Child-Access-Prevention Laws, Youths' Gun Carrying, and School Shootings

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Abstract

Despite public interest in keeping guns out of schools, little is known about the effects of gun control on youths' gun carrying or school violence. Using data from the Youth Risk Behavior Survey (YRBS) for 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 report 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 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.

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, ap-

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[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-grader Jose Reyes brought a 9 mm semiautomatic hand-

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proximately 70 percent of shootings at kindergarten through grade 12 schools in the United States were committed by minors (Everytown for Gun Safety Support Fund 2015a). More often than not, the shooters obtained the guns from their own homes or the homes of relatives (Violence Policy Center 2001; Wood 2001; Copeland 2014). According to survey data, roughly one-third of households with children report that firearms are kept in or around the home (Okoro et al. 2005). Among homes with children and firearms, 43 percent report having at least one gun in an unlocked location (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 youths' access to firearms, 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 (Giffords Law Center to Prevent Gun Violence 2015). Proponents of CAP laws argue that they limit not only intentional shootings but also accidental ones (Shaffer 2000). On the other hand, opponents of gun control contend that CAP laws impinge on a gun owner's constitutional right to bear arms. They argue that, in emergency situations, safe-storage requirements encumber a potential victim's ability to use a firearm for self-defense (Shaffer 2000; 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 studies have estimated the effect of CAP laws on youths' 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 there is substantial state-level variation in the timing of policy adoption, the laws generate predictions as to which age groups should be most affected, and heterogeneity in standards for criminal

gun 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 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 US Army Reserve program (Slauson 2014). On January 23, 2018, 15-year-old Gabe Parker used a handgun to kill two students at Marshall County High School. The gun was taken from a closet in his 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 his father (Fernandez, Fausset, and Bidgood 2018).

[2] Economists have studied the 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 on crime (McClellan and Tekin 2017; Cheng and Hoekstra 2013), but none have examined the effects of gun control on school violence.

liability (negligent storage versus reckless endangerment) generates predictions as to where the effects should be most strongly felt.

This study begins by examining the relationship between CAP laws and gun carrying among US high-school students using state and national data from the Youth Risk Behavior Survey (YRBS) for 1993–2013, a period when 17 states and the District of Columbia 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 under the age of 18 report carrying a gun in the past month. From 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 result 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 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 report 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 report having missed school in the past month because they felt 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 those 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 policy effects of substantial magnitude.

2. Background

2.1. Youths' Gun Violence

Highly publicized school shootings have increased awareness about youths' firearm access and use, and a large literature exists on the individual-level correlates of youths' 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 (Hemenway et al. 1996; Ruggles and Rajan 2014), academic performance (Hemenway et al. 1996), and drug dealing (Vaughn et al. 2012) are all strong predictors of youths' 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. In contrast, teens who obtain guns illegally are often socialized into gun use by their peers, more

[3] Data are available for before and after a child-access-protection (CAP) law went into effect for the District of Columbia and 13 of these 17 states.

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

[5] See Emmert and Lizotte (2015) for a thorough discussion of the research on the demographics of youths who carry weapons and the risk factors for juveniles' weapon carrying.

likely to be criminally active, and more likely to bring guns to school (Lizotte et al. 1994, 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) finds that increased levels of violence in and around schools lead to lower graduation rates in the United States. Beland and Kim (2016) find that fatal shootings in US high schools are associated with increased dropout rates and reduced test scores, while Abouk and Adams (2013) find evidence to suggest that school shootings induce private-school enrollment. Poutvaara and Ropponen (2018) conclude that a highly publicized school shooting in Finland decreased the academic performance of students in other schools.[6]

Students' 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 guns themselves (Bergstein et al. 1996; Hemenway et al. 1996, 2011).

2.2. Child-Access-Protection 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 the District of Columbia 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's owner with fines, imprisonment, or a both. For instance, according to California's recently signed Firearm Safe and Responsible Access Act, violators risk a potential $1,000 fine and/or 6 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. Under 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 2000). However, 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" (Giffords Law Center to Prevent Gun Violence 2015). Child-access-prevention laws have also been used to penalize manufac-

[6] Relatedly, Gershenson and Tekin (2018) find that the 2002 Beltway Sniper attacks in Washington, DC, reduced school-level proficiency rates in schools within 5 miles of an attack. They conclude that traumatic community events such as mass shootings have the potential to disrupt students' learning.

turers and dealers who fail to include safety devices when selling their firearms (Shaffer 2000).[7]

Prosecutions under CAP laws 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 of individuals being charged with unsafe gun storage in a number of CAP-law states, see Amaral (2014), Associated Press (2017), Bell (2016), Boren (2017), Cutts and Majchrowicz (2016), Harmacinski (2013), James (1996), Lopez and Goff (2014), Ly (2013), *Wilkes Journal-Patriot* (2017), WRAL.com (2009), and Young (2012).[8]

The absence of state panel data on adults' 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 2005 Early Childhood Longitudinal Study, Prickett, Martin-Storey, and Crosnoe (2014) find that unsafe firearm storage is 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 focus on school-related outcomes, several use state-level data to examine the relationship between CAP laws and gun-related deaths. Using data for 1979–94, Cummings et al. (1997) find that CAP laws are associated with a decrease in accidental shooting deaths by roughly 23 percent among children 14 and younger. Using data for 1979–97, Webster and Starnes (2000) also examine the relationship between CAP laws and accidental shooting deaths among children 14 and younger. They find a negative association between CAP laws and accidental firearm deaths, but this association is driven entirely by one state (Florida). Webster et al. (2004) find that CAP laws are associated with an 11 percent decrease in the gun-related suicide rate among 14- to 17-year-olds for 1976–2001. These authors also find that CAP laws are associated with a similar decrease in the gun-related suicide rate among 18- to 20-year-olds, which raises the possibility that their findings for the younger age group are spurious. Using

[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. Child-access-prevention laws may apply to all firearms, loaded firearms, or handguns only. Some states require that firearms must be stored with a locking device in place. The age at which one is considered a minor also varies by state. While most states define a minor as anyone under 18, some states have a lower age threshold (Giffords Law Center to Prevent Gun Violence 2015). In our Youth Risk Behavior Survey (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 classify minors as children 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 18, we potentially capture a lower-bound policy effect. However, dropping those states from our estimation sample or restricting each state's sample of students on the basis of its definition of a minor yields a similar pattern of results.

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

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

data for 1979–96, Lott and Whitley (2001) find little evidence to suggest that CAP laws are associated with accidental gun deaths or suicides among teens. On the other hand, they find that CAP laws are consistently associated with more rapes, robberies, and burglaries. Lott (2003) analyzes data for 1977–98 and finds results similar to those reported in Lott and Whitley (2001). However, Pepper (2005) shows that the estimates in Lott (2003) are sensitive to model specification (for a critical review of the research on state gun laws, see National Research Council [2005]).

Most recently, DeSimone, Markowitz, and Xu (2013) use annual hospital discharge data for 1988–2003 to estimate the relationship between CAP laws and nonfatal gun injuries. They find that CAP laws are 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 find no effects on self-inflicted gun injuries among adults or on self-inflicted injuries without a gun.

Our study contributes to this literature by being the first to examine the effect of CAP laws on youths' gun carrying and school violence. We also compile the first census of school-associated shooting deaths in the United States over the last 2 decades and explore the relationship between CAP laws and those events.

3. Youth Risk Behavior Survey Analysis

3.1. Data

The data for our primary analysis come from the national and state YRBS and cover 1993–2013. Government agencies rely on these data to track trends in teen behaviors including physical activity, unhealthful eating, tobacco use, alcohol and drug use, sexual activity, and violence. Previous studies such as Simon, Crosby, and Dahlberg (1999), Dinkes et al. (2009), and Sabia and Bass (2017) use these data to examine determinants of weapon carrying and student victimization on school grounds.[10]

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.[11] 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.[12] Roughly half of the states have granted the CDC permission to release their data, while the remaining states require direct requests. For both the na-

[10] Researchers have used these data to study a wide 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).

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

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

tional and state YRBS, the final stage of sampling selects classes within schools; all students in the sampled classes are eligible to participate (CDC 2013).

For the national and the majority of state surveys, trained data collectors travel to each participating school to administer the questionnaires.[13] The data-collection procedures are designed to protect respondents' 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 that 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's administrator as they complete the questionnaire. When finished with the survey, students seal the answer sheet in an envelope and place it in a box. For further details on the YRBS data-collection protocols, see CDC (2013).[14]

Following previous studies, we combine the national and state YRBS data so that identification comes from as many policy 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 (Colorado, Delaware, Georgia, Illinois, Massachusetts, Minnesota, Mississippi, Nevada, North Carolina, Tennessee, Texas, and Utah) and the District of Columbia contributed data to the national YRBS before and after the adoption of a CAP law, and 12 states (Colorado, Delaware, Georgia, Illinois, Massachusetts, Mississippi, North Carolina, Nevada, New Hampshire, Tennessee, Texas, and Utah) 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 posttreatment data for 13 states and the District of Columbia (see Table 1).[15] Six of these states and the District of Columbia impose criminal liability for negligent storage, while seven states impose criminal liability for reckless endangerment. The YRBS sample contains over 1 million observations and includes individuals from all 50 states and the District of Columbia.

We measure youths' 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 equals one if the respondent reported carrying a gun at least once in the past 30 days and zero otherwise.[16]

Respondents were also asked about weapon carrying, both overall and on school property, which we use to generate two separate indicators: "During the

[13] In some states, the questionnaires are sent directly to schools, and teachers administer the survey following a standardized script (CDC 2013).

[14] Research on survey settings conducted by the Centers for Disease Control and Prevention finds that students are more likely to report health-risk behaviors when answering questions at school rather than at home (Brener et al. 2006).

[15] Online Appendix Table OA1 shows the number of observations in our sample for each state-year cell.

[16] We also examine the intensive margin of gun carrying and the intensive margin for outcomes for which we have measures of frequency. These results, which are available on request from the authors, suggest that CAP-law effects tend to be largest on the extensive margin.

Table 1
Adoption of Child-Access-Prevention Laws

	Effective Date	Type of Law
California	January 1, 1992	Negligent storage
Colorado[a]	October 13, 2000	Reckless endangerment
Connecticut	October 1, 1990	Negligent storage
Delaware[a]	July 2, 1998	Reckless endangerment
District of Columbia[a]	January 28, 2009	Negligent storage
Florida	October 1, 1989	Negligent storage
Georgia[a]	May 1, 1994	Reckless endangerment
Hawaii	July 1, 1992	Negligent storage
Illinois[a]	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[a]	October 21, 1998	Negligent storage
Minnesota[a]	May 20, 1993	Negligent storage
Mississippi[a]	July 2, 1994	Reckless endangerment
Missouri	September 28, 1981	Reckless endangerment
Nevada[a]	July 1, 1995	Reckless endangerment
New Hampshire[a]	January 1, 2001	Negligent storage
New Jersey	January 17, 1992	Negligent storage
North Carolina[a]	December 1, 1993	Negligent storage
Oklahoma	July 7, 1993	Reckless endangerment
Rhode Island	July 1, 1995	Negligent storage
Tennessee[a]	July 1, 1994	Reckless endangerment
Texas[a]	September 1, 1995	Negligent storage
Utah[a]	October 21, 1993	Reckless endangerment
Virginia	July 1, 1992	Reckless endangerment
Wisconsin	March 1, 1992	Reckless endangerment

Source. Data are from Giffords Law Center to Prevent Gun Violence (2015).

[a] Data from the Youth Risk Behavior Survey are available before and after the law went into effect.

past 30 days, on how many days did you carry a weapon such as a gun, knife, or club?" and "During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club on school property?" The variable Carry Any Weapon (Carry Any Weapon at School) equals one if the respondent reported carrying a weapon (on school property) at least once in the past 30 days and zero otherwise. The obvious disadvantage of these two measures is that we cannot separate gun-carrying effects of CAP laws from knife- or club-carrying effects. Thus, we can observe only 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 were 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?" The variable Weapon Threat at School equals one if the respondent reported being threatened or injured at least once in the past 12 months and zero otherwise. Finally, respondents were 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?" The variable Missed School because of Safety equals one if respondents reported missing school at least once in the last 30 days and zero otherwise.[17]

Table 2 provides definitions of the variables and descriptive statistics 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 had carried a gun at least once in the past 30 days, 17.6 percent had carried a weapon (gun, knife, or club) in the past 30 days, 6.0 percent had 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 had missed school because of feeling unsafe in the past 30 days.[18] 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 which age groups should be most influenced by CAP laws. The laws are less binding for students 18 and older than for students under 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 because of Safety) differently across the two age groups.

Figures 1 and 2 show trends in our dependent variables for the national and state YRBS, respectively, and indicate that the national and state YRBS are capturing the same broad changes in our outcomes over time. During the 1990s,

[17] It is worth noting that the wording of the questions for our outcomes of interest is identical across the national and state YRBS. The numbers of observations are smaller for some of the outcomes, including Carry Gun and Carry Any Weapon, because these questions are not asked in a number of state surveys. The question for Carry Gun was not asked in Connecticut (2007–11), Idaho (2001, 2003), Maine (2009–13), Nebraska (2013), Nevada (2003–11), New Hampshire (2009–13), North Carolina (2003, 2009), North Dakota (1999–2013), Ohio (2011), Rhode Island (2007–11), South Dakota (2005–13), Utah (2001, 2009, 2011), Vermont (1993–2001, 2005–11), and Wisconsin (2013). The question for Carry Any Weapon was not asked in Connecticut (2011), Idaho (2003), Maine (2009–13), Nebraska (2013), New Hampshire (2009, 2013), North Dakota (1999–2013), Rhode Island (2013), South Dakota (2001, 2005–13), and Vermont (1997–2001, 2005–11).

[18] The nonresponse 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, .3 percent for Weapon Threat at School, and .4 percent for Missed School because of Safety. Nonresponse 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 nonresponse is correlated with CAP laws. In models in which we regress the probability of nonresponse for each of our outcomes of interest on the CAP Law indicator, we find no evidence to suggest that these laws predict nonresponse to the survey. This, to an extent, quells concerns that students in CAP-law states are more likely to lie when asked gun-carrying-related questions in the YRBS. These results are available from the authors on request. For general information on data quality in the YRBS, see CDC (2013).

Table 2
Descriptive Statistics, 1993–2013

	CAP Law	No CAP Law	Full Sample	Definition
Dependent variable:				
Carry Gun	.056	.053	.055	Equals one if the respondent carried a gun in the past 30 days and zero otherwise
	(.230)	(.225)	(.228)	
Carry Any Weapon	.181	.169	.176	Equals one if the respondent carried a weapon such as a gun, knife, or club in the past 30 days and zero otherwise
	(.385)	(.375)	(.381)	
Carry Any Weapon at School	.059	.062	.060	Equals one if the respondent carried a weapon such as a gun, knife, or club on school property in the past 30 days and zero otherwise
	(.236)	(.241)	(.238)	
Weapon Threat at School	.071	.072	.071	Equals one if the respondent was threatened or injured with a weapon such as a gun, knife, or club on school property in the past 12 months and zero otherwise
	(.257)	(.258)	(.257)	
Missed School because of Safety	.054	.051	.053	Equals one if the respondent missed school in the past 30 days because of feeling unsafe at school or on the way to or from school and zero otherwise
	(.226)	(.219)	(.224)	
Independent variable:				
White	.829	.830	.829	Equals one if the respondent is white and zero otherwise
	(.377)	(.376)	(.376)	
Black	.097	.110	.102	Equals one if the respondent is black and zero otherwise
	(.295)	(.313)	(.303)	
Other Race	.038	.034	.036	Equals one if the respondent is not white, black, or Hispanic and zero otherwise
	(.190)	(.180)	(.186)	
Hispanic	.037	.026	.033	Equals one if the respondent is Hispanic and zero otherwise
	(.189)	(.160)	(.177)	

Age 12	.001	.001	.001	Equals one if the respondent is 12 years old and zero otherwise
	(.032)	(.030)	(.031)	
Age 13	.002	.006	.003	Equals one if the respondent is 13 years old and zero otherwise
	(.039)	(.076)	(.057)	
Age 14	.102	.116	.108	Equals one if the respondent is 14 years old and zero otherwise
	(.302)	(.321)	(.310)	
Age 15	.250	.254	.251	Equals one if the respondent is 15 years old and zero otherwise
	(.433)	(.435)	(.434)	
Age 16	.266	.266	.266	Equals one if the respondent is 16 years old and zero otherwise
	(.442)	(.442)	(.442)	
Age 17	.244	.235	.240	Equals one if the respondent is 17 years old and zero otherwise
	(.429)	(.424)	(.427)	
Age 18+	.137	.122	.130	Equals one if the respondent is 18+ years old and zero otherwise
	(.343)	(.327)	(.337)	
Grade 8	.000	.005	.002	Equals one if the respondent is in grade 8 and zero otherwise
	(.000)	(.067)	(.044)	
Grade 9	.268	.265	.267	Equals one if the respondent is in grade 9 and zero otherwise
	(.443)	(.442)	(.442)	
Grade 10	.260	.270	.264	Equals one if the respondent is in grade 10 and zero otherwise
	(.439)	(.444)	(.441)	
Grade 11	.249	.243	.246	Equals one if the respondent is in grade 11 and zero otherwise
	(.432)	(.429)	(.431)	
Grade 12	.221	.215	.218	Equals one if the respondent is in grade 12 and zero otherwise
	(.415)	(.411)	(.413)	
Male	.504	.506	.505	Equals one if the respondent is male and zero otherwise
	(.500)	(.500)	(.500)	
Percentage Nonwhite	.203	.191	.198	Percentage of the state's population that is nonwhite
	(.082)	(.084)	(.083)	
Mean Age	36.5	37.1	36.7	Mean age in the state
	(2.10)	(1.33)	(1.84)	
Percentage Male	.492	.487	.490	Percentage of the state's population that is male
	(.212)	(.006)	(.006)	
Unemployment	.065	.063	.064	State's unemployment rate
	(.022)	(.020)	(.021)	

Table 2 (*Continued*)

	CAP Law	No CAP Law	Full Sample	Definition
Per Capita Income	42,694	42,104	42,447	State's real income per capita in 2013 dollars
	(6,302)	(7,549)	(6,857)	
Percentage Bachelor's Degree	.290	.271	.282	Percentage of the state's 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 in 2013 dollars
	(9,004)	(10,364)	(9,896)	
School Lunch Program	.099	.097	.098	National School Lunch Program participation rate
	(.021)	(.015)	(.018)	
ZT School Violence Law	.961	.921	.944	Equals one if the state has a zero-tolerance school-violence law and zero otherwise
	(.182)	(.261)	(.219)	
Antibullying Law	.397	.194	.312	Equals one if the state has an antibullying law and zero otherwise
	(.479)	(.381)	(.452)	
Police Expenditures	306	313	309	State's police expenditures per capita in 2013 dollars
	(77.0)	(110)	(92.3)	
Police Employment	2.20	2.74	2.43	Number of police per 1,000 residents in the state
	(.314)	(.993)	(.733)	
Property Crime	1,018	774	907	State's adult property-crime arrests per 100,000 residents
	(339)	(320)	(352)	
Violent Crime	449	326	393	State's adult violent-crime arrests per 100,000 residents
	(226)	(167)	(210)	
Democrat	.402	.453	.423	Equals one if the state has a Democratic governor and zero otherwise
	(.486)	(.495)	(.490)	
Mental Health Parity Law	.524	.502	.515	Equals one if the state has a mental health parity law and zero otherwise
	(.499)	(.497)	(.498)	
Beer Tax	.292	.271	.283	Beer taxes in 2013 dollars
	(.195)	(.221)	(.207)	

ZT Drunk-Driving Law	.951 (.212)	.917 (.266)	.937 (.237)	Equals one if the state has a zero-tolerance drunk-driving law and zero otherwise
Shall Issue Law	.600 (.490)	.435 (.496)	.531 (.499)	Equals one if the state has a shall-issue gun law and zero otherwise
Stand Your Ground Law	.304 (.449)	.186 (.388)	.255 (.429)	Equals one if the state has a stand-your-ground gun law and zero otherwise
Minimum Possession Age Law[a]	18.0 (.000)	18.2 (.757)	18.1 (.543)	Minimum age to possess a handgun
Background Check Law	.426 (.494)	.581 (.493)	.491 (.500)	Equals one if the state requires background checks for private sales of firearms and zero otherwise
Gun Buyback Program	.517 (.500)	.374 (.484)	.457 (.498)	Equals one if a major city in the state held a gun-buyback program and zero otherwise
Trigger Lock Law	.176 (.381)	.000 (.000)	.103 (.304)	Equals one if the state requires trigger locks to accompany dealers' and private firearm sales and zero otherwise
N	508,614	541,900	1,050,514	

Note. Weighted means are shown, with standard deviations in parentheses. Observations are coded as having a law in effect if a child-access-protection (CAP) law was in effect for any portion of the year.

[a] Regression models control for a set of mutually exclusive dummy variables that cover all possible state minimum possession ages (14, 16, 18, and 21).

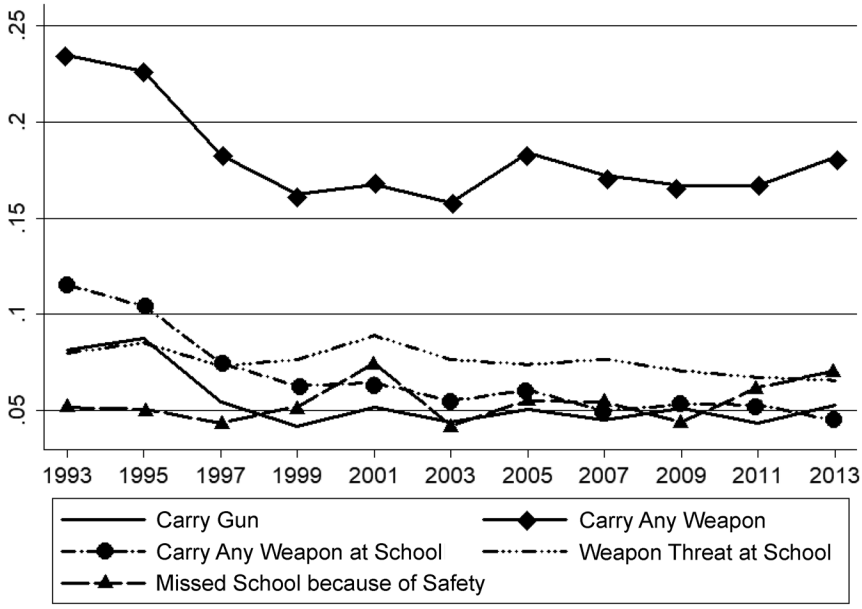

Figure 1. Outcomes in the national Youth Risk Behavior Survey

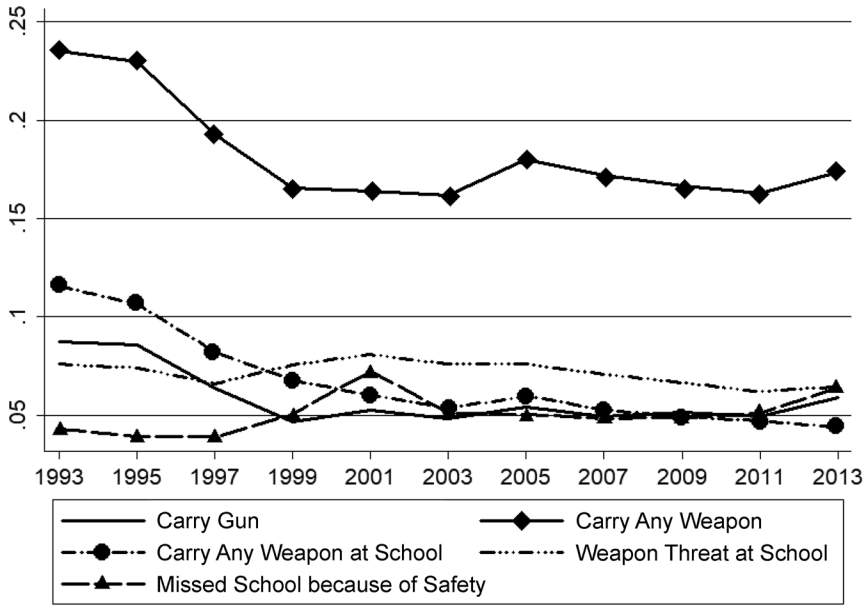

Figure 2. Outcomes in the state Youth Risk Behavior Survey

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. Empirical Strategy

Our empirical analysis is based on the approach taken by previous researchers interested in the effects of gun laws (for examples, see Ludwig 1998; Marvell 2001; Mustard 2001; Cheng and Hoekstra 2013; DeSimone, Markowitz, and Xu 2013; McClellan and Tekin 2017). To estimate the relationship between CAP laws and outcomes for high school students, we exploit the spatial and temporal variation of the laws in a difference-in-differences framework. Our estimating equation is

$$Y_{ist} = \beta_0 + \beta_1 \text{CAP Law}_{st} + \boldsymbol{X1}'_{ist}\boldsymbol{\beta}_2 + \boldsymbol{X2}'_{st}\boldsymbol{\beta}_3 + \boldsymbol{v}_s + \boldsymbol{w}_t + \boldsymbol{v}_s \times t + \varepsilon_{ist}, \quad (1)$$

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, Carry Any Weapon at School, Weapon Threat at School, Missed School because of Safety).[19] Because these outcomes are binary, we estimate equation (1) as a probit model in which 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.[20] 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 $\boldsymbol{v}_s$ and $\boldsymbol{w}_t$ represent state fixed effects and year fixed effects, respectively, and state-specific linear time trends are denoted $\boldsymbol{v}_s \times t$. The state-specific linear time trends are included to control for state-level factors that evolve smoothly over time, such as sentiment toward 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) show 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 (Percentage Nonwhite, Mean Age, Percentage Male), policing resources (Police Expenditures, Police Employment), the adult crime rate (Property Crime, Violent Crime), political preferences (Democrat),[21] whether the state mandates insurance coverage

[19] The YRBS defines a weapon as an object such as a gun, knife, or club. Ideally, we would like to observe only 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.

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

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

to include mental health benefits at parity with physical health benefits (Mental Health Parity Law), alcohol policies (Beer Tax, ZT Drunk-Driving Law),[22] economic conditions (Unemployment, Per Capita Income), education levels and school environment (Percentage Bachelor's Degree, Student-Teacher Ratio, Teacher Salary, School Lunch Program, ZT School Violence Law,[23] Antibullying Law), and other gun laws (Shall Issue Law, Stand Your Ground Law, Background Check Law,[24] 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 OA2 lists the data sources for the state-level covariates.[25]

All regressions are estimated such that the standard errors are corrected for clustering at the state level (Bertrand, Duflo, and Mullainathan 2004).[26] To ensure that the combined YRBS data are nationally representative, we use population data from the National Cancer Institute's Surveillance Epidemiology and End Results program[27] and assign population weights to each respondent according to 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 $\boldsymbol{X2}_{st}$, 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

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

[23] Zero-tolerance school-violence laws severely punish offenses such as weapon carrying (for example, 1-year expulsions).

[24] 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 the District of Columbia have extended the federal law to cover at least some private sales. Eight states (California, Colorado, Connecticut, Delaware, New York, Oregon, Rhode Island, and Washington) and the District of Columbia require universal background checks at the point of sale for all classes of firearms, two states (Maryland and Pennsylvania) require universal background checks at the point of sale for handguns, four states (Hawaii, Illinois, Maryland, and New Jersey) require a state license or permit to purchase any firearm, and four states (Iowa, Michigan, Nebraska, and North Carolina) require a state license or permit to purchase a handgun (Giffords Law Center to Prevent Gun Violence 2018).

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

[26] We also estimate equation (1) as a linear probability model, as statistical inference with ordinary least squares models is based on minimal assumptions regarding the error-correlation process. These estimates are generally similar to, and often larger in magnitude than, the marginal effects produced by probit models.

[27] For more information, see National Cancer Institute, Surveillance, Epidemiology, and End Results Program, U.S. Population Data—1969–2016 (http://seer.cancer.gov/popdata/).

Table 3
Child-Access-Prevention Laws, Gun Carrying, and School Safety

	Carry Gun	Carry Any Weapon	Carry Any Weapon at School	Weapon Threat at School	Missed School because of Safety
Students under 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
Students 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
Students under 18 versus students 18+ (DDD):					
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

Note. Estimates are marginal effects from a probit regression based on data from the Youth Risk Behavior Survey for 1993–2013. Dependent variable means are reported. All models control for the full set of covariates, state fixed effects, year fixed effects, and state-specific linear time trends. The difference-in-difference-in-differences (DDD) models also control for interactions between an under-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.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

this assumption: we examine whether effects are stronger for students under the age of 18, for whom CAP laws are more likely to bind, than for students 18 and older, as well as experiment with formal difference-in-difference-in-differences (DDD) models; we conduct placebo tests on CAP-law leads, including tests for whether salient violent events predict the adoption of CAP laws; and we provide falsification tests on behaviors that should be unaffected by CAP laws.

3.3. Results

Table 3 presents the main results from the YRBS analysis. Because CAP laws specifically target households with minors, we present results based on a cutoff of age 18. While the results for high-school students who are 18 and older do not represent a perfect falsification test (because they 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.[28]

For students under the age of 18, CAP laws are associated with a .010 decrease

[28] For example, from our calculations, wave 1 of the National Longitudinal Study of Adolescent Health, 50.4 percent of 18-year-olds surveyed reported having a younger sibling.

in the probability that the 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 18.[29] The 90 percent confidence interval around this estimate is [−.018, −.001], which means that we can rule out CAP-law effects smaller than 1.9 (=.001/.054) percent in absolute value. These laws are also associated with a .020 decrease in the probability that the student reported carrying any weapon within the past 30 days, an 11 percent decrease relative to the mean. From the 90 percent confidence interval around this estimate, we can rule out CAP-law effects smaller than .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 weapon carrying, at least across the range of weapons examined in the YRBS.[30] These laws are also negatively associated with 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 that 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 that a student reported missing school within the past 30 days because he or she felt unsafe.

Table 3 illustrates that CAP laws have no impact on the weapon-carrying behavior or safety of students 18 and older.[31] The across-the-board null findings provide confidence that our estimates for students under 18 are not spurious and potentially reflect a causal relationship between CAP laws and gun-related outcomes among high-school students.[32] Formal DDD estimates in Table 3 also suggest that our findings are not contaminated by unmeasured school- or state-level

[29] Online Appendix Table OA3 presents means for the outcomes by age.

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

[31] According to our own calculations using the US 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 and 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.

[32] We also explore whether the relationship between CAP laws and our outcomes of interest depends on gender or race. Across all outcomes, we fail to reject the hypothesis that CAP laws are more effective for male than for female students or for white than for black students. In addition, we split the sample along several risky behaviors and test 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; Anderson 2010). We find 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 to seek access to firearms. Finally given interest in the link between mental health and gun violence, we also split our sample on the basis of recent suicide ideation (Konnikova 2014; Said 2015). For our three

Table 4
Examining Heterogeneous Effects by Type of Child-Access-Prevention Law

	Carry Gun	Carry Any Weapon	Carry Any Weapon at School	Weapon Threat at School	Missed School because of Safety
Students under 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
Students 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
Students under 18 versus students 18+ (DDD):					
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

Note. Estimates are marginal effects from a probit regression based on data from the Youth Risk Behavior Survey for 1993–2013. Dependent variable means are reported. All models control for the full set of covariates, state fixed effects, year fixed effects, and state-specific linear time trends. The difference-in-difference-in-differences (DDD) models also control for interactions between an under-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.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

policies.[33] An advantage of the DDD model is that it allows us to net 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 state gun laws targeted at non-

measures of weapon carrying, we fail to reject the hypothesis of equal CAP-law effects across the two samples.

[33] The difference-in-difference-in-differences (DDD) estimates are based on a pooled sample in which the coefficient of interest represents the effect of CAP laws on students under 18 relative to students 18 and older (that is, an interaction between the CAP Law variable and an under-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, 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 ages 18 and older via younger siblings or lagged changes in parental behavior, we would expect the DDD estimates to be conservative.

minors. Given the flexibility of this specification, it could be argued that these estimates are preferred to those from the difference-in-differences models. The results of the DDD estimates indicate that CAP laws are negatively associated with all of our outcomes of interest and are negatively and statistically significantly associated with the outcomes Carry Gun, Weapon Threat at School, and Missed School because of Safety.

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" (Giffords Law Center to Prevent Gun Violence 2015). 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 18.[34]

3.4. *Sensitivity and Robustness Checks*

In Table 5, we explore whether the CAP Law DDD estimates presented in Table 3 are sensitive to the chosen set of controls. A concern is that some of the variables in $\boldsymbol{X2}_{st}$ could be endogenous (for example, the other gun laws). Table 5 begins by showing estimates from models without any of the individual- or state-level covariates listed in Table 2 and then sequentially adds common groupings of these time-varying controls.[35] Our estimates are generally stable across specifications. In Table 6, we repeat this exercise to examine the sensitivity of the Negligent Storage DDD estimates in Table 4. Online Appendix Tables OA5 and OA6 show estimates separately for students under 18 and students 18 and older. 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 con-

[34] A caveat to these results is that identification for the Negligent Storage and Reckless Endangerment indicators is based on only six states (and the District of Columbia) and seven states, respectively. Because of the relatively small number of clusters, we also experiment with using the score bootstrap method suggested by Kline and Santos (2012). Online Appendix Table OA4 reports results from Tables 3 and 4 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.

[35] Table 5 shows results with and without controlling for the other gun laws. For the full sample of respondents, we find little consistent evidence that any of the other gun-related policies are 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 on request.

Table 5
Sensitivity of Child-Access-Prevention Law Estimates to Sequentially Adding Controls

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

Note. Estimates are marginal effects from a probit difference-in-difference-in-differences regression comparing students under 18 with students 18 and older based on data from the Youth Risk Behavior Survey for 1993–2013. Sociodemographic controls are individual-level race, age, grade, and gender dummies; Percentage Nonwhite; Mean Age; and Percentage Male. School-violence policy controls are ZT School Violence Law and Antibullying Law. Crime and political controls are Police Expenditures, Police Employment, Property Crime, Violent Crime, and ZT Drunk-Driving Law. The political control is Democrat. Economic controls are Unemployment, Per Capita Income, and Beer Tax. Education controls are Percentage Bachelor's Degree, Student-Teacher Ratio, Teacher Salary, and School Lunch Program. The mental health policy control is Mental Health Parity Law. Gun-related policy controls are Shall Issue Law, Minimum Possession Age Law, 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 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.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

taminated by school- or state-specific shocks that impact minor teens but not individuals 18 and older. Table 7 presents results based on regressions in which we replace CAP Law with an indicator Year of Law Change, two leads of this indicator, and two lags. The variable Year of Law Change equals one for the year in which a CAP law went into effect and zero otherwise.[36] The primary purpose of this exercise is to test whether any of the outcomes were trending in the years prior to the change in law. Consistent with the parallel-trends assumption, there is little evidence to suggest that our outcomes of interest were trending in a sys-

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

Table 6
Sensitivity of Negligent Storage Estimates to Sequentially Adding Controls

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

Note. Estimates are marginal effects from a probit difference-in-difference-in-differences regression comparing students under 18 with students 18 and older based on data from the Youth Risk Behavior Survey for 1993–2013. Demographic controls are individual-level race, age, grade, and gender dummies; Percentage Nonwhite; Mean Age; and Percentage Male. School-violence policy controls are ZT School Violence Law and Antibullying Law. Crime controls are Police Expenditures, Police Employment, Property Crime, Violent Crime, and ZT Drunk-Driving Law. The political control is Democrat. Economic controls are Unemployment, Per Capita Income, and Beer Tax. Education controls are Percentage Bachelor's Degree, Student-Teacher Ratio, Teacher Salary, and School Lunch Program. The mental health policy control is Mental Health Parity Law. Gun-related policy controls are Shall Issue Law, Minimum Possession Age Law, 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 interactions between an under-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.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

tematic fashion leading up to the passage of CAP laws. We also see that there is a lagged policy effect, which suggests that 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 of the Negligent Storage indicator produce a similar pattern of results (Online Appendix Table OA7).

In Table 8, we conduct falsification tests on behaviors for which we would expect no causal effect of CAP laws. 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, five or more drinks in one sitting) in the past

Table 7
Leads and Lags of Child-Access-Prevention Laws

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

Note. Estimates are marginal effects from a probit regression based on data for students under age 18 from the Youth Risk Behavior Survey for 1993–2013. Dependent variable means are reported. All models control for the full set of covariates, 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.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

month, any drunk driving in the past month, whether the respondent never or rarely wears a seat belt, and having multiple sex partners in the past 3 months. If CAP laws are found to be negatively associated with these outcomes for students under 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.[37] These results provide further support for the hypothesis that the parallel-trends assumption is satisfied.

3.5. School Shootings and Violent-Crime Rates as Predictors of Child-Access-Prevention 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 CAP laws are enacted in response to shocks that affect minor (but not nonminor) high-school students, then our estimates may be biased. In column 1 in Table 9, we present results in which 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 for a detailed description of the school-shooting data set). In column 2, we include three lags of

[37] Online Appendix Table OA8 shows results for this exercise based on the Negligent Storage indicator. We also find that CAP laws are not associated with measures of helmet use, exercise, or diet-pill use or eating disorders.

Table 8
Risky-Behavior Falsification Tests

	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

Note. Estimates are marginal effects from a probit regression based on data on students under 18 from the Youth Risk Behavior Survey for 1993–2013. Dependent variable means are reported. All models control for the full set of covariates, 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.

this school-shooting indicator. There is little evidence in either set of results to support the notion that school-shooting deaths predict the passage of CAP laws.

In Table 9, we also explore whether the count of school-associated shooting deaths and the violent-crime arrest rate for minors are associated with CAP laws.[38] The results suggest that these measures do not predict the adoption of CAP laws.

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 threatened or injured with a weapon on school property or miss school for fear of their safety. Here we test whether the CAP-law effect extends to school shootings.

4.1. Data on School Shootings

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 report on school-associated violent deaths and covers 1992–2010.[39] To achieve the most complete coverage of these events possible, we use the following additional data sources to include events missing from National School Safety Center (2010):

[38] The sample size for the violent-crime regressions is smaller because not all states provided data to the Uniform Crime Report each year during 1991–2013.

[39] The National School Safety Center report 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 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" (National School Safety Center 2010).

Table 9

School Shootings, Violent-Crime Rates, and Child-Access-Prevention Laws

	(1)	(2)
Shooting deaths:		
Any death	−.001	−.013
	(.020)	(.014)
1-Year lag		.008
		(.019)
2-Year lag		.004
		(.014)
3-Year lag		−.002
		(.013)
Mean	.454	.499
N	1,173	1,020
Counts of shooting deaths:		
Number involving any death	−.002	−.002
	(.009)	(.006)
1-Year lag		−.006
		(.013)
2-Year lag		.001
		(.009)
3-Year lag		.001
		(.006)
Mean	.454	.499
N	1,173	1,020
Violent-crime arrest rate:		
Rate	.001	.0004
	(.001)	(.0003)
1-Year lag		.0001
		(.0003)
2-Year lag		.0003
		(.0003)
3-Year lag		.0004
		(.0003)
Mean	.451	.503
N	1,132	961

Note. Column 1 presents estimates from ordinary least squares regressions based on data for 1991–2013; column 2 presents estimates from an ordinary least squares regressions based on data for 1994–2013. Dependent variable means are reported. Data are for shooters or arrestees under age 18. All models control for the full set of state-level covariates, 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.

Lieberman (2006), National School Safety and Security Services (2010), Van Fleet and Van Fleet (2010), Klein (2012), D. Stafford & Associates (2014), Angels of Columbine (2016), Doll (2015), Everytown for Gun Safety Support Fund (2015b), and Laurine (2017). These sources, in addition to our own searches of

newspaper archives, allow us to extend our coverage from 1991 to 2013. A benefit of our data set is that it represents a balanced panel and covers a longer period of time than the YRBS data, which allows us to capture additional policy variation.[40]

For the analysis below, we restrict our focus to school shootings in which a death occurred (homicide, suicide, or accidental).[41] Our final data set includes information about when and where the shooting took place, the age of the shooter, and (when available) whether the shooting was reported to be 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 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 167 school-associated shooting deaths in which the shooter was under 18 and 187 deaths in which the shooter was 18 or older. We observe 76 school shootings involving more than one death and 12 involving four or more deaths.[42]

4.2. 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-differences model:

$$Y_{st} = \beta_0 + \beta_1 \text{CAP Law}_{st} + \boldsymbol{X}_{st}\boldsymbol{\beta}_2 + \boldsymbol{\nu}_s + \boldsymbol{w}_t + \nu_s \times t + \varepsilon_{st}. \quad (2)$$

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 vector $\boldsymbol{X}_{st}$ includes the same state-level controls used in equation (1).[43] Again, the parallel-trends assumption must be satisfied to generate unbiased estimates of β_1. All regressions are estimated as linear proba-

[40] We observe pre- and posttreatment data for 23 states (California, Colorado, Delaware, Georgia, Hawaii, Illinois, Indiana, Kentucky, Maryland, Massachusetts, Minnesota, Mississippi, Nevada, New Hampshire, New Jersey, North Carolina, Oklahoma, Rhode Island, Tennessee, Texas, Utah, Virginia, and Wisconsin) and the District of Columbia.

[41] 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 National School Safety Center (2010), 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 three observations. Our results are similar when we drop those three 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.

[42] 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 (2017), and Willingham and Ahmed (2017). Other research has focused on "rampage" school shootings, defined as "expressive, non-targeted attacks on a school institution" (Muschert 2007, p. 63). For qualitative studies on rampage school shootings, see Larkin (2009) and Newman and Fox (2009).

[43] Descriptive statistics for the state-level controls are provided in Online Appendix Table OA9.

Table 10

Descriptive Statistics for State-Level School-Shooting Analysis, 1991–2013

	CAP Law	No CAP Law	Full Sample	Definition
Shooter under 18:				
Any Death	.230	.185	.211	Equals one if there was a school shooting involving a death in state *s* during year *t*
	(.421)	(.388)	(.408)	committed by someone under 18 and zero otherwise
Suicide	.140	.072	.112	Equals one if there was a school shooting involving a suicide in state *s* during year *t*
	(.348)	(.259)	(.316)	committed by someone under 18 and zero otherwise
Homicide	.141	.128	.136	Equals one if there was a school shooting involving a homicide in state *s* during year *t*
	(.348)	(.334)	(.342)	committed by someone under 18 and zero otherwise
Shooter 18+:				
Any Death	.326	.191	.271	Equals one if there was a school shooting involving a death in state *s* during year *t*
	(.469)	(.393)	(.444)	committed by someone 18 or older and zero otherwise
Suicide	.142	.068	.112	Equals one if there was a school shooting involving a suicide in state *s* during year *t*
	(.350)	(.251)	(.315)	commited by someone 18 or older and zero otherwise
Homicide	.278	.168	.233	Equals one if there was a school shooting involving a homicide in state *s* during year *t*
	(.448)	(.374)	(.423)	commited by someone 18 or older and zero otherwise
N	543	630	1,173	

Note. Weighted means are shown, with standard deviations in parentheses. CAP = child-access prevention.

Table 11
Child-Access-Prevention Laws and the Probability of a School Shooting

	Shooter under 18			Shooter 18+		
	Any Death	Suicide	Homicide	Any Death	Suicide	Homicide
CAP Law	−.008	.046	−.049	.064	.065	−.014
	(.131)	(.080)	(.085)	(.136)	(.076)	(.092)
Mean	.211	.112	.136	.271	.112	.233

Note. Estimates are from an ordinary least squares regression based on school-associated shooting death data for 1991–2013. Dependent variable means are reported. All models control for the full set of covariates, 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. $N = 1{,}173$.

bility models and are weighted by the population of state s in year t.[44] Standard errors are corrected for clustering at the state level (Bertrand, Duflo, and Mullainathan 2004).

4.3. Results

Table 11 presents estimates of β_1 from equation (2). We disaggregate school-shooting events resulting in a death into those involving suicide or homicide. 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 nontrivial 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].[45] With many explanatory variables and a relatively small number of events, it is difficult to detect an effect.

We next subject the null findings to a number of sensitivity checks. First, because our school-shooting data set includes some gang-related events, we focus on shootings for which there was no mention of gang involvement.[46] Non-gang-related shootings are more often considered 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 find no evidence to suggest that CAP laws are effective at reducing the likelihood of these events. Second, we replace CAP Law

[44] To retain our 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 do, 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 include a fatal school-shooting event committed by minors and nonminors, respectively. Results from these specifications are qualitatively similar to those presented here and are available from the authors on request.

[45] Of the six other gun-related policies that we include as controls, the coefficient estimates on Shall Issue Law, Trigger Lock Law, and Stand Your Ground Law are consistently negative in sign but never statistically distinguishable from 0.

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

with an indicator for the year of the change in law and a series of leads and lags. Unlike the YBRS results in Table 7, we find no evidence of a lagged CAP-law effect.[47] Third, we examine whether there are heterogeneous effects by the type of law adopted. Our results suggest that neither negligent-storage nor reckless-endangerment CAP laws are associated with fewer school-shooting deaths.[48] 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 25 percent of parents are "very concerned" about school violence (C. S. Mott Children's Hospital 2017). These fears are perhaps driven by the fact that school shootings have been reported at nearly a weekly rate since 2012 (Everytown for Gun Safety Support Fund 2015b). While there is a wealth of research on the individual-level correlates of youths' gun 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 CAP laws. Using data from the national and state YRBS for 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 because of 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 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.

[47] These results are reported in Online Appendix Table OA10.

[48] For the sake of brevity, we omit the results for non-gang-related shootings and negligent-storage versus reckless-endangerment laws. Because some states define a minor using an age threshold under 18, we also experimented with restricting each state's sample of shootings using its own definition of a minor. Under this scenario, we find little evidence to suggest that CAP laws are associated with fewer school-associated shooting deaths. Finally, the results for shooters who are 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 (ages 21 and older or 24 and older). All of these results are available from the authors on request.

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