Fall 2019

Gun Policy and Youth Behavior

Ioannis Koutsonikolis

GUN POLICY AND YOUTH BEHAVIOR

by

Ioannis Koutsonikolis

A thesis submitted in partial fulfillment of the requirements
for graduation with Honors in the Economics

Jeffrey DeSimone
Thesis Mentor

Fall 2019

All requirements for graduation with Honors in the
Economics have been completed.

Steven Stong
Economics Honors Advisor

Gun Policy and Youth Behavior

For Completion of Honors in the Major: Economics
University of Iowa

Ioannis Koutsonikolis
Advised by Professor Jeffrey DeSimone

December 18, 2019

Contents

Produced using LaTeX. Thank you LaTeX!

Abstract: This thesis examines gun carrying and related behaviors in national and state Youth Risk Behavior Survey (YRBS) data, utilizing a new firearm law database from the RAND Corporation. I have three primary goals. The first is to replicate the results found in Anderson & Sabia (2018). Second, I explore the effects of background check and mandatory safety training firearm regulations. Finally, I probe into whether YRBS is a valuable measure of youth gun carrying behavior as evidence of the potential for violent behavior. I was unable to replicate the results of Anderson & Sabia (2018), and I did not find a consistent effect of background check regulation on dependent variables of interest (such as youth gun carrying, or perceived school safety). I do find significant effects of mandatory safety trainings on improving the school environment. However, the causal mechanism is unclear in this case. Overall, the results suggest that reported gun carrying in the YRBS does not necessary reflect gun carrying with nefarious intent.

1 Introduction

Among the most contentious and persistent issues that have come to the forefront of the public discourse over the past decade is the regulation of firearms. In that time, there have been several instances of multiple mass shooting events occurring within short time periods, so that the news cycle is dominated by coverage of these events for many days. In the recent past, several mass shootings in public spaces have represented record breaking incidences of violence, such as the shootings in Las Vegas and at PULSE nightclub in Orlando (CNN, 2019). Even as I write this, news broke regarding a high school shooting in Santa Clarita California, in which five students were injured, two fatally (Lam, 2019).

Much of the discussion surrounding these events has centered around what kinds of state and federal regulations on firearms could potentially prevent repeated occurrences. While it is unlikely that any individual policy proposal could stop all mass shooting incidents, we might see that on average some policies have greater effects than others on decreasing the occurrence of such events.

Specifically with respect to mass shooting events in K-12 schools, the issue of regulations having a significant impact on gun related behaviors of youth is complicated by the fact that in most cases minors are not allowed legal ownership of a firearm (with some exceptions for possession such as hunting). So to reduce incidences of firearm related school violence proposed regulations would likely need some intermediate causal mechanism.

A class of firearm regulations known as Child Access Prevention (CAP) laws put a burden on gun owners to either safely store a firearm or to not recklessly provide minors access to a firearm. These regulations place criminal liability on the owner of a gun if a minor should illegally possess or commit a crime with a firearm. CAP laws, however, are typically not mentioned in the news cycles when mass shooting events occur. More standard policy proposals include universal background checks, gun registries, or even mandatory safety trainings.

The causal mechanism for some of these policies in reducing school related gun violence is unclear and is, in part, the motivation for this study. I hope to begin parsing potential causal relationships that exist between these policy objectives and their intended consequence of a reduction in youth and school related gun violence. I do this by applying existing variation in these gun policies in existence and timing of implementation across U.S. states to YRBS data, as described in section 3.

2 A Brief Review of Anderson & Sabia (2018)

A significant portion of this paper is devoted to replicating Anderson & Sabia, who use YRBS data from 1993 - 2013 combined with information from the Giffords Law Center website to analyze the effect of CAP laws on youth gun carrying.[1] Their primary result is that the presence of a CAP law is associated with a statistically significant, 19% decrease in reported youth gun carrying in the past 30 days. They also find some statistically significant improvements in the safety of the school environment based on the presence of a CAP law, such as decreases in the proportions of students reporting skipping school because they felt unsafe and that were threatened by a weapon at school.

Additionally, Anderson & Sabia (2018) conduct an analysis of the effect of CAP laws on school shootings , finding noisy but insignificant effects on the change in likelihood of a mass shooting event based on the presence of a CAP law. Furthermore, shooting events are separated into a subset of those that are not gang related, and in this setting the effects of the presence of a CAP law are insignificant as well.

[1] https://lawcenter.giffords.org

3 Data

3.1 YRBS

The primary data source for this project is the Youth Risk Behavior Survey (YRBS).[2] The YRBS is administered in odd numbered years to high school students in the United States both nationally and by individual states (to generate nationally and state-representative estimates, respectively, of prevalence rates of risky behaviors and outcomes).[3] At the national level, a three-stage cluster design is utilized. In the first stage, YRBS-created primary sampling units, which consist of a single county or a collection of adjacent counties, are randomly selected. From these PSU's, schools are selected with probability proportional to enrollment. Finally, in the third stage, one or two classes per grade per school are selected at random to participate. The process is similar in generating representative state samples, but without the first stage. The survey is administered to high schoolers during the spring of an academic year, typically sometime from February through May (Centers for Disease Control and Prevention, 2013).

The survey seeks to monitor the prevalence of risky behaviors and outcomes in youth. Of primary interest in the following analysis is the prevalence of youth gun carrying behaviors, as well as the prevalence of school environment outcomes. Other examples of monitored behaviors include drug use, sexual activity, and violence.

With respect to the primary dependent variables in this analysis, I use YRBS data from 1993-2015. Data from this period are used because during this time a series of questions regarding gun and gun carrying behavior were asked in a consistent manner. Specifically, the survey question of particular interest asks, "During the past 30 days, on how many days did you carry a gun?". Unfortunately, the wording of this question was changed in the 2017 iteration of the YRBS, resulting in non-comparability with responses from previous years. All other questions, which are used to generate binary dependent variables and are described in the following paragraph or table, were asked consistently during the 1993-2015 period.

A primary outcome of interest is gun carrying among youth. In particular I look to find a causal relationship between implementation of gun related legislation and youth gun carrying behavior. The dependent variable in this case is constructed from the aforementioned gun carrying question. Possible multiple choice responses from the YRBS include 0 days, 1 day, 2 or 3 days, 4 or 5 days, or 6 or more days. These responses were coded as a binary variable with

[2] https://www.cdc.gov/healthyyouth/data/yrbs/index.htm

[3] Not every state is surveyed by the national YRBS every year. Likewise not every state administers its own YRBS every year. A table describing observations by state-year is in Appendix 7.1. Because my empirical model, as outlined in section 4, includes state fixed effects as well as state-specific linear time trends, three years of data are required for a given state to contribute to model identification.

0 days being 0, and all other responses (i.e. any positive number of days carrying a gun in the past 30 days) coded as 1.

Table 3.1: Description of Binary Dependent Variables from YRBS

Question Number	Question Text	0 Value	1 Value
q13	During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club?	Did not carry a weapon during the past 30 days	Carried a weapon at least once during the past 30 days
q14	During the past 30 days, on how many days did you carry a gun?	Did not carry a gun during the past 30 days	Carried a gun at least once during the past 30 days
q15	During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club on school property?	Did not carry a weapon on school property during the past 30 days	Carried a weapon on school property at least once during the past 30 days
q16	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?	Did not skip school during the past 30 days for safety concerns	Skipped school at least once during the past 30 days for safety concerns
q17	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?	Was not threatened or injured with a weapon on school property in the past year	Was threatened or injured with a weapon on school property at least once during the past year
q18	During the past 12 months, how many times were you in a physical fight?	Was not in a physical fight during the past year	Was in a physical fight at least once during the past year
q19	During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?	Was not involved in a 'serious' physical fight during the past year	Was involved in a 'serious' physical fight at least once during the past year
q20	During the past 12 months, how many times were you in a physical fight on school property?	Was not in a physical fight on school property during the last year	Was in a physical fight on school property at least once during the last year

An excerpt of the YRBS codebook can be found in the Appendix, section 7.1. This excerpt details the exact questions, and possible answers, presented to respondents in the YRBS survey. Table 3.1 details the corresponding binary variables that I coded based on these YRBS questions.

With respect to the YRBS data in general, there are several questions requesting demographic information from participants. I used these demographic variables as individual level controls in the analysis. These variables included a binary variable for sex (male=0, female=1), a categorical variable for grade (covering grades 9-12), a categorical variable for age (covering ages 12-17 and 18+ as a group), and a 4-level categorical variable for race (White, Black/African American, Hispanic/Latino, other). These categorical variables are included as sets of binary variables - indicating an observation falling into one of the variable's categories (e.g. for a 9^{th} grader, the 9^{th} grade indicator would have value 1 and all other grade values would have a value of 0). I dropped observations that were missing any of this demographic information.

In early iterations of this analysis, I also dropped observations from 12 and 13 year olds. Because he standard age that one starts high school is between 14 and 15, I argued that 12 and 13 year old high schoolers would not be representative of the average high school student. After further consideration I realized that there was a potential inconsistency in proceeding in this manner. I could remove the 12 and 13 year olds from the sample, but because there is no longitudinal mapping of respondents, it would be difficult to also remove the "former" 12 and 13 year old high schoolers, as their ages would appear to be in the normal range after a year or two of enrollment. Accommodations could be made to drop specific age-grade combinations (e.g. any 12 and 13 year olds, any 14 year olds in 10th grade, 15 year olds in 11th grade, etc.), but natural variation in age of enrollment in high school may make it unreasonable to drop 'young' high schoolers in general.

3.2 Gun Policy Databases

Deviating from the previous literature, I take advantage of two new datasets which aggregate information on gun regulation legislation by state. These sources are the Boston University (BU) State Firearm Laws Database and the RAND Corporation's State Firearm Law Database.[4] The RAND Corporation updates their database periodically; in this analysis, Version 2.0, released on October 10, 2019, was used.

These databases allow me to simultaneously verify the robustness of the results of prior literature, where research questions overlap, and evaluate the accuracy of the databases in question. With these results in mind, I can move forward, examining new questions relating to the efficacy of gun control legislation on youth behaviors.

In attempting to attain the first goal of this article, which is to replicate the relevant results from Anderson & Sabia (2018), the first iterations of the analysis were conducted using gun law variables from the BU Database. However, I found significant mismatches in the identified gun laws between the coding in Anderson & Sabia (2018) and the BU database. Furthermore, the BU Database appears to describe only CAP Laws that regulate "negligent storage". Considering

[4]http://www.statefirearmlaws.org & https://www.rand.org/pubs/tools/TL283-1.html respectively

this, I switched my primary data source to the RAND Corporation's database. In particular, using the RAND database, I generated separate variables for CAP laws categorized as "reckless endangerment", this typically means that an adult is held criminally liable for the knowing provision of a firearm to a minor, and as "negligent storage", which typically holds a gun owner liable for the improper storage of a firearm. There is some variation in the severity of the punishment, and in the definition of a minor, by state. I also code a number of other binary state law variables that are analyzed. These include background check laws, which impose some verification of eligibility to purchase or possess a firearm at some point in the purchasing process (some states require a background check to receive a permit to purchase a firearm while others require a background check at the point of purchase), and mandatory safety training laws, which typically require the completion of some sort of firearm safety training in order to be eligible to purchase or possess a firearm. Tables 3.2 through 3.10 list the states that have each type of law that I examine, along with the date the law became effective in each case.

Table 3.2: CAP Negligent Storage (RAND)

State	Effective Date	Coded Effective Year
California	1/1/92	1992
Connecticut	10/1/90	1991
DC	9/24/76	1977
°Delaware	7/12/94	1995
Florida	10/1/89	1990
Hawaii	6/29/92	1993
Iowa	4/5/90	1991
°Illinois	1/1/00	2000
°Massachusetts	10/21/98	1999
Maryland	10/1/92	1993
°Minnesota	8/1/93	1994
°North Carolina	12/1/93	1994
°New Hampshire	1/1/01	2001
New Jersey	1/17/92	1993
Nevada	10/1/91	1992
°Rhode Island	6/19/95	1996
°Texas	9/1/95	1996
Virginia	7/1/92	1993
Wisconsin	4/16/92	1993

States marked ° are those whose policies vary within the sample period

Table 3.3: CAP Reckless Endangerment (RAND)

State	Effective Date	Coded Effective Year
°Colorado	9/13/93	1994
°Georgia	5/1/94	1995
°Indiana	3/15/94	1995
°Kentucky	7/15/94	1995
Missouri	9/28/81	1982
° Mississippi	7/1/94	1995
°Oklahoma	11/1/12	2013
°Pennsylvania **	10/11/95	1996
°Tennessee	4/15/94	1995
°Utah	10/21/93	1994

** Pennsylvania's law is described as "permissive" in the RAND database

States marked ° are those whose policies vary within the sample period

I generate indicator variables that are coded as 1 when a state-year combination has an active gun law of a given type, and as 0 otherwise. Because the YRBS is administered in the spring of an academic year, and the fact that there is variation in when exactly the survey is taken, any law that is made effective after January of a given year is coded as effective only beginning in the next year. For even numbered years, this is the same as being coded on the next odd numbered year because of the biannual administration of the survey, but for clarity and simplicity, I use the following year. Laws used in the analysis conducted by Anderson & Sabia can be found in appendix 7.2.

Table 3.4: Background Check for a Permit to Purchase a Handgun (RAND)

State	Effective Date	Coded Effective Year
°Connecticut	10/1/95	1996
°DC*	6/26/08	2009
Hawaii	7/1/27	1928
Iowa	4/5/90	1991
Illinois**	7/1/68	1969
Massachusetts	1/1/69	1969
°Maryland	10/1/13	2014
Michigan	9/5/27	1928
° Missouri***	8/28/07	2008
° North Carolina	12/1/95	1996
Nebraska	9/5/91	1992
New Jersey	3/30/27	1928
New York	7/1/34	1935
Rhode Island	1/26/59	1959

* DC is absent from our YRBS sample.

** Illinois modified this law in 2013

*** Missouri Repealed a 1981 law in late 2007

States marked ° are those whose policies vary within the sample period

Table 3.5: Background Check for sales of Handguns from Private Dealers (RAND)

State	Effective Date	Coded Effective Year
California	1/1/91	1991
° Colorado	3/20/13	2014
°Connecticut	10/1/95	1996
°Delaware	7/1/13	2014
Hawaii	7/1/27	1927
Iowa	4/5/90	1991
°Maryland	10/1/96	1997
Michigan	9/5/27	1928
°Missouri *	8/28/07	2008
New Jersey	10/1/18	2019
Nevada	1/1/17	2017
New York	7/1/34	2935
Oregon	5/11/15	2016
Pennsylvania	10/11/95	1996
Rhode Island	1/26/59	1959
Tennessee **	11/1/98	1999
Vermont	4/11/18	2019

* Missouri Repealed a 1981 law in late 2007

** Tennessee Repealed a 1959 law in late 1998

States marked ° are those whose policies vary within the sample period

I omit consideration of laws requiring background checks on licensed dealers, as there was federal legislation in the mid and late 1990's that severely restricts the variation available in the sample period. Once the federal legislation took effect, there is no longer any state-year variation in policy. According to the RAND Corporation's database any subsequent state legislation that was passed was precautionary in that it mandated some sort of background check on the sale of firearms from licensed dealers if the Brady Act, which in 1994 instituted a 5-day waiting

period on firearms purchases, was repealed. Specifically the subsequent 1998 enactment of the NICS background check system mandated background checks for all firearm sales from federally licensed dealers (103rd Congress of the United States, 1993).[5]

Table 3.6: Background Check for Sales of Long Guns from Private Dealers (RAND)

State	Effective Date	Coded Effective Year
°Arizona	4/19/94	1995
California	1/1/91	1991
°Colorado	3/20/13	2014
°Connecticut	12/1/00	2001
°Delaware	7/1/13	2014
Hawaii	7/1/27	1928
°Idaho	4/7/94	1995
Nevada	1/1/17	2017
°New York	3/16/13	2014
Oregon	5/11/15	2016
°Pennsylvania	10/11/95	1996
Rhode Island	7/1/90	1991
Vermont	4/11/18	2019
°Washington	12/4/14	2015

States marked ° are those whose policies vary within the sample period

Table 3.7: Background Check for a Permit to Purchase a Long Gun (RAND)

State	Effective Date	Coded Effective Year
° Connecticut	4/1/14	2015
DC*	9/24/76	1977
Hawaii	7/1/27	1928
Illinois	7/1/68	1969
New Jersey	1/1/70	1970

* DC is absent from our YRBS sample.
States marked ° are those whose policies vary within the sample period

Table 3.8: Safety Training Required: Long Guns (RAND)

State	Effective Date	Coded Effective Year
°California	1/1/15	2015
°DC*	3/31/09	2010
°Massachusetts	6/1/98	1999

* DC is absent from our YRBS sample.
States marked ° are those whose policies vary within the sample period

Table 3.9: Safety Training Required to Carry: Handguns (RAND)

State	Effective Date	Coded Effective Year
° Connecticut	10/1/1998	1999

States marked ° are those whose policies vary within the sample period

[5]https://www.fbi.gov/services/cjis/nics

Table 3.10: Safety Training Required: Handguns (RAND)

State	Effective Date	Coded Effective Year
°California	1/1/03	2003
°Connecticut	10/1/94	1995
°DC*	3/31/09	2010
°Hawaii	7/1/95	1996
°Massachusetts	6/1/98	1999
°Maryland	10/1/13	2014
Rhode Island	1/1/80	1980

* DC is absent from our YRBS sample.
States marked ° are those whose policies vary within the sample period

3.3 WISQARS

The CDC maintains the Web-based Injury Statistics Query and Reporting System (WISQARS).[6] This database holds information on fatal and non-fatal injuries in the United States, as well as data on population estimates by state and various demographic characteristics. I use these data as a weighting mechanism in the statistical analysis that follows. Specifically, mimicking the methodology of Anderson & Sabia (2018),I assign to each observation a population weight based on state, year, race, age, and sex. The purpose of this is ensure that the YRBS data are nationally representative. Additionally, I obtain annual state population totals from the WISQARS database to generate the per capita variable which is discussed next.

3.4 Hunting Licenses

Siegel et al. (2014) finds that hunting licenses per capita contribute to a combined measure serving as a proxy for gun prevalence in a state, suggesting that on its own it can serve as a valuable state-level control. From the US Fish And Wildlife Service's National Hunting License Report, the same source as was used in Siegel et al. (2014), aggregate numbers of hunting licenses by state and year were obtained.[7] These data were combined with state-year populations to generate a hunting licenses per capita control variable. Considering that the proxy developed by Siegel et al. was simply a linear combination of the fraction of total suicides committed by firearm and the per capita hunting license measure, it is appropriate to use per capita hunting licenses as a control in this linear regression setting .

[6] https://www.cdc.gov/injury/wisqars/index.html
[7] https://wsfrprograms.fws.gov/Subpages/LicenseInfo/Hunting.htm

4 Methods

The RAND database provides a valuable tool to analyze the impact of a number of gun laws on potentially related youth behaviors. The empirical approach used in this analysis is similar to those undertaken in other analyses of the impact of gun laws on individual level outcomes (DeSimone, Markowitz, & Xu, 2013; Anderson & Sabia, 2018). In general, the econometric model specification is as follows:

$$\Theta_{ist} = \beta_0 + \beta_1[Policy]_{st} + \beta_2\vec{\mathbf{\Gamma}} + \vec{\boldsymbol{\sigma_s}} + \vec{\boldsymbol{\tau_t}} + \vec{\boldsymbol{\sigma_s}} \times t + u_{ist}$$

This model specification uses the existing state-year variation in the United States regarding active gun policies of different categories. The indices represent observations across individuals (i), states (s), and time/years (t).

Θ_{ist} describes the individual level outcomes on the dependent variable, e.g. whether or not an individual has carried a gun in the past 30 days. All dependent variables are described in table 3.1. Although the specific outcomes will vary, every dependent variable has been coded as a binary variable with 0 representing failure to engage in some behavior in a given time frame and 1 representing any engagement in a given behavior in a give time frame (e.g. a student skipping school because they felt unsafe within the last 30 days). β_0 is the constant term. β_1 is the primary coefficient of interest and represents the increase or decrease in probability of engaging in some behavior depending on the presence or absence of a given gun related policy. The variable $[Policy]$ represents one of the previously described gun policy measures. The variation in $[Policy]$ by state and year, based on the enactment or repeal of a given type of firearm regulation, is exactly the variation I am exploiting to conduct this analysis.

$\vec{\mathbf{\Gamma}}$ represents a vector containing all individual level controls, i.e. the age, race, grade and sex of the individual. $\vec{\boldsymbol{\sigma_s}}$ and $\vec{\boldsymbol{\tau_t}}$ are state and year fixed effects, respectively, so that the interaction $\vec{\boldsymbol{\sigma_s}} \times t$ represents linear trends over time for each state, and u is the error term.

5 Results

5.1 Replication

As mentioned, the first portion of this paper is a partial replication of Anderson & Sabia (2018). The following table is an attempted replication of their primary results. Their table (Table 3) can be found in appendix 7.2.

Table 5.1: CAP Laws and YRBS Outcomes - Replication

	Carry Gun	Carry Weapon	Carry Weapon at School	Threatened by Weapon at School	Skip School - Unsafe
Under 18					
Any	0.00263	-0.00275	0.00560	-0.0103*	-0.00938
CAP Law	(0.00780)	(0.00877)	(0.00731)	(0.00491)	(0.00700)
Mean	0.053	0.170	0.056	0.071	0.059
N	635691	821049	867925	893474	916711
18 +					
Any	-0.00726	-0.00407	-0.00351	-0.00745	-0.00159
CAP Law	(0.00925)	(0.0120)	(0.0103)	(0.00554)	(0.00886)
Mean	0.065	0.182	0.067	0.065	0.061
N	97006	114685	122416	125421	128248

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are also reported. Standard errors are corrected for clustering by state. Population weights from WISQARS are used. DDD results were not replicated.

In general, I was unable to replicate the results produced by Anderson & Sabia. On the primary dependent variable of interest "Carry Gun", which indicates having carried a gun during the past 30 days, Anderson & Sabia report a coefficient of -0.010, with significance at the 10% level, while I find a coefficient that is smaller, positive, and highly insignificant. In general, the effect sizes under my specification are smaller, with signs that are not systematically related to those presented in Anderson & Sabia (2018). It is important to note that one would expect a negative and significant effect on gun carrying in the under 18 cohort, as this is the behavior and group that the law is meant to impact.

Some other results were more comparable. In particular, I find that the presence of a CAP law decreases the probability of being threatened by a weapon at school with a coefficient and

standard error of -0.0103 (0.0049), with Anderson and Sabia finding -0.014 (0.005). For this age group, the effect on missing school because of safety likewise had a similar effect size, but was insignificant. The results for the over 18 group more closely replicate their work, but the effect sizes I find are smaller. Nonetheless, the effects are insignificant, which is consistent with Anderson & Sabia (2018).

There are some differences between our specifications that may contribute to the differing results, but individually do not seem to be problematic. There are immediate differences in the sample. I was able to use YRBS data from 2015, so that my sample period is 1993-2015. In Anderson & Sabia, YRBS data from 1993-2013 are used. Alone, this does not seem as though it should radically alter the results, as I add one year of additional respondent data.

I also tend have fewer observations; in general, for any given model specification, the number of observations tends to differ. This is likely due to the fact that I am only able to use YRBS data that is publicly available: Anderson & Sabia obtained data from some states that do not contribute to the CDC-published combined file that I analyze. However, this discrepancy varies by the particular YRBS question. For example, on the "Carry Gun" variable, I have approximately 37,000 fewer observations, but on the variable "Carry Weapon" I have approximately 21,000 more observations. Additionally, there are differences in CAP law coding, although I do not have an explanation for this. Regarding the sample, I have omitted YRBS data from Washington D.C. because the BU gun law database that I originally intended to use does not have laws coded for D.C. and as such, policy variation from Washington D.C. has been excluded from this analysis. Finally, Anderson and Sabia use fractional values for the $[Policy]$ variable in the year that a given policy was enacted, while I code the presence of a policy in a given state-year as 1 if the policy was enacted on January 1 of that year, and 0 if otherwise. Then the following state-year policy observation is coded as 1.

Although our model specifications are quite similar, there are some differences. I utilize a Linear Probability Model, while Anderson & Sabia utilize probit. LPM rarely produces coefficients that are substantially different from probit marginal effects with the same data and specification, so this difference in method seems unlikely to cause the discrepancy.

Table 5.2 is the attempted replication wherein the effects of reckless endangerment and negligent storage CAP laws are examined in isolation to see if there are heterogeneous effects by law type. The results from Anderson & Sabia (2018) (Table 4) can be found in the appendix 7.2.

Again, the primary result, which is the effect of the presence of a CAP law on the proportion of YRBS respondents having carried a gun in the past 30 days, did not replicate. Anderson & Sabia find a coefficient of -0.013 (0.006), which is significant at the 5 % level, for the effect of a negligent storage CAP law on reported gun carrying by students under 18, while I find a coefficient of -0.00938 (0.007), which is insignificant at the 10 percent level. Unlike in the previous table, the direction of the effect is consistent, but nonetheless, the significance of the effect does not hold.

Table 5.2: Reckless Endangerment and Negligent Storage CAP Laws - Replication

		Carry Gun	Carry Weapon	Carry Weapon at School	Threatened by Weapon at School	Skip School - Unsafe
Under 18	Negligent Storage	0.00832	0.000142	0.00853	-0.00680	-0.00157
		(0.00859)	(0.0104)	(0.0102)	(0.00665)	(0.00743)
	N	635691	821049	867925	893474	916711
	Reckless Endangerment	-0.00609	-0.00618	-0.000316	-0.0119^{+}	-0.0177**
		(0.00787)	(0.00725)	(0.00836)	(0.00699)	(0.00531)
	N	635691	821049	867925	893474	916711
18 +	Negligent Storage	0.00709	0.00114	-0.00312	-0.000770	0.00602
		(0.00579)	(0.00799)	(0.0133)	(0.00655)	(0.0100)
	N	97006	114685	122416	125421	128248
	Reckless Endangerment	-0.0302*	-0.0121	-0.00294	-0.0166^{+}	-0.0149*
		(0.0143)	(0.0292)	(0.0139)	(0.00933)	(0.00703)
	N	97006	114685	122416	125421	128248

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are as in Table 5.1. Standard errors are corrected for clustering by state. Population weights from WISQARS are used. DDD results were not replicated.

I do find that the presence of a reckless endangerment CAP law is associated with a significant decrease in the likelihood of a student under the age of 18 skipping school because of feeling unsafe, but absent a causal mechanism (i.e. a decrease in gun carrying), the cause of this effect is unclear. The result is similar in the over 18 group, and in that group, the presence of a reckless endangerment CAP law is associated with a decrease in gun carrying. This is discussed further in the next section. The results for the 18+ group of students, under a negligent storage CAP law, replicate Anderson & Sabia (2018) in that they are generally insignificant and of a similar magnitude. However, the direction of the effect does not always match.

5.1.1 Consistency of Downstream Effects

In light of the absence of a statistically significant effect of CAP laws on youth gun carrying, one would expect that the CAP laws would also not have a "downstream" effect on dependent variables that describe the school environment. This section provides a slight extension to Anderson & Sabia (2018) in evaluating these additional outcome variables. The following table 5.3 confirms this hypothesis with respect to heterogeneous effects. One can see an absence of any general pattern for which the presence of a CAP law is associated with a statistically significant and negative relationship with the dependent variables listed. Furthermore, when the age groups over 18 and under 18 are separated, there is no general pattern that suggests that CAP law will improve the school environment.

Table 5.3: CAP Laws and YRBS Further Outcomes

		Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
	Under 18	-0.00938	-0.0103*	0.000514	-0.00121	-0.000315
		(0.00700)	(0.00491)	(0.00374)	(0.00559)	(0.00978)
Any	N	916711	893474	713193	872960	784310
CAP	18 +	-0.00159	-0.00745	-0.00154	0.0152*	0.0283+
Law		(0.00886)	(0.00554)	(0.00690)	(0.00690)	(0.0157)
	N	128248	125421	110085	124947	117624
	Under 18	-0.00157	-0.00680	0.00648+	-0.00297	-0.000143
		(0.00743)	(0.00665)	(0.00370)	(0.00560)	(0.0128)
Negligent	N	916711	893474	713193	872960	784310
Storage	18 +	0.00602	-0.000770	-0.00735	0.0262**	0.0332+
		(0.0100)	(0.00655)	(0.00592)	(0.00494)	(0.0189)
	N	128248	125421	110085	124947	117624
	Under 18	-0.0177**	-0.0119+	-0.00810*	0.00166	-0.000475
		(0.00531)	(0.00699)	(0.00333)	(0.0101)	(0.00960)
Reckless	N	916711	893474	713193	872960	784310
Endanger-	18 +	-0.0149*	-0.0166+	0.00956	-0.0107	0.00890
ment		(0.00703)	(0.00933)	(0.0121)	(0.0118)	(0.0206)
	N	128248	125421	110085	124947	117624

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

However, in evaluating the school environment, it may not be suitable to separate the age groups. This was done in earlier portions of this analysis because we would expect CAP laws to bind youth under the age of 18 in particular, by the nature of the typical definition of a minor. However, the school environment is integrated, so it may be reasonable to view the results pertaining to the school environment in an aggregated table. Doing so in Table 5.4, the presence of a reckless endangerment CAP law has a negative and significant effect on three dependent variables that describe the school environment: skipping school because of feeling unsafe, being threatened with a weapon, and being in a serious fight in the last 12 months.

All of these downstream effects would be consistent with a decrease in gun carrying by students. But lacking a concrete causal mechanism, this issue needs further analysis. The primary issue is that I find a significant decrease in gun carrying among individuals over 18, who I do not expect CAP laws to bind.

Table 5.4: CAP Laws and YRBS Further Outcomes - All Ages

	Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
Any CAP Law	-0.00793	-0.0104*	-0.000202	0.000927	0.00349
	(0.00747)	(0.00462)	(0.00244)	(0.00523)	(0.00940)
Negligent Storage	0.000303	-0.00620	0.00387	0.00140	0.00497
	(0.00793)	(0.00608)	(0.00234)	(0.00532)	(0.0129)
Reckless Endangerment	-0.0177**	-0.0132+	-0.00612+	-0.0000700	0.000192
	(0.00544)	(0.00680)	(0.00332)	(0.00907)	(0.00756)
N	1044959	1018895	823278	997907	901934

Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used. DDD results were not replicated.

5.2 Background Checks

In Anderson & Sabia (2018), state-level firearm background checks laws are used as controls, but marginal effects are not presented. Here I provide a slight extension of their work by using the presence of background checks as a primary independent variable to explain the variation in gun carrying behavior in youth as well as in school environment responses.

Here there is a different hypothesis regarding which groups would be bound by the presence of a background check law: We might expect to see a negative and significant effect on gun carrying in the past 30 days by those over 18, rather than under 18, in this specification. This is because in the case of long guns, purchase and possession is legal by those over the age of 18. In the case of handguns, I do not expect to see this effect considering the federal age for purchase from licensed dealers is 21, not 18. However, the age for possession and unlicensed (private) transfer does vary by state ("Giffords Law Center to Prevent Gun Violence", 2018).[8] If direct purchase is not the causal mechanism, then background checks "screening out" individuals who may be more likely to, intentionally or unintentionally, allow their firearm to fall into the possession of a young person would be required.

Results are presented in Tables 5.5 and 5.6. The only background check law that had a statistically significant effect on gun carrying was the presence of a law requiring background checks on sales from private dealers for long guns. This supports the hypothesis above that those ages 18 and above are bound by these laws.

[8] Adding state level variation in minimum age for handgun possession and purchase as a control may be a valuable addition to this analysis and is discussed in section 6

Table 5.5: Effect of Background Check Laws on Gun & Weapon Carrying

		Carry Gun	Carry Weapon	Carry Weapon at School
	Under 18	0.00598	0.0174**	0.00494+
		(0.00733)	(0.00515)	(.0025)
Background	N	635691	821049	867925
Checks for	18 +	-0.0112	0.0294**	0.0122+
a Permit to		(0.00732)	(0.00675)	(0.00658)
Purchase:	N	97006	114685	122416
Handguns	All Ages	0.00389	0.0179**	0.00501+
		(0.00726)	(0.00451)	(0.00298)
	N	732697	935734	990341
	Under 18	-0.00791	-0.0229**	-0.00178
		(0.00507)	(0.00661)	(0.0123)
Background	N	635691	821049	867925
Checks for	18 +	-0.0159	-0.0630*	-0.0337**
Sales from		(0.0235)	(0.0249)	(0.00885)
Private Dealers:	N	97006	114685	122416
Handguns	All Ages	-0.00910	-0.0298**	-0.00700
		(0.00676)	(0.00425)	(0.0116)
	N	732697	935734	990341
	Under 18	-0.00591	0.0112	0.00751
		(0.00732)	(0.0138)	(0.00771)
†Any	N	635691	821049	867925
Background	18 +	-0.0337	-0.0426	-0.0157
Checks		(0.0229)	(0.0338)	(0.0128)
on Handguns	N	97006	114685	122416
	All Ages	-0.00922	0.00309	0.00393
		(0.00828)	(0.0152)	(0.00706)
	N	732697	935734	990341

Standard errors in parentheses $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

† "Any Handgun background Checks" is a binary variable that is 1 if either of "Background Checks for a Permit to Purchase: Handguns" or "Background Checks for Handgun Sales from Private Dealers" is 1.

The reported effects of background check laws on youth weapon carrying and weapon carrying at school are perplexing. Table 5.5 shows positive and significant effects of the presence of a background check law on weapon carrying and weapon carrying at school when the law requires a background check for a permit to purchase a handgun, but in general negative and significant effects when the law requires a background check to purchase a handgun from a private seller. If the results were consistent across these two types of laws, then an exploration on potential substitution effects between handguns and other "weapons" may have been warranted. However, such heterogeneity in the specific type of background check law on youth weapon carrying may suggest that the aggregate null effect (shown in "Any background Checks on Handguns") is most plausible.

Table 5.6: Effect of Background Check Laws on Gun & Weapon Carrying - Continued

		Carry Gun	Carry Weapon	Carry Weapon at School
	Under 18	-0.00163	0.00912^{+}	0.0116**
		(0.00347)	(0.00530)	(0.00320)
Background	N	635691	821049	867925
Checks for	18 +	-0.0317**	-0.0343	0.00262
Sales from		(0.00864)	(0.0205)	(0.00812)
Private Dealers:	N	97006	114685	122416
Long Guns	All Ages	-0.00485	0.00475	0.0107**
		(0.00352)	(0.00622)	(0.00331)
	N	732697	935734	990341
	Under 18	0.00158	0.0170	0.0121
		(0.00950)	(0.0135)	(0.00825)
	N	635691	821049	867925
‡Any	18 +	-0.0296	-0.0387	-0.00482
Background		(0.0222)	(0.0335)	(0.0137)
Checks	N	97006	114685	122416
	All Ages	-0.00219	0.00880	0.00944
		(0.0101)	(0.0150)	(0.00798)
	N	732697	935734	990341

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

‡ "Any Background Checks" is a binary variable that is 1 if any of the other variables in this table are 1.

The odd results carry into the analysis of the school environment. There are statistically significant reductions in skipping school because of feeling unsafe, and being threatened with a weapon on school property within the last year, when a background check is required for a permit to purchase a handgun. The effects for the other handgun background checks are also negative, which is at least a consistent result, although they are in general insignificant. However, there are statistically significant increases in reporting any fight in the last year or any serious fight.

Table 5.7: Effect of Background Check Laws on School Environment

	Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
Background Checks for a Permit to Purchase: Handguns	-0.0108^{+}	-0.0177^{+}	0.0203**	-0.00905	0.0778**
	(0.00548)	(0.00951)	(0.00595)	(0.00889)	(0.0251)
Background Checks for Sales from Private Dealers: Handguns	-0.0118	0.0176	-0.00295	-0.0130	-0.0816**
	(0.0102)	(0.0217)	(0.0143)	(0.0200)	(0.0221)
†Any Background Checks on Handguns	-0.0146	-0.00440	0.0154**	0.00356	-0.00149
	(0.00996)	(0.0318)	(0.00405)	(0.00930)	(0.0300)
Background Checks for Sales from a Private Dealer: Long Guns	-0.00696	0.0163^{+}	-0.00800	0.0219^{+}	0.0278
	(0.00721)	(0.00917)	(0.0113)	(0.0130)	(0.0277)
‡ Any Background Checks	-0.0114	0.00694	0.0142**	0.00988	-0.00335
	(0.0106)	(0.0311)	(0.00402)	(0.0107)	(0.0323)
N	1044959	1018895	823278	997907	901934

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

5.3 Mandatory Safety Training

The RAND Corporation's database on state firearm laws provides information on an interesting class of laws - those that require a user to undergo mandatory safety training. Typically, these laws require the possession of some sort of safety certification for a purchase to be completed, a permit to purchase to be issued, or for a private sale to occur legally (RAND Corporation, 2019).

Table 5.8: Safety Training and Gun/Weapon Carrying

		Carry Gun	Carry Weapon	Carry Weapon at School
	Under 18	-0.00273	0.00465	0.00175
		(0.00298)	(0.00492)	(0.00136)
Safety	N	635691	821049	867925
Training	18+	0.00992*	0.0203*	0.00505
Required :		(0.00473)	(0.00949)	(0.00327)
Handguns	N	97006	114685	122416
	All Ages	-0.000647	0.00609	0.00188
		(0.00280)	(0.00475)	(0.00126)
	N	732697	935734	990341
	Under 18	0.00310	-0.00758^{+}	-0.00587*
		(0.00284)	(0.00422)	(0.00249)
Safety	N	635691	821049	867925
Training	18 +	0.0162**	-0.00390	0.000703
Required:		(0.00462)	(0.00820)	(0.00428)
Long Guns	N	97006	114685	122416
	All Ages	0.00485^{+}	-0.00704^{+}	-0.00514*
		(0.00256)	(0.00415)	(0.00223)
	N	732697	935734	990341

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are also reported. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

For example, in California purchase, but not possession, of a firearm requires a Firearm Safety Certificate (FSC), which is valid for five years after being issued. Issuance requires passing a DOJ written test on firearm safety, the contents of which include basic safety principles, operation and handling of firearms, and descriptions of firearms laws (California DOJ, 2019). In Connecticut, in addition to passing a written exam, obtaining a license to hunt with firearms requires 16 hours of instruction (State of Connecticut, 2019a). Analogously, to apply for a permit to purchase a firearm in Connecticut, one must complete one of a list of approved courses (State of Connecticut, 2019b). In some states, even those under the age of 18 may be required to partake in the safety training, depending on the context. The effect of these regulations on youth carrying behavior are reported in table 5.8 and effects on the school environment are reported in table 5.9.

The results from table 5.8 show that required safety trainings (for both handguns and long guns) are associated with a statistically significant increase in reported gun carrying in the past 30 days, for the group of respondents ages 18 and above. The effects on weapon carrying at

school and in general appear to be ambiguous. Safety trainings for long guns are associated with statistically significant decreases in weapon carrying in general and at school for respondents under the age of 18, but these effects are not mirrored in the effects of mandatory handgun safety trainings.

Table 5.9: Safety Training and School Environment

	Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
Safety Training	-0.00231	-0.00263	0.000816	0.00357	0.0661**
Required: Handguns	(0.00643)	(0.00638)	(0.00244)	(0.00486)	(0.00478)
N	1044959	1018895	823278	997907	901934
Safety Training	-0.0147**	-0.00972**	-0.00786**	-0.0148**	-0.0366**
Required: Long Guns	(0.00299)	(0.00294)	(0.00253)	(0.00273)	(0.00697)
N	1044959	1018895	823278	997907	901934

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used. Heterogeneous effects by age can be found in table 7.3 in the appendix

The results in table 5.9 show that requiring safety training for long guns is consistently associated with statistically significant improvements in the school environment with respect to the measures of safety are being considered. Handgun training, however, is not associated with improvements in the school environment. The only significant effect is an *increase* in the self reported participation in any fight in the last year.

The causal mechanism is unclear in this case. It seems reasonable to assume that students are not carrying long guns on school campuses. I hypothesize that the driver of this effect is likely to do with hunting or shooting sports and the required participation in mandatory safety training for participation in those sports. During participation, one might expect that the participants pick up some safety-related skills which transfer to the school environment. However, this mechanism is unsubstantiated and more research into the content of these trainings and characteristics of the participants would be required.

5.4 A Digression on Hunting Licenses

The main consideration in this section of this analysis is whether or not the variable for gun carrying generated from YRBS survey data is a valuable indicator that the gun carrying has some sort of nefarious intent. If one wants to interpret YRBS gun carrying to measure the safety of the school environment, or as a causal mechanism for t school shootings or gun violence, then the YRBS responses must reflect these behaviors in particular. The main results I present here appear in tables 5.10 and 5.11. In these two tables one can see that hunting licenses per capita

consistently has explanatory power for youth gun carrying.

As such, youth gun carrying as is reported in the YRBS may not be a good representation of youth gun carrying with respect to violent intent. The question may be picking up the participation of youth in hunting, rather than anything else. It may be unreasonable, then, to use youth gun carrying as a causal mechanism in evaluating the effects of gun policy on the school environment.

There appear to be heterogeneous effects by age on this result, as the variable "Hunting License Per Capita" is always significant at the 5 or 10 percent level for the under 18 cohort, but is never significant for the cohort of respondents who are 18 years of age or older.

As a verification of the robustness of this result, in the same framework, I check that hunting licenses per capita have no statistically significant explanatory power over other dependent variables such as skipping school because of safety and indeed these results were null, as expected. This certainly is an interesting association, but requires further investigation.

Table 5.10: Hunting Licenses and Reported Gun Carrying

Dependent Variable: Carry Gun		
	Under 18	18 +
Any	0.00327	-0.00648
CAP Law	(0.00788)	(0.00940)
Hunting License	0.00000148^{+}	0.00000201
Per Capita	(0.000000741)	(0.00000161)
Reckless	-0.00557	-0.0300*
Endangerment	(0.00730)	(0.0141)
Hunting License	0.00000132^{+}	0.00000208
Per Capita	(0.000000774)	(0.00000191)
Negligent	0.00890	0.00818
Storage	(0.00890)	(0.00570)
Hunting License	0.00000153*	0.00000239
Per Capita	(0.000000751)	(0.00000153)
N	635691	97006

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

Table 5.11: Hunting Licenses and Reported Gun Carrying - Continued

Dependent Variable: Carry Gun		
	under18	over18
Background		
Check Permit to	0.00728	-0.00956
Purchase: Handguns	(0.00692)	(0.00674)
Hunting License	0.00000147*	0.00000207
Per Capita	(0.000000677)	(0.00000146)
Background		
Check Private	-0.00717	-0.0152
Seller: Handgun	(0.00434)	(0.0228)
Hunting License	0.00000129+	0.00000206
Per Capita	(0.000000651)	(0.00000152)
Any Background	-0.00402	-0.0318
Check: Handgun	(0.00717)	(0.0240)
Hunting License	0.00000129+	0.00000151
Per Capita	(0.000000686)	(0.00000147)
Background		
Check Private	-0.00114	-0.0311**
Seller: Long Gun	(0.00340)	(0.00867)
Hunting License	0.00000136*	0.00000173
Per Capita	(0.000000665)	(0.00000149)
Any Background	0.00364	-0.0276
Check	(0.00917)	(0.0232)
Hunting License	0.00000148*	0.00000158
Per Capita	(0.000000716)	(0.00000147)
Observations	635691	97006

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

6 Discussion and Conclusion

The first goal of this analysis was to replicate, in part, the work of Anderson & Sabia (2018). The primary result in this respect was that I was largely unable to reproduce their most important results. They find a statistically significant 19% decrease in the proportion of youth reporting carrying a gun in the past 30 days when a CAP law is present. My corresponding result was near zero and not statistically significant, even at the 10% level. Some secondary results were more closely replicated. There were a number of differences in the methodologies and data used. Methodologically, I use a LPM while Anderson & Sabia specify a probit. Additionally, I use YRBS data from 1993-2015 while Anderson and Sabia use data from 1993-2013. My sample excludes Washington D.C., and thus I do omit some variation in firearm legislation.

The second goal was to provide an extension of Anderson & Sabia (2018) by exploring the effect of background checks on youth gun carrying behavior and school environment "downstream outcomes". The conclusion from this section was that the effects of background checks on youth gun carrying and the school environment were null.

The third objective was an exploration of mandatory safety trainings. The effect of these laws on youth gun carrying was ambiguous, but the effect on the safety of the school environment shows that mandatory safety trainings for long guns improve the school environment across all measures of safety that I used. Unfortunately, the causal mechanism is unclear in this case because of the lack of clarity of these laws' effect on youth gun carrying. Further research as to the content of these trainings is required.

Finally, I explore the usefulness of YRBS gun and weapon carrying data as a measure of youth gun carrying with nefarious intent. I find that hunting licenses per capita consistently have a statistically significant effect on youth reported gun carrying. As such, it seems that the YRBS variable describing youth gun carrying may be picking up youth participation in hunting. I would suggest further investigation into the value of YRBS gun carrying data as a measure of gun carrying with nefarious or violent intent. Information that includes what types of firearms that youth report carrying might be valuable in this pursuit.

In considering these results, it is important to note that null results do not mean that these policies are not important or do not have substantive impact on *other* measurable outcomes not related to youth behavior or the school environment. Other research is necessary to evaluate these questions.

There are some clear extensions of the work conducted in this analysis. First, I intend to add Washington D.C. back to the sample. Additionally, I would like to include state-level controls that vary over time such as for economic conditions. Another state-level control of value might be the minimum age of possession and purchase for firearms (separated by policies

for handguns and long guns). There may be value in exploring the causal mechanism for the effect of mandatory safety trainings on bettering the school environment as well. Additionally there is value in considering the effects of a variety of other firearm legislation such as concealed carry (which have been examined extensively by economists over the past two decades), trigger locks, and gun buyback programs. Anderson & Sabia (2018) use these as state level controls but do not report associated marginal effects.

References

103rd Congress of the United States. (1993, November). *Public law 103-159 (brady act).*

Anderson, D. M., & Sabia, J. J. (2018). *Child-Access-Prevention Laws, Youths' Gun Carrying, and School Shootings* (Vol. 61; Tech. Rep.). Retrieved from `http://www.journals.uchicago.edu/t-and-c`

California DOJ. (2019, January). *Firearm safety certificate study guide.* Office of the Attorney General, Bureau of Firearms.

Centers for Disease Control and Prevention. (1993-2015 Youth Risk Behavior Survey Data, Available at: www.cdc.gov/yrbs. Accessed Fall 2019).

Centers for Disease Control and Prevention. (2013). *Methodology of the youth risk behavior surveillance system — 2013.*

Centers for Disease Control and Prevention, National Center for Injury Prevention and Control. (2019). Web-based injury statistics query and reporting system (wisqars): Fatal injurey data. *https://www.cdc.gov/injury/wisqars/index.html*.

CNN. (2019). *Mass shootings in the us fast facts.* Web. Retrieved 12/1/2019, from `https://www.cnn.com/2019/08/19/us/mass-shootings-fast-facts/index.html`

DeSimone, J., Markowitz, S., & Xu, J. (2013). Child Access Prevention Laws and Nonfatal Gun Injuries. *Southern Economic Journal*, *80*(1), 5–25. doi: 10.4284/0038-4038-2011.333

FBI. (n.d.). *National Instant Criminal Background Check System (NICS).* Retrieved 12/1/2019, from `https://www.fbi.gov/services/cjis/nics`

Giffords law center to prevent gun violence. (2018). *https://lawcenter.giffords.org*.

Lam, K. (2019, November). *Santa clarita gunman used unregistered 'ghost gun' in saugus high school shooting, sheriff says.* Retrieved 11/22/2019, from `\\\url{https://www.usatoday.com/story/news/nation/2019/11/21/saugus-shooting-ghost-gun-santa-clarita-teen-nathaniel-berhow/4265433002/}`

RAND Corporation. (2019). Rand state firearm law database. *https://www.rand.org/pubs/tools/TL283-1.html*.

Siegel, M. (2019). State firearms laws database. *http://www.statefirearmlaws.org*.

Siegel, M., Ross, C. S., & King, C. (2014). A new proxy measure for state-level gun ownership in studies of firearm injury prevention. *Injury Prevention*, *20*(3), 204–207. doi: 10.1136/injuryprev-2013-040853

State of Connecticut. (2019a, November). *Ce/fs firearms hunting safety courses.* Retrieved 12/12/2019, from `https://www.ct.gov/deep/cwp/view.asp?a=2700&q=429848&deepNav_GID=1633`

State of Connecticut. (2019b, May). *State Pistol Permit.* Retrieved 12/12/2019, from

https://portal.ct.gov/DESPP/Division-of-State-Police/Special-Licensing-and-Firearms/State-Pistol-Permit

U.S. Fish and Wildlife Service. (2019). Historical hunting license data. *https://wsfrprograms.fws.gov/Subpages/LicenseInfo/Hunting.htm*.

7 Appendix

7.1 YRBS

Table 7.1: YRBS Observations by State and Year

state	1993	1995	1997	1999	2001	2003	2005	2007	2009	2011	2013	2015	Total
Alabama	5,120	3,917	4,421	2,099	1,862	1,721	1,026	483	2,528	1,654	1,845	1,810	28,486
Alaska	0	1,596	0	0	0	1,439	0	1,268	1,218	1,279	1,183	1,343	9,326
Arizona	435	0	1,107	131	408	3,714	3,502	3,545	2,846	3,876	1,744	2,698	24,006
Arkansas	395	2,536	2,336	1,435	1,670	281	1,503	1,979	1,927	1,327	1,854	2,746	19,989
California	2,119	1,202	1,985	2,479	2,198	1,730	1,553	2,110	2,802	1,877	2,463	5,779	28,297
Colorado	259	101	268	0	658	0	0	0	193	297	304	270	2,350
Connecticut	0	0	1,887	0	0	0	2,442	1,997	2,319	2,000	2,377	2,429	15,451
Delaware	0	214	0	2,131	2,863	3,333	2,633	2,357	2,257	2,421	2,590	2,638	23,437
Florida	529	547	675	863	5,130	5,461	4,982	5,098	5,591	7,409	6,840	6,854	49,979
Georgia	911	445	344	811	486	423	1,849	349	1,319	135	371	402	7,845
Hawaii	0	0	0	311	0	0	0	0	236	0	0	0	547
Idaho	3,970	0	0	0	1,843	1,702	1,667	1,384	2,102	1,921	2,090	2,050	18,729
Illinois	4,721	3,310	0	230	440	315	492	2,956	4,432	4,500	3,872	4,022	29,290
Indiana	0	0	0	0	178	422	170	400	0	277	824	189	2,460
Iowa	0	242	2,284	0	0	0	1,588	1,666	0	1,513	0	0	7,293
Kansas	171	0	204	0	0	340	1,909	1,692	2,196	2,133	2,089	0	10,734
Kentucky	0	0	1,455	0	0	1,563	3,766	3,842	1,726	1,973	2,257	2,465	19,047
Louisiana	0	381	580	624	0	691	158	0	433	0	0	0	2,867
Maine	249	1,528	2,060	198	1,525	1,830	1,325	1,267	8,445	9,079	8,343	9,112	44,961
Maryland	145	0	823	0	0	261	1,398	1,486	1,590	2,793	51,769	54,356	114,621
Massachusetts	370	274	1,632	0	255	213	257	718	0	289	0	264	4,272
Michigan	144	1,110	4,385	3,134	3,864	3,772	3,479	3,723	3,636	4,711	4,627	4,879	41,464
Minnesota	322	0	0	0	0	0	95	0	188	0	292	745	1,642
Mississippi	1,749	1,746	1,827	2,178	2,133	1,471	0	1,923	1,763	1,846	2,183	2,040	20,859
Missouri	183	5,386	1,459	2,175	2,095	1,798	1,963	1,865	1,681	344	1,825	1,594	22,368
Montana	2,489	2,493	2,402	2,820	2,810	2,678	2,987	3,846	1,785	4,022	4,745	4,308	37,385
Nebraska	3,560	0	0	0	0	2,913	3,706	0	0	3,719	1,824	1,634	17,356
Nevada	2,005	1,507	1,453	1,652	1,676	1,947	1,529	1,729	2,403	207	2,069	1,787	19,964
New Hampshire	2,662	2,072	0	0	0	1,298	1,249	1,581	1,450	1,359	1,590	14,310	27,571
New Jersey	0	0	737	235	2,324	305	1,822	689	2,203	1,730	2,027	208	12,280
New Mexico	661	0	280	0	155	104	0	220	605	0	0	315	2,340
New York	1,244	508	4,043	3,995	309	9,987	9,939	13,688	15,335	13,161	10,409	10,406	93,024
North Carolina	2,712	1,873	339	509	3,191	2,522	4,466	3,975	5,550	3,324	2,171	5,891	36,523
North Dakota	0	1,509	0	1,789	1,580	1,649	1,710	1,722	1,767	1,863	1,919	2,064	17,572
Ohio	524	553	556	564	224	299	279	0	0	0	158	227	3,384
Oklahoma	0	0	223	0	397	1,366	1,923	2,842	1,397	1,136	1,465	1,934	12,683
Oregon	188	0	0	0	184	0	268	0	247	0	0	0	887
Pennsylvania	362	671	272	487	0	316	423	210	3,104	450	264	3,278	9,837
Rhode Island	0	0	1,494	75	1,361	1,775	2,316	2,133	3,106	3,814	2,357	4,004	22,435
South Carolina	4,619	5,434	6,003	5,313	0	887	1,567	1,206	1,070	1,437	1,553	1,311	30,400
South Dakota	1,336	1,177	1,589	1,643	1,596	2,100	1,567	1,577	2,122	1,502	1,273	1,257	18,739
Tennessee	3,735	354	578	265	611	1,919	1,924	2,182	2,176	2,874	1,847	4,371	22,836
Texas	2,762	1,585	949	2,715	2,048	2,628	1,746	1,586	1,331	1,786	393	1,226	20,755
Utah	4,338	3,144	1,349	1,479	1,042	1,537	1,710	2,097	1,544	1,657	2,118	0	22,015
Vermont	0	0	0	0	0	256	0	0	0	0	0	0	256
Virginia	0	64	0	746	0	245	349	439	98	1,603	7,776	4,310	15,630
Washington	376	83	108	0	54	0	101	0	246	167	195	102	1,432
West Virginia	3,107	2,071	1,808	1,308	261	1,719	1,549	1,598	2,071	2,375	1,753	1,803	21,423
Wisconsin	3,272	0	1,603	1,855	2,327	2,278	2,593	2,234	3,074	3,615	2,776	0	25,627
Wyoming	0	1,674	2,033	1,590	2,684	1,507	2,455	2,174	2,802	2,439	2,924	2,317	24,599
Total	61,744	51,307	57,551	47,839	52,442	74,715	85,935	89,836	106,914	107,894	155,348	175,748	1,067,273

Q13. During the past 30 days, on how many days did you carry **a weapon** such as a gun, knife, or club?

A. 0 days
B. 1 day
C. 2 or 3 days
D. 4 or 5 days
E. 6 or more days

Variable label: Weapon carrying

QN13: Numerator: Students who answered B, C, D, or E for Q13
Denominator: Students who answered A, B, C, D, or E for Q13
Summary text: Percentage of students who carried a weapon (such as a gun, knife, or club on at least 1 day during the 30 days before the survey)
Variable label: Carried a weapon

Q14. During the past 30 days, on how many days did you carry **a gun**?

A. 0 days
B. 1 day
C. 2 or 3 days
D. 4 or 5 days
E. 6 or more days

Variable label: Gun carrying

QN14: Numerator: Students who answered B, C, D, or E for Q14
Denominator: Students who answered A, B, C, D, or E for Q14
Summary text: Percentage of students who carried a gun (on at least 1 day during the 30 days before the survey)
Variable label: Carried a gun

Q15. During the past 30 days, on how many days did you carry a weapon such as a gun, knife, or club **on school property**?

A. 0 days
B. 1 day
C. 2 or 3 days
D. 4 or 5 days
E. 6 or more days

Variable label: Weapon carrying at school

QN15: Numerator: Students who answered B, C, D, or E for Q15
Denominator: Students who answered A, B, C, D, or E for Q15
Summary text: Percentage of students who carried a weapon on school property (such as a gun, knife, or club on at least 1 day during the 30 days before the survey)
Variable label: Carried a weapon on school property

Q16. 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?

A. 0 days
B. 1 day
C. 2 or 3 days
D. 4 or 5 days
E. 6 or more days

Variable label: Safety concerns at school

QN16: Numerator: Students who answered B, C, D, or E for Q16
Denominator: Students who answered A, B, C, D, or E for Q16
Summary text: Percentage of students who did not go to school because they felt unsafe at school or on their way to or from school (on at least 1 day during the 30 days before the survey)
Variable label: Did not go to school because they felt unsafe at school or on their way to or from school

Q17. 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**?

A. 0 times
B. 1 time
C. 2 or 3 times
D. 4 or 5 times
E. 6 or 7 times
F. 8 or 9 times
G. 10 or 11 times
H. 12 or more times

Variable label: Threatened at school

QN17:
Numerator: Students who answered B, C, D, E, F, G, or H for Q17
Denominator: Students who answered A, B, C, D, E, F, G, or H for Q17
Summary text: Percentage of students who were threatened or injured with a weapon on school property (such as a gun, knife, or club one or more times during the 12 months before the survey)
Variable label: Were threatened or injured with a weapon on school property

Q18. During the past 12 months, how many times were you in a physical fight?

A. 0 times
B. 1 time
C. 2 or 3 times
D. 4 or 5 times
E. 6 or 7 times
F. 8 or 9 times
G. 10 or 11 times
H. 12 or more times

Variable label: Physical fighting

QN18:
Numerator: Students who answered B, C, D, E, F, G, or H for Q18
Denominator: Students who answered A, B, C, D, E, F, G, or H for Q18
Summary text: Percentage of students who were in a physical fight (one or more times during the 12 months before the survey)
Variable label: Were in a physical fight

Q19. During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?

A. 0 times
B. 1 time
C. 2 or 3 times
D. 4 or 5 times
E. 6 or more times

Variable label: Injurious physical fighting

QN19:
Numerator: Students who answered B, C, D, or E for Q19
Denominator: Students who answered A, B, C, D, or E for Q19
Summary text: Percentage of students who were injured in a physical fight (one or more times during the 12 months before the survey; injuries had to be treated by a doctor or nurse)
Variable label: Were injured in a physical fight

Q20.	During the past 12 months, how many times were you in a physical fight **on school property**?	
	A. 0 times	
	B. 1 time	
	C. 2 or 3 times	
	D. 4 or 5 times	
	E. 6 or 7 times	
	F. 8 or 9 times	
	G. 10 or 11 times	
	H. 12 or more times	
	Variable label:	Physical fighting at school
QN20:	Numerator:	Students who answered B, C, D, E, F, G, or H for Q20
	Denominator:	Students who answered A, B, C, D, E, F, G, or H for Q20
	Summary text:	Percentage of students who were in a physical fight on school property (one or more times during the 12 months before the survey)
	Variable label:	Were in a physical fight on school property

7.2 Tables from Anderson & Sabia (2018)

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.

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.

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.

7.3 School Environment - Heterogeneous Effects

Table 7.2: Heterogeneous Effects by Age of Background Check Laws on School Environment Outcomes

		Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
	Under 18	-0.00926^{+}	-0.0158	0.0238**	-0.00753	0.0831**
Background		(0.00483)	(0.00955)	(0.00469)	(0.00957)	(0.0285)
Checks for	N	916711	893474	713193	872960	784310
a Permit to	18 +	-0.00590	-0.0236	0.000680	0.0313*	0.0539**
Purchase:		(0.0163)	(0.0238)	(0.0118)	(0.0140)	(0.0107)
Handguns	N	128248	125421	110085	124947	117624
Background	Under 18	-0.0103	0.0151	-0.00779	-0.0116	-0.0923**
Checks for		(0.0114)	(0.0183)	(0.0135)	(0.0191)	(0.0225)
Handgun Sales	916711	893474	713193	872960	784310	
from Private	18 +	-0.00772	0.0443	0.0279	-0.0233	-0.0145
Dealers		(0.0120)	(0.0463)	(0.0286)	(0.0307)	(0.0228)
	N	128248	125421	110085	124947	117624
	Under 18	-0.0115	-0.00426	0.0138*	0.00362	-0.00567
		(0.0103)	(0.0272)	(0.00564)	(0.00924)	(0.0339)
†Any Handgun	N	916711	893474	713193	872960	784310
Background	18 +	-0.0239	0.0146	0.0292	0.00774	0.0391*
Checks		(0.0199)	(0.0607)	(0.0288)	(0.0221)	(0.0188)
	N	128248	125421	110085	124947	117624

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are also reported. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

† "Any Handgun background Checks" is a binary variable that is 1 if either of "Background Chekcs for a Permit to Purchase: Handguns" or "Background Checks for Handgun Sales from Private Dealers" is 1.

Table 7.3: Heterogenous Effects by Age of Background Check Laws on School Environment Outcomes - Continued

		Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
Background	Under 18	-0.00870	0.0159^{+}	-0.0123*	0.0295*	0.0379
Checks for		(0.00735)	(0.00914)	(0.00588)	(0.0128)	(0.0313)
Sales from	N	916711	893474	713193	872960	784310
Private Dealers:	18 +	0.0243	0.0465^{+}	0.0246	-0.00725	-0.0229
Long Guns		(0.0177)	(0.0247)	(0.0548)	(0.0318)	(0.0301)
	N	128248	125421	110085	124947	117624
	Under 18	-0.00860	0.00821	0.0125*	0.0111	-0.00554
		(0.0108)	(0.0272)	(0.00554)	(0.0107)	(0.0341)
‡Any	N	916711	893474	713193	872960	784310
Background	18 +	-0.0185	0.0182	0.0293	0.00771	0.0276
Checks		(0.0199)	(0.0573)	(0.0260)	(0.0231)	(0.0359)
	N	128248	125421	110085	124947	117624

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are also reported. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.

‡ "Any Background Checks" is a binary variable that is 1 if any of the other variables in this table are 1.

Table 7.4: Safety Training and Gun/Weapon Carrying - Heterogeneous Effects by Age

		Skip School - Unsafe	Threatened with a Weapon - Last 12 mo.	Serious Fight - Last 12 mo.	Fight at School - Last 12 mo.	Any Fight - Last 12 mo.
	Under 18	-0.00286	-0.00197	-0.000572	0.00142	0.0589**
		(0.00580)	(0.00744)	(0.00251)	(0.00271)	(0.00553)
Safety	N	916711	893474	713193	872960	784310
Training	18 +	0.0115	-0.00582	0.00689^{+}	0.0350	0.111**
Required:		(0.00918)	(0.00710)	(0.00375)	(0.0226)	(0.00655)
Handguns	N	128248	125421	110085	124947	117624
	Under 18	-0.0161**	-0.0129**	-0.00848**	-0.0175**	-0.0380**
		(0.00303)	(0.00306)	(0.00255)	(0.00276)	(0.00725)
Safety	N	916711	893474	713193	872960	784310
Training	18 +	-0.00415	0.0149**	-0.00214	0.00398	-0.0285**
Required:		(0.00297)	(0.00279)	(0.00290)	(0.00298)	(0.00786)
Long Guns	N	128248	125421	110085	124947	117624
	Under 18	-0.00286	-0.00197	-0.000572	0.00142	0.0589**
Any		(0.00580)	(0.00744)	(0.00251)	(0.00271)	(0.00553)
Safety	N	916711	893474	713193	872960	784310
Training	18 +	0.0115	-0.00582	0.00689^{+}	0.0350	0.111**
Required		(0.00918)	(0.00710)	(0.00375)	(0.0226)	(0.00655)
	N	128248	125421	110085	124947	117624

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Reported coefficients are results of the LPM described in the Methodology section, including individual level controls for age, race, sex, grade. Dependent variable means are also reported. Standard errors are corrected for clustering by state. Population weights from WISQARS are used.