

Castle Doctrine Legislation: Unintended Effects for Gun Ownership?



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Justice Policy Journal • Volume 11, Number 2 (Fall)

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Abstract

Between 2000 and 2010, more than 20 states passed new or revised legislation referred to as the Castle Doctrine. These statutes provide citizens protection from criminal prosecution and civil liability in cases where an individual uses physical force to protect self or home. Advocated by the National Rifle Association, these statutes were intended to protect citizens using firearms as self-defense. Little research to date has examined their effects. This paper tests whether Castle Doctrine legislation affected gun ownership and acquisition, as approximated by the number of Federal background checks and the proportion of suicides attributable to firearms. Analyses treat both outcomes as time series spanning 2000-2010 with states as panels. Results indicate that Castle Doctrine legislation is associated with a long-term increase in the number of Federal background checks. Results for the proportion of suicides attributable to firearms are limited. Implications of these results and avenues for future research are discussed.

service. It would find compassion, instead of antipathy, for its beleaguered veterans. Following that, the nation would provide the resources necessary to meet the demands of a moral society that is committed to aiding its beleaguered veterans in reclaiming their civilian lives.

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Introduction

On the night of February 26th, 2012, 28-year-old George Zimmerman fatally shot unarmed 17-year-old Trayvon Martin in the gated Florida community where Zimmerman served as neighborhood watch coordinator (Kennedy, 2013). Zimmerman testified that he shot Martin in self-defense as the two were having a physical altercation (Kennedy, 2013). He was eventually acquitted after a highly publicized trial. Over a year later, in November 2013, Theodore Wafer fatally shot 19-year-old Renisha McBride through the front door of his home in Dearborn Heights, Michigan (Guarino, 2014). Wafer told police that he believed McBride was attempting to break into his home. Wafer was found guilty of second-degree murder and other charges in August 2014 (Goodman, 2014). Although unrelated incidents, both cases have a key factor in common: each defendant claimed to be protected by the "Castle Doctrine."

The term Castle Doctrine refers to legal statutes that explicitly protect an individual's right to use force to protect his/ her home (i.e. "a man's home is his castle") and person without necessarily having to retreat from the situation (Levin, 2010). Individuals whose circumstances meet certain requirements under these laws may be exempt from criminal prosecution, civil litigation, or both (Levin, 2010). Although laws like these have existed for centuries, many states have chosen to revise or expand legislation since 2005. Little is known about the effects of these legal changes for violent crime, citizen perceptions of victimization risk or other related factors.

A number of these unknowns came to the forefront of public debate as a result of the Trayvon Martin shooting noted above. In that particular case George Zimmerman called 911 to report a suspicious person walking in his neighborhood (CNN Library, 2014). That person was Trayvon Martin, an unarmed African American male walking home from a convenience store. Although instructed not to leave his vehicle, Zimmerman did so anyway, confronted Martin and fatally shot Martin in an altercation that followed (CNN Library, 2014). Zimmerman was not initially arrested or charged for the crime. Under Florida's Castle Doctrine statute Zimmerman had no duty to retreat before using deadly force to defend himself. Further, police were not permitted to arrest Zimmerman without probable cause to refute his claims of self-defense (Flock, 2012). Zimmerman was not charged for the crime until six weeks later (CNN Library, 2014). These events spurred lengthy public debate concerning racial profiling and shooting victims, legal immunity for aggressors, as well as gun carrying. In spite of this debate, many of the alleged effects of the statutes are unknown or unclear.

In particular, it remains unknown how Castle Doctrine legislation may affect gun ownership, especially since these statutes were backed by the National Rifle Association as legal protection for owners of firearms. Past research has shown that demand for and ownership of firearms can be sensitive to social and political context. McDowall and Loftin (1983), for instance, showed demand for handguns in Detroit was sensitive to a sense of collective security; demand increased when citizens felt that crime was a problem or when the number of police decreased. Past research also showed that fear of crime was predictive of protective ownership of firearms (Cao, Cullen, & Link, 1997; Lizotte, Bordua, & White, 1981; Young, 1985). Research by Holbert and colleagues (2004) has shown an association between handgun ownership and viewing crime-related content on television. Although Blair and Hyatt (1995) found that gun advertisements did not affect attitudes towards guns, it is unknown how Castle Doctrine legislation and its surrounding publicity might affect gun ownership or acquisition. Unlike television ads or series, state legislation carries with it the authority of government. By providing criminal and civil protections for use of force, Castle Doctrine legislation may make gun ownership more appealing as a form of self-defense. This may be the case particularly if guns are perceived as a deterrent to victimization.

Addressing this research question is imperative since gun ownership may have effects for violence. Some studies, for instance, found a positive association between rates of gun ownership and homicide rates (Philip J. Cook & Ludwig, 2006; Matthew Miller, Azrael, & Hemenway, 2002; Siegel, Ross, & King, 2013). Positive associations have also been found between gun ownership and suicide, so much so that the proportion of suicides attributable to firearms is a commonly used proxy for gun ownership (Kleck, 2004; M. Miller, Azrael, Hepburn, Hemenway, & Lippmann, 2006). Other research, however, found that using a gun for protection decreased the likelihood of certain violent crimes being completed or resulting in victim injury (Kleck & Gertz, 1995). Given these associations, it is important to assess how the recent wave of Castle Doctrine legislation has affected gun ownership and acquisition across U.S. states.

In addition to testing the impact of Castle Doctrine legislation on gun ownership, this paper makes several other contributions to existing literature. First, as there is no national gun ownership registry, many authors (Kleck, 2004) have attempted to measure ownership and acquisition through alternative measures. I use two such measures: the proportion of suicides committed by firearm and the number of FBI firearm background checks. Replication of analyses across these two constructs lends credence to results. Second, I assess effects of Castle Doctrine statutes nationwide rather than limiting myself to a single state or small collection of states.

This helps to avoid some threats to internal validity, such as selection or history, by analyzing states of widely varying characteristics passing legislation at different time points. Lastly, I include the legal characteristics of Castle Doctrine statutes as predictors in my models to determine whether effects vary by type of legislation. As I detail shortly, some states have passed fairly weak versions of the Castle Doctrine while others have passed versions that extend far beyond those observed in early law.

History & Development of Castle Doctrine Legislation

Today's modern Castle Doctrine statutes have their origins in medieval English common law. At that time, English common law made a distinction between self-defense and defense of one's home (Catalfamo, 2006). Law recognized that an individual had the right to protect his or her home from attack and unlawful entry. For self-defense law dictated that a person must make some attempt to retreat or withdraw from the situation before using force (Catalfamo, 2006). This was not the case for defense of residence. Law granted homeowners the right to defend their place of residence without retreat and gave homeowners the right to use force to defend their homes even if the intruder did not him/herself present a physical threat (Catalfamo, 2006). The term "Castle Doctrine" itself is attributed to Sir Edward Coke, the Attorney General of England in 1604 who stated in *Semayne's Case* (1604) that "the house of every one is to him as his castle and fortress, as well for his defence against injury and violence as for his repose" (Coke, 1604). This saying is often written as "a man's home is his castle" and referred to as the Castle Doctrine.

As with many aspects of English common law, this notion of a Castle Doctrine took hold in the early United States and gained favor through the 1800's (Boots, Bihari, & Elliott, 2009; Levin, 2010). In 1895, the U.S. Supreme Court upheld a person's right to use force in defense of one's home without a duty to retreat in *Beard v United States* (1895) (Justia, 2014). In 1921, the United States Supreme Court ruled in *Brown v. United States* that the duty to retreat was an unnecessary requirement in self-defense cases overall (Ross, 2007). Similar rulings have continued to uphold various aspects of the Castle Doctrine. Each state, however, developed its own version of these protections and state laws varied in the degree to which retreat was required, in what circumstances, and what legal protections ensued if force was used in the prescribed circumstances. Although these legal protections remained unchanged through the last century, many states have recently chosen to revise or expand these protections.

Some of these new laws simply reiterate that an individual has a reasonable right to self-defense in the face of physical attack provided he or she attempts to retreat or deescalate the conflict. Other states have extended these protections by removing the duty to retreat or by granting Castle Doctrine protections in locations beyond the home, such as a vehicle or workplace (Ross, 2007). Florida was the first of these states to pass such legislation (National Rifle Association, 2006). Introduced as Senate Bill 436, Florida's legislation was drafted in conjunction with National Rifle Association (NRA) lobbyist and former president Marion Hammer. Explaining her reasoning for advocating the law, Marion stated:

The courts have manipulated the law into a position where the law favors criminals rather than victims and law abiding citizens (...) Out on the street, the courts have imposed a duty to retreat. That basically says if you are attacked, you have to try to turn around and run before defending yourself. When you turn your back on a criminal, you make yourself infinitely more vulnerable. The bill we passed yesterday will allow you to decide whether or not you can get away or whether or not you're safer if you stand your ground and fight.(Democracy Now, 2005, p. np)

Florida's legislation, passed in 2005, became the basis for model Castle Doctrine legislation promoted nationwide through the American Legislative Exchange Council (American Legislative Exchange Council, 2012).

The wording of the Florida statute, in particular, states that an individual "has the right to stand his or her ground and meet force with force, including deadly force if he or she reasonably believes it is necessary to do so" (Florida Legislature, 2013). Under this law, an individual has no duty to retreat when attacked provided he/she is not engaged in illegal activity and is in a location he/she has a legal right to be (Florida Legislature, 2013). Legislation with this phrasing has been termed "stand your ground" legislation. While not all states passed as extensive a version of the Castle Doctrine, by 2007, 30 states had considered such legislation (Ross, 2007) and between 2000 and 2010, 25 states passed some form of Castle Doctrine legislation (see Table 1).

Table 1: Castle Doctrine Legislation 2000-2010 by State

State	Region	Effective Date	No Duty to Retreat Extended Beyond the Home	No Duty to Retreat Anywhere One Has a Legal Right To Be	Presumes Reasonable Fear of Bodily Harm	No Civil Liability
Alabama	South	4/4/06	Yes	Yes	No	Yes
Alaska	West	6/22/06	Yes	No	Yes	Yes
Arizona	West	4/24/06	Yes	Yes	Yes	Yes
Florida	South	3/23/05	Yes	Yes	Yes	Yes
Georgia	South	3/24/06	Yes	Yes	No	Yes
Idaho	West	4/14/06	No	No	No	Yes
Illinois	Midwest	7/28/04	No	No	No	Yes
Indiana	Midwest	3/28/06	Yes	Yes	No	Yes
Kansas	Midwest	3/30/06	Yes	Yes	No	Yes
Kentucky	South	4/14/06	Yes	Yes	Yes	Yes
Louisiana	South	3/28/06	Yes	Yes	Yes	Yes
Maine	Northeast	9/21/07	No	No	No	Yes
Maryland	South	5/21/10	No	No	No	Yes
Michigan	Midwest	7/20/06	Yes	Yes	No	Yes
Mississippi	South	3/28/06	Yes	Yes	Yes	Yes
Missouri	Midwest	7/3/07	Yes	No	Yes	Yes
Montana	West	4/27/09	Yes	Yes	Yes	No
North Dakota	Midwest	4/27/07	Yes	No	Yes	Yes
Ohio	Midwest	6/10/08	Yes	No	Yes	Yes
Oklahoma	South	5/12/06	Yes	Yes	Yes	Yes
South Carolina	South	6/9/06	Yes	Yes	Yes	Yes
South Dakota	Midwest	2/28/06	Yes	Yes	No	No
Tennessee	South	5/23/07	Yes	Yes	Yes	Yes
Texas	South	3/20/07	Yes	Yes	Yes	Yes
West Virginia	South	3/28/08	Yes	Yes	No	No

These developments were not made without controversy, however (Boots et al., 2009). A Lexis-Nexis search for “castle doctrine” in Florida newspaper articles yielded 355 results as of August 2014; similar searches led to 338 articles for Pennsylvania and nearly 1,000 articles nationwide. Advocates of Castle Doctrine legislation, as can be observed from Marion Hammer’s quote (Democracy Now,

2005), argue that the decision to use force to defend oneself is made in the moment, at a time of great risk, and that we should not force citizens to risk injury or death by attempting to flee. Opponents, however, argue that the laws will make prosecution more difficult and lead to an escalation of violence by removing legal penalties for using force. In Florida, prosecutors and law enforcement actually opposed the new law for this reason (Weaver, 2008). However, the claims made by either side of the debate are empirical questions and research is needed to address the true effects of the legislation.

Effects of the Castle Doctrine

To date there is very limited research evidence regarding the effects of Castle Doctrine legislation. Weaver (2008), as one example, described qualitative evidence from Florida suggesting that Castle Doctrine legislation resulted in delayed arrest and more challenges for prosecutors at the decision-to-file point. Roman (2013), using the Supplemental Homicide Reports associated with the Uniform Crime Reports (UCR), found that the percentage of homicides ruled justifiable was quite low on average (~2%), but significantly higher in states with “stand your ground” legislation. Further, there appeared to be racial disparities (based on shooter and victim) in whether a shooting would be ruled as justified (Roman, 2013). A more hotly debated issue related to the Castle Doctrine statutes, however, is their potential to escalate or diminish the occurrence of violent crime

Cheng and Hoesktra (2013) examined the effects of Castle Doctrine legislation on state-level violent crime rates using UCR data. They found no evidence that the laws acted as a deterrent for burglary, robbery, or aggravated assault. Instead, results indicated a roughly 8% increase in murder and non-negligent manslaughter in states passing Castle Doctrine legislation. It remains unclear what portion of this increase may be attributable to justifiable homicides, which the authors note may be underreported (Cheng & Hoekstra, 2013). Ren, Zhang, and Zhao (2012), focusing on Texas, examined the effects of Castle Doctrine legislation and a related shooting incident on violent crime in Houston and Dallas. The authors found a decrease in residential and business burglaries following the shooting incident, but only in Houston where the shooting occurred (Ren et al., 2012). The legislation itself did not seem to produce a deterrent effect until the shooting occurred (Ren et al., 2012).

Chamlin (2013), in contrast, found that Castle Doctrine legislation passed in Arizona in 2006 resulted in a lasting increase in robbery. The effect was immediate for armed robberies but delayed for weaponless robberies. Interestingly, the

author also found a lasting increase in counts of suicide following the passing of Castle Doctrine legislation. As the authors discuss, one possibility for this result is an increase in gun availability as an unintended consequence of the legislation. No research to date has empirically examined this possibility, a gap addressed by the present study. To place this research in context, however, I first outline current knowledge about gun ownership in the United States.

Gun Ownership

By some estimates (Krouse, 2012) there are 310 million non-military firearms in the United States; the estimated current population of the U.S. is 316 million (U.S. Census Bureau, 2014). Unfortunately, the number of individuals/ households owning guns can only be approximated; there is currently no national gun owner registry in the United States. As a result research on gun ownership and acquisition nationwide often draws on survey data. Cook and Ludwig (1996), for instance, reported results from the 1994 National Survey of Private Ownership of Firearms (NSPOF), a national telephone-based survey of 2,568 adults. At that time, the results indicated that roughly 25% of American adults personally owned a firearm (P. J. Cook & Ludwig, 1996). Further, approximately 10% of American adults owned more than 75% of the nation's firearms (P. J. Cook & Ludwig, 1996). Gun ownership was most common among middle-aged, middle-class whites (generally males) from rural areas (P. J. Cook & Ludwig, 1996). According to the survey findings, 46% of gun owners reported owning a gun primarily for protection against criminal victimization (P. J. Cook & Ludwig, 1996).

Similarly, Miller and colleagues (2006) reported results from the 2004 National Firearms Survey, finding that 38% of households and 26% of individuals reported owning at least one firearm. In line with the results of the 1994 study, ownership of guns was fairly concentrated; many gun owners reported owning multiple firearms (M. Miller et al., 2006). Ownership, again, was more common among males than females. As with the NSPOF results, 46% of the survey respondents reported that their primary reason for owning a gun was for self-defense (M. Miller et al., 2006). The second most commonly reported reason was sport. According to research by Dixon and Lizotte (1987), gun ownership was not related to violent values that might form a "subculture of violence."

Beyond these two examples, much of what we know about trends in gun ownership is drawn from the General Social Survey (GSS). The GSS began asking questions about gun ownership in 1973 and has continued to do so periodically ever since. In 1973, 49% of respondents reported having a gun or revolver in their

home or garage; in 2012, 34% said they had a gun in their home or garage and 22% reported personal gun ownership (Gewurz, 2013b). Similarly, the Pew Research Center has tracked gun ownership since the early 1990's and reported that 33% of American households had a gun in 2013 (Gewurz, 2013b). Gallup reported a higher percentage of 43% for 2013, however (Gewurz, 2013b).

Demographics of gun ownership reported by the Pew Research Center are consistent with those noted above from other surveys. Most gun owners are male and gun ownership is far more common among whites than minorities (Gewurz, 2013b). Ownership is more common among those ages 30+ than among adults under age 30 (Gewurz, 2013b). Rates of ownership are highest in the Midwest and South (27% and 29% respectively) and is substantially more common in rural areas (Gewurz, 2013b). Gun ownership also varies by political affiliation; 51% of gun owners identify with the Republican party while 61% of those in non-gun households identify with the Democratic party (Gewurz, 2013b).

Although these demographic characteristics are well matched with existing NRA membership, it remains unclear how NRA-backed Castle Doctrine legislation and its surrounding publicity might affect gun ownership. Approximately 9% of Americans have someone in their household who is a member of the NRA (Gewurz, 2013a). Even among non-members, however, Castle Doctrine legislation may raise awareness of firearms as a self-defense measure or imply that firearms can be used as a deterrent. Further, the laws remove a barrier to firearms as self-defense: legal responsibility. These factors may make gun ownership more appealing. As a result, I hypothesize that passing Castle Doctrine statutes will be associated with increased gun ownership/ acquisition.

According to the Pew Research Center (Gewurz, 2013a), 55% of those in non-gun households believe that stricter gun controls laws would make self-defense more difficult for homeowners. 40% of those in non-gun households say they would feel comfortable having a gun in the home (Gewurz, 2013a); 55% of those under 30 say the same. What these statistics suggest is that many of those currently in non-gun households are not opposed to gun ownership, personal or otherwise. Thus, it is quite possible that Castle Doctrine statutes may lead to increased gun ownership or acquisition, a proposition I test in the pages to follow.

Data

Gun Ownership/Acquisition

As there is no national gun ownership registry, proxy measures of state-by-state gun ownership and acquisition are drawn from two sources. The first is the National Instant Criminal Background Check System (NICS) managed by the Federal Bureau of Investigation (Federal Bureau of Investigation, 2014). Mandated by the Brady Handgun Violence Prevention Act of 1993 and implemented in 1998, the purpose of NICS is to provide an avenue through which authorized firearms dealers and sellers can ensure that there are no legal barriers to gun ownership (criminal record, etc.) for a potential buyer (Federal Bureau of Investigation, 2014). I use the monthly count of such background checks as a proxy measure of gun acquisition. To correct for a skewed distribution, the logarithm of this measure will be used as a dependent variable in analyses.

This measure has several weaknesses, however. First, private party sales and gun show sales may not result in a NICS background check. Only licensed Federal dealers are required to conduct Federal background checks. Second, a NICS background check may be used to issue a concealed carry permit rather than to authorize a firearm purchase. Lastly, an individual may purchase multiple weapons with a single background check or the same individual may acquire several weapons at different time points (resulting in multiple background checks). These scenarios cannot be distinguished in the data. As a result of these weaknesses, the NICS background check counts do not perfectly correlate with the number of guns acquired or the number of individuals who acquire guns. To offset this concern, I complement my analyses by considering an alternative measure of gun ownership.

Following the recommendation of Kleck (2004) in his review of over 20 proxies for gun ownership, I use the percent of suicide deaths that occur by firearm (hereafter abbreviated FS/S) as a proxy for the proportion of state residents who own guns. Kleck finds that this is the best cross-sectional indicator of gun ownership (2004). Although he concludes that no currently available measure is acceptable for measuring longitudinal trends (Kleck, 2004), I utilize this measure and the FBI weapons checks as complementary dependent variables to lend credence to my findings. Yearly counts of deaths by suicide overall and suicide deaths by firearm are obtained from the Centers for Disease Control (CDC). As this is a yearly rather than monthly measure, analyses focusing on this dependent variable will assess annual change and measures will be aggregated from months to years accordingly.

Castle Doctrine Legislation

The primary predictor in all analyses is a dummy variable indicator of when Castle Doctrine legislation passed for each state in the years 2000-2010. Effective dates (see Table 1) are drawn from Cheng and Hoesktra (2013). For states where no legislation passed in this time period, this variable is coded as 0 for all time points. For states that did change their statutes ($n = 25$), I test for both a step effect (0's for all time periods before the effective dates and 1's thereafter) and a pulse effect (1's for a brief period around the effective dates). In both instances I assess possible lagged effects and pulse effects of various lengths.

Controls

In any study of crime-related legislation and its effects one must ask whether crime rates themselves may be causal factors. I control for the number of violent crimes occurring each month using data drawn from the Uniform Crime Reports. The violent crime total I use as a control is the monthly sum of all murders and non-negligent homicides, robberies, and assaults. Given reporting discrepancies and/or missing data for this measure, I omit the following states from analyses: Alabama, Florida, Minnesota, and Kansas. Washington, D.C., is also omitted from analyses due to missing data on one of the dependent variables.

As has been discussed previously, Castle Doctrine statutes vary widely across states. To assess the potential impact of legal variation, I include dummy variables that indicate whether the new/ changed statutes have three key characteristics (see Table 1 for a summary by state). The first of these variables indicates whether the statute removes the duty to retreat anywhere a person has the legal right to be. This measure is highly correlated ($r = 0.57$) with removing the duty to retreat somewhere beyond the home, so I do not include a separate dummy variable for lesser extensions of "no duty to retreat." Since laws also vary in whether they require there to be imminent fear of bodily harm, I also control for this factor. Lastly, I include a dummy variable indicating whether the statute removes civil liability. All of these dummy variables are coded as 0 for no and 1 for yes. As would be expected, these measures are only available for states passing some sort of Castle Doctrine legislation. Information for this set of control variables is drawn from Cheng and Hoekstra (2013), although I include in my analyses four states which only make changes to the civil liability portion of the statute (Idaho, Illinois, Maryland, Maine). These four states were excluded in the Cheng and Hoekstra analyses since they did not make substantial changes to their statutes. I include them in analyses since even a small legal change may affect gun ownership or acquisition in some way.

Lastly, I control for a variety of state demographic characteristics that may affect rates of gun ownership or acquisition. These include region (Northeast, Midwest, South, West), percent male, percent black, percent Hispanic and total population. In all tables, South is the reference category for region. Each of these measures is drawn from U.S. Census Data; the U.S. Census provides population estimates for years in which no census was conducted. With the exception of region, all are yearly measures.

Method

Given data availability and the time frame in which most states have passed Castle Doctrine legislation (2005-present), my analyses will focus on the years 2000 to 2010 ($n = 132$ months, 11 years) across 46 states. For NICS weapons checks, time points are months. Time points are years for the firearm suicide proxy. Since my primary interest is in the effect of an event (passing of legislation) on a time series (gun ownership/ acquisition), I first conducted unit root tests to verify that both outcome series were difference-stationary within state (Raffalovich, 1994); both series pass this test.

The data were also tested for serial auto-correlation. This refers to the possibility that the error terms for different time periods may actually be correlated (Drukker, 2003). In time series data this is particularly likely for adjacent time periods. Failure to account for positive serial correlation can result in standard errors that are too low and effects that appear to be statistically significant when they are not (Drukker, 2003). The data were tested for serial autocorrelation using a test developed by Wooldridge (2002) and the presence of serial correlation was confirmed. Details regarding how this is accounted for in models are detailed below.

A second concern with state-based time series, however, is spatial auto-correlation, which refers to the possibility that adjacent states may be more similar than non-adjacent states. In the words of Waldo Tobler (1970), the first law of geography is that "everything is related to everything else, but near things are more related than distant things." Given that the data consist of many panels (states) and relatively few time points, the data were tested for spatial auto-correlation with a variety of methods. These included the Lagrange multiplier test described by Breusch and Pagan (1980), two semiparametric tests developed by Friedman (1937) and Frees (2004), as well as a parametric testing procedure described by Pesaran (2004). All consistently indicate serial auto-correlation which is accounted for in the models detailed below.

Analyses for the NICS outcome employ panel-data linear models estimated using feasible generalized least squares (StataCorp, 2013a). To account for serial autocorrelation, these models are specified to assume AR(1) autocorrelation within states and that the coefficient of the AR(1) process is specific to each state. This assumption is consistent with the results of the serial correlation tests described above. To account for spatial autocorrelation, each model also specifies a heteroskedastic error structure with cross-sectional (cross-state) correlation. The basic equation on which these models are based is given by:

$$y_{it} = x_{it}\beta + \epsilon_{it}$$

Where $i = 1, \dots, 46$ states and $t = 1, \dots, 132$ months, the number of months observed for each state. The coefficient β is assumed to be the same for all states. Cross-sectional correlation and autocorrelation are addressed by specifying variance structure. The variance structure, assuming cross-sectional correlation, is given by (StataCorp, 2013a):

$$\begin{bmatrix} \sigma_1^2 \mathbf{I} & \sigma_{1,2} \mathbf{I} & \cdots & \sigma_{1,46} \mathbf{I} \\ \sigma_{2,1} \mathbf{I} & \sigma_2^2 \mathbf{I} & \cdots & \sigma_{2,46} \mathbf{I} \\ \vdots & \vdots & \ddots & \vdots \\ \sigma_{46,1} \mathbf{I} & \sigma_{46,2} & \cdots & \sigma_{46}^2 \mathbf{I} \end{bmatrix}$$

The individual identity matrices along the diagonal above are replaced with more general structures to allow for serial correlation (AR(1)) where the correlation parameter is unique for each state (StataCorp, 2013a). These models require more time points than states and that the data are balanced; both conditions are met by the data.

For the FS/S outcome, tests reveal no significant autocorrelation. However, this time series has more states than time points. As a result, the models used above for the NICS outcome are not appropriate. Instead, I use a random effects model estimated using generalized least squares and robust standard errors (StataCorp, 2013b; Wooldridge, 2010).

Results

Trends in NICS Checks and Firearm Suicides

Yearly trends in the number of NICS weapons checks and the percentage of suicides attributable to firearms are shown in Figures 1 and 2, respectively. Although the average number of NICS checks per year was higher in Castle Doctrine states long before 2005 (when Florida passed its Castle Doctrine legislation), there

is a sudden shift upward from that point onward. This shift is far more pronounced in Castle Doctrine states than in states that have not passed such legislation. For FS/S, however, trends are not as clear. Overall, FS/S appears to be on a decreasing trajectory across states, both before and after 2005. For Castle Doctrine states, FS/S seems somewhat less variable year to year. Empirical testing is needed to assess whether trends for either outcome are associated with Castle Doctrine legislation.

Figure 1: Yearly Average Number of NICS Checks Across States by Legislation Status

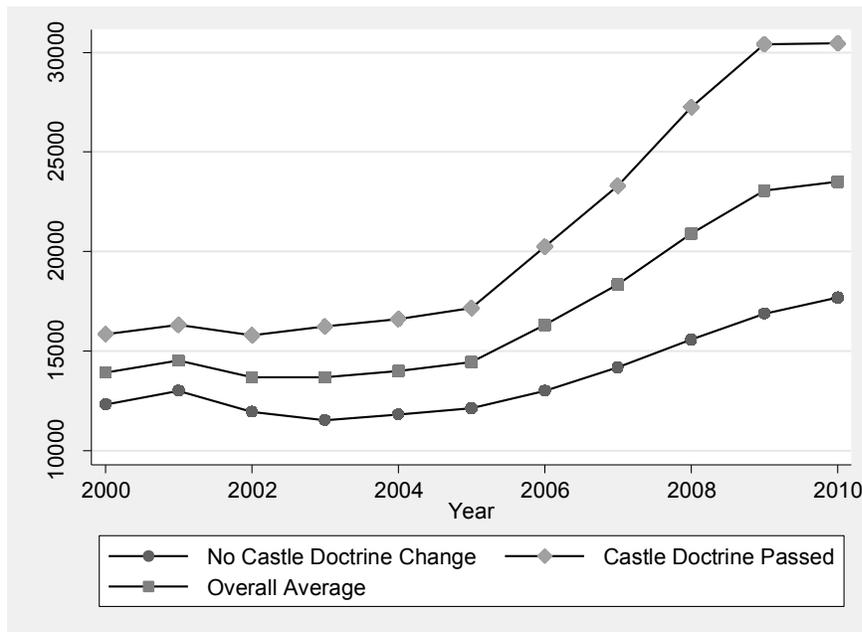
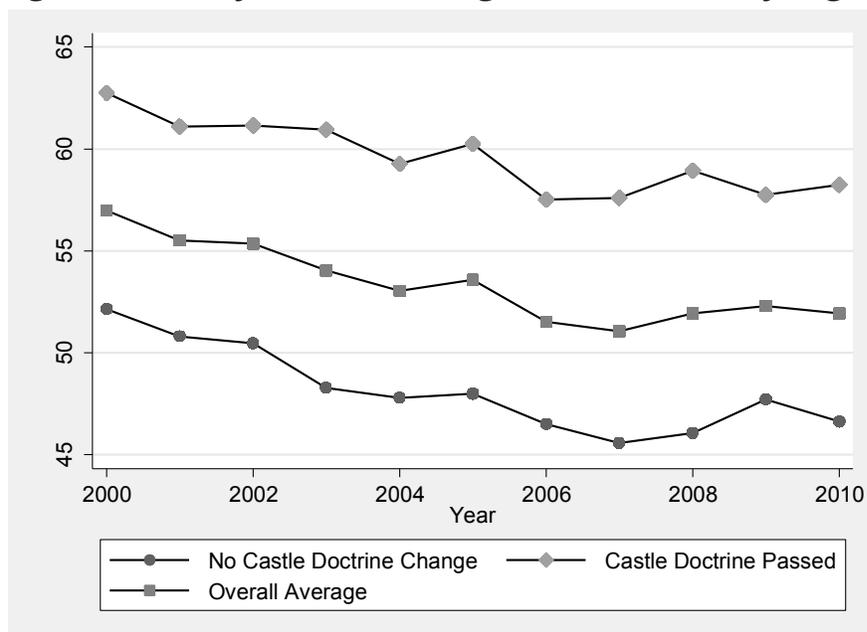


Figure 2: Yearly FS/S Percentage Across States by Legislation Status



NICS Background Checks

I first test for the presence of an immediate and permanent step effect of Castle Doctrine legislation on NICS Background Checks. In other words, does passing Castle Doctrine legislation lead to a permanent increase/ decrease in gun background checks that begins as the legislation becomes effective? Results, shown in Table 2, indicate that passing Castle Doctrine legislation leads to a statistically significant, modest, permanent increase in the number of NICS checks, even when controlling for key state demographic characteristics and the violent crime count. Examining legal variation among states passing legislation, it is apparent that removing civil liability is associated with an increase in the number of NICS checks. This is consistent with the argument that removing criminal and civil liability removes a barrier to gun ownership as a form of self-defense. Legislation removing the duty to retreat any place one has a legal right to be (“stand your ground” legislation) and legislation having a presumption of reasonable fear both have a negative association with the number of NICS checks. However, states with stand your ground legislation are also states with higher FS/S overall, perhaps indicating that these states already have high gun ownership rates.

Although the above results are informative, it is quite possible that the effects of Castle Doctrine legislation are temporary rather than permanent. Effects testing for one-month, two-month, and three-month pulse effects (temporary increase/

decreases) are shown in Table 2. Here I am still assuming an immediate effect at the time the legislation becomes effective (variations on this assumption are tested later). As these results show, any apparent one-month or two-month pulse effect is negligible and non-significant in the presence of controls (models not shown). Even when a three-month temporary effect surfaces, it is not apparent when legal variation controls are introduced (model not shown). However, these analyses were based on the assumption that effects would emerge at the effective date of the legislation. It is possible that effects might be delayed, particularly if publicity surrounding Castle Doctrine laws peaks after the effective date.

To test this possibility for a lagged but permanent effect, I create and test the effects of one, two, three, four, five and six month lags. For the sake of brevity I do not display all of these models in tables. However, the results of these models indicate increasing effect size peaking at four months post-legislation and declining in magnitude thereafter for the NICS checks outcome. Effects resulting from a four-month lag assumption are displayed in Table 3. Although effects are substantively the same as those shown in Table 2, larger effect sizes and better model fit indicate that effects are more delayed than immediate. Frequencies of NICS checks by month indicate that weapons checks peak annually in the months of October, November, and December. A four-month lag post effective date (see Table 1) falls within this range for a number of Castle Doctrine states.

Even though pulse effects were not identified by previous models, I also tested for lagged pulse effects of one month, two months, and three months in length. Rather than display all of these models, Table 3 shows results for a three-month long pulse effect lagged by four months post the effective date. As with the permanent effect models, effect sizes peaked at four months post-legislation. Pulse effects did not emerge as statistically significant for pulses of shorter duration than three months. This is suggestive of a longer-term effect of the legislation as observed when I modeled the impact as a permanent step. As before, the legislation is associated with an increase in NICS checks, even after accounting for key controls. Again, it is apparent that removing civil liability is associated with an increase in the number of NICS checks. Removing the duty to retreat any place one has a legal right to be ("stand your ground" legislation) and having a presumption of reasonable fear are both associated with decreased NICS checks.

As far as controls variables are concerned, there is significant variation by region (South is the reference category). There appears to be a greater effect for the Midwest compared to the South and a decreased impact for the Northeast compared to the South, while the South and West appear indistinguishable. There is no consistent variation by percent male, although this measures changes little

with time. Percent black and percent Hispanic, however, are both positively associated with NICS checks. This is consistent with a “racial threat” or increased fear of victimization perspective (Blalock, 1967). Percent other race has a negative association with NICS checks, perhaps because this category includes Asians who may be similar to whites on other characteristics. The violent crime count has a positive association with the number of NICS checks, as would be expected.

In sum, results indicate that the passing of Castle Doctrine legislation is associated with a long-term, if not permanent, increase in the number of NICS weapons checks. These effects hold even in the presence of controls for violent crime and key state demographic characteristics. Examining variation among states passing such legislation, results show that removing civil liability is associated with an increase in NICS weapons checks while removing the duty to retreat in all locations has a negative association with the number of NICS background checks. There is no consistent impact of the presumption of reasonable fear.

Percentage of Suicides Attributed to Firearms

Results of GLS random effects models for the FS/S outcome are presented in Table 4. In contrast to the NICS check counts, there appears to be a negative association between the passing of Castle Doctrine legislation and the percent of suicides attributable to firearms. This is the case regardless of whether the effect is modeled as permanent or temporary, lagged or immediate. These effects hold in the presence of controls only when modeled as a permanent immediate step. There is no evidence of a lagged effect for this outcome. Among Castle Doctrine states there are no statistically significant differences by type of legislation. These effects are consistent with the trends observed in Figure 2; in that figure there is no clear “jump” at the time Castle Doctrine statutes are passed (in contrast to the NICS outcome).