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ABSTRACT

Many states have passed child access prevention (CAP) laws, which hold the gun owner responsible if a child gains access to a gun that is not securely stored. Previous CAP law research has focused exclusively on gun-related deaths even though most gun injuries are not fatal. We use annual hospital discharge data from 1988-2001 to investigate whether CAP laws decrease non-fatal gun injuries. Results from Poisson regressions that control for various hospital, county and state characteristics, including state-specific fixed effects and time trends, indicate that CAP laws substantially reduce non-fatal gun injuries among both children and adults. Our interpretation of the estimates as causal impacts is supported by the absence of effects on self-inflicted gun injuries among adults, non-gun self-inflicted injuries, and knife assaults, the failure of violent crime levels and law leads to attain significance or alter estimated law coefficients, and larger coefficient magnitudes in states where the law covers older children.

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1. Introduction

During the 1989–2000 period, 18 states passed child access prevention (CAP) or "safe storage" laws in an attempt to reduce gun-related accidents, suicides and crime among children. CAP laws hold the gun owner responsible if a child gains access to a gun that is not securely stored, with owners typically liable only if the gun is loaded and, in some states, an injury occurs. The penalty for violating the law is usually a misdemeanor, but in some states is a felony if someone is injured or the gun is large capacity (e.g. a machine gun). In states other than Hawaii, the law does not apply if the gun is stored in a locked container or with a child-safety trigger lock, even if an injury takes place. The intention of CAP laws is to make guns as difficult as possible for children to obtain, while still providing fairly straightforward accessibility of guns to adults.

Only a few previous studies have rigorously examined the effects of CAP laws on gun related injuries, all using annual state-level data on gun-related deaths. Using 1979–1994 data on children age 14 and under, Cummings et al. (1997) found that CAP laws reduce unintentional shooting deaths by 23 percent. In 1979–1997 data, Webster and Starnes (2000) similarly established that CAP laws were associated with a 17 percent decline in unintentional firearm death rates, but attributed the entire effect to a 51 percent mortality reduction in Florida after the 1989 enactment of its CAP law, which allows felony prosecution of violators. Webster et al. (2004) analyzed 1976–2001 data and found that among 14–17 year olds, CAP laws decreased gun suicides by 11 percent but had no effect on non-gun suicides. They also reported that among 18–20 year olds, who are not covered by the laws in any state, CAP laws were associated with a 13 percent fall in gun suicides and a nine percent reduction in non-gun suicides. In contrast, Lott and Whitley (2001) failed to detect a relationship between CAP laws and accidental gun deaths or suicides among youth age less than 15 and 15–19 during 1979–1996.

A characteristic shared by the above studies is the restriction of attention to deaths, presumably for a variety of reasons. Deaths are more publicized and, obviously, more serious than non-fatal injuries. Mortality statistics by cause are easily available at the state level at which CAP laws vary. Moreover, analyzing suicides allows for focusing on injuries in which the age of the person discharging the gun is known, which is useful because of the age-specific applicability of CAP laws. Yet, most gun injuries are non-fatal. In 2001, for instance, there were 14,571 non-fatal gun injuries but only 6,000 gun deaths among youth age 20 and under, along with 47,997 non-fatal gun injuries compared with 26,617 gun deaths among adults (NCIPC 2004a, NCIPC 2004b).

Non-fatal injuries are relevant for CAP laws from at least two policy analysis perspectives. From a public health standpoint, non-fatal injuries constitute costs of child gun access that CAP laws might mitigate. Gun injuries clearly cause considerable pain and suffering for many victims who survive. The value of preventing some non-fatal gun injuries might approach that of saving a life, particularly those involving damage to the brain or spinal cord. For instance, Cook et al. (1999) estimated that in 1994, U.S. gun injuries were responsible for $2.3 billion in lifetime medical costs, 85 percent of which were accounted for by hospitalized gunshot survivors. Studies that concentrate on gun fatalities thus ignore a potential impact of CAP laws that conveys a substantial social benefit. From a behavioral response viewpoint, the ideal variable to analyze would be the total number of gunshots fired either accidentally or with intent to harm another individual, particularly considering that the difference between death and injury can be simply a matter of random variation in aim or reaction time of emergency medical personnel. For assaults and unintentional injuries, the undercount resulting from the inability to observe missed shots is much less severe when non-fatal injuries are considered along with deaths.

This paper uses annual hospital discharge data from 1988–2001 to evaluate the effects of state CAP laws on non-fatal gun injuries among both children and adults. These data are the most complete source of non-fatal gun injury information available. They are useful for this analysis because they cover years both before and after CAP law implementation in many CAP law states and also include states without CAP laws. However, relative to the data examined by the studies cited above, our hospital discharge data on deaths are incomplete because many victims who are fatally injured never make it to the hospital. We therefore restrict attention to non-fatal injuries even though, as argued above, it would be ideal to analyze both fatal and non-fatal gun injuries simultaneously.

Our main sample consists of hospitals in the 11 states surveyed each year starting in one of the first two years of the data collection period. Seven of these states implemented CAP laws during the sample period. Our analysis compares changes in gun injury counts between the periods before and after CAP laws took effect in these states with contemporaneous injury count changes in the four sample states with no CAP law. Specifically, we estimate Poisson regressions of the number of hospital gun injury discharges on a CAP law indicator. We also control for various characteristics of states, counties, and hospitals, the latter of which are important because each year's sample is a separate cross section. In order to identify effects using cross-state variation in within-state injury count changes over time, and thus hold constant state characteristics that are time invariant or trend linearly, the model includes a fixed effect and linear time trend for each sample state.

Results indicate that CAP laws reduce gun injuries among both children and adults by between 30 and 40 percent. Various results from auxiliary analyses suggest that these estimates represent causal effects. When CAP laws are implemented, self-inflicted gun injuries fall by 64 percent for youth age 18 and under but do not decrease for adults. Non-firearm self-inflicted injuries and knife assaults are unrelated to CAP laws for both age groups. When added to baseline models, violent crime levels and one and two-year time leads for law indicators rarely attain significance, and estimated law coefficients remain largely unchanged. Finally, the size of the effect on youth self-inflicted gun injuries is larger in states that consider 15 year olds to be minors, and thus covered by the law, rather than adults.

2. Data

Our gun injury data come from the Agency for Healthcare Research and Quality's Nationwide Inpatient Sample (NIS), a database of inpatient hospital stays that serves as a 20 percent sample of all U.S. short-term community hospitals including those that provide specialty care (e.g. to children) and are academic medical centers or public hospitals. Data collection began in 1988 and continues annually. In each year of our sample period, the NIS containes information on between five and eight million inpatient visits to between 759 and 1,012 hospitals.[1]

Hospitals from eight states were surveyed in 1988. States never exit the sample once they enter, and hospitals from new states are added almost every year. Consequently 33 states are include as of 2001, the last year of data in our sample. The universe of hospital discharges is observed for all hospitals surveyed. Each discharge record contains up to fifteen diagnosis codes through which injuries from specific sources, including guns, are identifiable via E-codes. Patient-level observations are aggregated to the hospital level to create annual counts of gun-

[1] ER visits are included only if they result in admissions, which means that less severe injuries are not observed.

related injuries. Sampling recurs every year, so that no hospital remains in the sample for several consecutive years. Hence the data are more appropriately considered to be pooled cross-sections than a panel, and we analyze them as such.

Coben et al. (2003) evaluated the validity of using NIS diagnosis codes to calculate gun injury counts by comparing relevant characteristics of the 1997 NIS to those from the 1997 National Electronic Injury Surveillance System (NEISS), a national probability sample gathered from emergency departments across the U.S. The two data sources yielded comparable conclusions. For example, the NIS implied 35,810 gun injuries nationwide of which 86 percent involved males, while analogous totals from the NEISS were 32,326 and 89 percent. The authors concluded that the NIS is the best available source of inpatient data: “Due to its larger sampling frame and the ability to provide important clinical and epidemiologic information not available form other data sources, NIS may serve as a gold standard for inpatient data” (pg. 7).

The information provided by the NIS on surveyed hospitals includes measures of hospital location, size, and ownership type. Location is categorized as rural, urban non-teaching, or urban teaching. Size is classified as small, medium or large in terms of the number of beds, with the thresholds that divide the categories varying by location. Ownership type is stratified as public, private non-profit, and private for-profit in areas with large sample sizes (southern rural, southern and western urban non-teaching), with a single private category used in smaller sample size areas (midwestern and western rural) and no ownership type identified in remaining areas. In addition, we observe whether or not the hospital is a children’s hospital. Indicators for each group, less one per measure, serve as explanatory variables in the regressions. The logged number of annual discharges is also held constant to proxy for total injury admissions.

We also control for several characteristics of the county and state in which each hospital is located. These include the logged population and the annual unemployment rate of both the county and state, along with the fraction of state residents age 25 and above who have graduated from college and the proportion of the state population living in rural areas.

We obtain information on CAP laws in each NIS state from Webster et al (2004). Table 1 lists, for each state that entered the NIS by 1997, the effective date of and oldest age covered by the CAP law if the state has one, as well as the year of NIS entry and total number of observations (i.e. hospital/year cells) during 1988–2001.

The timing of CAP law implementation in NIS states dictates the composition of our analysis samples. Eight states contributed data to the original NIS in 1988, and three more were added in 1989. Because states never drop out of NIS once they enter, annual data exist for 13 consecutive years from these 11 states (of which eight states supply an additional year for a total of 14 years). Seven of these 11 states have CAP laws, in each of which the law took effect after the state had contributed at least a year of data. During 1993–1997, 10 additional states had entered NIS. However, six of the seven CAP laws in the original 11 states, and all three laws in these 10 later-entering states, were implemented by October 1992.[2] Thus, not only can changes in gun injury counts from before to after CAP laws took effect not be observed in any of these 10 latter states, but moreover only one other CAP law change takes place after their data series start. We therefore use data from only the original 11 states to generate our main estimates, though we show one set of results for a sample that also includes the 10 later-entering states.

3. Empirical Strategy

Our gun injury data are count variables that comprise only a small proportion of total

[2] Another 11 states entered NIS during 1999–2001, but these states were omitted because of the limited number of years that they contributed data.

hospital discharges and are sometimes equal to zero in a given hospital and year. These characteristics make a count data model appropriate for the regression analysis. In particular, we use the Poisson quasi-maximum likelihood estimator (QMLE), which is consistent regardless of whether the injury counts actually have a Poisson distribution (Wooldridge 2002). To permit overdispersion, a common feature of count data that is not accommodated by the Poisson MLE, standard errors are adjusted for heteroskedasticity of unknown form. Because CAP laws vary across states, but not hospitals within states, we also allow for state-level clustering when calculating standard errors.

Our regression equation is thus

(1) $$G_{hy} = \alpha_0 + \alpha_1 CAP_{sy} + \mathbf{H_{hy}}\boldsymbol{\alpha_3} + \mathbf{X_{sy}}\boldsymbol{\alpha_4} + \mathbf{Z_{cy}}\boldsymbol{\alpha_5} + \mathbf{S_s}\boldsymbol{\alpha_6} + \mathbf{Y_y}\boldsymbol{\alpha_7} + \mathbf{S_s}\cdot(y - 1988)\boldsymbol{\alpha_8} + \varepsilon_{hy}.$$

G_{hy} signifies the number of non-fatal gun injury discharges from hospital h in year y. CAP_{sy} is a binary indicator that state s has a CAP law in year y for all years except for the year that the law takes effect, in which CAP_{sy} equals the fraction of the year that the law was operational. The $\mathbf{H_{hy}}$, $\mathbf{X_{sy}}$ and $\mathbf{Z_{cy}}$ vectors represent the previously described hospital (h), state (s) and county (c) level characteristics, all of which vary by year. $\mathbf{S_s}$ and $\mathbf{Y_y}$ are vectors of indicators for each state and year except one, while $\mathbf{S_s}\cdot(y - 1988)$ is a vector of interactions between $\mathbf{S_s}$ and the time trend (y – 1988), i.e. state-specific linear trends. Lastly, ε_{ht} is a disturbance term that is assumed to be uncorrelated with the explanatory variables.

State indicators control for time-invariant state-specific gun injury determinants, while year indicators capture annual gun injury changes that are common across states. Because these indicators are included, regression coefficients are identified by within-state differences in gun injuries from before to after CAP law passage between states that did and did not enact CAP laws. This avoids potential bias from time-invariant correlates of CAP laws and gun injuries that differ across states, which could possibly arise if CAP law effects were identified using only cross sectional variation. For example, if gun control sentiments are more favorable among residents of states that passed CAP laws than other states, CAP laws and gun injuries might have a negative cross sectional correlation even if the laws themselves do not affect behavior. Conversely, disproportionate passage of CAP laws by states with particularly high gun injury rates would create a positive reverse causal effect of gun injuries on CAP law presence across states. To allow for gun injury trends to differ across states and be correlated with CAP law enactment, most models also control for state-specific linear time trends.

Because CAP laws apply only when the individual discharging the gun is younger than age 14–18, with the exact age limit varying by state, we form separate injury counts for two age groups, younger than 18 (youth) and at least 18 (adults).[3] However, an important limitation is that these ages pertain to the victim, not the shooter. We thus separately examine gun injuries that are and are not self-inflicted, as the former is the only type for which the age of the shooter is observed. CAP laws are not expected to impact self-inflicted gun injuries among adults, even if they do reduce analogous injuries among youth. Age distinctions for non-self-inflicted gun injuries are less meaningful; for example, Lott and Whitley (2001) cite an average age difference between victim and shooter of 6.6 years for fatal gun injuries. This suggests that many victims of youth-inflicted gunshot wounds are adults, so that both youth and adult non-self-inflicted gun injuries could decline in response to effective CAP laws.[4] For these injury types, we estimate separate models for three intent categories, unintentional, assault and undetermined, as well as for the combined total.

[3] This means that, in states besides California and Massachusetts that have CAP laws, some injuries among those we call "youth" occur to individuals who are old enough to not be considered youth by the CAP law. The laws might still affect such youth if they have siblings who are sufficiently young to be covered. As described below, we later exploit the variation in these age thresholds across states.

[4] This issue also affects the studies of unintentional deaths discussed in the literature review.

The remainder of our analysis constitutes various investigations regarding the extent to which CAP laws merely signal other changes that are correlated with injury trends. First, counts of two non-firearm injuries, overall self-inflicted and knife assaults, are examined as a "counterfactual" experiment. Because CAP laws pertain strictly to gun access, a significant influence on non-gun injuries would cast doubt on the interpretation of CAP law coefficients as causal effects of the laws. Next, we add the logged state violent crime count, obtained from various editions of the FBI's Uniform Crime Reports, as an explanatory variable to the original specifications to test whether CAP law implementations coincide with shifts in violent crime rates that dictate changes in injury rates. Subsequently, we return to the original specifications and add one- and two-year law lead variables, which represent the fraction of each year that occurred within one year and between one and two years before the effective law date in states with CAP laws, to filter out correlations between laws and injury counts created by prevailing injury trends that began before the laws took effect and are not fully accounted for by the state-specific trend variables that are already included.

We proceed to restrict attention to 14–17 year olds, among whom we scrutinize self-inflicted injuries in various state subgroups to assess whether CAP law effects are larger in states with higher age thresholds at which the law no longer defines someone as a "child" for whose gun access the parent is responsible. This age group is completely beyond the CAP law age limit of 14 in Illinois and Wisconsin, partially covered by the laws in Iowa (age 15) and Florida and New Jersey (age 16), and entirely under the jurisdiction of the laws in California and Massachusetts (age 18). If CAP law coefficients represent true law impacts, they will presumably grow when Illinois and Wisconsin are excluded from the sample, but shrink when California and Massachusetts are excluded, all else equal. Finally, because the distinction between CAP law violations being a felony in California and Florida but only a misdemeanor in the other states represents the main reason that all else is not equal even when holding the age limit constant, we also test whether the more serious potential penalties in the felony states are associated with larger effects.

4. Results

Before proceeding to the estimation results, we briefly discuss the summary statistics presented in Table 2, which correspond to the main sample of 11 states that entered the NIS by 1989. The top and middle panels show statistics for the various injury types (i.e. the dependent variables) for youth and adults, respectively. As implied by the small injury means, especially relative to the maximums, the majority of hospitals report no gun injuries during the year. On average only about 0.1 percent of hospital discharges are gun injuries. Almost 60 percent of gun injuries are assaults, with unintentional injuries comprising most of the remainder and self-inflicted injuries being relatively rare, partially because the majority of self-inflicted gun injuries are fatal and thus do not show up in these data.[5]

The bottom panel of Table 2 describes the explanatory variables. About half of the observations constitute hospitals in states with a CAP law in effect. Half of sample hospitals are non-teaching hospitals in urban areas, with 70 percent of the remainder located in urban areas. About 55 percent of hospitals are private non-profit, another 20 percent are public, and around nine percent are private for-profit, with 14 percent not classified and the remaining four percent known to be private but not further classified. Less than one percent of hospitals specialize in treating children.

Tables 3–6 show estimated coefficients, standard errors and implied semi-elasticities, i.e.

[5] These numbers pertain only to the sample hospitals and are not comparable to national deaths or injury statistics.

$[\exp(\alpha_1) - 1]$, for the CAP law indicators. These are from Poisson regressions of equation 1 using various samples, dependent variables and model specifications.[6] Each cell represents parameters from a different regression model. In Table 3, row headings list the dependent variable; odd (even) numbered columns pertain to youth (adults).

The main results of the study are contained in the first two columns of Table 3, which use the main sample and specification as described earlier. In column 1 of the first row, the effect of CAP laws on self-inflicted gun injuries among youth is negative and highly significant, with a magnitude that is striking. The implied 64 percent reduction in youth self-inflicted injuries associated with CAP laws would represent an impressive preventative impact, but is so large as to beg the question of credibility. Our first supporting result appears in column 2, which shows that, in contrast to the huge effect for youth, CAP laws have a small and highly insignificant association with self-inflicted injuries among adults. This is as expected: CAP law provisions have no direct bearing on adult gun use other than to possibly make accessing guns slightly less convenient. The implication is that the youth coefficient is not merely the by-product of a negative correlation between CAP law implementation and unobserved trends in gun-related suicide attempts that are common to youth and adults but not controlled for adequately.

The estimated effects of CAP laws on various categories of gun injuries that are not self-inflicted, displayed in the second through fifth rows of Table 3, are also consistent with the result for youth self-inflicted injuries. Youth injury coefficients, in column 1, are negative and significant other than a marginally insignificant effect on unintentional injuries. This suggests that the self-inflicted injury effect is not simply a spurious correlate of a trend that is unique to youth suicide attempts. Yet the coefficient magnitudes are uniformly smaller for the non-self-

[6] Estimates for variables other than the CAP law indicators are available from the authors.

inflicted injuries, which is expected: while any impact of CAP laws on self-inflicted shootings will by definition be reflected in youth injury counts, some of the effect on non-self-inflicted shootings will spill over to adult injury counts because some youths will shoot adults rather than other youth. Indeed, in column 2, the CAP law coefficients are negative and significant for all non-self-inflicted categories for adults, with magnitudes that slightly exceed those for youth. In particular, for total non-self-inflicted injuries, the youth semi-elasticity of –0.33 is only one-half of that for self-inflicted injuries, while the adult semi-elasticity of –0.38 contrasts with the insignificant self-inflicted injury coefficient. McClurg (1999) similarly reports that adult gun victimizations may be reduced when CAP laws prevent unauthorized users from obtaining guns via theft. The response to CAP laws of gun injuries among adults is especially important in light of their much higher prevalence than among youth, i.e. the sample gun injury count is five times higher for adults than for youth.

An alternative explanation for these results is that they are not truly causal effects but rather are related to omitted factors that are correlated with CAP laws. The rest of the analysis seeks to address this possibility in various ways. The last two rows of Table 3 present results from regressions in which non-gun self-inflicted injuries and knife assaults are the dependent variables. These serve as the basis for additional counterfactual experiments: while these measures should reflect general trends in suicide attempts and assaults, respectively, neither should be influenced by CAP laws. Significant negative coefficients would therefore cast doubt on the causal nature of the earlier estimates by providing evidence that CAP laws are related to unmeasured injury trends. However, for both youth and adults, CAP laws are unrelated to either type of non-gun injury, diminishing the likelihood that underlying trends in violence directed against selves or others contribute to the estimated effects of CAP laws on gun injuries.

Remaining columns in Table 3 examine the sensitivity of the CAP law effects to changes in the sample and specification. Columns 3 and 4 repeat the column 1 and 2 exercises in the full sample of 21 states, including the 10 that entered the NIS no earlier than 1993. Earlier we argued that their exclusion should have little impact on our pseudo-experimental design because of the timing of the data and CAP law implementation, and indeed estimated coefficients change very little, other than the effect on unintentional injuries among youth increasing by about one-third and becoming marginally significant. Columns 5 and 6 return to the original 11 state sample but excludes state specific trends from the right hand side of the regression equation. The high significance levels attained by these trends in all models suggest that such exclusion is inappropriate. But it is reassuring that, although the magnitudes of the coefficients change somewhat, the only meaningful differences are that the youth unintentional injury effect further increases and becomes highly significant while the undetermined injury effect decreases for both age groups and loses significance for adults. Notably, the self-inflicted injury coefficient is still highly significant for youth and insignificant for adults, the total gun injury effects are one-third larger and maintain significance, and the non-gun injury coefficients remain insignificant.

Table 4 further examines the robustness of our results. The first two columns reflect models that control for the logged violent crime count in each state. Our intent is to capture underlying trends in violence that may be correlated with both gun injuries and CAP laws. In particular, the passage of CAP laws coincides with widespread declines in violent crime rates during the 1990s with which gun injury reductions are presumably associated. However, including violent crimes does not alter our conclusions: the CAP law coefficients in column 1 of Table 4 are virtually unchanged from the corresponding estimates in columns 1 (youth) and 2 (adults) of Table 3. Column 2 of Table 4 shows that state violent crime levels are largely unrelated to hospital injury counts and of inconsistent sign for the two injury types, adult assaults and youth undetermined, in which the correlation is significant. The similarity in CAP law coefficients for these two injury types between Tables 3 and 4 indicates that the correlation between state violence and hospital gun injuries is rather low.

Columns 3–5 of Table 4 take a different approach to examining the possibility of spurious correlation. Here we include two "lead" indicator variables that represent the fraction of the year that took place within one year, and between one and two years, before the effective law date. A reduction in gun injuries prior to the effective date might indicate that other factors are responsible for the negative CAP law coefficients. For instance, the buildup to CAP law passage typically includes publicity about specific gun injuries that CAP laws might have prevented. Such publicity could contribute to declines in gun injuries separately from any actual effect of the laws. Similarly, CAP law passage might be symptomatic of changing attitudes towards gun ownership that had already started to diminish gun injuries and would have continued to do so regardless of the presence of the laws. More generally, negative lead coefficients could indicate that gun injuries were already falling prior to the laws becoming effective, and thus cast doubt on whether the negative law coefficients reflect actual law effects.[7]

The results in columns 3–5 of Table 4, however, show that only for assaults is there a pre-existing downward trend in gun injuries. In both these cases, the one-year lead is significant only at the 10 percent level, and the two-year lead is small and insignificant (and positive for youth). More importantly, in neither case does the estimated CAP law effect change much. The youth coefficient is slightly larger, while the adult coefficient is smaller by about one-eighth in

[7] On the other hand, the time lag between when laws are enacted and effective, which in a few cases is more than a year, could create anticipation effects among gun owners who mistakenly take the enactment date as the effective date or to ensure compliance when the law goes into effect. These anticipation effects would be manifested by negative lead coefficients, but constitute true law effects because the corresponding injury reductions would not have occurred in the absence of the laws.

semi-elasticity terms and is still highly significant. In contrast, several of the one-year leads, and all but two of the two-year leads, are positive, and the two-year lead in the youth total injury equation is positive and significant. Even if this latter result indicates that increasing gun injuries prompted legislatures to pass CAP laws, our previous interpretation of the negative CAP law effects in Table 3 are unaffected.

Once again, the estimated CAP law parameters change little when the leads are added. Notably, the total injury coefficients increase slightly in magnitude, and remain highly significant even though the standard errors become much larger. The effect on self-inflicted injuries falls by about one-fourth, in semi-elasticity terms, but this is likely the artifact of removing the two relatively high-injury years directly preceding CAP law implementation from the pre-law comparison group. The decline in significance of the effects on undetermined injuries is largely the result of a substantial increase in the size of the standard errors. The combination of positive lead coefficients and negative CAP law coefficients strengthens the conclusion that it is the CAP laws, rather than other omitted factors, that lead to gun injury reductions.

Table 5 contains the results of another set of robustness checks. In this table we exploit the variation across states in the age at which the CAP law is phased out, i.e. the minimum age at which a person who accesses a gun is no longer considered by the law to be the responsibility of the parent. As Table 1 shows, among the 11 main sample states, Iowa and Wisconsin have the most narrow laws, covering only gun owners with children ages 13 and under, while California and Massachusetts have the broadest laws, defining children as those ages 17 and under. In order to isolate the age group affected by the discrepancy in age threshold across states, the dependent variable in Table 5 represents injuries among 14–17 year olds, rather than all children ages 17 and under. Moreover, Table 5 restricts attention to self-inflicted injuries, the only category for which the age of the person discharging the gun is known.

The first row of Table 5 provides a baseline estimate: using our original sample of 11 states and the specification from column 1 of Table 3, CAP laws reduce self-inflicted shootings among 14–17 year olds by 67 percent. This is quite similar to the estimate in column 1 of Table 3, because very few self-inflicted gun injuries occur among youth ages 13 and under. The next row reports results for the same regression, but with the CAP law indicator recoded to zero for observations from Iowa and Wisconsin. Because 14 year olds in these states are considered adults for CAP law purposes, the laws are not relevant for gun access among 14–17 year olds. It thus might be more appropriate to categorize these states as not having CAP laws from the perspective of this age group. As expected, this recoding yields larger negative effects of the CAP laws. The third row repeats this exercise with the CAP law for hospitals from Illinois, in which 14 year olds are covered by the law but 15–17 year olds are not, recoded to zero. The resulting estimate is slightly smaller than that of the preceding row, but still somewhat larger than the baseline estimate.[8]

The increase in CAP law coefficients when states with fewer youth actually covered by the laws are included with the control states is consistent with the CAP laws having a causal effect on youth self-inflicted gun injuries. By the same token, law coefficients in these states with lower age limits for the law should be relatively small. We examine this by re-estimating the same regression in samples that omit states that do not phase out their laws until age 16 or 18. The fourth row shows the estimate when observations from California and Massachusetts, the two states in which the entire age 14–17 group is covered by the law, are dropped from the

[8] When Illinois, Iowa and Wisconsin are alternatively dropped from the sample, the results are quite similar: the CAP law coefficient (standard error) is –1.373 (.649). Similarly, the coefficient (standard error) when just Iowa and Wisconsin, the states with an age 14 law limit, are dropped is –1.405 (.617).

sample. The magnitude of the CAP law effect is slightly smaller than that in the baseline model, although the primary reason for its insignificance is a large increase in the standard error. In the last row, further omitting hospitals from Massachusetts and New Jersey, the states that phase out their CAP laws at age 16, results in an additional decrease in coefficient magnitude and significance, with the semi-elasticity now about 25 percent smaller than when all 11 main sample states are included. This coefficient pattern gives us further confidence that the CAP law coefficients can be attributed to actual effects of the laws on gun access among youth. At the same time, the still sizable semi-elasticity in the last row is not necessarily unrealistic. Knowledge of the exact law provisions among gun-owning parents is likely imperfect, and gun access in the states with age limits of 14 and 15 is expected to remain restricted among youth age 14–17 who have siblings age 13 or under.

A potential explanation for the relatively large drop in coefficient size when moving from the penultimate row in Table 5 to the last row is that the CAP law in Florida, which was the first law to take effect and allows felony prosecution when child access results in an injury, has a disproportionate impact. This hypothesis stems from the previously documented finding of Webster and Starnes (2000) that a significant negative effect of CAP laws on mortality from unintentional firearm injuries in national data is entirely attributable to a very large effect of the law in Florida. To examine this possibility, column 1 of Table 6 re-estimates the original models of Table 3, columns 1 and 2, but with observations from Florida omitted from the sample. Coefficient sizes are indeed smaller for unintentional injuries and remaining categories besides assaults: semi-elasticites decline by about 25 percent for overall gun injuries but somewhat less than that for other categories except unintentional injuries. However, the estimate is insignificant only on unintentional injuries among youth, for which the effect was insignificant even in the full sample, and declines in significance level for other injury types are primarily attributable to standard error increases.

However, these results suggest the possible relevance of one additional law characteristic that is heterogeneous across states, namely whether the penalty for violation is a felony under certain circumstances. Besides Florida, California is the only other state in which felony prosecution is possible, and only when serious injury or death ensues. Still, the coefficient decrease when Florida hospitals are excluded begs the question of the importance of the potential for felony prosecution, particularly the associated increase in possible fine (e.g. $1,000 v. $10,000 in California) and jail time. Thus, we re-estimate the column 1 regressions after excluding observations from both California and Florida, and show the results in column 2. Besides a sharp drop in the effect on assaults, however, the coefficient magnitudes either remain relatively unchanged or increase in comparison with the sample from which only Florida hospitals are omitted. Overall gun injury semi-elasticities have fallen, but only by nine percent for adults and six percent for youth. Recoding the CAP law indicator to zero in all other states besides California and Florida increases the coefficients, as expected, though the effects on assault are still smaller than when Florida observations were omitted. Yet, omission of hospitals from states in which CAP law violation is a misdemeanor regardless of circumstance, which results in a comparison of California and Florida against the four main sample states that never enacted CAP laws, generally reduces the estimated law effects to levels close to or, in many cases, below those in column 2. In contrast, if the possibility of a felony violation was the driving force behind the results, excluding California and Florida observations in column 2 would presumably produce much smaller coefficients than excluding all CAP law states except California and Florida. It therefore appears that, although the possibility of felony prosecution might be important, the laws affect behavior even in states where the maximum penalty is a misdemeanor.

5. Conclusion

We have used annual hospital discharge data from 1988–2001 to investigate whether CAP laws decrease non-fatal gun injuries. Results from Poisson regressions that control for various hospital, county and state characteristics, including state-specific fixed effects and time trends, indicate that CAP laws substantially reduce non-fatal gun injuries among both children and adults. Our interpretation of the estimates as causal impacts is supported by the absence of effects on self-inflicted gun injuries among adults, non-gun self-inflicted injuries, and knife assaults, the failure of violent crime levels and law leads to attain significance or alter estimated law coefficients, and larger coefficient magnitudes in states where the law covers older children.

Cook et al. (1999) estimate that the average lifetime medical cost for a hospitalized gunshot survivor is $35,367 in 1994 dollars. The semi-elasticities in columns 3 and 4 of Table 3 (in combination with the relevant unreported injury count totals) imply that CAP laws prevented 829 gun injuries during 2001 in the 10 states from the full sample that have them (two self-inflicted and 71 others among youth, 756 among adults), which translates to $37,371,154 in medical costs saved in 2004 dollars. Analogously, CAP laws in the 11 states from the full sample that do not have them would have saved $20,646,548 in medical costs on hospitalized gun injuries that would not have occurred. These figures are conservative as estimates of annual nationwide gun injury costs since they ignore non-medical costs, non-fatal injuries in the 31 states not in the full sample and all fatal injuries.

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Table 1: NIS states and years

State	First year in NIS	Date CAP law took effect	Oldest age covered	Number of Observations
Main sample (11 states)				
Arizona	1989			189
California	1988	October 1, 1992	17	1,511
Colorado	1988			344
Florida	1988	October 1, 1989	15	2,027
Illinois	1988	January 1, 2000	14	1,059
Iowa	1988	April 5, 1990	13	1,038
Massachusetts	1988	October 1, 1992	17	443
New Jersey	1988	January 17, 1992	15	344
Pennsylvania	1989			798
Washington	1988			375
Wisconsin	1989	April 16, 1992	13	1,087
Full sample (21 states)				
Connecticut	1993	October 1, 1990	15	66
Georgia	1997			446
Hawaii	1997	June 29, 1992	15	17
Kansas	1993			533
Maryland	1993	October 1, 1992	15	275
Missouri	1995			279
New York	1993			485
South Carolina	1993			322
Tennessee	1995			373
Utah	1997			76

CAP law effective dates are from Webster et al. (2004). The main sample consists of the 11 states that contributed data to NIS during its first two years. The full sample also includes the 10 additional states that contributed data to NIS by 1998 (the year before the last CAP law was enacted in a sample state) other than Oregon, which is dropped because some hospitals have suspect (very high) injury counts in some years. All states are sampled annually once they enter NIS.

Table 2: Summary statistics for 11 state sample (*n* = 9,215)

Variable	Sum	Mean	S.D.	Minimum	Maximum
Injury counts, age < 18					
Self-inflicted firearm	238	.026	.187	0	4
Unintentional firearm	3,346	.363	1.978	0	76
Assault by firearm	5,615	.609	4.936	0	294
Undetermined firearm	995	.108	1.233	0	62
Total firearm (except self-inflicted)	9,950	1.080	6.261	0	299
Non-firearm self-inflicted	41,980	4.556	7.935	0	122
Assault by knife	3,226	.350	1.951	0	67
Injury counts, age ≥ 18					
Self-inflicted firearm	2,633	.286	1.104	0	18
Unintentional firearm	14,311	1.553	12.306	0	574
Assault by firearm	30,121	3.269	23.212	0	1,249
Undetermined firearm	4,954	.538	5.070	0	212
Total firearm (except self-inflicted)	49,345	5.3548	31.095	0	1,291
Non-firearm self-inflicted	241,612	26.219	38.346	0	425
Assault by knife	37,489	4.068	24.088	0	1,224
Explanatory variables					
State has CAP law		.495		0	1
Rural hospital		.352		0	1
Urban non-teaching hospital		.499		0	1
Small hospital size category		.369		0	1
Medium hospital size category		.324		0	1
Private for-profit hospital		.087		0	1
Public hospital		.192		0	1
Private non-profit hospital		.547		0	1
Unknown hospital ownership type		.138		0	1
Children's hospital		.008		0	1
Total discharges		7,436	8,071	3	91,530
County population		1,015,460	1,987,394	1,529	9,655,870
County unemployment rate		5.78	2.84	0	29.4
County unemployment rate is missing		.034		0	1
State population		12,598,538	9,289,106	2,768,393	34,501,130
State unemployment rate		5.46	1.48	2.5	9.2
Fraction state pop. 25+ college grad.		.227	.040	.163	.387
Fraction state population in rural area		.191	.110	.053	.398
State violent crimes		103,211	96,104	6,553	345,624

This sample consists of the 11 states that contributed data to NIS each year during 1989–2001.

Table 3: Effects of CAP laws on gun injuries

Sample:	11 states		21 states		11 states	
Sample Size:	9,215		12,087		9,215	
State-specific trends:	Included		Included		Excluded	
Age group:	< 18	≥ 18	< 18	≥ 18	< 18	≥ 18
Injury category	(1)	(2)	(3)	(4)	(5)	(6)
Self-inflicted firearm	-1.030**	-.054	-1.024**	-.113	-.790**	-.154
	(.370)	(.184)	(.364)	(.172)	(.260)	(.115)
	-.643	*-.052*	*-.641*	*-.106*	*-.546*	*-.143*
Unintentional firearm	-.165	-.341**	-.228*	-.366**	-.378**	-.538**
	(.109)	(.108)	(.121)	(.131)	(.121)	(.109)
	-.152	*-.289*	*-.204*	*-.307*	*-.315*	*-.416*
Assault by firearm	-.374**	-.455**	-.397**	-.476**	-.656**	-.537**
	(.099)	(.093)	(.110)	(.146)	(.137)	(.165)
	-.312	*-.365*	*-.327*	*-.379*	*-.481*	*-.415*
Undetermined firearm	-.884**	-.910**	-.728**	-.815**	-.505*	-.413
	(.267)	(.138)	(.237)	(.154)	(.278)	(.277)
	-.587	*-.597*	*-.517*	*-.557*	*-.397*	*-.338*
Total firearm	-.394**	-.482**	-.378**	-.466**	-.587**	-.587**
(except self-inflicted)	(.058)	(.057)	(.112)	(.135)	(.139)	(.122)
	-.326	*-.383*	*-.315*	*-.373*	*-.444*	*-.444*
Non-firearm self-inflicted	.038	-.044	.032	-.049	.086	.006
	(.058)	(.035)	(.065)	(.042)	(.061)	(.054)
	.039	*-.043*	*.032*	*-.048*	*.090*	*.006*
Assault by knife	-.042	-.209	.014	-.146	-.029	-.186
	(.240)	(.162)	(.301)	(.246)	(.248)	(.222)
	-.041	*-.189*	*.014*	*-.136*	*-.028*	*-.169*

Coefficients are from Poisson injury counts regressions that also include the hospital, county and state characteristics listed in Table 2 and state and year indicators. Standard errors adjusted for state-level clustering and heteroskedasticity of unknown form (which accounts for potential overdispersion) are in parentheses. * and ** denote statistical significance at the 10 and 5 percent levels, respectively, for a two-tailed test. Percentage effects on injury counts of a discrete law indicator change from 0 to 1 are in italics.

Table 4: Sensitivity of CAP law effects to inclusion of violent crime and leads

Specification includes:		State violence		CAP law leads		
Explanatory variable:		CAP law	Log (violent crimes)	2 years pre-law	1 year pre-law	CAP law
Injury category	Age	(1)	(2)	(3)	(4)	(5)
Self-inflicted firearm	< 18	-1.030**	.030	.280	.414	-.671*
		(.377)	(1.761)	(.445)	(.438)	(.408)
		-.643				*-.489*
Unintentional firearm	< 18	-.165	.034	-.085	.031	-.187
		(.108)	(.728)	(.157)	(.210)	(.200)
		-.152				*-.170*
	≥ 18	-.343*	-.333	.017	.215	-.210
		(.109)	(.488)	(.250)	(.216)	(.198)
		-.290				*-.190*
Assault by firearm	< 18	-.351**	1.087	.134	-.365*	-.496**
		(.095)	(1.051)	(.161)	(.208)	(.148)
		-.296				*-.391*
	≥ 18	-.437**	1.325**	-.063	-.348*	-.658**
		(.093)	(.649)	(.138)	(.190)	(.170)
		-.354				*-.482*
Undetermined firearm	< 18	-.866**	-2.136*	.475	-.244	-.762
		(.235)	(1.182)	(.352)	(.276)	(.477)
		-.579				*-.533*
	≥ 18	-.913**	-.349	.166	.159	-.724*
		(.143)	(.887)	(.325)	(.289)	(.407)
		-.599				*-.515*
Total firearm (except self-inflicted)	< 18	-.391**	.361	.183*	-.220	-.418**
		(.053)	(.673)	(.104)	(.165)	(.145)
		-.324				*-.342*
	≥ 18	-.477**	.607	.055	-.104	-.509**
		(.051)	(.444)	(.133)	(.186)	(.175)
		-.379				*-.399*

Coefficients are from Poisson injury counts regressions that also include the hospital, county and state characteristics listed in Table 2, state and year indicators and state-specific linear trends. Standard errors adjusted for state-level clustering and heteroskedasticity of unknown form (which accounts for potential overdispersion) are in parentheses. * and ** denote statistical significance at the 10 and 5 percent levels, respectively, for a two-tailed test. Percentage effects on injury counts of a discrete law indicator change from 0 to 1 are in italics. The sample is the 9,215 hospital/year observations from the 11 states included in NIS from 1989–2001.

Table 5: Effects of CAP laws with different age thresholds on self-inflicted injuries among 14–17 year olds

Specification	
Main sample, specification from column 1 of Table 3	-1.110**
	(.442)
	-.671
CAP law indicator recoded to zero for states with age 14 limit	-1.424**
(Iowa, Wisconsin)	(.538)
	-.759
CAP law indicator recoded to zero for states with age 14 or 15 limit	-1.359**
(Illinois, Iowa, Wisconsin)	(.547)
	-.743
Omits states with age 18 limit	-1.015
(California, Massachusetts)	(.668)
n = 7,261	*-.638*
Omits states with age 16 or 18 limit	-.704
(California, Florida, Massachusetts, New Jersey)	(.795)
n = 4,890	*-.506*

Coefficients are from Poisson injury counts regressions that also include the hospital, county and state characteristics listed in Table 2, state and year indicators and state-specific linear trends. Standard errors adjusted for state-level clustering and heteroskedasticity of unknown form (which accounts for potential overdispersion) are in parentheses. * and ** denote statistical significance at the 10 and 5 percent levels, respectively, for a two-tailed test. Percentage effects on injury counts of a discrete law indicator change from 0 to 1 are in italics.

Table 6: Sensitivity of CAP law effects to whether penalty is felony or misdemeanor

Specification:		Omits Florida	Omits felony states	CAP = 0 if not felony	Omits non-felony states
Sample size:		7,188	5,677	9,215	5,244
Injury category	Age	(1)	(2)	(3)	(4)
Self-inflicted firearm	< 18	-.786*	-.964	-1.177**	-.906
		(.406)	(.664)	(.401)	(.599)
		-.544	*-.618*	*-.692*	*-.596*
Unintentional firearm	< 18	-.057	-.180	-.228*	-.030
		(.159)	(.172)	(.132)	(.125)
		-.056	*-.165*	*-.204*	*-.029*
	≥ 18	-.187*	-.196	-.386*	-.297**
		(.097)	(.120)	(.218)	(.107)
		-.171	*-.178*	*-.320*	*-.257*
Assault by firearm	< 18	-.358*	-.121	-.339*	-.290
		(.184)	(.267)	(.186)	(.206)
		-.301	*-.114*	*-.288*	*-.251*
	≥ 18	-.431**	-.308	-.326*	-.291*
		(.217)	(.236)	(.192)	(.174)
		-.350	*-.265*	*-.278*	*-.252*
Undetermined firearm	< 18	-.799*	-.787**	-.970**	-.665
		(.452)	(.272)	(.322)	(.668)
		-.550	*-.545*	*-.621*	*-.486*
	≥ 18	-.638**	-.930	-.961**	-.392
		(.307)	(.235)	(.230)	(.274)
		-.472	*-.605*	*-.618*	*-.324*
Total firearm (except self-inflicted)	< 18	-.281**	-.263	-.470**	-.291
		(.124)	(.189)	(.146)	(.199)
		-.245	*-.231*	*-.375*	*-.252*
	≥ 18	-.348**	-.311*	-.495**	-.359**
		(.154)	(.171)	(.167)	(.182)
		-.294	*-.268*	*-.390*	*-.302*

Coefficients are from Poisson injury counts regressions that also include the hospital, county and state characteristics listed in Table 2, state and year indicators and state-specific linear trends. Standard errors adjusted for state-level clustering and heteroskedasticity of unknown form (which accounts for potential overdispersion) are in parentheses. * and ** denote statistical significance at the 10 and 5 percent levels, respectively, for a two-tailed test. Percentage effects on injury counts of a discrete law indicator change from 0 to 1 are in italics. Felony states are California and Florida, while non-felony states are Illinois, Iowa, Massachusetts, New Jersey and Wisconsin.