 30 days, and equal to 0 otherwise.[18]

Table 2 provides descriptive statistics and definitions for the YRBS data. Means are reported by whether a CAP law was in place during the year of the survey. According to the YRBS data, 5.5 percent of high school students carried a gun at least once in the past 30 days, 17.6 percent carried a weapon (that is, a gun, knife, or club) in the past 30 days, 6.0 percent carried a weapon on school property in the past 30 days, 7.1 percent were threatened or injured with a weapon on school property at least once in the past year, and 5.3 percent missed school due to feeling unsafe in the past 30 days.[19] An advantage of the first three outcomes (*Carry Gun*, *Carry Any Weapon*, and *Carry Any Weapon at School*) is that there are clear predictions as to

[18] It is worth noting that the wording of the questions for our outcomes of interest is identical across the national and state YRBS. The number of observations are smaller for some of the outcomes, including *Carry Gun* and *Carry Any Weapon*, because these items are not available in a number of state YRBS surveys. The question for *Carry Gun* was not asked in CT (2007- 2011), ID (2001, 2003), ME (2009- 2013), NE (2013), NV (2003-2011), NH (2009-2013), NC (2003, 2009), ND (1999-2013), OH (2011), RI (2007-2011), SD (2005-2013), UT (2001, 2009, 2011), VT (1993-2001, 2005-2011), and WI (2013). The question for *Carry Any Weapon* was not available in CT (2011), ID (2003), ME (2009-2013), NE (2013), NH (2009, 2013), ND (1999-2013), RI (2013), SD (2001, 2005-2013), and VT (1997-2001, 2005-2011).

[19] The non-response rate for the outcomes of interest are as follows: 1.5 percent for *Carry Gun*, 1.8 percent for *Carry Any Weapon*, 1.1 percent for *Carry Any Weapon at School*, 0.3 percent for *Weapon Threat at School*, and 0.4 percent for *Missed School Due to Safety*. In addition, the non-response rates are similar across the national and state YRBS. A concern is whether students underreport and, in particular, if this measurement error is correlated with CAP laws. While we cannot directly test underreporting, we can test whether non-response is correlated with CAP laws. In models where we regressed the probability of non-response for each of our outcomes of interest on a CAP law indicator, we found no evidence to suggest that these laws predict survey non-response. This, to an extent, quells concerns that students in CAP law states are more likely to lie when asked about gun carrying-related questions in the YRBS. These results are available from the authors upon request. For general information on data quality in the YRBS, see Centers for Disease Control and Prevention (2013).

which age groups should be most influenced by CAP laws. The laws are less binding for students 18 years of age and older than for students under the age of 18. However, because some of these older students live with younger individuals, there may be spillover effects. It is less clear that CAP laws should impact the latter two outcomes (*Weapon Threat at School* and *Missed School Due to Safety*) differently across the two age groups.

Figures 1 and 2 show trends in our dependent variables for the national and state YRBS, respectively. These figures illustrate that the national and state YRBS are each capturing the same broad changes in our outcomes over time. During the 1990s, rates of weapon carrying declined substantially; at the same time, safety-related absences and weapons-related threats at school rose. After 2001, the rates for all of our outcomes of interest remained steady.

3.2. YRBS Empirical Strategy

Our empirical analysis is based on the approach taken by previous researchers interested in the effects of gun laws.[20] Specifically, to estimate the relationship between CAP laws and high school student outcomes, we exploit the spatial and temporal variation of these laws in a difference-in-difference framework. Our estimating equation is:

$$(1) \qquad Y_{ist} = \beta_0 + \beta_1 CAP\ Law_{st} + \boldsymbol{X1'_{ist}\beta_2} + \boldsymbol{X2'_{st}\beta_3} + \nu_s + w_t + \nu_s \cdot t + \varepsilon_{ist},$$

where i indexes individuals, s indexes states, and t indexes years. The dependent variable, Y_{ist}, represents one of the five possible outcomes listed in Table 2 (*Carry Gun*, *Carry Any Weapon*,

[20] For examples, see Ludwig (1998), Marvell (2001), Mustard (2001), Cheng and Hoekstra (2013), DeSimone, Markowitz, and Xu (2013), and McClellan and Tekin (2017).

Carry Any Weapon at School, *Weapon Threat at School*, *Missed School Due to Safety*).[21] Because these outcomes are binary, we estimate equation (1) as a probit model where the errors are assumed to be distributed normally. The variable of interest, $CAP\ Law_{st}$, is an indicator for whether state *s* was enforcing a CAP law during year *t*.[22] In alternative specifications, we allow the type of CAP law to vary by whether the state enforces a negligent storage or reckless endangerment criminal liability standard. The vectors v_s and w_t represent state fixed effects and year fixed effects, respectively, and state-specific linear time trends are denoted by $v_s \cdot t$. The state-specific linear time trends are included to control for state-level factors that evolve smoothly over time, such as sentiment towards gun control. An advantage of using the combined national and state YRBS data set is that it often contains thousands of individuals per state-year. If we observed smaller state-year cell sizes, a concern would be the ability to obtain consistent parameter estimates from a nonlinear model, such as the probit, with fixed effects. Neyman and Scott (1948) showed in their seminal work that estimation of nonlinear models with limited data can result in the well-known incidental parameters problem.

The vector $\boldsymbol{X1}_{ist}$ includes individual-level controls for race, age, grade, and gender, while $\boldsymbol{X2}_{st}$ includes state-level controls for demographics (*% Nonwhite*, *Mean Age*, *% Male*), policing resources (*Police Expenditures*, *Police Employment*), the adult crime rate (*Property Crime*, *Violent Crime*), political preferences (*Democrat*)[23], whether the state mandates insurance

[21] The YRBS defines a weapon as an object such as a "gun, knife, or club." Ideally, we would like to only observe gun carrying or gun threats on school property. However, if CAP laws restrict gun access among teens and other weapons such as knives or clubs serve as substitutes for firearms, then our estimates based on the outcomes *Carry Any Weapon*, *Carry Any Weapon at School*, and *Weapon Threat at School* will be conservative. We address this issue further in section 3.3 below.

[22] This variable takes on fractional values during the year in which a CAP law took effect.

[23] Previous studies suggest a correlation exists between political ideology and crime (Wright et al. 2017). We define the variable *Democrat* as equal to 1 if state *s* had a democratic governor in year *t*, and equal to 0 otherwise.

coverage to include mental health benefits at parity with physical health benefits (*Mental Health Parity Law*), alcohol policies (*Beer Tax, ZT Drunk Driving Law*)[24], economic conditions (*Unemployment, Per Capita Income*), education levels and school environment (*% Bachelor's Degree, Student-Teacher Ratio, Teacher Salary, School Lunch Program, ZT School Violence Law*[25], *Anti-Bullying Law*), and other gun laws (*Shall Issue Law, Stand Your Ground Law, Background Check Law*[26], *Minimum Possession Age Law, Trigger Lock Law, Gun Buyback Program*). Table 2 provides means and definitions for the variables included in $\boldsymbol{X1_{ist}}$ and $\boldsymbol{X2_{st}}$. Online Appendix Table AO2 lists the data sources for the state-level covariates.[27]

All regressions are estimated such that the standard errors are corrected for clustering at the state level (Bertrand, Duflo, and Mullainathan 2004).[28] To ensure the combined YRBS data

[24] Researchers have relied on beer taxes to proxy variations in the price of alcohol (Ruhm 1996; Markowitz, Kaestner, and Grossman 2005). In addition, zero-tolerance drunk driving laws have been found to affect alcohol consumption of minor teens (Carpenter 2004)

[25] Zero-tolerance school violence laws severely punish offenses such as weapon carrying (for example, one-year expulsions).

[26] Federal law requires federally licensed dealers to perform background checks on all firearm sales, but it does not apply to private sellers. Eighteen states and D.C. have extended the federal law to cover at least some private sales. Eight states (CA, CO, CT, DE, NY, OR, RI, and WA) and D.C. require universal background checks at the point of sale for all classes of firearms; two states (MD and PA) require universal background checks at the point of sale for handguns; four states (HI, IL, MA, and NJ) require a state license or permit to purchase any firearm; four states (IA, MI, NE, and NC) require a state license or permit to purchase a handgun (Law Center to Prevent Gun Violence 2016a).

[27] We also experimented with controlling for state-level measures of the stock of guns, the prevalence of metal detectors in schools, and anti-gun sentiment. The stock of guns in a state is proxied by the number of annual firearm background checks (Lang 2013a), while data on metal detector prevalence and anti-gun sentiment come from the National Center for Education Statistics' School Survey on Crime and Safety and the General Social Survey, respectively. In general, the results presented below were robust to controlling for these three variables and are available from the authors upon request. We opted to not include them in our preferred set of covariates due to missing data issues. For instance, because information on the number of firearm background checks is only available going back to 1999, we were forced to linearly impute the data for 1993, 1995, and 1997. Similar interpolation was used for the measure of school metal detector prevalence (available after 1999) and anti-gun sentiment (available in 1991, 1993, and every even year thereafter).

[28] We also estimated equation (1) as a linear probability model, as statistical inference with OLS is based on minimal assumptions regarding the error correlation process. These estimates were generally similar to, and often larger in magnitude than, the marginal effects produced by probits.

are nationally representative, we used population data from the National Cancer Institute's Surveillance Epidemiology and End Results (SEER) Program (http://seer.cancer.gov/popdata/) and assigned population weights to each respondent based on state of residence, age, gender, and race (Hansen, Sabia, and Rees 2017; Anderson and Elsea 2015, Anderson, Hansen, and Rees 2015).

Beyond controlling for $\boldsymbol{X1_{ist}}$ and $\boldsymbol{X2_{st}}$, in order for equation (1) to generate unbiased estimates of the effect of CAP laws on gun carrying and school violence, the parallel trends assumption must be satisfied. We take three approaches to test this assumption: (1) examine whether effects are stronger for students under the age of 18, for whom CAP laws are more likely to bind, as compared to students 18 years of age and older, as well as experiment with formal difference-in-difference-in-difference models; (2) conduct placebo tests on CAP law leads, including tests for whether salient violent events predict the adoption of CAP laws; (3) provide falsification tests on behaviors that should be unaffected by CAP laws.

3.3. YRBS Results

Table 3 presents the main results from the YRBS analysis. Because CAP laws specifically target households with children who are minors, we present results based on an age 18 cutoff. Panel I shows results for students under the age of 18, while panel II shows results for students 18 years of age and older. While the results in panel II do not represent a perfect falsification test (because high school students 18 years of age and older may live in households with younger siblings or parents' gun storage behaviors may occur with a lag), we expect CAP laws to bind less for this age group.[29]

[29] For example, according to wave 1 of the National Longitudinal Study of Adolescent Health, 50.4 percent of 18-year-olds surveyed reported having a younger sibling.

For students under the age of 18 (panel I), CAP laws are associated with a .010 decrease in the probability a high school student reported carrying a gun within the past 30 days. This estimate is statistically significant at the 10 percent level and reflects an 18.5 (=.010/.054) percent decrease relative to the mean rate of gun carrying among students under the age of 18.[30] The 90 percent confidence interval around this estimate is [-.018, -.001], meaning that we can rule out CAP law effects smaller than 1.9 (=.001/.054) percent in absolute value. CAP laws are also associated with a .020 decrease in the probability a high school student reported carrying any weapon (that is, a gun, knife, or a club) within the past 30 days, an 11 percent decrease relative to the mean. Based on the 90 percent confidence interval around this estimate, we can rule out CAP law effects smaller than 0.6 (=.001/.175) percent in absolute value. While tests of differences in these coefficients cannot rule out substitution across types of weapons, the results suggest that CAP laws are effective at reducing net weapons carrying, at least across the range of weapons examined in the YRBS.[31] CAP laws are also negatively associated with high school students having reported carrying a weapon specifically on school property, but this estimate is not statistically significant.

Regarding student safety, we find that CAP laws are associated with a .014 decrease in the probability a student reported being threatened or injured with a weapon on school property within the past year. This represents roughly a 19 percent decrease relative to the mean. We also find that CAP laws are associated with a (statistically insignificant) .008 decrease in the probability a student reported missing school within the past 30 days because he/she felt unsafe.

[30] Online Appendix Table AO3 presents means for the outcomes by age.

[31] Based on the outcomes *Carry Gun* and *Carry Any Weapon*, we created a variable equal to one if the student carried a weapon in the past 30 days, exclusive of guns, and equal to zero otherwise. When we regressed this measure on *CAP Law* (and the full set of controls), the relationship was negative, but statistically insignificant at conventional levels.

Panel II in Table 3 illustrates that CAP laws have no impact on the weapon carrying behavior or safety of students 18 years of age and older.[32] The across-the-board null findings provide confidence that our estimates in panel I are not spurious and potentially reflect a causal relationship between CAP laws and gun-related outcomes among high school students.[33] Formal difference-in-difference-in-difference (DDD) estimates, shown in panel III of Table 3, also suggest that our findings are not contaminated by unmeasured school- or state-level policies.[34] An advantage of the DDD model is that it allows us to subtract out any confounding effect of unobserved state or school policies aimed at, for instance, decreasing gun carrying among teenagers and young adults in general, including the introduction of metal detectors in schools or non-minor targeted state gun laws. Given the flexibility of this specification, it could be argued that these estimates are preferred to those from the DD models. The results in panel III indicate

[32] Based on our own calculations using the U.S. Census Bureau's American Community Survey, 41 and 7 percent of 18- and 19-year-olds, respectively, attended high school in the United States in 2013. If individuals 18 years of age or older in CAP law states have systematically different rates of school attendance and gun carrying than similarly aged individuals in non-CAP law states, then estimates for this age group could be biased. In 2013, 24.8 and 23.5 percent of 18- and 19-year-olds in CAP law and non-CAP law states, respectively, attended high school.

[33] We also explored whether the relationship between CAP laws and our outcomes of interest depends on gender or race. Across all outcomes, we failed to reject the hypothesis that CAP laws were more effective for male as compared to female students or white as compared to black students. In addition, we split the sample along several risky behaviors and tested the "selective recruitment" hypothesis; that is, that students who are most likely to carry guns are those least likely to be influenced by the law (for examples, see Dee (1998), Carpenter and Stehr (2008), and Anderson (2010)). We found that the CAP law effects for the weapon-carrying outcomes are isolated among students who reported past month substance use and drinking and driving. One possible explanation for these results is that CAP laws particularly influence the behavior of parents who believe their children have a propensity for seeking access to firearms. Lastly, given interest on the link between mental health and gun violence, we also split our sample based on recent suicide ideation (Konnikova 2014; Said 2015). For our three measures of weapon carrying, we failed to reject the hypothesis of equal CAP law effects across the two samples.

[34] The difference-in-difference-in-difference estimates are based on a pooled sample, where the coefficient of interest represents the effect of CAP laws on students under the age of 18 relative to students 18 years of age and older (that is, an interaction between the CAP law variable and an under-age-18 indicator). We note, however, that this empirical strategy is more appropriate for the outcomes related to gun and weapon carrying and less so for the outcomes related to a safe school environment. This is because students over the age cutoff may be as likely to be threatened with a weapon on school property or miss school for fear of their safety. Moreover, if there are spillover effects to students 18 years of age and older via younger siblings or lagged changes in parental behavior, we would expect the DDD estimates to be conservative.

that CAP laws are negatively associated with all of our outcomes of interest, and negatively and statistically significantly associated with the *Carry Gun, Weapon Threat at School*, and *Missed School Due to Safety* outcomes.

In Table 4, we replace *CAP Law* with two mutually exclusive indicators, *Negligent Storage* and *Reckless Endangerment*, to examine whether heterogeneous effects by the type of CAP law in place exist. As discussed above, negligent storage laws are the strongest form of CAP legislation and impose criminal liability when a minor gains access to a negligently stored firearm. On the other hand, some states impose a weaker standard for criminal liability and prohibit persons from "intentionally, knowingly, and/or recklessly providing some or all firearms to children" (Law Center to Prevent Gun Violence 2013). The results in Table 4 are consistent with the notion that negligent storage laws are generally more effective than reckless endangerment laws when it comes to reducing gun carrying among high school students and promoting a safer school environment. Again, we see that the effects are concentrated among students under the age of 18.[35]

3.4. Sensitivity and Robustness Checks

In Table 5, we explore whether the *CAP Law* estimates presented in Table 3 (panel III) are sensitive to the chosen set of controls. A concern is that some of the variables in $\boldsymbol{X2_{st}}$ could be potentially endogenous (for example, the other gun laws). We begin Table 5 by showing

[35] A caveat to these results is that identification for the *Negligent Storage* and *Reckless Endangerment* indicators are coming from only 6 (and D.C.) and 7 states, respectively. Due to the relatively small number of clusters, we also experimented with using the score bootstrap method suggested by Kline and Santos (2012). Online Appendix Table AO4 reports the results from Tables 2 and 3 that are based on standard clustering at the state level but includes a row of p-values calculated from the score bootstrap procedure for comparison. The score bootstrap is an adaptation of the wild bootstrap and allows for extension to nonlinear models, such as the probit. See Cameron, Gelbach, and Miller (2008) and Cameron and Miller (2015) for discussions of the wild cluster bootstrap method.

estimates from models without any of the individual- or state-level covariates listed in Table 2, and then sequentially add common groupings of these time-varying controls.[36] Our estimates are generally stable across specifications. In Table 6, we repeat this exercise to examine the sensitivity of the *Negligent Storage* estimates from Table 4 (panel III). Online Appendix Tables AO5 and AO6 show estimates separately for students under age 18 and students 18 years of age or older, respectively. Again, the results show that estimated CAP law effects are generally insensitive to the choice of controls.

In addition to the DDD model, we perform two robustness checks in Tables 7 and 8 that are designed to explore whether our CAP law effects could be contaminated by school- or state-specific shocks that impact minor teens, but not individuals 18 years of age or older.

Table 7 presents results based on regressions where we replace *CAP Law* with an indicator *Year of Law Change*, 2 leads of this indicator, and 2 lags. *Year of Law Change* is equal to 1 the year in which a CAP law went into effect and is equal to 0 otherwise.[37] The primary purpose of this exercise is to test whether any of the outcomes were trending in the years prior to the law change. Consistent with the parallel trends assumption, there is little evidence to suggest that our outcomes of interest were trending in a systematic fashion leading up to the passage of CAP laws. We also see that there is a lagged policy effect, suggesting the impact is felt in the years after the law is implemented rather than immediately. This appears to especially be the case for outcomes related to a student's own safety. Comparable analyses using leads and lags

[36] The last two rows of Table 5 show results with and without controlling for the other gun laws. For the full sample of YRBS respondents, we found little consistent evidence that any of the other gun-related policies were effective at decreasing gun or weapon carrying among high school students. While these estimates are not reported for the sake of brevity, they are available from the authors upon request.

[37] This variable takes on fractional values during the year in which a CAP law took effect.

of the *Negligent Storage* indicator produce a similar pattern of results (Online Appendix Table AO7).

In Table 8, we conduct falsification tests on behaviors for which we would expect no causal effect of CAP laws. Specifically, we consider binary outcomes for the following risky behaviors: any cigarette use in the past month, any illicit drug use in the past month, any binge drinking (that is, 5 or more drinks in one sitting) in the past month, any drunk driving in the past month, whether the respondent "never" or "rarely" wears a seat belt, and multiple sex partners in the past 3 months. If CAP laws were found to be negatively associated with these outcomes for students under the age of 18, it could suggest that estimates produced by equation (1) are spurious. However, the findings in Table 8 suggest no evidence of a statistically significant association between any of these behaviors and CAP laws.[38] These results provide further support for the hypothesis that the parallel trends assumption is satisfied.

3.5. Do School Shootings or Violent Crime Rates Predict CAP Laws?

In Table 9, we explore whether salient events, such as school shooting fatalities or violent crime rates, predict the passage of CAP laws. If these laws are enacted in response to shocks that affect minor (but not non-minor) high school students, then the above estimates may be biased. The first column in panel I of Table 9 illustrates results where the enactment of a CAP law is regressed on whether a school-associated shooting death occurred in state *s* during year *t* (see Section 4 below for a detailed description of the school shooting data set). The second column in

[38] Online Appendix Table AO8 shows results for this exercise based on the *Negligent Storage* indicator. We also found that CAP laws were not associated with measures of helmet use, exercise, or diet pill use/eating disorders.

panel I includes three lags of this school shooting indicator. In either set of results, there is little evidence to support the notion that school shooting deaths predict the passage of CAP laws.

In panels II and III of Table 9, we explore whether the count of school-associated shooting deaths and the violent crime arrest rate for minors, respectively, are associated with CAP laws.[39] The results suggest that these measures do not predict CAP laws going into effect.

4. School Shooting Analysis

The estimates above indicate that CAP laws play an important role in decreasing the likelihood that high school students report past-month gun carrying and past-month weapon carrying on school property. Our results also suggest that CAP laws decrease the likelihood that students report being threated or injured with a weapon on school property and miss school for fear of their safety. Here, we test whether the CAP law effect extends to school shootings.

4.1. School Shooting Data

To our knowledge, this study is the first to compile a comprehensive account of school-associated shooting deaths in the United States during the period of interest. Our primary data source is the National School Safety Center's (NSSC) report on School Associated Violent Deaths and covers the period 1992 through 2010.[40] To achieve as nearly complete coverage of

[39] The sample size for the violent crime regressions is smaller because not all states reported to the UCR each year during the period 1991-2013.

[40] The NSSC (2010) report, which is based on newspaper accounts, can be found at: http://www.schoolsafety.us/media-resources/school-associated-violent-deaths. The NSSC defines a school-associated violent death as

> "...any homicide, suicide, or weapons-related violent death in the United States in which the fatal injury occurred: on the property of a functioning public, private or parochial elementary or secondary school, Kindergarten through grade 12 (including alternative schools); on the way to or from regular sessions at such a school; while person was attending or was on the way to or from

these events as possible, we called upon the following additional data sources to include events missing from the NSSC's (2010) report: Lieberman (2008), National School Safety and Security Services (2010), Van Fleet and Van Fleet (2010), Klein (2012), Stoptheshootings.org (2013), Stafford and Associates (2014), Columbine-angels.com (2015), Doll (2015), Everytown.org (2015b), and Laurine (2017). These sources, in addition to our own searches of newspaper archives, allowed us to extend our coverage from 1991 to 2013. A benefit of this data set is that it represents a balanced panel and covers a longer period of time than the YRBS data, allowing us to capture additional policy variation.[41]

For the analysis below, we restrict our focus to school shootings where a death occurred (homicide, suicide, or accidental).[42] Our final data set includes information on when and where the shooting took place, the age of the shooter, and (when available) whether the shooting was reported as gang-related. We define a school shooting as an event that takes place on school property. This includes shootings on school buses and in areas outside of the main building, such as school parking lots and athletic fields. Table 10 presents descriptive statistics and definitions for our outcome measures. For our sample, we identify a total of 167 school-associated shooting deaths where the shooter was under the age of 18 and 187 deaths where the

an official school-sponsored event; as obvious direct result of school incidents, functions or activities, whether on or off school bus/vehicle or school property" (NSSC 2010).

[41] Specifically, we observe pre- and post-treatment data for 23 states (CA, CO, DE, GA, HI, IL, IN, KY, MD, MA, MN, MS, NV, NH, NJ, NC, OK, RI, TN, TX, UT, VA, and WI) and D.C.

[42] To ensure accuracy, we double-checked each observation with at least one other information source. For instance, if a shooting event was initially recorded from the NSSC's (2010) report, we checked it against at least one of the other sources listed above. If the event was not found in one of our other sources, we searched online newspaper archives for confirmation. Of the 354 school-associated shooting deaths used in our analysis, we found at least two sources for all but 3 observations. Our results were similar when dropping these 3 observations. While it is difficult to say with certainty that our data set captures every school-associated shooting death during the period under study, to our knowledge, it represents the most comprehensive list to date.

shooter was 18 years of age or older. We observe 76 school shootings involving more than one death and 12 involving four or more deaths.[43]

4.2. School Shooting Empirical Strategy

To explore the relationship between CAP laws and school-associated shooting deaths, we generate a state-by-year panel from the data set described above and estimate the following difference-in-difference model:

$$(2) \qquad Y_{st} = \beta_0 + \beta_1 CAP\ Law_{st} + \boldsymbol{X_{st}\beta_2} + v_s + w_t + v_s \cdot t + \varepsilon_{st},$$

where *s* indexes states and *t* indexes years. The binary dependent variable, Y_{st}, indicates whether there was a school shooting death in state *s* during year *t*, defined as one of the six possible school shooting outcomes listed in Table 10. The variable of interest, $CAP\ Law_{st}$, is defined as above and v_s, w_t, and $v_s \cdot t$ represent state fixed, year fixed effects, and state-specific linear time trends, respectively. The vector $\boldsymbol{X_{st}}$ includes the same state-level controls used in equation (1).[44] Similar to equation (1), the parallel trends assumption must be satisfied in order to generate unbiased estimates of β_1 in equation (2). All regressions are estimated as linear probability

[43] A "mass" murder is generally defined as four or more murders occurring during the same incident, with no distinctive period between the murders. For research and press coverage on mass school shootings in the United States, see Lankford (2015), Los Angeles Times Staff (2016), and Willingham (2016). Other research has focused on "rampage" school shootings, defined as "expressive non-targeted attacks on a school institution" (Muschert 2007). For qualitative studies on rampage school shootings, see Larkin (2009) and Newman and Fox (2009).

[44] Descriptive statistics for the state-level controls are provided in Online Appendix Table AO9.

models and are weighted by the population of state *s* in year *t*.[45] Standard errors are corrected for clustering at the state level (Bertrand, Duflo, and Mullainathan 2004).

4.3. School Shooting Results

Table 11 presents estimates of β_1 from equation (2). Panel I shows results based on events where the shooter was under the age of 18 and panel II shows results based on events where the shooter was 18 years of age or older. We disaggregate all death-related school shooting events (first column) into those involving a suicide (second column) and a homicide (third column). In general, we find no statistically significant evidence to suggest that CAP laws are associated with fewer school-associated shooting deaths. However, we note that these estimates are sufficiently imprecise to conclusively rule out non-trivial effects of CAP laws. The 90 percent confidence interval associated with the relationship between CAP laws and homicides committed by shooters under the age of 18 is [-.168, .180].[46] With many explanatory variables and a relatively small number of events, the ability to detect an effect is difficult.

We next subjected the null findings to a number of sensitivity checks. First, because our school shooting data set includes some gang-related events, we focused on shootings where there was no mention of gang involvement.[47] Non-gang-related shootings are more often considered

[45] To retain sample size, we opted to use a linear probability model because the state fixed effects perfectly predicted the outcome for states with no school-associated shooting deaths. Probit models did, however, yield similar results. In addition, we explored Poisson and negative binomial models to take advantage of the full count of school shooting events involving a fatality. Approximately 21 and 23 percent of state-year observations included a fatal school shooting event committed by minors and non-minors, respectively. Results from these specifications were qualitatively similar to those presented below and are available from the authors upon request.

[46] Of the 6 other gun-related policies that we include as controls, the coefficient estimates on *Shall Issue Law*, *Trigger Lock Law*, and *Stand Your Ground Law* were consistently negative in sign, but never statistically distinguishable from zero.

[47] We were able to link 27 of the school-associated shooting deaths to gang involvement.

"random acts of violence" and are less likely to be related to the community's underlying trend of violent crime (Midlarsky and Klain 2005). We found no evidence to suggest that CAP laws are effective at reducing the likelihood of these events. Second, we replaced *CAP Law* with an indicator for the year of the law change and a series of leads and lags. Unlike the YBRS results in Table 7, we found no evidence of a lagged CAP law effect.[48] Third, we examined whether there were heterogeneous effects by the type of law in place. Our results suggested that neither negligent storage nor reckless endangerment CAP laws are associated with fewer school shooting deaths.[49]

In sum, while CAP laws appear to decrease gun carrying among high school students and generally promote a safer school environment, they do not have an observable impact on school-associated shooting deaths.

5. Conclusion

The National Poll on Children's Health recently indicated that 1 in 4 parents are "very concerned" about school violence for their children.[50] These fears are perhaps driven by the fact that school shootings have been reported at nearly a weekly rate since 2012 (Everytown.org 2015b). While there is a wealth of research on the individual-level correlates of youth gun

[48] These results are reported in Online Appendix Table AO10.

[49] For the sake of brevity, we have omitted the results on non-gang related shootings and negligent storage versus reckless endangerment laws. Because some states define a minor based on an age threshold lower than 18, we also experimented with restricting each state's sample of shootings based on their own definition of a "minor." Under this scenario, we found little evidence to suggest that CAP laws are associated with fewer school-associated shooting deaths. Lastly, the results for shooters ages 18 and older are similar if we restrict this age range to 18- to 21-year-olds or 18- to 24-year-olds, or if we focus on older shooters (that is, ages 21 and older or 24 and older). All of these results are available upon request.

[50] Results from the poll are available at: https://mottpoll.org/sites/default/files/documents/082117_TopParentConcerns.pdf.

carrying (Emmert and Lizotte 2015), we know very little about whether specific policies may be leveraged to curb this behavior.

This paper draws on data from two sources to examine the effects of child access prevention laws. Using data from the Youth Risk Behavior Surveys for the period 1993-2013, we find that CAP laws are associated with substantial decreases in rates of gun carrying among high school students, and these results are primarily driven by states with stricter forms of enforcement. We also find that CAP laws are associated with fewer reports of being threatened or injured with a weapon on school property and school absences due to feeling unsafe. From an education perspective, these results are vital as school climate is a well-known predictor of academic success.

Finally, to supplement our YRBS analysis, we explore the relationship between CAP laws and school-associated shooting deaths. Using a novel data set that covers the period 1991-2013, we find no statistically significant evidence that CAP laws reduce school-associated shooting deaths. However, because these estimates are imprecise, we cannot rule out beneficial (or adverse) effects of CAP laws on school shooting deaths. Future research examining the effectiveness of other gun and anti-school violence policies will be critical to curbing these tragic and costly events.

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Figure 1. Outcomes in the National YRBS

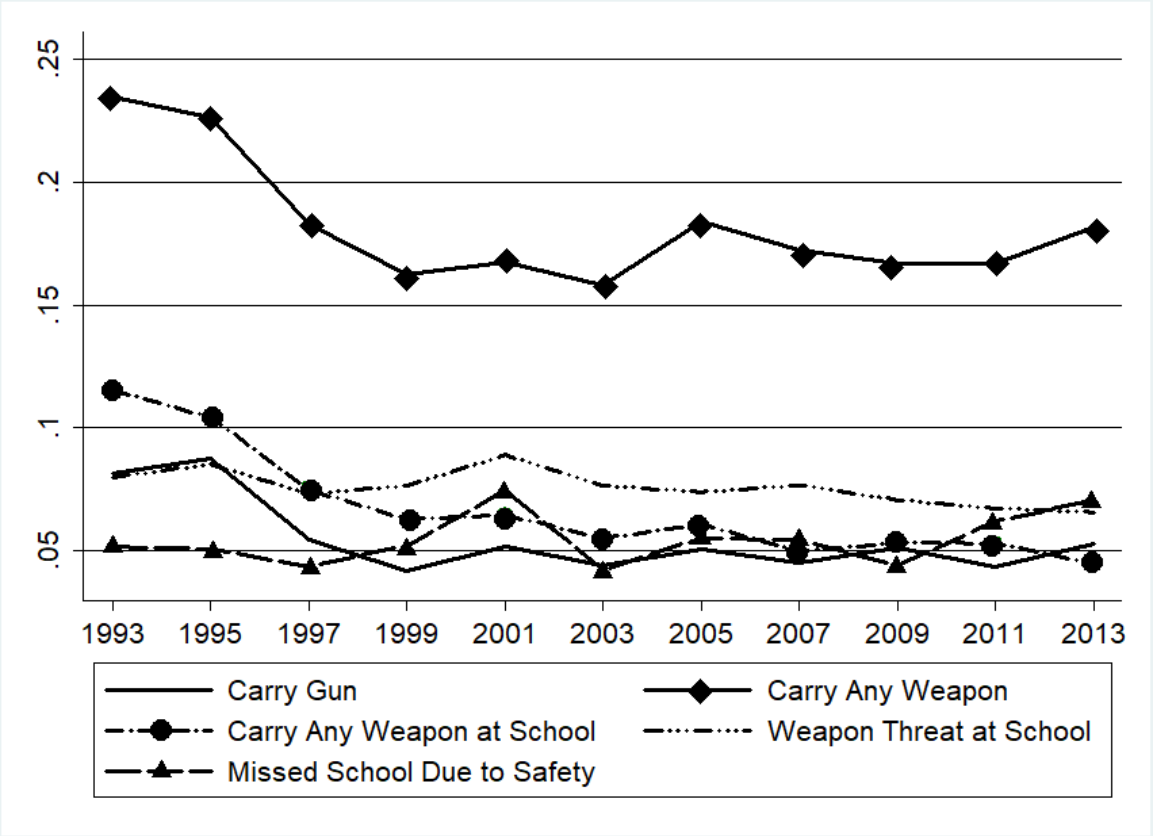

Figure 2. Outcomes in the State YRBS

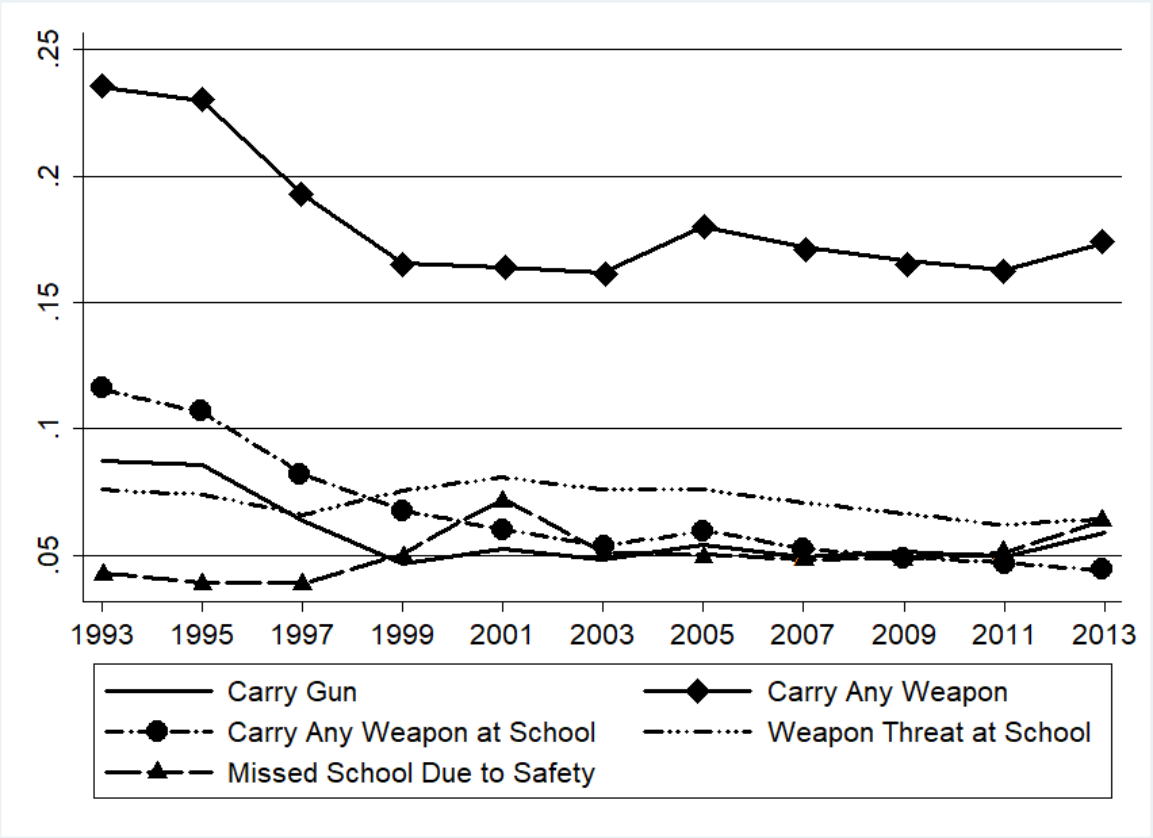

Table 1. Child Access Prevention Laws

	Effective Date	Type of CAP Law
California	January 1, 1992	Negligent Storage
Colorado	October 13, 2000	Reckless Endangerment
Connecticut	October 1,1990	Negligent Storage
Delaware	July 2, 1998	Reckless Endangerment
D.C.	January 28, 2009	Negligent Storage
Florida	October 1,1989	Negligent Storage
Georgia	May 1, 1994	Reckless Endangerment
Hawaii	July 1, 1992	Negligent Storage
Illinois	January 1, 2000	Negligent Storage
Indiana	March 7, 1994	Reckless Endangerment
Iowa	April 5, 1990	Negligent Storage
Kentucky	July 15, 1994	Reckless Endangerment
Maryland	April 1, 1992	Negligent Storage
Massachusetts	October 21, 1998	Negligent Storage
Minnesota	May 20, 1993	Negligent Storage
Mississippi	July 2, 1994	Reckless Endangerment
Missouri	September 28, 1981	Reckless Endangerment
Nevada	July 1, 1995	Reckless Endangerment
New Hampshire	January 1, 2001	Negligent Storage
New Jersey	January 17, 1992	Negligent Storage
North Carolina	December 1, 1993	Negligent Storage
Oklahoma	July 7, 1993	Reckless Endangerment
Rhode Island	July 1, 1995	Negligent Storage
Tennessee	July 1, 1994	Reckless Endangerment
Texas	September 1, 1995	Negligent Storage
Utah	October 21, 1993	Reckless Endangerment
Virginia	July 1, 1992	Reckless Endangerment
Wisconsin	March 1, 1992	Reckless Endangerment

Notes: States for which YRBS data (1993-2013) are available before and after a CAP law went into effect are denoted in italicized font. Data on CAP laws were obtained from the Law Center to Prevent Gun Violence (2013).

Table 2. Descriptive Statistics for YRBS Analysis, 1993-2013

	CAP Law = 1[a]	*CAP Law* = 0	*Full Sample*	Description
Dependent variables				
Carry Gun	.056	.053	.055	= 1 if respondent carried a gun in the past 30 days, = 0 otherwise
	(.230)	(.225)	(.228)	
Carry Any Weapon	.181	.169	.176	= 1 if respondent carried a weapon such as a gun, knife, or club in the past
	(.385)	(.375)	(.381)	30 days, = 0 otherwise
Carry Any Weapon at School	.059	.062	.060	= 1 if respondent carried a weapon such as a gun, knife, or club on school
	(.236)	(.241)	(.238)	property in the past 30 days, = 0 otherwise
Weapon Threat at School	.071	.072	.071	= 1 if respondent was threatened or injured with a weapon such as a gun,
	(.257)	(.258)	(.257)	knife, or club on school property in the past 12 months, = 0 otherwise
Missed School Due to Safety	.054	.051	.053	= 1 if respondent missed school in the past 30 days due to feeling unsafe at
	(.226)	(.219)	(.224)	school or on the way to or from school, = 0 otherwise
Independent variables				
White	.829	.830	.829	= 1 if respondent is white, = 0 otherwise
	(.377)	(.376)	(.376)	
Black	.097	.110	.102	= 1 if respondent is black, = 0 otherwise
	(.295)	(.313)	(.303)	
Other Race	.038	.034	.036	= 1 if respondent is an "other" race, = 0 otherwise
	(.190)	(.180)	(.186)	
Hispanic	.037	.026	.033	= 1 if respondent is Hispanic, = 0 otherwise
	(.189)	(.160)	(.177)	
Age 12	.001	.001	.001	= 1 if respondent is 12 years old, = 0 otherwise
	(.032)	(.030)	(.031)	
Age 13	.002	.006	.003	= 1 if respondent is 13 years old, = 0 otherwise
	(.039)	(.076)	(.057)	
Age 14	.102	.116	.108	= 1 if respondent is 14 years old, = 0 otherwise
	(.302)	(.321)	(.310)	
Age 15	.250	.254	.251	= 1 if respondent is 15 years old, = 0 otherwise
	(.433)	(.435)	(.434)	
Age 16	.266	.266	.266	= 1 if respondent is 16 years old, = 0 otherwise
	(.442)	(.442)	(.442)	
Age 17	.244	.235	.240	= 1 if respondent is 17 years old, = 0 otherwise

	(.429)	(.424)	(.427)	
Age 18+	.137	.122	.130	= 1 if respondent is 18+ years old, = 0 otherwise
	(.343)	(.327)	(.337)	
Grade 8	.000	.005	.002	= 1 if respondent is in grade 8, = 0 otherwise
	(.000)	(.067)	(.044)	
Grade 9	.268	.265	.267	= 1 if respondent is in grade 9, = 0 otherwise
	(.443)	(.442)	(.442)	
Grade 10	.260	.270	.264	= 1 if respondent is in grade 10, = 0 otherwise
	(.439)	(.444)	(.441)	
Grade 11	.249	.243	.246	= 1 if respondent is in grade 11, = 0 otherwise
	(.432)	(.429)	(.431)	
Grade 12	.221	.215	.218	= 1 if respondent is in grade 12, = 0 otherwise
	(.415)	(.411)	(.413)	
Male	.504	.506	.505	= 1 if respondent is male, = 0 otherwise
	(.500)	(.500)	(.500)	
% Nonwhite	.203	.191	.198	Percent of the state population that is nonwhite
	(.082)	(.084)	(.083)	
Mean Age	36.5	37.1	36.7	State mean age
	(2.10)	(1.33)	(1.84)	
% Male	.492	.487	.490	Percent of the state population that is male
	(.212)	(.006)	(.006)	
Unemployment Rate	.065	.063	.064	State unemployment rate
	(.022)	(.020)	(.021)	
Per Capita Income	42,694	42,104	42,447	State real income per capita (2013 dollars)
	(6,302)	(7,549)	(6,857)	
% Bachelor's Degree	.290	.271	.282	Percent of the state population with a bachelor's degree
	(.056)	(.053)	(.055)	
Student-Teacher Ratio	16.0	14.7	15.4	Average Student to Teacher Ratio
	(2.99)	(2.70)	(2.94)	
Teacher Salary	57,623	62,532	59,673	Average Teacher Salary (2013 dollars)
	(9,004)	(10,364)	(9,896)	
School Lunch Program	.099	.097	.098	National School Lunch Participation Rate
	(.021)	(.015)	(.018)	
ZT School Violence Law	.961	.921	.944	= 1 if state has a zero-tolerance school violence law, = 0 otherwise
	(.182)	(.261)	(.219)	
Anti-Bullying Law	.397	.194	.312	= 1 if state has an anti-bullying law, = 0 otherwise

	(.479)	(.381)	(.452)	
Police Expenditures	306	313	309	State police expenditures per capita (2013 dollars)
	(77.0)	(110)	(92.3)	
Police Employment	2.20	2.74	2.43	State police employees per 1,000 population
	(.314)	(.993)	(.733)	
Property Crime	1,018	774	907	State adult property crime arrests per 100,000 population
	(339)	(320)	(352)	
Violent Crime	449	326	393	State adult violent crime arrests per 100,000 population
	(226)	(167)	(210)	
Democrat	.402	.453	.423	= 1 if state has a democratic governor, = 0 otherwise
	(.486)	(.495)	(.490)	
Mental Health Parity Law	.524	.502	.515	= 1 if state has a mental health parity law, = 0 otherwise
	(.499)	(.497)	(.498)	
Beer Tax	.292	.271	.283	Beer taxes (2013 dollar)
	(.195)	(.221)	(.207)	
ZT Drunk Driving Law	.951	.917	.937	= 1 if state has a zero-tolerance drunk driving law, = 0 otherwise
	(.212)	(.266)	(.237)	
Shall Issue Law	.600	.435	.531	= 1 if state has a shall issue gun law, = 0 otherwise
	(.490)	(.496)	(.499)	
Stand Your Ground Law	.304	.186	.255	= 1 if state has a stand-your-ground gun law, = 0 otherwise
	(.449)	(.388)	(.429)	
Minimum Possession Age[b]	18.0	18.2	18.1	Minimum age to possess a handgun
	(.000)	(.757)	(.543)	
Background Check Law	.426	.581	.491	= 1 if state requires background checks for private sales on firearms, = 0
	(.494)	(.493)	(.500)	otherwise
Gun Buyback Program	.517	.374	.457	= 1 if a major city in the state held a gun buyback program, = 0 otherwise
	(.500)	(.484)	(.498)	
Trigger Lock Law	.176	.000	.103	= 1 if state requires trigger locks to accompany dealer and private firearm
	(.381)	(.000)	(.304)	sales, = 0 otherwise
N	508,614	541,900	1,050,514	

[a] If a CAP law is in effect for any portion of the year, the observation is included in this column. [b] In our regression models, we control for a set of mutually exclusive dummy variables that cover all possible state minimum possession ages (that is, 14, 16, 18, and 21).

Notes: Weighted means presented along with standard deviations.

Table 3. CAP Laws, Gun Carrying, and School Safety

	Carry Gun	*Carry Any Weapon*	*Carry Any Weapon at School*	*Weapon Threat at School*	*Missed School Due to Safety*
	Panel I: Students under age 18				
CAP Law	-.010*	-.020*	-.005	-.014***	-.008
	(.005)	(.011)	(.005)	(.005)	(.006)
Mean	.053	.175	.059	.072	.053
N	672,373	799,904	889,523	892,550	916,544
	Panel II: Students ages 18+				
CAP Law	.006	.005	-.003	.012	.020
	(.006)	(.020)	(.012)	(.012)	(.013)
Mean	.064	.187	.072	.065	.054
N	104,263	119,320	130,534	131,751	133,970
	Panel III: Students under age 18 vs. students ages 18+ (DDD estimates)				
CAP Law	-.015**	-.025	-.003	-.028**	-.030***
	(.006)	(.016)	(.011)	(.011)	(.011)
Mean	.055	.176	.060	.071	.053
N	776,636	919,224	1,020,057	1,024,301	1,050,514

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

Notes: Each cell represents a marginal effect from a probit regression based on data from the YRBS for the period 1993-2013. Dependent variable means are reported. All models control for the covariates listed in Table 2, state fixed effects, year fixed effects, and state-specific linear time trends. The models in panel III also control for interactions between an under-age-18 indicator and all right-hand-side variables. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 4. Examining Heterogeneous Effects by Type of CAP Law

	Carry Gun	*Carry Any Weapon*	*Carry Any Weapon at School*	*Weapon Threat at School*	*Missed School Due to Safety*
	Panel I: Students under age 18				
Negligent Storage	-.013**	-.026**	-.002	-.009*	-.009
	(.006)	(.012)	(.005)	(.005)	(.007)
Reckless Endangerment	-.005	-.012	-.009	-.022**	-.005
	(.007)	(.015)	(.008)	(.010)	(.010)
Mean	.053	.175	.059	.072	.053
N	672,373	799,904	889,523	892,550	916,544
	Panel II: Students ages 18+				
Negligent Storage	.003	.006	-.010	.018	.030**
	(.008)	(.027)	(.017)	(.013)	(.013)
Reckless Endangerment	.009	.004	.007	.003	.005
	(.009)	(.025)	(.013)	(.015)	(.015)
Mean	.064	.187	.072	.065	.054
N	104,263	119,320	130,534	131,751	133,970
	Panel III: Students under age 18 vs. students ages 18+ (DDD estimates)				
Negligent Storage	-.016**	-.032	.006	-.029**	-.041***
	(.007)	(.021)	(.016)	(.012)	(.011)
Reckless Endangerment	-.013	-.016	-.015	-.025	-.010
	(.009)	(.022)	(.010)	(.019)	(.017)
Mean	.055	.176	.060	.071	.053
N	776,636	919,228	1,020,057	1,024,301	1,050,514

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

Notes: Each column within each panel represents marginal effects from a probit regression based on data from the YRBS for the period 1993-2013. Dependent variable means are reported. All models control for the covariates listed in Table 2, state fixed effects, year fixed effects, and state-specific linear time trends. The models in panel III also control for interactions between an under-age-18 indicator and all right-hand-side variables. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 5. Sensitivity of *CAP Law* Estimates to Sequentially Adding Controls (Students Under Age 18 vs. Students Ages 18+ (DDD Estimates))

	Carry Gun	*Carry Any Weapon*	*Carry Any Weapon at School*	*Weapon Threat at School*	*Missed School Due to Safety*
Baseline model	-.010	-.010	-.002	-.015	-.022***
	(.009)	(.012)	(.014)	(.013)	(.008)
+ Sociodemographic controls	-.014**	-.021*	-.001	-.017	-.025***
	(.007)	(.012)	(.014)	(.014)	(.009)
+ School violence policies	-.014**	-.020*	-.001	-.016	-.026***
	(.006)	(.012)	(.014)	(.014)	(.009)
+ Crime and political controls	-.017***	-.016	-.004	-.019	-.028***
	(.006)	(.012)	(.013)	(.013)	(.008)
+ Economic controls	-.017***	-.018	-.002	-.021	-.028***
	(.005)	(.012)	(.013)	(.013)	(.008)
+ Education controls	-.017***	-.023	-.003	-.023*	-.031***
	(.006)	(.015)	(.015)	(.012)	(.010)
+ Mental health policy	-.016***	-.023	-.002	-.023*	-.029***
	(.006)	(.014)	(.015)	(.012)	(.011)
+ Gun controls	-.015**	-.025	-.003	-.028**	-.030***
	(.006)	(.016)	(.011)	(.011)	(.011)
N	776,636	919,228	1,020,057	1,024,301	1,050,514

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

Notes: Each column represents marginal effects from a probit regression based on data from the YRBS for the period 1993-2013. Demographic controls: individual-level race, age, grade, and gender dummies, *% Nonwhite*, *Mean Age*, and *% Male*. School violence policies: *ZT School Violence Law* and *Anti-Bullying Law*. Crime controls: *Police Expenditures*, *Police Employment*, *Property Crime*, *Violent Crime*, and *ZT Drunk Driving Law*. Political control: *Democrat*. Economic controls: *Unemployment Rate*, *Per Capita Income*, and *Beer Tax*. Education controls: *% Bachelor's Degree*, *Student-Teacher Ratio*, *Teacher Salary*, and *School Lunch Program*. Mental health policy: *Mental Health Parity Law*. Gun-related policies: *Shall Issue Law*, *Minimum Possession Age*, *Background Check Law*, *Trigger Lock Law*, and *Gun Buyback Program*. All models control for state fixed effects, year fixed effects, state-specific linear time trends, and for interactions between an under-age-18 indicator and all right-hand-side variables. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 6. Sensitivity of *Negligent Storage* Estimates to Sequentially Adding Controls (Students Under Age 18 vs. Students Ages 18+ (DDD Estimates))

	Carry Gun	*Carry Any Weapon*	*Carry Any Weapon at School*	*Weapon Threat at School*	*Missed School Due to Safety*
Baseline model	-.021***	-.009	.005	-.027*	-.026***
	(.008)	(.014)	(.021)	(.014)	(.009)
+ Sociodemographic controls	-.020***	-.019	.005	-.028**	-.031***
	(.006)	(.014)	(.021)	(.014)	(.009)
+ School violence policies	-.019***	-.017	.005	-.028**	-.031***
	(.006)	(.014)	(.021)	(.014)	(.009)
+ Crime and political controls	-.020***	-.015	.003	-.028**	-.031***
	(.005)	(.012)	(.018)	(.013)	(.008)
+ Economic controls	-.020***	-.016	.004	-.030**	-.033***
	(.005)	(.012)	(.018)	(.013)	(.008)
+ Education controls	-.021***	-.026	.003	-.031**	-.039***
	(.005)	(.016)	(.020)	(.013)	(.011)
+ Mental health policy	-.020***	-.026*	.004	-.031**	-.038***
	(.005)	(.016)	(.021)	(.013)	(.012)
+ Gun controls	-.016**	-.032	.006	-.029**	-.041***
	(.007)	(.021)	(.016)	(.012)	(.011)
N	776,636	919,228	1,020,057	1,024,301	1,050,514

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

Notes: Each column represents marginal effects from a probit regression based on data from the YRBS for the period 1993-2013. Demographic controls: individual-level race, age, grade, and gender dummies, *% Nonwhite*, *Mean Age*, and *% Male*. School violence policies: *ZT School Violence Law* and *Anti-Bullying Law*. Crime controls: *Police Expenditures*, *Police Employment*, *Property Crime*, *Violent Crime*, and *ZT Drunk Driving Law*. Political control: *Democrat*. Economic controls: *Unemployment Rate*, *Per Capita Income*, and *Beer Tax*. Education controls: *% Bachelor's Degree*, *Student-Teacher Ratio*, *Teacher Salary*, and *School Lunch Program*. Mental health policy: *Mental Health Parity Law*. Gun-related policies: *Shall Issue Law*, *Minimum Possession Age*, *Background Check Law*, *Trigger Lock Law*, and *Gun Buyback Program*. All models control for *Reckless Endangerment*, state fixed effects, year fixed effects, state-specific linear time trends, and for interactions between an under-age-18 indicator and all right-hand-side variables. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 7. Leads and Lags of *CAP Law*
(Students Under Age 18)

	Carry Gun	*Carry Any Weapon*	*Carry Any Weapon at School*	*Weapon Threat at School*	*Missed School Due to Safety*
2 Years before CAP Law	.004 (.009)	.006 (.016)	.005 (.007)	-.007 (.009)	-.006 (.010)
1 Year before CAP Law	-.007 (.012)	-.012 (.030)	.006 (.015)	-.000 (.015)	-.029** (.012)
Year of Law Change	.007 (.010)	.014 (.019)	-.001 (.009)	-.001 (.011)	.006 (.012)
1 Year after CAP Law	-.021** (.010)	-.040* (.024)	-.002 (.010)	-.018 (.011)	-.028** (.012)
2+ Years after CAP Law	-.007 (.010)	-.020 (.025)	.003 (.010)	-.024*** (.009)	-.033*** (.011)
Mean	.053	.175	.059	.072	.053
N	672,373	799,904	889,523	892,550	916,544

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

Notes: Each column represents marginal effects from a probit regression based on data from the YRBS for the period 1993-2013. Dependent variable means are reported. All models control for the covariates listed in Table 2, state fixed effects, year fixed effects, and state-specific linear time trends. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 8. CAP Laws and Risky Behavior Falsification Tests
(Students Under Age 18)

	Cigarette Use	*Illicit Drug Use*	*Binge Drinking*	*Drunk Driving*	*Seat Belt Use*	*Sex Partners*
CAP Law	.004	-.054	-.002	-.002	-.019	.007
	(.012)	(.054)	(.011)	(.007)	(.013)	(.006)
Mean	.213	.258	.249	.091	.105	.075
N	886,205	711,238	904,960	883,577	892,147	773,650

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

Notes: Each cell represents a marginal effect from a probit regression based on data from the YRBS for the period 1993-2013. Dependent variable means are reported. All models control for the covariates listed in Table 2, *Reckless Endangerment*, state fixed effects, year fixed effects, and state-specific linear time trends. Regressions are weighted using population estimates from the National Cancer Institute's Surveillance Epidemiology and End Results Program. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 9. Do School Shootings or Violent Crime Rates Predict CAP Laws?

Panel I	*CAP Law*	*CAP Law*
Shooting Involving Any Death	-.001	-.013
(*Shooters Under Age 18*)	(.020)	(.014)
Shooting Involving Any Death	…	.008
(*Shooters Under Age 18*), *1 Year Lag*		(.019)
Shooting Involving Any Death	…	.004
(*Shooters Under Age 18*), *2 Year Lag*		(.014)
Shooting Involving Any Death	…	-.002
(*Shooters Under Age 18*), *3 Year Lag*		(.013)
Mean	.454	.499
N	1,173	1,020
Panel II		
Number of Shootings Involving Any Death (*Shooters*	-.002	-.002
Under Age 18)	(.009)	(.006)
Number of Shootings Involving Any Death (*Shooters*	…	-.006
Under Age 18), *1 Year Lag*		(.013)
Number of Shootings Involving Any Death (*Shooters*	…	.001
Under Age 18), *2 Year Lag*		(.009)
Number of Shootings Involving Any Death (*Shooters*	…	.001
Under Age 18), *3 Year Lag*		(.006)
Mean	.454	.499
N	1,173	1,020
Panel III		
Violent Crime Arrest Rate	.001	.0004
(*Under Age 18*)	(.001)	(.0003)
Violent Crime Arrest Rate	…	.0001
(*Under Age 18*), *1 Year Lag*		(.0003)
Violent Crime Arrest Rate	…	.0003
(*Under Age 18*), *2 Year Lag*		(.0003)
Violent Crime Arrest Rate	…	.0004
(*Under Age 18*), *3 Year Lag*		(.0003)
Mean	.451	.503
N	1,132	961

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

Notes: The first column within each panel represents an estimate from an OLS regression based on data for the period 1991-2013. The second column within each panel represents estimates from an OLS regression based on data for the period 1994-2013. Dependent variable means are reported. All models control for the state-level covariates listed in Table 2, state fixed effects, year fixed effects, and state-specific linear time trends. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 10. Descriptive Statistics for State-Level School Shooting Analysis, 1991-2013

	CAP Law = 1	*CAP Law* = 0	Full Sample	Description
Shooting Involving Any Death (*Shooters under age 18*)	.230 (.421)	.185 (.388)	.211 (.408)	= 1 if there was a school shooting involving a death in state *s* during year *t* that was committed by someone under the age of 18, = 0 otherwise
Shooting Involving Suicide (*Shooters under age 18*)	.140 (.348)	.072 (.259)	.112 (.316)	= 1 if there was a school shooting involving a suicide in state *s* during year *t* that was committed by someone under the age of 18, = 0 otherwise
Shooting Involving Homicide (*Shooters under age 18*)	.141 (.348)	.128 (.334)	.136 (.342)	= 1 if there was a school shooting involving a homicide in state *s* during year *t* that was committed by someone under the age of 18, = 0 otherwise
Shooting Involving Any Death (*Shooters ages 18+*)	.326 (.469)	.191 (.393)	.271 (.444)	= 1 if there was a school shooting in state *s* during year *t* that was committed by someone 18+ years of age, = 0 otherwise
Shooting Involving Suicide (*Shooters ages 18+*)	.142 (.350)	.068 (.251)	.112 (.315)	= 1 if there was a school shooting involving a suicide in state *s* during year *t* that was committed by someone 18+ years of age, = 0 otherwise
Shooting Involving Homicide (*Shooters ages 18+*)	.278 (.448)	.168 (.374)	.233 (423)	= 1 if there was a school shooting involving a homicide in state *s* during year *t* that was committed by someone 18+ years of age, = 0 otherwise
N	543	630	1,173	

Notes: Weighted means with standard deviations in parentheses.

Table 11. CAP Laws and the Probability of a School Shooting

Panel I	*Shooting Involving Any Death* (*Shooters Under Age 18*)	*Shooting Involving Suicide* (*Shooters Under Age 18*)	*Shooting Involving Homicide* (*Shooters Under Age 18*)
CAP Law	-.008	.046	-.049
	(.131)	(.080)	(.085)
Mean	.211	.112	.136
N	1,173	1,173	1,173
Panel II	*Shooting Involving Any Death* (*Shooters Ages 18+*)	*Shooting Involving Suicide* (*Shooters Ages 18+*)	*Shooting Involving Homicide* (*Shooters Ages 18+*)
CAP Law	.064	.065	-.014
	(.136)	(.076)	(.092)
Mean	.271	.112	.233
N	1,173	1,173	1,173

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

Notes: Each cell represents an estimate from an OLS regression based on school-associated shooting death data for the period 1991-2013. Dependent variable means are reported. All models control for the covariates listed in Online Appendix Table AO9, state fixed effects, year fixed effects, and state-specific linear time trends. Regressions are weighted using state populations. Standard errors, corrected for clustering at the state level, are in parentheses.