Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine

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Abstract

From 2000 to 2010, more than 20 states passed so-called "castle doctrine" or "stand your ground" laws. These laws expand the legal justification for the use of lethal force in self-defense, thereby lowering the expected cost of using lethal force and increasing the expected cost of committing violent crime. This paper exploits the within-state variation in self-defense law to examine their effect on homicides and violent crime. Results indicate the laws do not deter burglary, robbery, or aggravated assault. In contrast, they lead to a statistically significant 8 percent net increase in the number of reported murders and non-negligent manslaughters.

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1. Introduction

A long-standing principle of English common law, from which most U.S. self-defense law is derived, is that one has a "duty to retreat" before using lethal force against an assailant. The exception to this principle is when one is threatened by an intruder in one's own home, as the home is one's "castle". In 2005, Florida became the first in a recent wave of states to pass laws that explicitly extend castle doctrine to places outside the home, and to expand self-defense protections in other ways. Since then, more than 20 states have followed in strengthening their self-defense laws by passing versions of "castle doctrine" or "stand-your-ground" laws. These laws eliminate the duty to retreat from a list of specified places, and frequently also remove civil liability for those acting under the law and establish a presumption of reasonable fear for the individual claiming self-defense. For ease of exposition, we subsequently refer to these laws as castle doctrine laws.

These laws alter incentives in important ways. First, the laws reduce the expected cost of using lethal force. They lower the expected legal costs associated with defending oneself against criminal and civil prosecution, as well as the probability that one is ultimately found criminally or civilly liable for the death or injury inflicted. In addition, the laws increase the expected cost of committing violent crime, as victims are more likely to respond by using lethal force. The passage of these laws may also increase the salience of the legal protections in place, which may itself affect the decision of whether to use lethal force or commit violent crime. The purpose of our paper is to examine empirically whether people respond to these changes, and thus whether the laws lead to an increase in homicide, or to deterrence of crime more generally.

In doing so, our paper also informs a vigorous policy debate over these laws. Proponents argue these statutes provide law-abiding citizens with additional necessary protections from civil and criminal liability. They argue that since the decision to use lethal force is a split-second one that is made under significant stress, the threatened individual should be given additional legal leeway. Critics argue that existing self-defense law is sufficient to protect law-abiding citizens, and extending legal protections will unnecessarily escalate violence. These potential consequences have been of particular interest recently following some highly publicized cases.[1] In examining the empirical consequences of these laws, this study informs the debate over their costs and benefits.

We use state-level crime data from 2000 to 2010 from the FBI Uniform Crime Reports to empirically analyze the effects of castle doctrine laws on two types of outcomes. First, we examine whether these laws deter crimes such as burglary, robbery, and aggravated assault. In doing so, we join a much larger literature on criminal deterrence generally (e.g., Becker, 1968; Ehrlich, 1973; Di Tella and Schargrodsky, 2004; Donohue and Wolfers, 2009). More specifically, however, we join a smaller literature focused on whether unobserved victim precaution can deter crime. For example, Ayres and Levitt (1998) examine whether LoJack reduces overall motor vehicle thefts, while others have examined whether laws that make it easier to carry concealed weapons deter crime (Bronars and Lott, 1998; Dezhbakhsh and Rubin, 1998; Lott and Mustard, 1997; Ludwig,

[1] The most publicized case is that of Trayvon Martin, an unarmed teenager who was shot and killed by a neighborhood watch volunteer (Alvarez, 2012).

1998).[2]

We then examine whether lowering the expected cost of using lethal force results in additional homicides, defined as the sum of murder and non-negligent manslaughter. We also examine the effects of the laws on other outcomes in order to shed light on *why* homicides are affected by the laws.

To distinguish the effect of the laws from confounding factors, we exploit the within-state variation in the adoption of laws to apply a difference-in-differences identification strategy. Intuitively, we compare the within-state *changes* in outcomes of states that adopted laws to the within-state *changes* in non-adopting states over the same time period. Moreover, we primarily identify effects by comparing changes in castle doctrine states to other states in the same region of the country by including region-by-year fixed effects. Thus, the crucial identifying assumption is that in the absence of the castle doctrine laws, adopting states would have experienced changes in crime similar to non-adopting states in the same region of the country.

Our data allow us to test and relax this assumption in several ways. First, graphical evidence and regression results show that the outcomes of the two groups did not diverge in the years prior to adoption. In addition, we show that our findings are robust to the inclusion of time-varying covariates such as demographics, policing, economic conditions, and public assistance, as well as to the inclusion of contemporaneous crime levels unaffected by castle doctrine laws that proxy for general crime trends. This

[2] Our view is that relative to shall-issue concealed carry laws, the potential for castle doctrine law to deter crimes is quite large. For example, in Texas only 1.5 percent of adults age 18 and older have a concealed carry permit, and presumably only a fraction of those carry a gun on a regular basis (Texas Department of Public Safety, 2006; Texas Department of State Health Services, 2006; and authors' calculations). In contrast, Gallup polls indicate that from 2000 to 2009, 44 percent of households own a gun that could be used in self-defense against a burglar or assailant (Saad, 2011). Moreover, strengthened self-defense laws lower the cost of using a concealed carry weapon.

suggests that other known determinants of crime rates were orthogonal to the within-state variation in castle doctrine laws. Along similar lines, we offer placebo tests by showing that castle doctrine laws do not affect crimes that ought not be deterred by the laws, such as vehicle theft and larceny. Failing to find effects provides further evidence that general crime trends were similar in adopting and non-adopting states. Finally, we allow for state-specific linear time trends.

Results indicate that the prospect of facing additional self-defense does not deter crime. Specifically, we find no evidence of deterrence effects on burglary, robbery, or aggravated assault. Moreover, our estimates are sufficiently precise as to rule out meaningful deterrence effects.

In contrast, we find significant evidence that the laws lead to more homicides. Estimates indicate that the laws increase homicides by a statistically significant 8 percent, which translates into an additional 600 homicides per year across states that expanded castle doctrine. The magnitude of this finding is similar to that reported in a recent paper by McClellan and Tekin (2012), who examine these laws' effect on firearm-related homicide using death certificate data from Vital Statistics.[3,4] We further show that this divergence in homicide rates at the time of castle doctrine law enactment is larger than any divergence between the same groups of states at any time in the last 40 years, and that

[3] One advantage of using FBI UCR data is that it allows us to assess both how the laws affect the use of lethal force and whether they deter violent crime. In addition, the nature of the UCR data enables us to measure all homicides, rather than just those caused by firearms. The data also allow us to examine homicide subclassifications and relative changes in reported justifiable homicide from the SHR, along with assumptions about the degree of underreporting, to address the issue of whether the additional homicides are legally justified. The primary disadvantage of the UCR homicide data is that while the annual state-level data we use are regarded as accurate and there is no reason to believe that any total homicide reporting issue at any level should be systematically correlated with changes in castle doctrine law, the monthly data from Vital Statistics are more complete. However, we obtain nearly identical estimates to those reported when we exclude observations in the year in which the state adopted the law, indicating that this is not a problem.

[4] Our findings contrast with those of Lott (2010) in More Guns, Less Crime, who reports that castle doctrine laws adopted from 1977 through 2005 reduced murder rates and violent crime.

magnitudes of this size arise rarely by chance when randomly assigning placebo laws in similarly-structured data sets covering the years prior to castle doctrine expansion. In short, we find compelling evidence that by lowering the expected costs associated with using lethal force, castle doctrine laws induce more of it.

Finally, we perform several exercises to examine the possibility that the additional reported criminal homicides induced by the laws were in fact legally justified, but were misreported by police to the FBI. We conclude on the basis of these findings that it is unlikely, albeit not impossible, that all of the additional homicides were legally justified but were misreported by police as murder or non-negligent manslaughter.[5]

Collectively, these findings suggest that incentives do matter in one important sense: lowering the threshold for the justified use of lethal force results in more of it. On the other hand, there is also a limit to the power of incentives, as criminals are apparently not deterred when victims are empowered to use lethal force to protect themselves.

These findings also have significant policy implications. The first is that these laws do not appear to offer any hidden spillover benefits to society at large in the form of deterrence. On the other hand, the primary potential downside of the law is the increased number of homicides. Thus, our view is that any evaluation of these laws ought to weigh the benefits of increased leeway and protections given to victims of actual violent crime against the net increase in loss of life induced by the laws.

[5] Of course, there is also the issue of whether all legally justified homicides under expanded castle doctrine are socially desirable, which is beyond the scope of this paper.

2. Castle Doctrine Law and Data

2.1 Castle Doctrine Law

The principle of “duty to retreat” originates from English common law, whereby one had to “retreat to the wall” and thus no longer be able to retreat safely before responding to an attacker with deadly force (Vilos and Vilos, 2010). The exception to this rule is if the attack is inside one’s home, or “castle”, in which case there is no longer a duty to retreat. The duty to retreat, and the castle doctrine exception, remained influential in state statute and (primarily) case law, though the case law in some states diverged from the duty to retreat requirement over time.[6] In 2005, a wave of states began passing laws that explicitly put “castle doctrine” into state statute. More importantly, these laws also expanded castle doctrine by removing the duty to retreat from a list of specified places such as one’s vehicle, place of work, and in some cases, any place one has a legal right to be. Nearly all of these laws also strengthened legal protections in other ways as well. Some laws added a presumption of reasonable fear of imminent serious injury or death, which shifts the burden of proof to the prosecutor to show someone acted unreasonably.[7] Similarly, many laws also grant immunity from civil liability when using defensive force in

[6] For example, excluding the changes to Florida law in 2005, Koons (2006) classified 23 states as having required a defendant to retreat prior to using lethal force when retreat could be done in complete safety (Alabama, Alaska, Arkansas, Connnecticut, Delaware, Florida, Hawaii, Iowa, Maine, Maryland, Massachusetts, Minnesota, Missouri, Nebraska, New Hampshire, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina), 7 states as occupying a “middle ground” where retreat is a factor in determining whether lethal force is justified (Louisiana, Michigan, Oregon, South Dakota, Texas, Wisconsin, Wyoming), and 20 states as not requiring a duty to retreat (Arizona, California, Colorado, Georgia, Idaho, Illinois, Indiana, Kansas, Kentucky, Mississippi, Montana, Nevada, New Mexico, Oklahoma, Tennessee, Utah, Vermont, Virginia, Washington, and West Virginia). Our view is that removing duty to retreat in statute offers broader and more certain legal protections than does case law, and we note that nearly all of these changes to statute strengthened legal protections in other ways as well (e.g., adding a presumption of reasonableness, removing civil liability). We also note that even in the context of this varied legal background, passage of these laws is likely to heighten citizen awareness. Nevertheless, in Appendix Table A1 we show results using different subgroups of treatment states. The main finding there is that the estimated increase in homicide rates is larger for adopting states classified by Koons (2006) as previously requiring duty to retreat (11.7%) than for the other states (5.3%), though unsurprisingly given the sample size, the estimates are not statistically different from each other.

[7] For example, the law passed in Florida states that “a person is presumed to have held a reasonable fear of imminent peril of death or bodily injury to himself or herself or another when using defensive force that is intended or likely to cause death or bodily injury to another.”

a way justified under law. Collectively, these laws lower the cost of using lethal force to protect oneself, though they also lower the cost of escalating violence in other conflicts.[8]

Our understanding is that the main rationale for these laws was to provide additional legal leeway to potential victims in self-defense situations, not to deter crime. Thus, there is little reason to believe that the enactment of these laws coincided with either other policies expected to affect crime or homicides, or with expectations about future crime.[9] Nevertheless, we devote considerable effort toward assessing whether the estimated effects we find could be due to some other confounding factor(s), rather than to the changes in law.

To determine if and when states passed castle doctrine laws, we searched news releases and other sources such as the Institute for Legislative Action of the National Rifle Association to determine whether a state appeared to have passed a law that strengthened self-defense law these ways. Specifically, we coded the specific attributes of each state statute found, and classified whether the law i) removed the duty to retreat from somewhere outside the home, ii) removed the duty to retreat from any place one has a legal right to be, iii) added a presumption of reasonable fear for the person using lethal force, and iv) removed civil liability for those acting under the law. We then classified a state as having a castle doctrine law if they remove the duty to retreat in some place outside the home. Our goal in doing so was to create a list of states that expanded castle doctrine and generally passed meaningful changes to their self-defense law that would be widely

[8] These laws also typically state that the protections do not apply to those who are committing a crime at the time, or who instigated the conflict.

[9] For example, the National Rifle Association (NRA) was a major proponent of these laws (Goode, 2012). We are unaware of any statement by the NRA that suggests their support for the laws is due to a belief that the law will deter crime, or that the law is a necessary response to recent changes in violent crime. Rather, our understanding is that supporters view castle doctrine/stand-your-ground as an issue of individual and victim rights.

reported.[10]

Table 1 shows the states classified as having enacted castle doctrine laws between 2000 and 2010. We classify 21 states as having passed castle doctrine laws, as each of these states expanded castle doctrine protections to places outside the home.[11] Of these, 17 states removed the duty to retreat in any place one has the legal right to be, 13 included a presumption of reasonable fear, and 18 explicitly removed civil liability. Our main analysis groups all of these laws together, and thus captures the average intent-to-treat effect of passing a law similar to those passed in these 21 states. However, since that approach is perhaps unnecessarily blunt, in appendix Table A1 we show results for different subgroups of these states. We note, however, that due to the high degree of collinearity and the potential for interaction effects, we do not attempt to distinguish between the effects caused by the different attributes of these laws.

2.2 Data

Outcome data come from the FBI Uniform Crime Reports (UCR) and cover all 50 states from 2000 – 2010.[12] Specifically, we use homicide, burglary, robbery, and aggravated assault data from the official UCR data published online by the FBI.[13] In

[10] We are aware of four states that passed laws removing civil liability that that made no other changes to self-defense law over this time period, including Idaho (2006), Maryland (2010), Maine (2007), and Illinois (2004). We do not code those states as castle doctrine states. We also do not classify Wyoming as having passed a castle doctrine law, though we note that they removed civil liability and added a presumption of reasonable fear (provisions that removed the duty to retreat were stripped out prior to passage) (Vilos and Vilos, 2010). We thank McClellan and Tekin (2012) for helpful conversations about the specific attributes of laws passed in different states.

[11] To avoid confusion over which states are driving the within-state variation used in our study, we intentionally leave states off Table 1 if they had passed a law that expanded castle doctrine prior to 2000 or after 2010, which are outside our sample period.

[12] There are relatively few cases of missing data. Data on whether robbery was committed with a gun were missing from 2000 to 2005 for Illinois. Justifiable homicide data were missing for Florida, so we requested and received those data directly from the Florida Department of Law Enforcement Office.

[13] These data include corrections by the FBI to adjust for under-reporting by police agencies. We note, however, that results are qualitatively and quantitatively similar if we instead use data from the Supplemental Homicide Report and

addition, for the other variables not available from the online UCR, we use data from the FBI's Master files (Return A and Supplemental Homicide Report).

We use these data to test whether making it easier for individuals to use lethal force in self-defense deters crime or increases homicide. For deterrence, we focus on three criminal outcomes. The first is burglary, which is defined as "the unlawful entry of a structure to commit a felony or a theft" (FBI, 2004). The second is robbery, defined as "the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear" (FBI, 2004). Finally, we also examine aggravated assault, which the FBI defines as "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury", and is typically accompanied by the use of a weapon (FBI, 2004).[14] In all cases, one might expect rational criminals to be less likely to commit such crimes under expanded castle doctrine, as the increased scope for the use of justifiable lethal force on the part of the victim raises the expected cost to the criminal.

The homicide measure we use is total homicides, defined as the sum of murder and non-negligent manslaughter. We also look at murder separately to determine exactly how police are classifying the additional homicides.

An increase in criminal homicide could represent the escalation of violence by criminals, the escalation of violence in otherwise non-lethal conflicts, or, possibly, an

Return A from the FBI Master files, which were acquired directly from the FBI and include statistics reported after the deadline, but do not correct for under-reporting. For example, estimates corresponding to the homicide estimates in the 6 columns of Panel A in Table 5 are 0.0875, 0.0928, 0.0854, 0.0967, 0.0910, and 0.0729, respectively. All estimates but the last are significant at the 5 percent level.

[14] Results are similar using data on all assaults, including simple assault, which were obtained from Return A of the FBI Master files. We also note that it is possible that the laws could increase aggravated assaults by escalating violence in conflicts.

increase in legally justified homicide that is misreported as murder or non-negligent manslaughter.[15] In order to shed light on that issue, we look at two other outcomes, both of which measure the escalation of violence by criminals in response to castle doctrine. The ratio of robberies committed with a gun measures whether criminals respond by being more likely to carry and use weapons during the commission of a crime, as one might expect if they believe they will be faced with lethal force by the victim. We also look at felony-type and suspected felony-type murders, which also measure the escalation of violence by criminals. We expect to see increases in these outcomes if castle doctrine laws induce criminals to be more likely to carry and use deadly weapons during the commission of crimes.

In addition, we also ask whether the laws increase homicides that are reported to the FBI as "justifiable homicides by private citizens", which the FBI defines as "the killing of a felon during the commission of a felony" (Uniform Crime Reporting Handbook, 2004).[16] The major disadvantage of these data is that they are widely believed to be underreported; Kleck (1988) estimates that around one-fifth of legally justified homicides are reported that way to the FBI. However, note that we use these data only to look for evidence of *relative* changes in legally justified homicide. We then use those estimates, along with assumptions about the degree of underreporting, to determine if the entire

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[15] The general possibility that disputes can escalate dramatically in environments perceived to be dangerous is discussed in O'Flaherty and Sethi (2010).

[16] The Uniform Crime Reporting Handbook emphasizes that by definition, justifiable homicide occurs in conjunction with other offenses, and those other offenses must be reported. Additionally, the handbook gives examples of specific hypothetical events that would and would not qualify as justifiable homicide under the guidelines. An example given of an incident that would qualify as a justifiable homicide is "When a gunman entered a store and attempted to rob the proprietor, the storekeeper shot and killed the felon" (Uniform Crime Reporting Handbook, 2004). An example of what would NOT qualify as a justifiable homicide is "While playing cards, two men got into an argument. The first man attacked the second with a broken bottle. The second man pulled a gun and killed his attacker. The police arrested the shooter; he claimed self-defense" (Uniform Crime Reporting Handbook, 2004). We note that under expanded castle doctrine, the hypothetical shooter may have been justified as acting in self-defense, though again the reporting handbook explicitly states that this would not qualify as a justifiable homicide under the guidelines.

increase in criminal homicides can be explained as (misreported) legally justified homicides.

The data also allow us to perform several placebo, or falsification tests. Specifically, we use data on the rate of larceny and motor vehicle theft to determine whether castle doctrine laws appear to affect those crimes.[17] In both cases we expect to find no effects so long as the identifying assumptions of our difference-in-difference research design hold, which we discuss at length in the next section.

Finally, we have data on several time-varying control variables. Specifically, we observe the number of full-time equivalent police per 100,000 state residents (Uniform Crime Reports, 2000-2010). We also include both contemporaneous and lagged measures of the number of persons incarcerated in state prison per 100,000 residents (Bureau of Justice Statistics Bulletin, 2000-2010). These variables capture the effects of deterrence and incapacitation caused by additional policing or incarceration. In addition, we have two variables from the American Community Survey of the U.S. Census Bureau that measure local legal opportunities, including median family income and the poverty rate. We also have data on the share of white and black men in the 15-24 and 25-44 age groups for each state over time (American Community Survey, 2000-2010). Finally, we measure the generosity of public assistance in each state by measuring per capita spending on assistance and subsidies and per capita spending on public welfare (US Census, 2000 – 2010).

[17] While it may be possible for castle doctrine law to deter these crimes as well, our view is that deterrence should be considerably less likely for these crimes than for burglary, robbery, and aggravated assault.

3. Identification

To distinguish the effect of the castle doctrine laws from confounding factors, we exploit the within-state variation induced by the fact that 21 states passed such laws between 2000 and 2010. Specifically, we use a difference-in-differences research design that asks whether outcomes change more in states that adopt castle doctrine laws than in states that do not, and focus primarily on within-region comparisons.

Formally, we estimate fixed effects ordinary least squares (OLS) panel data models, where we follow convention and use the log of the outcome per 100,000 population as the dependent variable.[18] For homicide, we also estimate negative binomial models. Ordinary least squares models are estimated with and without weighting by state population.[19] The OLS model estimated is

$$\text{Outcome}_{it} = \beta_1 CDL_{it} + \beta_1 X_{it} + c_i + u_t + \varepsilon_{it}$$

where CDL_{it} is the treatment variable that equals the proportion of year t in which state i has an effective castle doctrine law, X_{it} is the vector of control variables, and c_i and u_t control for state and year fixed effects, respectively. In addition, in most models we also include Census region-by-year fixed effects, to allow states in different regions of the country to follow different trajectories and account for differential shocks by region over time.[20] Note that for states that enacted the law partway through a year, we set CDL equal to the proportion of the year in which the law was in effect, though estimates are almost identical

[18] See, for example, Ayres and Levitt (1998), Duggan (2001), and Lott and Mustard (1997). An alternative specification is to use the log of homicide count as the dependent variable, and control for the log of population. Estimates from that specification that correspond to those in column 3 of Table 5 are 0.097 and 0.0602 for weighted and unweighted OLS regressions, compared to estimates reported in Table 5 of 0.0937 and 0.0600.

[19] Specifically, we use analytic weights where average state population over the time period is the weight. This was done using the aweight command in Stata.

[20] There are four Census Regions: West, Midwest, Northeast, and South.

when we exclude the year of adoption.[21] Robust standard errors are clustered at the state level, though we also do additional exercises in the spirit of Bertrand, Duflo, and Mullainathan (2004) to ensure standard errors are being estimated accurately, as well as to perform inference using placebo estimates from pre-castle doctrine data. This last approach of using distributions of placebo estimates to do inference is similar in spirit to the permutation inference approach used in the synthetic control method by Abadie, Diamond, and Hainmueller (2010).

Since we primarily rely on specifications that include state fixed effects and region-by-year fixed effects, the identifying assumption is that in the absence of the castle doctrine laws, adopting states would have experienced changes in crime similar to non-adopting states in the same region of the country. Our data allow us to test and relax this identifying assumption in several ways. First, we look for graphical evidence of whether the two groups diverged prior to treatment. Along similar lines, we offer a formal statistical test by including an indicator in equation (1) for the two years prior to the passage of the laws. That is, we ask whether states that pass the laws diverge even *before* they pass the laws. If they do, it suggests that the identifying assumption of our research design is violated.

We also examine whether time-varying determinants of crime are orthogonal to the within-state variation in castle doctrine laws. Under our identifying assumption, factors such as economic conditions, welfare spending, and policing intensity should not change more over time in adopting states than non-adopting states, as this would suggest that crime

[21] Specifically, when we drop observations containing the year of adoption, estimates corresponding to column 3 of Table 5 are 0.0947, 0.0569, and 0.0895, compared to reported estimates in Table 5 of 0.0937, 0.600, and 0.0879, respectively.

in the two groups might have diverged even in the absence of treatment. Thus, we examine whether adding these controls changes our estimates in a meaningful way. To the extent that our difference-in-differences estimates remain unchanged, it provides some assurance that our research design is reasonable.[22]

Along similar lines, we also show results from specifications that include contemporaneous motor vehicle theft and larceny as controls. While it is possible that castle doctrine laws could affect these crimes, we would expect any such effects to be second-order and at most small in magnitude. Thus, we use these crime measures as controls that pick up any differential trends in crime in adopting and non-adopting states. We also perform falsification exercises using these crimes as outcomes to explicitly test whether castle doctrine laws appear to affect crimes unrelated to self-defense. If our identifying assumption holds, we would expect to see no effects on these crimes.

Finally, we allow for state-specific linear time trends, thereby allowing each state to follow a different trend.

4. Results

4.1 Falsification Tests

One way to test the identifying assumption is to directly examine whether crimes that ought not be affected by the laws—and thus proxy for general crime trends—appear to be affected by the laws.[23] Finding effects on crimes that ought to be exogenous to castle

[22] The primary concern is not that observed determinants vary systematically over time—we can control for those variables directly—but that if they do, it may suggest that unobserved determinants also change systematically over time in the treatment and control groups.
[23] Similar tests are performed by Ayres and Levitt (1998), when they look for effects of Lojack on crimes other than motor vehicle theft.

doctrine law would invalidate our research design.

Thus, we examine whether castle doctrine laws appear to affect larceny or motor vehicle theft. While it is possible that these outcomes are affected directly by self-defense laws, we argue that such effects should be second-order, at best.

Results are shown in Table 3, which uses a format similar to subsequent tables showing other outcomes. Columns 1 through 6 represent OLS estimates that are weighted by population, while Columns 7 through 12 are unweighted OLS estimates. The first column of each group controls for only state and year fixed effects. The second column adds region-by-year fixed effects, while the third column adds time-varying controls. The fourth column additionally includes an indicator variable for the two years before the castle doctrine law was adopted; the fifth drops the leading indicator but adds controls for contemporaneous larceny and motor vehicle theft. Finally, the last column controls for state fixed effects, region-by-year fixed effects, time-varying controls, and state-specific linear time trends.

Estimates for larceny are close to zero and statistically insignificant across all specifications. Estimates of the effect on the log of the motor vehicle theft rate are more interesting. Results in columns 1 and 7 in which only state and year fixed effects are included provide suggestive evidence of increases in motor vehicle theft of 5 to 8 percent, the latter of which is significant at the 10 percent level. However, including region-by-year fixed effects in columns 2 and 8 causes the estimate to drop to zero or even turn negative, and both are statistically insignificant. This suggests that accounting for differences in regional trends in some way may be important in assessing the impact of castle doctrine laws.

4.2 Deterrence

We now examine whether strengthening self-defense law deters crime. We examine three types of crime: burglary, robbery, and aggravated assault. To the extent that criminals respond to the higher actual or perceived risk that victims will use lethal force to protect themselves, we would expect these crimes to decline after the adoption of castle doctrine.

Results are shown in Table 4, where the first 6 columns show estimates from an OLS regression weighted by state population, while the last 6 columns are from unweighted OLS regressions. Results in Column 1 in Panel A for burglary are similar to the finding for motor vehicle theft, in that estimates range from 6 to 8 percent and are statistically significant at the 5 percent level. Again, however, including region-by-year effects in columns 2 and 8 reduces the estimates considerably, and all are statistically indistinguishable from zero at the 5 percent level.

Importantly, there is little evidence of deterrence effects in any specification for any outcome: of the 36 estimates reported, none are negative and statistically significant at the 10 percent level. The estimates are sufficiently precise as to rule out large deterrence effects. For example, in our preferred specification in column 3, the lower bounds of estimates on burglary, robbery, and aggravated assault are -2.1 percent, -1.9 percent, and -2.5 percent. Put differently, our estimates and standard errors from column 3 indicate that if we were to perform this castle doctrine policy experiment many times, we would expect that 90 percent of the time we would find deterrence effects of less than 0.7 percent, 0.4 percent, and 0.5 percent for burglary, robbery, and aggravated assault, respectively. In short, these estimates provide strong evidence against the possibility that castle doctrine laws cause economically meaningful deterrence effects. Thus, while castle doctrine law may well have benefits to those legally justified in protecting themselves in self-defense, there is no evidence that the law provides positive spillovers by deterring crime more generally.[24]

4.3 Homicide

We now turn to whether strengthening self-defense laws increases criminal homicide. Given that the laws reduce the expected costs associated with using violence, economic theory would predict that there would be more of it.

We start by showing the raw data in a set of figures. Figure 1 shows log homicide rates over time for adopting states and non-adopting states, by year of adoption. For example, Figure 1a shows the log homicide rate for the 2005 adopting state (Florida), relative to states that did not adopt the law from 2000 – 2010. While it is somewhat difficult to appreciate the magnitude of changes over time from the graphs and keeping in mind that the adoption year is only partially treated, two patterns emerge. The first is that with the exception of the two states adopting in 2008 (Ohio and West Virginia),[25] the

[24] It is worth noting that it is difficult to measure the benefits of these laws to actual victims of violent crime. These benefits could include fewer or less serious physical or psychological injuries, or lower legal costs. We make no attempt to measure these benefits in this paper.

[25] It is little surprise given the small sample sizes involved in this exercise that there would be some set of sets that did not track non-adopting states perfectly in trajectory for the entire period prior to treatment. In addition, we note that while homicide rates did increase in both Ohio and West Virginia from 2000/2001 to 2003, rates there tracked the rest of the country quite closely in changes from 2003 through 2007.

homicide rates of adopting states have a similar trajectory to those of non-adopting states *prior* to the adoption of the law.[26] That is, there is little reason to believe that the homicide rates of adopting states would have increased relative to non-adopting states in the absence of treatment.

Second, Figure 1 shows that there is a large and immediate increase in homicides for states adopting in 2005, 2006, and 2009. Similarly, while the 6 states that adopted in 2007 or 2008 did not appear to experience much of a relative increase in the year of adoption or the year afterward, they notably did not experience the relative drop in homicide rates that other states nationwide did in 2009 and 2010. Of course, given the small samples involved, it is difficult to infer much about short-term versus long-term patterns across these different sets of states, but it is clear from the raw data that castle doctrine states experienced a relative increase in homicides after adoption.[27]

Figure 2 shows the estimated divergence between adopting and non-adopting states over time, where t=0 is the year of treatment. Specifically, Figure 2 graphs coefficients from a difference-in-differences model in which we control for state and region-by-year fixed effects and time-varying covariates, and then allow for divergence 3 and 4 years prior to adoption, 1 and 2 years prior to adoption, the year of adoption, the 1st and 2nd years after adoption, and 3 or more years after adoption. Estimates are relative to

[26] As shown in Figure 1, adopting states have homicide rates that are about 30 percent higher than non-adopting states. However, because we are using a difference-in-differences research design that conditions on year and state fixed effects, differences in *levels* is not a concern for identification. Instead, what would worry us is if the homicide rate in adopting states increased more than in non-adopting states even before treatment, as that would suggest that the groups might have continued to diverge afterward, regardless of the change in law. We see no evidence of that, which suggests that the relative increase seen after 2005 is caused by castle doctrine law. Moreover, note that homicide estimates remained similar even after controlling for time-varying police and incarceration rates and other controls, including region-by-year fixed effects, and allowing for state-specific linear time trends.

[27] We note that estimates remain similar when Florida is excluded from the sample. For example, the estimate from population-weighted least squares declines only slightly from 9.37% to 8.69%, which is still statistically significant at the 1 percent level.

the average difference in log homicide rates 5 or more years prior to law adoption.

Consistent with Figure 1, there is little evidence of divergence in the years prior to adoption. For example, there was almost no divergence in the 4 years prior to adoption using the negative binomial model, and only around 1 to 2 percent using weighted OLS. For weighted OLS, the divergence increases to 10 percent after the year of treatment, and to around 8 percent in the negative binomial model. This offers of preview of the estimated effect on homicide of around 8 percent. There is more modest evidence of divergence prior to adoption using unweighted OLS, though there still appears to be a discrete change at the year of treatment from around 2.5 percent to 7 percent. The difference between the estimated pre-adoption divergence in weighted and unweighted specifications appears to be largely due to the small population states of North and South Dakota.[28]

We now turn to estimating the average effect of the laws in a difference-in-differences regression framework. Results are shown in Panels A, B, and C of Table 5, which show population-weighted OLS estimates, unweighted OLS estimates, and estimates from a negative binomial model. Estimates from the negative binomial regression are interpreted in the same way as those from a log-linear OLS model. Results from the population-weighted OLS model shown in Panel A indicate that the laws increased homicide rates by 8 to 10 percent; all 6 estimates are statistically significant at the 5 percent level, and 3 are significant at the 1 percent level. Estimates from unweighted OLS regressions shown in Panel B range from 5 to 9 percent, though all are

[28] In North Dakota, homicide rates per 100,000 population went from 0.87 in 2000-2002 to 1.5 in 2003-2006, prior to law adoption in 2007. Similarly, homicide rates went from 0.96 in 2000-2001 to 1.89 in 2002 – 2005 in South Dakota, who adopted the law in 2006. South Dakota averages 20 homicides per year and North Dakota averages less than 10, so we suspect the changes in the pre-adoption period were idiosyncratic.

measured imprecisely: t-statistics range from 0.6 to 1.5. Estimates in Panel C from a negative binomial model indicate castle doctrine leads to a 6 to 11 percent increase in homicide. All negative binomial estimates that include region-by-year fixed effects are significant at the 5 percent level, and that which does not (column 1) is significant at the 10 percent level. Finally, we note that homicide estimates are similar for various subsets of the adopting states, as shown in Appendix Table 1. The only difference is the estimates are somewhat larger, albeit not statistically so, for the subset of adopting states identified by Koons (2006) as not previously requiring duty to retreat in either statute or (more typically) case law.[29]

We have also done additional tests in order to ensure that we are making correct inferences about statistical significance. Toward that end, we do tests in the spirit of Bertrand et al. (2004), in which we randomly select 11-year panels from 1960 to 2004, and then randomly assign states to the treatment dates found in our data, without replacement. Thus, we assume that one state expanded castle doctrine on October 1st of the 6th year of the 11-year panel (just as Florida actually adopted in 2005, the 6th year of our panel), and that 13 more states adopted in the 7th year of the 11-year panel, etc. We generate distributions of estimates, and ask how often we reject the null hypothesis of no effect at the 5 percent level, as well as what proportion of the placebo estimates are larger than the actual estimated effect of (real) castle doctrine expansion. The latter figure corresponds to a p-value and is similar to the method used in synthetic control methods (Abadie et al., 2010), as well as by Chetty, Looney, and Kroft (2009).

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[29] This is consistent with what one would expect in that states that arguably received a more significant change in law experienced larger (albeit not statistically different) effects. See results in Appendix Table A1.

The resulting placebo distributions from 1,000 random draws are shown in Figure 3, and correspond to Table 5 results from column 2 of Panels A, B, and C, respectively. Results from population-weighted OLS placebo estimates suggest that robust clustered standard errors may be a bit too small: 10.0 percent of simulated estimates are significant at the 5 percent level. However, the estimate of 9.46 percent in column 2 ranks in the 95.4th percentile of placebo estimates, which means only 4.6 percent of placebo estimates are larger than it is.

Results for unweighted OLS simulation results are also interesting. On the one hand, simulations suggest that clustered standard errors from unweighted OLS regressions are accurate: 5.7 percent of the simulated estimates are significant at the 5 percent level. At the same time, however, the estimate of 8.1 percent shown in Table 5 corresponds to the 95.1st percentile, which would give it a p-value of 4.9 percent using the permutation-based approach to inference. This suggests that results in Panel B of Table 5 understate the degree of statistical significance.

Finally, simulations for the fixed effect negative binomial model corresponding to column 2 in Panel C indicate that 7.6 percent of placebo estimates are significant at the 5 percent level, while 14.1 percent are significant at the 10 percent level. As shown in Figure 3, the estimate of 7.3 percent in Table 5 ranks at the 95.7th percentile, as fewer than 5 percent of placebo estimates were larger than the actual estimate in the simulations.

On the basis of these exercises, we conclude that it is unlikely that we would have obtained estimates of the magnitude and statistical significance shown in Panels A, B, and C of Table 5 due to chance.

We have also performed simulations to see if the homicide rates of these particular 21 states *ever* diverged in the way they did after adopting castle doctrine in the late 2000s. To do so, we created 40 panel data sets, each covering separate 11-year time periods between 1960 and 2009. In each 11-year panel, we assume that Florida adopts castle doctrine on October 1st of the 6th year, and that the 13 states that adopted in 2006 adopted in the 7th year, etc. None of the 40 estimates corresponding to either the OLS population-weighted regressions or from the negative binomial regression were larger than those shown in column 2 of Table 5. In the case of the OLS unweighted regressions, only 1 of the 40 placebo estimates was larger than the actual estimate of 8.1 percent shown in Column 2, Panel B, of Table 5.[30] The average estimated divergence across the 40 years was -0.008, -0.004, and -0.005 across the unweighted OLS, weighted OLS, and negative binomial models.[31] Thus, there is no evidence that the homicide rates in castle doctrine states show a general tendency to increase relative to their regional counterparts: in the last 40 years they have almost never done so as much as they did immediately after castle doctrine.

Given the robustness of the estimates to various specifications, it is worth considering what one would have to believe for a confounding factor to cause the observed increase in homicide rates, rather than expansions to castle doctrine. That is, one would have to believe that something else caused homicides to increase relative to non-adopting states immediately after the laws were enacted, but not in the years prior to enactment. In

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30 The one larger estimate was 10.5 percent, and was from the 1975 to 1985 time period.

31 Estimates for the most recent 5 panels (1995 – 2005 through 1999 – 2009) were 0.022, 0.015, 0.004, -0.027, and -0.069 for weighted OLS, 0.01247, 0.02391, 0.00826, -0.02142, and -0.04719 for unweighted OLS, and 0.004, -0.003, -0.0185, -0.0562, and -0.106 for negative binomial. In these latter panels, we exclude all state-year observations when expanded castle doctrine was actually in effect, so as not to bias placebo estimates upward due to the real treatment effect.

addition, this confounder must have only caused a divergence in homicide rates in the late 2000s coincidental with the passage of castle doctrine law, and not at any point in the 40 years prior. Furthermore, this confounder must cause an increase in homicides in castle doctrine states after adoption, but not cause a similar increase in states *in the same region of the country* that did not expand castle doctrine at that time. Additionally, the confounder must cause adopting states to diverge from their own pre-adoption trend in homicide rate, coincidental with the enactment of castle doctrine law. The confounder must also increase homicides in adopting states after adoption without causing proportionate increases in motor vehicle theft, larceny, robbery, burglary, or aggravated assault. Finally, the confounder must be uncorrelated with changes in the economic conditions, welfare generosity, and the rates of incarceration and policing in adopting states immediately following adoption. We are unable to think of any confounding factor that would fit this description, and thus we interpret the increase in homicides as the causal effect of expanded castle doctrine.

4.4 Homicide: Interpretation

Collectively, we view these findings as compelling evidence that castle doctrine laws increase homicide. However, we note that one downside of the homicide measure is that it could potentially include homicides that are justified under the new self-defense law, but were improperly reported as criminal homicides rather than justifiable homicides. If all the additional homicides were misreported as criminal homicides, the increase may not be viewed by everyone as unambiguously bad. We note, however, that the net increase cannot be driven by a one-to-one substitution of homicides of assailants for homicides of innocent victims. In contrast, in order for the entire increase in homicide to be driven by life-saving use of force, there would have to be at least some cases of multiple killed assailants by a would-be-killed victim.

To shed light on this issue, we look directly for evidence for or against the different interpretations of the increase in reported homicide. We start by examining whether the laws increase the number of homicides classified as murders. This classification available in the Return A files excludes non-negligent manslaughter classifications that one might think would be used more often in potential self-defense killings not classified as justifiable homicides. Estimates in Panel A of Table 6 indicate a similarly sized increase in murder, which suggests that police are largely classifying these additional homicides as murders.

We then turn to assessing whether criminals appear to escalate violence in response to castle doctrine laws. For example, a rational criminal may respond to a real or perceived increase in the likelihood of encountering a victim willing to use lethal force by using a deadly weapon himself. Thus, we examine whether expanded castle doctrine increases felony-type and suspected felony-type murders, which appeared to be committed during a felony. Results are shown in Panel B of Table 6. The estimate from column 1, which controls only for state and year fixed effects, is 10 percent and is statistically indistinguishable from zero. Estimates from specifications including region-by-year fixed effects are more suggestive of a criminal escalation effect: estimates in columns 2 through 5 are around 20 percent and are statistically significant at the 10, 5, 1, and 5 percent levels, respectively, though we note the estimate goes to zero when allowing for state-specific time trends in column 6. We also examine whether criminals are more likely to use guns during robberies.[32] Results in Panel C of Table 6 indicate that there is little evidence of this type of escalation, at least once one compares states to others in their same region.[33] In short, while we find suggestive evidence of escalation by criminals, it is not conclusive.

Finally, we turn to evidence on whether the laws increase the reported number of justifiable homicides. The problem with these data is that justifiable homicides are believed to be underreported: Kleck (1988) estimates that only one-fifth of legally justified homicides by civilians are reported. Only 200 to 300 homicides are classified this way every year in the U.S., compared to around 14,000 total criminal homicides. However, even though the *level* of justifiable homicides may be underreported, *relative* changes in justifiable homicide may still be informative. As a result, we focus on examining the relative increase in reported justifiable homicide, and then estimate how many additional legally justified homicides there really are by scaling the pre-castle doctrine figure by estimates of underreporting.

Results are shown in Panels D and E of Table 6. Panel D shows estimates from unweighted regressions in which the number of justifiable homicides is the dependent variable. Estimated effects range from 1 to 4.3 additional justifiable homicides, which is relative to a baseline average of 4.9 justifiable homicides per state in the year prior to castle

[32] We also look at the proportion of assaults in which a gun was used and find no evidence of an increase, though the baseline rate is small (3 percent). We also note that examining these ratios as outcome variables could be problematic if the laws were found to reduce robbery or aggravated assault. However, as we show in Table 4 there is no effect on robberies or aggravated assaults.

[33] It is difficult to think of how using other FBI classifications could help answer this question. For example, the FBI classifies some non-felony-type homicides as having originated in an argument. It is difficult to know, however, whether the argument would have resulted in serious injury to the killer, had that person not used lethal force, or if the argument escalated from, say, a fistfight into a homicide. Yet most would agree that the latter is more disturbing than the former.

doctrine expansion. The estimate in our preferred specification in column 3 is 3.2, is statistically significant at the 5 percent level, and represents a 65 percent increase.[34, 35]

Panel E reports estimates from a negative binomial model. Estimates range from an insignificant 28 percent increase to a significant 57 percent increase.

Using these estimates, we now turn to assessing whether the relative increases observed in Table 6 can explain the entire increase in homicide, given estimates of the degree of underreporting of legally justified homicide. The largest estimated relative increase from a specification in Table 6 that controls for region-by-year fixed effects is 70 percent, which is relative to a baseline total of 103 justifiable homicides across the 21 states in the year prior to castle doctrine enactment. We assume that i) police departments are not *less* likely to report an otherwise-identical homicide as justifiable after castle doctrine expansion, and ii) the relative increase in legally justified homicide due to the change in law is no lower for reporting agencies than for non-reporting agencies. We view the first of these assumptions as likely to hold, and the second as reasonable, though we emphasize that they are in fact assumptions. Combining these assumptions with our estimates in Table 5 suggests that the true castle-doctrine-induced relative increase in legally justified homicide across the 21 states should be no larger than 70 percent.

Kleck (1988) reports that approximately one-fifth of legally justified homicides are reported correctly, while the others are classified as (criminal) homicides. Given the 103 reported pre-castle doctrine justifiable homicides, that suggests that the true figure is 515.

[34] In contrast, we find no evidence of an increase in justifiable homicide by police, consistent with the identifying assumption. Results are shown in Table A2 of the web appendix.
[35] Estimates from weighted OLS are broadly similar. Specifically, estimates corresponding to those in columns 1 through 5 of Table 6 were 9.6***, 6.0**, 4.6*, 4.8, and 4.6*, respectively, where asterisks denote statistical significance. The population-weighted baseline state average was 10.0 justifiable homicides per year.

A 70 percent increase means that castle doctrine expansion causes an additional 361 legally justified homicides, of which 289 (80 percent) would be (mis)reported as homicides. Recall that estimates from Table 5 indicate that castle doctrine law causes approximately an 8 percent increase in homicide, which translates to an additional 611 homicides given the 7,632 pre-castle doctrine homicides. Thus, under these assumptions, our best estimate is that no more than half of the additional homicides caused by castle doctrine law were legally justified.

Of course, different assumptions yield different conclusions. For example, assuming that only 10 percent of legally justified homicides are reported correctly, along with a 70 percent relative increase and the second assumption outlined above, would suggest that all of the additional homicides were legally justified.

To summarize our results, we find no evidence that strengthening self-defense law deters crime. On the other hand, we find that a primary consequence of castle doctrine laws is to increase homicide by a statistically and economically significant 7 to 10 percent. Relative increases in justifiable homicide along an estimate of the degree of underreporting suggest that it is unlikely, but not impossible, that the additional reported criminal homicides consist entirely of legally justified homicides. We emphasize, however, that one's conclusion on that issue depends on assumptions about the nature and degree of underreporting of legally justified homicides.

5. Conclusion

In recent years, more than 20 states have strengthened their self-defense laws by adopting castle doctrine laws. These statutes widen the scope for the justified use of lethal force in self-defense by stating the circumstances under which self-defense is justified and removing the duty to retreat from a list of protected places outside the home. In addition, in many cases they also establish a presumption of reasonable fear and remove civil liability. Thus, these laws could hypothetically deter crime or, alternatively, increase homicide.

Results presented indicate that expansions to castle doctrine do not deter crime. Furthermore, our estimates are sufficiently precise as to rule out moderate-sized deterrence effects. Thus, while our view is that it is *a priori* reasonable to expect that strengthening self-defense law would deter crime, we find this is not the case.

More significantly, results indicate that castle doctrine laws increase total homicides by around 8 percent. Put differently, the laws induce an additional 600 homicides per year across the 21 states in our sample that expanded castle doctrine over this time period. This finding is robust to a wide set of difference-in-differences specifications, including region-by-year fixed effects, state-specific linear time trends, and controls for time-varying factors such as economic conditions, state welfare spending, and policing and incarceration rates. These findings provide evidence that lowering the expected cost of lethal force causes there to be more of it.

A critical question is whether all the additional homicides that were reported as murders or non-negligent manslaughters could have been legally justified. Based on the results of various tests and exercises performed here, our view it is that this is unlikely, albeit not impossible.

With respect to policy, our findings suggest that an informed debate over these laws will weigh the benefits of increased protections given to victims against the net increase in violent deaths that result. More broadly, our findings indicate that incentives and expected costs matter when it comes to the decision of whether to use lethal force.

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Figure 1: Log Homicide Rates Before and After Adoption of Castle Doctrine Laws, by Year of Adoption

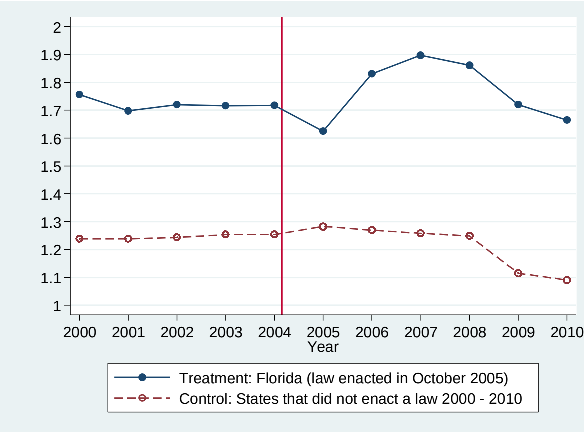


Figure 1a: 2005 State Adopting in 2005(Florida)

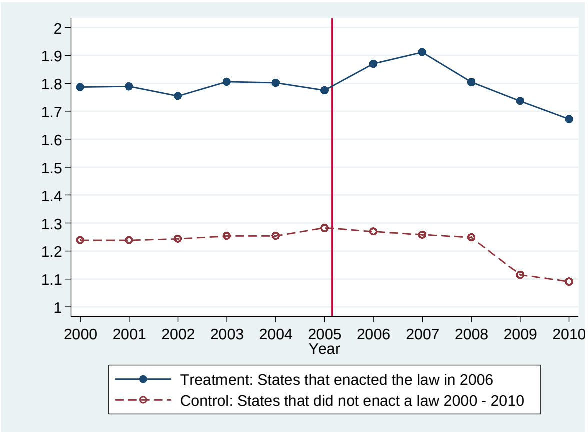


Figure 1b: States Adopting in 2006 (Alabama, Alaska, Arizona, Georgia, Indiana, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, South Dakota)

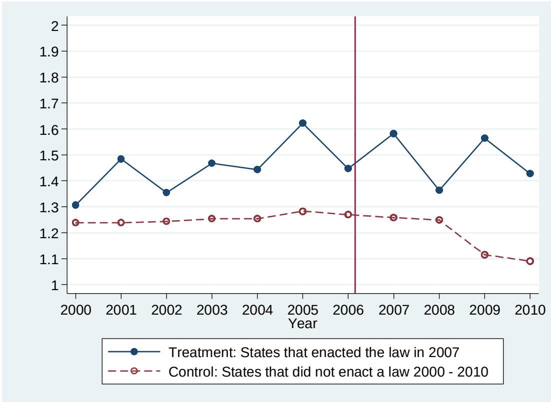


Figure 1c: States Adopting in 2007 (Missouri, North Dakota, Tennessee, Texas)

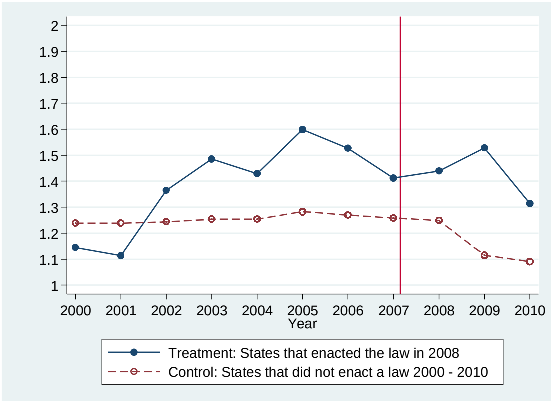


Figure 1d: States Adopting in 2008 (Ohio, West Virginia)

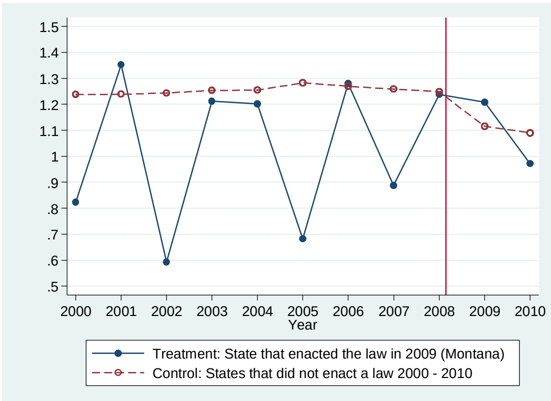


Figure 1e: State Adopting in 2009 (Montana)

Figure 2: Divergence in Log Homicide Rates Before and After Adoption of Castle Doctrine Laws, Relative to the Difference 5 or More Years Before Adoption

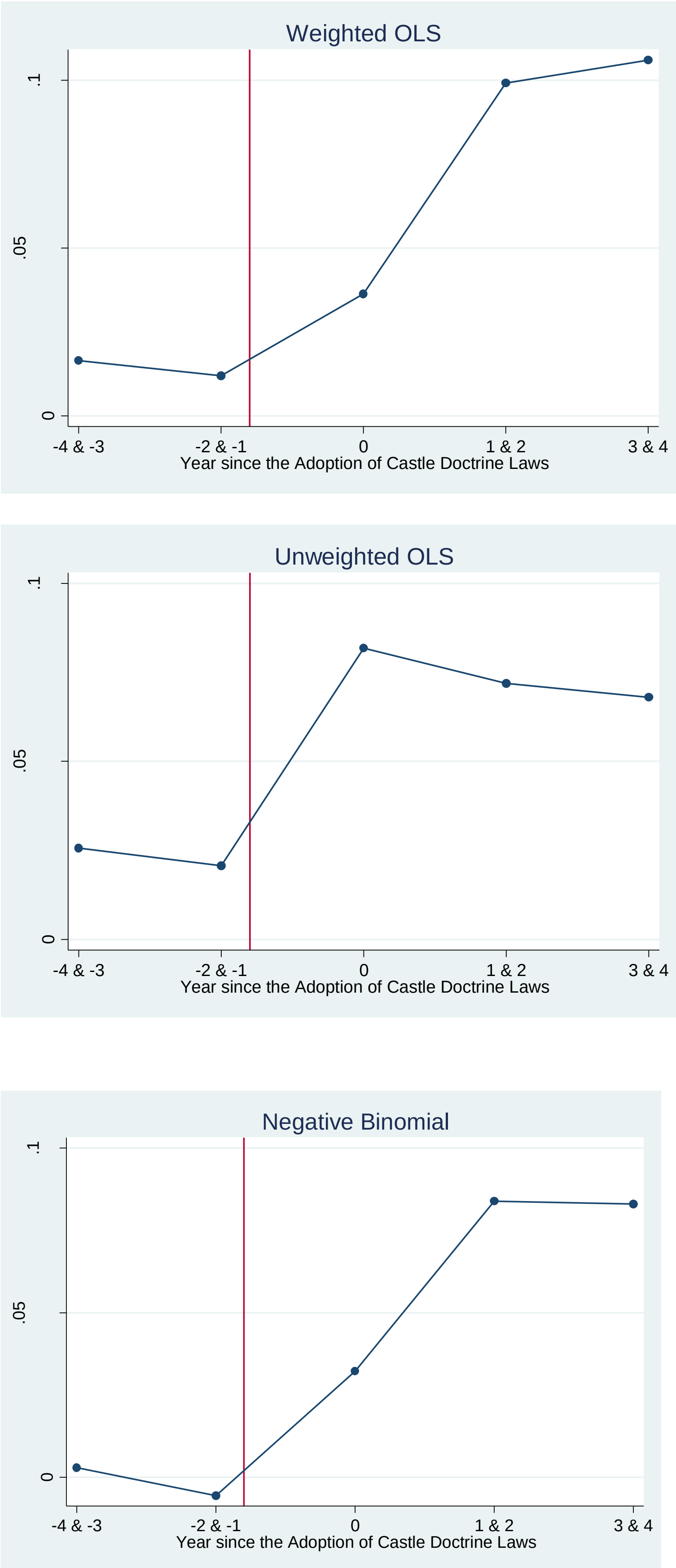
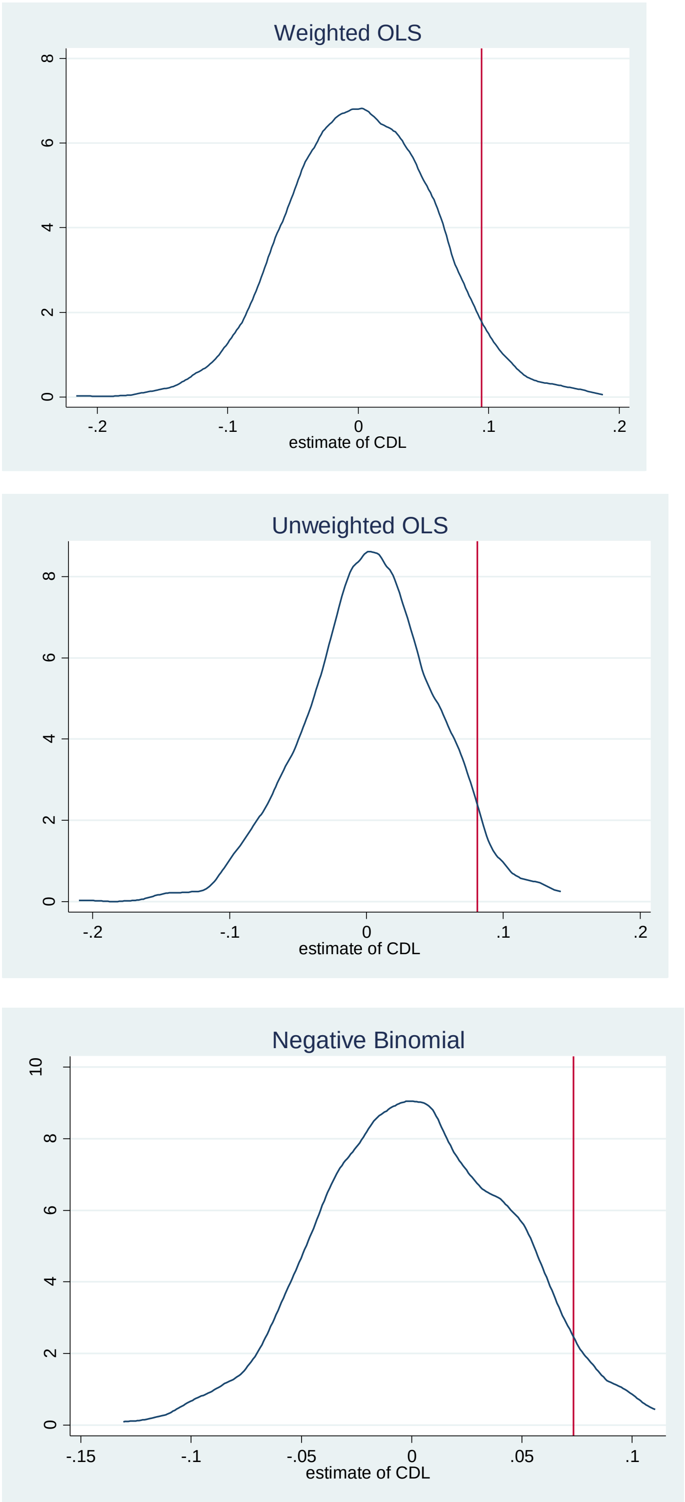


Figure 3: Empirical Distributions of Placebo Homicide Estimates



Notes: The vertical lines represent the actual estimated effects of castle doctrine on log homicide. These estimates are 0.0946, 0.0811, and 0.0734 and correspond to population-weighted OLS, unweighted OLS, and negative binomial estimation, respectively, as shown in Column 2 of Table 5. A total of 4.6 percent, 4.9 percent, and 4.3 percent of placebo estimates lie to the right of these estimates.

Table 1: States that Expanded Castle Doctrine Between 2000 and 2010

State	Effective Date	Removes duty to retreat somewhere outside home	Removes duty to retreat in any place one has a legal right to be	Presumption of reasonable fear	Removes civil liability
Alabama	6/1/2006	Yes	Yes	No	Yes
Alaska	9/13/2006	Yes	No	Yes	Yes
Arizona	4/24/2006	Yes	Yes	Yes	Yes
Florida	10/1/2005	Yes	Yes	Yes	Yes
Georgia	7/1/2006	Yes	Yes	No	Yes
Indiana	7/1/2006	Yes	Yes	No	Yes
Kansas	5/25/2006	Yes	Yes	No	Yes
Kentucky	7/12/2006	Yes	Yes	Yes	Yes
Louisiana	8/15/2006	Yes	Yes	Yes	Yes
Michigan	10/1/2006	Yes	Yes	No	Yes
Mississippi	7/1/2006	Yes	Yes	Yes	Yes
Missouri	8/28/2007	Yes	No	No	Yes
Montana	4/27/2009	Yes	Yes	Yes	No
North Dakota	8/1/2007	Yes	No	Yes	Yes
Ohio	9/9/2008	Yes	No	Yes	Yes
Oklahoma	11/1/2006	Yes	Yes	Yes	Yes
South Carolina	6/9/2006	Yes	Yes	Yes	Yes
South Dakota	7/1/2006	Yes	Yes	No	No
Tennessee	5/22/2007	Yes	Yes	Yes	Yes
Texas	9/1/2007	Yes	Yes	Yes	Yes
West Virginia	2/28/2008	Yes	Yes	No	No

Table 2: Descriptive Statistics

	Mean (Unweighted)	Mean (Weighted by Population)
Dependent Variables		
Homicides per 100,000 Population	4.8	5.5
	(2.5)	(1.9)
Justifiable Homicide by Private Citizens (count)	5.1	11.8
	(8.2)	(12.9)
Justifiable Homicide by Police (count)	8.0	23.4
	(16.9)	(34.3)
Robberies per 100,000 Population	107.2	143.1
	(59.6)	(47.5)
Aggravated Assault per 100,000 Population	267	296
	(131)	(114)
Burglary per 100,000 Population	710	744
	(240)	(235)
Larceny per 100,000 Population	2,334	2,328
	(533)	(532)
Motor Theft per 100,000 Population	331	381
	(178)	(174)
Proportion of Robberies in Which a Gun Was Used	0.35	0.37
	(0.13)	(0.13)
Control Variables		
Police per 100,000 residents	315	336
	(65)	(66)
Unemployment Rate (%)	5.49	5.93
	(1.99)	(2.10)
Poverty Rate (%)	12.4	12.9
	(3.0)	(2.6)
Median Household Income ($)	51,648	52,146
	(7873)	(6895)
Prisoners per 100,000 residents	439	461
	(169)	(150)
Government spending (assistance and subsidies) per capita	125	110
	(56)	(48)
Government spending (public welfare) per capita	1,319	1,344
	(391)	(409)
% Black Male Aged 15-24	2.60	0.97
	(4.61)	(2.11)
% White Male Aged 15-24	10.77	4.36
	(17.70)	(7.69)
% Black Male Aged 25-44	4.32	1.61
	(7.71)	(3.53)
% White Male Aged 25-44	21.97	8.88
	(36.40)	(15.90)

Notes: Each cell contains the mean with the standard deviation in parentheses. All variables have 550 observations except for the proportion of assaults in which a gun was used (544) and the proportion of robberies in which a gun was used (544).

Table 3: Falsification Tests: The Effect of Castle Doctrine Laws on Larceny and Motor Vehicle Theft

	OLS - Weighted by State Population						OLS - Unweighted					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Larceny	Log (Larceny Rate)						Log (Larceny Rate)					
Castle Doctrine Law	0.00300	-0.00660	-0.00910	-0.00858	-0.00401	-0.00284	0.00745	0.00145	-0.00188	0.00199	-0.00361	-0.0137
	(0.0161)	(0.0147)	(0.0139)	(0.0165)	(0.0128)	(0.0180)	(0.0227)	(0.0205)	(0.0210)	(0.0230)	(0.0201)	(0.0228)
0 to 2 years before adoption of castle doctrine law				0.00112						0.00924		
				(0.0105)						(0.0121)		
Observation	550	550	550	550	550	550	550	550	550	550	550	550
Panel B: Motor Vehicle Theft	Log (Motor Vehicle Theft Rate)						Log (Motor Vehicle Theft Rate)					
Castle Doctrine Law	0.0517	-0.0389	-0.0252	-0.0294	-0.0165	-0.00708	0.0767*	0.0138	0.00814	0.0151	0.00977	-0.00373
	(0.0563)	(0.0448)	(0.0396)	(0.0469)	(0.0354)	(0.0372)	(0.0413)	(0.0444)	(0.0407)	(0.0490)	(0.0391)	(0.0361)
0 to 2 years before adoption of castle doctrine law				-0.00896						0.0165		
				(0.0216)						(0.0278)		
Observation	550	550	550	550	550	550	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
Controls for Larceny or Motor Theft					Yes						Yes	
State-Specific Linear Time Trends						Yes						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 4: The Deterrence Effects of Castle Doctrine Laws: Burglary, Robbery, and Aggravated Assault

	OLS - Weighted by State Population						OLS - Unweighted					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Burglary	Log (Burglary Rate)						Log (Burglary Rate)					
Castle Doctrine Law	0.0780***	0.0290	0.0223	0.0181	0.0327*	0.0237	0.0572**	0.00961	0.00663	0.00293	0.00683	0.0207
	(0.0255)	(0.0236)	(0.0223)	(0.0265)	(0.0165)	(0.0207)	(0.0272)	(0.0291)	(0.0268)	(0.0330)	(0.0222)	(0.0259)
0 to 2 years before adoption of				-0.00906						-0.00884		
castle doctrine law				(0.0133)						(0.0195)		
Panel B: Robbery	Log (Robbery Rate)						Log (Robbery Rate)					
Castle Doctrine Law	0.0408	0.0344	0.0262	0.0197	0.0376**	0.0515*	0.0448	0.0320	0.00839	0.000483	0.00874	0.0267
	(0.0254)	(0.0224)	(0.0229)	(0.0257)	(0.0181)	(0.0274)	(0.0331)	(0.0421)	(0.0387)	(0.0462)	(0.0339)	(0.0299)
0 to 2 years before adoption of				-0.0138						-0.0189		
castle doctrine law				(0.0153)						(0.0237)		
Panel C: Aggravated Assault	Log (Aggravated Assault Rate)						Log (Aggravated Assault Rate)					
Castle Doctrine Law	0.0434	0.0397	0.0372	0.0330	0.0424	0.0414	0.0555	0.0698	0.0343	0.0326	0.0341	0.0317
	(0.0387)	(0.0407)	(0.0319)	(0.0367)	(0.0291)	(0.0285)	(0.0604)	(0.0630)	(0.0433)	(0.0501)	(0.0405)	(0.0380)
0 to 2 years before adoption of				-0.00897						-0.00391		
castle doctrine law				(0.0147)						(0.0249)		
Observations	550	550	550	550	550	550	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes						Yes	
State-Specific Linear Time Trends						Yes						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 5: The Effect of Castle Doctrine Laws on Homicide

	1	2	3	4	5	6
Panel A: Log Homicide Rate (OLS - Weighted)						
Castle Doctrine Law	0.0801**	0.0946***	0.0937***	0.0955**	0.0985***	0.100**
	(0.0342)	(0.0279)	(0.0290)	(0.0367)	(0.0299)	(0.0388)
0 to 2 years before adoption of castle doctrine law				0.00398		
				(0.0222)		
Observations	550	550	550	550	550	550
Panel B: Log Homicide Rate (OLS - Unweighted)						
Castle Doctrine Law	0.0877	0.0811	0.0600	0.0588	0.0580	0.0672
	(0.0638)	(0.0769)	(0.0684)	(0.0807)	(0.0662)	(0.0450)
0 to 2 years before adoption of castle doctrine law				-0.00298		
				(0.0350)		
Observations	550	550	550	550	550	550
Panel C: Homicide (Negative Binomial)						
Castle Doctrine Law	0.0565*	0.0734**	0.0879***	0.0854**	0.0937***	0.108***
	(0.0331)	(0.0305)	(0.0313)	(0.0385)	(0.0302)	(0.0346)
0 to 2 years before adoption of castle doctrine law				-0.00545		
				(0.0227)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Negative binomial estimates are interpreted in the same way as those in a log-linear OLS model. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates. Homicide data are from the published FBI Uniform Crime Reports.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 6: The Effect of Castle Doctrine Laws on Murder, Felony-Type Homicide, Proportion of Robberies Committed Using a Gun, and Justifiable Homicide by Private Citizens

	1	2	3	4	5	6
Panel A: Murder						
(OLS - Weighted)						
Castle Doctrine Law	0.0906**	0.0955**	0.0916**	0.105**	0.0981**	0.0813
	(0.0424)	(0.0389)	(0.0382)	(0.0425)	(0.0391)	(0.0520)
0 to 2 years before adoption of castle doctrine law				0.0277		
				(0.0309)		
Observations	550	550	550	550	550	550
Panel B: Log Felony-Type and Suspected Felony Type Homicides						
(OLS - Weighted)						
Castle Doctrine Law	0.0993	0.203*	0.220**	0.284***	0.222**	0.00121
	(0.112)	(0.109)	(0.0907)	(0.103)	(0.0871)	(0.0686)
0 to 2 years before adoption of castle doctrine law				0.143***		
				(0.0516)		
Observations	539	539	539	539	539	539
Panel C: Proportion of Robberies Using Gun						
(OLS - Weighted)						
Castle Doctrine Law	0.0444***	0.0218	0.0187	0.0247	0.0183	-0.00404
	(0.0145)	(0.0186)	(0.0153)	(0.0167)	(0.0155)	(0.0133)
0 to 2 years before adoption of castle doctrine law				0.0124		
				(0.0101)		
Observations	544	544	544	544	544	544
Panel D: Justifiable Homicide by Private Citizens						
(OLS - Unweighted, Dep. Variable = Count)						
Castle Doctrine Law	4.328***	3.370**	3.200**	3.374**	3.239**	0.960
	(1.467)	(1.300)	(1.202)	(1.335)	(1.216)	(1.219)
0 to 2 years before adoption of castle doctrine law				0.417		
				(0.709)		
Observations	550	550	550	550	550	550
Panel E: Justifiable Homicide by Private Citizens						
(Negative Binomial)						
Castle Doctrine Law	0.573***	0.428*	0.283	0.320	0.324	NA
	(0.210)	(0.244)	(0.235)	(0.254)	(0.228)	NA
0 to 2 years before adoption of castle doctrine law				0.0862		
				(0.136)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	
State-Specific Linear Time Trends						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Negative binomial estimates are interpreted in the same way as those in a log-linear OLS model. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates. NA indicates that the model did not converge. Castle doctrine states averaged 4.9 justifiable homicides in the year prior to enactment.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Appendix

Table A1: Differential Effects of Castle Doctrine Laws

Panel A: Differential Effects by Previous Treatment of Duty to Retreat

	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law in states classified as previously requiring duty to retreat	0.0195	0.0173	0.0315	0.117***	0.0196	7.976***
	(0.0265)	(0.0256)	(0.0358)	(0.0355)	(0.0156)	(2.898)
Castle doctrine law in states classified as not previously requiring duty to retreat	0.0271	0.0416	0.0471	0.0529	0.0173	-1.457
	(0.0305)	(0.0323)	(0.0423)	(0.0352)	(0.0259)	(3.368)
Observations	550	550	550	550	544	550

Panel B: Effect of Castle Doctrine Law That Removes Duty to Retreat in Any Place One Has a Legal Right to Be

	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that removes duty to retreat in any place one has a legal right to be	0.0189	0.0219	0.0293	0.0814***	0.0212	5.597**
	(0.0252)	(0.0264)	(0.0330)	(0.0269)	(0.0174)	(2.736)
Observations	506	506	506	506	500	506

Panel C: Differential Effects by Whether the Law Includes a Presumption of Reasonable Fear

	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that includes a presumption of reasonable fear	-0.00335	0.0169	0.0228	0.0959***	0.0192	6.970**
	(0.0278)	(0.0269)	(0.0322)	(0.0288)	(0.0137)	(3.208)
Other castle doctrine law	0.0646***	0.0415	0.0610	0.0900*	0.0178	0.651
	(0.0215)	(0.0272)	(0.0495)	(0.0524)	(0.0272)	(2.750)
Observations	550	550	550	550	544	550

Panel D: Effect of Castle Doctrine Law, Excluding States That Did Not Also Remove Civil Liability

	Log Burglary Rate	Log Robbery Rate	Log Aggravated Assault Rate	Log Homicide Rate	Proportion of Robberies with a Gun	# Justifiable Homicide by Private Citizens
Castle doctrine law that removes civil liability	0.0218	0.0266	0.0339	0.0844***	0.0203	4.686*
	(0.0223)	(0.0235)	(0.0315)	(0.0290)	(0.0152)	(2.658)
Observations	517	517	517	517	511	517
State and Region-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column in each panel represents a regression, each of which is weighted by state population. Robust standard errors are clustered at the state level. The unit of observation is state-year. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Adopting states classified by Koons (2006) as previously having case law or statute requiring duty to retreat in at least some circumstances include Alabama, Alaska, Florida, Louisiana, Michigan, Missouri, North Dakota, Ohio, South Carolina, South Dakota, and Texas.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A2: Justifiable Homicide by Police

Panel A: OLS - Weighted, Dep. Variable = Count						
Castle Doctrine Law	8.963*	2.770	1.252	-0.0162	1.182	1.129
	(4.501)	(2.829)	(2.600)	(2.834)	(2.643)	(2.878)
0 to 2 years before adoption of castle doctrine law				-2.692***		
				(0.785)		
Observations	550	550	550	550	550	550
Panel B: OLS - Unweighted, Dep. Variable = Count						
Castle Doctrine Law	1.726	-0.244	-0.415	-0.858	-0.380	-0.352
	(1.836)	(1.423)	(1.372)	(1.458)	(1.374)	(1.628)
0 to 2 years before adoption of castle doctrine law				-1.065		
				(0.695)		
Observations	550	550	550	550	550	550
Panel C: Negative Binomial - Unweighted						
Castle Doctrine Law	0.0328	-0.204**	-0.208*	-0.296***	-0.193*	-0.0751
	(0.164)	(0.101)	(0.107)	(0.113)	(0.104)	(0.144)
0 to 2 years before adoption of castle doctrine law				-0.204**		
				(0.0834)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	
State-Specific Linear Time Trends						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Robust standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, welfare and public assistance spending, median income, poverty rate, unemployment rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level