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ABSTRACT

Since Florida adopted the first castle doctrine law in 2005, more than 20 other states have passed similar self-defense laws that justify the use of deadly force in a wider set of circumstances. Elements of these laws include removing the duty to retreat in places outside of one's home, adding a presumption of reasonable belief of imminent harm necessitating a lethal response, and removing civil liability for those acting under the law. This paper examines whether aiding self-defense in this way deters crime or, alternatively, escalates violence. To do so, we apply a difference-in-differences research design by exploiting the within-state variation in law adoption. We find no evidence of deterrence; burglary, robbery, and aggravated assault are unaffected by the laws. On the other hand, we find that murder and non-negligent manslaughter are increased by 7 to 9 percent. This could represent either increased use of lethal force in self-defense situations, or the escalation of violence in otherwise non-lethal situations. Regardless, the results indicate that a primary consequence of strengthening self-defense law is increased homicide.

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1. Introduction

A long-standing principle of English common law, from which most of 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 state to codify this principle into state statute, and to expand self-defense protections in other ways. Twenty-two other states have passed similar laws through 2009. These laws are known as castle doctrine laws or "Stand Your Ground" laws. They generally widen the scope for the justified use of deadly force in self-defense by stating the conditions under which lethal force is justified. The laws sometimes also eliminate the duty to retreat from a list of specified places, establish a presumption of reasonableness as to the beliefs and actions of the individual claiming self-defense, and remove civil liability for those acting under the law.

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 their scope will unnecessarily escalate violence. These potential consequences have been of particular interest recently following some highly publicized cases.[1] The purpose of this paper is to empirically assess which of these possibilities, if any, is true.

[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).

We use state-level crime data from 2000 to 2009 from the FBI Uniform Crime Reports to empirically analyze the effects of castle doctrine on three separate types of outcomes. First we investigate whether the laws increase justifiable homicide by private citizens. Importantly, justifiable homicide is defined as "the killing of a felon, during the commission of a felony, by a private citizen". This FBI classification explicitly excludes homicides when there was no other felony being committed, even if the homicide was justified under current self-defense law. Thus, this definition is independent of any verdict or ruling, and is consistent before and after the passage of castle doctrine, though we note relatively few homicides—around 200 per year nationally—are classified in this way.

In addition, 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, 1998).[2]

Finally, we test whether or not castle doctrine law escalates violence by examining the impact of the laws on total homicides, defined as the sum of murder and non-negligent

[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 author's 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.

manslaughter. In addition, we assess whether criminals respond to the laws by using deadly weapons more often during the commission of crimes such as aggravated assault or robbery.

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. The crucial identifying assumption is that states that adopted castle doctrine laws would have experienced changes in crime similar to non-adopting states in the absence of passing the law. Our data allow us both to test and relax this identifying assumption in several ways. First, we empirically test whether outcomes in the two groups of states diverge in the year prior to adoption. In addition, we show that our findings are robust to the inclusion of both time varying covariates such as demographics and unemployment, as well as to the inclusion of contemporaneous crime levels unaffected by castle doctrine laws that proxy for general crime trends. 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. In addition, we allow for year-by-Census-region fixed effects, which means we identify effects by comparing changes in crime of adopting states to non-adopting states in the same region of the country. 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 increase homicides. Suggestive but inconclusive evidence indicates that castle doctrine laws increase the narrowly defined category of justifiable homicides by private citizens by 17 to 50 percent, which translates into as many as 50 additional justifiable homicides per year nationally due to castle doctrine. More significantly, we find the laws increase murder and manslaughter by a statistically significant 7 to 9 percent, which translates into an additional 500 to 700 homicides per year nationally across the states that adopted castle doctrine. Thus, by lowering the expected costs associated with using lethal force, castle doctrine laws induce more of it. This increase in homicides could be due either to the increased use of lethal force in self-defense situations, or to the escalation of violence in otherwise non-lethal conflicts. We suspect that self-defense situations are unlikely to explain all of the increase, as we also find that murder alone is increased by a statistically significant 6 to 11 percent. This is important because murder excludes non-negligent manslaughter classifications that one might think are used more frequently in self-defense cases. But regardless of how one interprets increases from various classifications, it is clear that the primary effect of strengthening self-defense law is to increase homicide.

These findings have significant policy implications. The first is that these laws do not appear to offer any hidden spillover benefits to society at large. Rather, the evidence indicates that the benefits of strengthening self-defense laws begin and end with

the added protections to those using self-defense to protect themselves. 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 self-defense against the increased loss of life caused by the laws.

2. Castle Doctrine Law and Identification

2.1 Castle Doctrine Law

U.S. self-defense law, which stems from English common law, has long favored the principle of "retreat to the wall", which means that only after no longer being able to retreat safely could one respond 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. In 2005, states began removing the duty to retreat from places outside the home, as well as strengthening self-defense laws in several other ways. For example, most laws added language that explicitly states individuals are justified in using deadly force in certain circumstances when they reasonably believe that they face a serious risk of imminent death or serious bodily harm. In addition, the laws did up to three other things: i) remove the duty to retreat in a list of special places such as one's vehicle, place of work or, in some cases, any place one has a legal right to be; ii) add 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;[3] iii) grant immunity from civil liability when using defensive force in a way justified under law.

[3] 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."

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.

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.[4]

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 castle doctrine law. We then found the text of the actual laws that were passed and determined whether the law contained text regarding standing one's ground, removing a duty to retreat, and/or enumerating the conditions under which one had the right to use lethal force in self-defense situations.[5] We then coded the specific attributes of each state statute. Results are shown in Table 1. Specifically, between 2005 and 2009, 23 states passed castle doctrine laws. Of those, 13 states include a presumption of reasonable fear, 19 explicitly remove civil liability, and 13 remove the duty to retreat in any place someone has a legal right to be. Our main analysis groups all of these laws together, and thus captures the average effect of passing a law similar to those passed in these 23 states. However, since that approach is

[4] 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 as an issue of gun and victim rights.

[5] This was performed by one of the authors and, independently, by a research assistant. Results were compared afterward; the only point of disagreement was Montana. Further research confirmed that Montana enacted a castle doctrine bill, House Bill 228, on April 9, 2009.

perhaps unnecessarily blunt, later in the paper we show results from categorizing state laws in these three ways.

2.2 Crime Data

Outcome data come from the FBI Uniform Crime Reports (UCR) and cover all 50 states from 2000 – 2009.[6] Specifically, we use homicide, burglary, robbery, and aggravated assault data from the official UCR data published online by the FBI.[7] In 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 strengthening state self-defense laws does any of three things. First, we ask whether these laws result in differential response on the part of the civilians. To do so, we use data on justifiable homicide by private citizens, which is defined as "the killing of a felon, during the commission of a felony, by a private citizen" (Uniform Crime Reporting Handbook, 2004). A natural concern is whether reporting a homicide as "justifiable" depends in part on the self-defense laws in the state at the time. The FBI Uniform Crime Reporting Handbook makes it clear this should not be the case. For example, the handbook emphasizes that "law enforcement agencies must report the willful (nonnegligent) killing of one individual by another, not the criminal liability of the person or persons involved" (Uniform Crime Reporting Handbook, 2004). In addition,

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

[7] 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 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 0.0915, 0.115, 0.106, 0.109, 0.0762, and 0.0867, respectively. All are significant at the 5 percent level.

the handbook emphasizes that by definition, justifiable homicide occurs in conjunction with other offenses, and those other offenses must be reported. The handbook explicitly states that "reporting agencies should take care to ensure that they do not classify a killing as justifiable or excusable solely on the claims of self-defense or on the action of a coroner, prosecutor, grand jury, or court" (Uniform Crime Reporting Handbook, 2004). Additionally, the handbook gives examples of specific hypothetical events that would and would not qualify as justifiable homicide under the guidelines.[8] Thus, our view is that so long as law enforcement agencies are reporting justifiable homicides as they are instructed, any effects of the castle doctrine laws on justifiable homicide should reflect real changes in self-defense response by civilians, not differential reporting. Moreover, given how strict the definition is and how few homicides are classified in this way—approximately 200 per year nationally, compared to about 14,000 cases of murder and non-negligent manslaughter—we expect difficulty in detecting any effects on self-defense killings.

We also examine whether strengthening self-defense laws deters criminals. Here, 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,

[8] 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). Note that in the absence of castle doctrine law, this may not qualify as a self-defense case (though it could, of course), but according to the guidelines should still have been reported as a justifiable homicide. 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). Note here that under castle doctrine, the shooter may well 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.

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).[9] In all cases, one might expect rational criminals to be less likely to commit such crimes, as the increased scope for the use of justifiable lethal force on the part of the victim raises the expected cost to the criminal.[10]

Our last set of outcomes is intended to measure the escalation of violence. The primary outcome we examine is total homicides, which is defined as the sum of murder and non-negligent manslaughter, although we also look at murder separately.[11] In addition, we examine the ratio of robberies committed with a gun and the ratio of assaults committed with a gun. We expect to see increases in these outcomes if castle doctrine laws escalate otherwise less violent situations, or if criminals respond to an increased propensity for victims to use lethal force in self-defense by being more likely to carry and use weapons in the commission of a crime. While any increase in the ratios of robberies or assaults committed with a gun would be clear evidence that criminals escalate violence, we note that an increase in homicides is less clear. On the one hand, an increase could represent the escalation of violence in otherwise reasonably safe situations. On the other hand, it could represent the increased use of lethal force in self-defense by potential victims of crimes such as assault.

The data also allow us to perform several placebo, or falsification tests. For

[9] Results are similar using data on all assaults, including simple assault, which were obtained from Return A of the FBI Master files.

[10] To the extent castle doctrine increases homicide, however, the hierarchy rule means that our results are biased in favor of finding deterrence effects. The hierarchy rule instructs reporting agencies to only code the highest, or most serious, offense in multiple-offense situations. Thus, a burglary that escalates into a homicide due to castle doctrine will be coded as a homicide, which potentially leads us to falsely estimate a deterrent effect on burglary.

[11] Homicide figures come from the UCR data published online and do not include justifiable homicides. Murder figures come from the FBI's Return A, since murder is not available as a separate category in the published UCR.

example, because the focus of castle doctrine laws is on civilians, and not law enforcement, we examine whether we detect effects of the laws on justifiable homicide by police. Similarly, we use data on the rate of larceny and motor vehicle theft to determine whether castle doctrine laws appear to affect those crimes.[12] 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 have measures of the number of full-time equivalent police per 100,000 state residents (Uniform Crime Reports, 2000-2009). 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, 2000-2009). 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. Lastly, we 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, 2000-2009)

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 23 states passed such laws between 2005 and 2009. Specifically, we use a difference-in-differences research design

[12] 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.

that asks whether outcomes change more in states that adopt castle doctrine laws than in states that do not.

Formally, we estimate a fixed effects Poisson panel data model to account for the nature of the count data we are using, which is particularly important for measures such as homicides, which are relatively infrequent, especially in smaller states.[13] In addition, for all outcomes other than justifiable homicides, we also use a fixed effects Ordinary Least Squares regression model. In both models we include year fixed effects and state fixed effects. The fixed effects Poisson model, revised to account for crime rates rather than counts, yields estimates comparable to those in a conventional log-linear specification using fixed effects OLS.[14] The Poisson model estimated is:

$$\log\left(\frac{\lambda_{it}}{n_{it}}\right) = \alpha + \delta CDL_{it} + \mathrm{X}_{it}\beta + c_i + u_t, \quad \mathrm{P}(Y_{it} = y_{it}) = \frac{e^{-\lambda_{it}}\lambda_{it}^{y_{it}}}{y_{it}!} \qquad (1)$$

where CDL_{it} is the policy dummy variable that equals 1 if state i has an effective castle doctrine law at year t,[15] λ_{it} is the Poisson parameter (also the expected mean) of justifiable homicide or crime count Y_{it}, n_{it} is the number of population in state i at time t, X_{it} is the vector of control variables, and c_i and u_t control for fixed state and year effects, respectively. Robust standard errors are clustered at the state level.

In addition, we estimate a standard OLS fixed effects model, where we use the log

[13] The primary downside of the Poisson model is the equivariance assumption. Still, we use the Poisson model rather than the negative binomial model because doing so allows us to cluster standard errors at the state level. In addition, we use robust variance estimation for the Poisson model, which relaxes the equivariance assumption somewhat (Cameron and Trivedi, 2010). Finally, we note that estimates from the negative binomial model are almost identical to the Poisson estimates presented. For example, the estimates corresponding to columns 1 through 6 in Panel A of Table 5 (homicide) are 0.102, 0.120, 0.118, 0.114, 0.0755, and 0.0706. The first five are significant at the 1 percent level; the 6th is significant at the 5 percent level.

[14] For example, see Michener and Tighe (1992).

[15] When laws were enacted partway through a calendar year, we used the proportion of the year in which the law was effect as the treatment variable, and one for each year thereafter.

of the number of crimes per 100,000 residents as the dependent variable,[16] and control for year fixed effects, state fixed effects as well as an indicator for whether castle doctrine law was in effect. Robust standard errors are clustered at the state level.

The identifying assumption of the difference-in-differences research design used here is that states that adopted castle doctrine law would have experienced similar changes in outcomes as non-adopting states had they not passed the castle doctrine law. Our data allow us to test and relax this identifying assumption in several ways. First, we offer a formal statistical test of this by including an indicator in equation (1) for the year 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 and policing intensity should not change more over time in adopting states than non-adopting states, as this would suggest that crime in the two groups might have diverged even in the absence of treatment. Thus, we compare estimates to specifications without these controls to specifications with these controls. To the extent that our difference-in-differences estimates remain unchanged, it provides some assurance that our research design is reasonable.[17]

Along similar lines, we also show results from specifications that include

[16] In those cases when there were zero crimes, we added one before taking the log.

[17] 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.

contemporaneous motor vehicle theft and larceny as controls. While it could be possible that castle doctrine laws affect these crimes, we would certainly 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 overall 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 note that a disproportionate number of states that adopted castle doctrine laws over this time period were in the midwestern and southern parts of the United States. While we view the above tests as informative about whether the states that did not adopt the new laws provide a good counterfactual for those that do, we also do two more things to better allow for differential trends of these states. First, in some specifications we allow for year-by-Census-region fixed effects.[18] This enables us to difference out any region-specific shocks that might differ over time, thereby allowing states in each region to trend differently over our time period. Intuitively, this means that we identify effects by comparing changes in crime in adopting states to changes in crime in non-adopting states from the same region. Finally, we allow for state-specific linear time trends, thereby allowing each state to follow a different trend.

4. Results

4.1 Justifiable Homicide

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

We first examine the effect of castle doctrine law on civilian response, as measured by justifiable homicides. Results are shown in Table 3. Each specification controls for state and year fixed effects. Column 2 additionally controls for time-varying factors including household income and the rate of policing, incarceration, and unemployment. Column 3 is the same as Column 2, except that it estimates a placebo effect in the year prior to the adoption of the Castle Doctrine. Column 4 additionally controls for contemporaneous motor vehicle theft and larceny rates to proxy for overall changes in state crime. Column 5 controls for region-by-year fixed effects and the time-varying controls, while Column 6 controls for state and year fixed effects, time-varying controls, and state-specific linear time trends.

Poisson estimates—which are interpreted similar to estimates in a log-linear OLS model in which estimates represent the percent change in the outcome induced by castle doctrine—are shown in Panel A of Table 3. Results indicate that castle doctrine increases justifiable homicide by private citizens by between 17 and 50 percent. Estimates in the first 4 specifications are statistically significant at the 1 percent level, though estimates are no longer significant when allowing for either region-by-year fixed effects (17 percent) or state-specific linear time trends (25 percent). Given that the average adopting state had 4.3 justifiable homicides by private citizens in the year prior to castle doctrine law, these estimates suggest that on average 1 to 2 additional felons are killed in each state each year as a result of castle doctrine.[19] By contrast, consistent with expectations there is less

[19] One could raise questions about whether some states that report zero justifiable estimates should instead be classified as having missing data. The state that is most concerning is New York, which reported an average of 14 justifiable homicides by private citizens from 2000 to 2006, and then zero in each year afterward. Other states reporting few or no justifiable homicides by private citizens include Montana, North Dakota, West Virginia, Vermont, and Utah. However, we note that the results are qualitatively similar when we exclude these 6 states; the estimate corresponding to column 2

evidence that castle doctrine increases justifiable homicide by police, as only 1 estimate of the 7 (27.5 percent) is statistically significant at the 5 percent level. Our view is that these results provide suggestive but not conclusive evidence that castle doctrine increases justifiable homicide by private citizens, and that the absolute magnitude of any effect is small.

4.2 Deterrence

We now move to the question of 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 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 the fixed effects Poisson model, while the second 6 columns show results from OLS. Results are consistent across specifications in that there is little evidence that castle doctrine deters crime. For example, of the 36 estimates presented, none are negative and significant at the 10 percent level.

Importantly, the estimates are sufficiently precise as to rule out large deterrence effects. For example, in our preferred specification in column 2, the lower bounds of estimates on burglary, robbery, and aggravated assault are 0.2 percent, minus 0.8 percent, and minus 2.7 percent. Put differently, our estimates and standard errors from column 2

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of Panel A in Table 3 is 0.366, which is significant at the 1 percent level, compared to an estimate of 0.504 in Table 3.

indicate that if we were to perform this castle doctrine policy experiment many times, we would expect to find deterrence effects larger than 1 percent less than 2.5 percent of the time for burglary and robbery, and only 10 percent of the time for aggravated assault. Thus, our findings 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 protecting themselves in self-defense, there is no evidence that the law provides positive spillovers by deterring crime more generally.

4.3 Escalation

We now turn to whether strengthening self-defense laws causes an escalation of violence. Given that the laws reduce the costs associated with using violence, economic theory would predict that we would get more of it. Perhaps the most obvious form of escalation—and one most commonly cited by critics of castle doctrine law—is that conflicts or crimes that might not have otherwise turned deadly may now do so. For example, a criminal may not have intended to kill someone he was robbing until the victim attempted to use a weapon in self-defense. Alternatively, individuals involved in a fight may perceive that lethal force is legally justified in situations it was not previously, and may thus use lethal force. As a result, the first measure of escalation we consider is homicides—murder plus non-negligent manslaughter. We note, however, that it is also possible for castle doctrine law to deter homicides. Thus, we estimate the net effect of escalation and deterrence on homicides.

Results are shown in Panels A and B of Table 5. Results indicate that castle

doctrine laws appear to increase homicides. Poisson estimates from columns 1 through 5 in Panel A of Table 5 indicate that castle doctrine laws lead to a 7 to 9 percent increase in homicides and non-negligent manslaughter, while the estimate in column 6 from the highly parameterized model that includes state-specific linear time trends is 13 percent. Estimates from all 6 specifications are statistically significant at the 1 percent level. Estimates from OLS estimation in Panel B are similar in magnitude and range from 7 to 13.5 percent. Due to the large standard errors, however, OLS estimates do not attain significance at conventional levels: t-statistics range from 1.42 to 1.60.

The increase in homicides can be seen graphically as well. Figure 1 shows the log of homicide rate for the 13 states that adopted castle doctrine in 2006, as well as for the 27 states that did not enact castle doctrine through 2009.[20] It shows that while the *trends* of the two groups track each other closely prior to castle doctrine, afterward homicides in adopting states increase relative to control states. Importantly, Figure 1 gives us little reason to believe that even in the absence of castle doctrine, adopting states would have experienced in increase in homicides after 2005 relative to non-adopting states. [21]

While we view the evidence that castle doctrine increases homicides as convincing, we note that one downside of the homicide measure is that it could well include homicides that are justified under the new self-defense law and yet may not meet the strict definition

[20] It is more difficult to show a graph for the entire sample of adopting states, as they enacted castle doctrine in different years. Therefore we focus on showing results graphically for the largest subset of states that passed the law in the same year.

[21] 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 castle doctrine. We see no evidence of that, which suggests that the relative increase seen after 2005 is caused by castle doctrine. Moreover, recall that homicide estimates remained similar even after controlling for time-varying police and incarceration rates, including region-by-year fixed effects, and allowing for state-specific linear time trends.

of justifiable homicide. Thus, this increase may not be viewed by everyone as unambiguously bad. For example, the increase could be driven by individuals protecting themselves from imminent harm by using lethal force.[22] On the other hand, the increase could be driven by the escalation of violence in situations that otherwise would not have ended in serious injury for either party.

We address this issue in two ways. First, we focus solely on murder, which excludes classifications of non-negligent manslaughter that are more likely to be used in self-defense killings that do not meet the strict definition of justifiable homicide. Results are shown in Panel C of Table 5, where all specifications show statistically significant increases of between 6 and 11 percent. In addition, we find suggestive evidence that felony homicide and suspected felony homicide—that is, homicides that were or were suspected to have been committed along with a felony, such as robbery or burglary—are increased as a result of the laws.[23] These homicides almost certainly represent an escalation of violence by criminals, as opposed to self-defense situations.[24] Combined, this suggests that the increase in homicides is unlikely to be entirely due to self-defense.

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 murder/homicides, rather than castle doctrine. That is, one would have to

[22] However, note that the net increase cannot be driven by a one-to-one substitution of homicides of assailants for homicides of innocent victims. In theory, however, the increase could be driven by substitution if there were more (killed) assailants than would-be-killed victims.

[23] Estimates corresponding to the first 6 columns of Table 5 are 0.064, 0.139, 0.153, 0.142, 0.095, and 0.039. The largest three estimates are significant at the 10 percent level. Obtaining precise estimates for this outcome is difficult in part because felony-type criminal homicides are only about 15 percent of total homicides.

[24] 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.

believe that something else caused homicides to increase relative to non-adopting states immediately after castle doctrine was enacted, but not in the years prior to enactment. 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 adopt 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. The confounder must also increase homicides in adopting states after adoption without causing proportionate increases in motor vehicle theft and larceny. Finally, the confounder must be uncorrelated with changes in the economic conditions 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 castle doctrine.

We also examine a less extreme form of escalation—whether criminals respond to the laws by being more likely to use a gun when committing robbery and assault. The intuition is that 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 while committing the crime. To the extent that this occurs, we would expect to see an increase in the ratios of robberies and assaults in which a gun was used after the law is passed.[25]

Results are shown in Panels D and E of Table 5. Our findings indicate there is

[25] Examining these ratios as outcome variables could be problematic if the laws were found to reduce robbery or aggravated assault. However, this is not a concern here as we show in Table 4 that there is no effect on robberies or aggravated assaults.

little evidence that criminals respond to the laws in this way. While all estimates are positive, only 1 of the 12 estimates is significant at the 5 percent level. Thus, while we find little evidence that criminals escalate violence on the extensive margin, we note that our estimates do not rule out such effects.

4.4 Falsification Tests

The identifying assumption of our study is that outcomes in adopting states would have trended similarly to non-adopting states in the absence of the laws. While we have performed several tests thus far to both examine the veracity of this assumption and relax it, we can also 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.[26] Finding effects on crimes that ought to be exogenous to castle 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 6. Consistent with the identifying assumption, castle doctrine law does not appear to affect any of these crimes. Specifically, of the 24 estimates reported, only 1 is significant at the 10 percent level.

4.5 Differential Effects of State Castle Doctrine Laws

[26] Similar tests are performed by Ayres and Levitt (1998), when they look for effects of Lojack on crimes other than motor vehicle theft.

Thus far we have examined the average effect of passing laws that extend the castle doctrine to places outside the home and in some cases create a presumption of reasonableness and remove civil liability for those acting under the law. However, as noted earlier, while there are broad similarities in these laws, their scope varies across states. One way in which the laws differ is in whether they remove the duty to retreat in any place one has a legal right to be, as opposed to only a list of protected places such as one's workplace or vehicle. As shown in Table 1, 13 of the 23 states that passed these laws from 2005 through 2009 extended castle doctrine and removed the duty to retreat in any place a person had a legal right to be.

To determine whether the effect of these laws was greater in these states, we define two different treatment indicators-one for whether the law extended to any place one had a legal right to be, and another that represented a narrower castle doctrine law. Results are shown in Panel A of Table 7, and indicate that there is little evidence that these laws with wider scope had stronger effects on justifiable homicide, escalation, or deterrence. For example, estimates of the effect on homicide are statistically indistinguishable from each other.

Another way in which the laws differed is in whether they created a presumption of reasonableness for those using lethal violence in self-defense situations. Thirteen states did so, and in Panel B of Table 7 we calculate separate treatment effects for these states as well as those that did not explicitly create a presumption of reasonableness. Again, we find no statistical differences between these two groups of states.

The final way in which the laws differed is in whether or not they explicitly

removed civil liability for someone using lethal force in self-defense under the law. Of the 23 states, 19 explicitly removed civil liability for those justified under the self-defense law.[27] Thus, in Panel C of Table 7 we exclude the four states that passed castle doctrine, but did not explicitly remove civil liability. Again, estimates are similar to the average effects presented earlier: we find that the laws increase homicide by 6 to 9 percent, and no evidence of deterrence effects.

In summary, we find no evidence that strengthening self-defense law deters crime. On the other hand, we find that the primary consequence of castle doctrine laws is to increase homicides—including perhaps a small absolute increase in justifiable homicides by private citizens, as well as a meaningful and statistically significant 7 to 9 percent increase in total 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 some cases they establish a presumption of reasonableness and remove civil liability. Thus, these laws could hypothetically deter crime or, alternatively escalate violence. To our knowledge, this is the first paper to examine empirically which of these possibilities, if

[27] We had initially categorized Arizona as not removing civil liability, as there is no mention of it in the law itself. However, when we cross-checked our coding with that described in Vilos and Vilos (2010) on the self-defense laws of every state, we found that Arizona law states that "No person in this state shall be subject to civil liability for engaging in conduct otherwise justified pursuant to the provisions of this chapter" when it discusses civil liability.

any, is true.

We find no evidence that castle doctrine law deters 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.

We find suggestive but inconclusive evidence that these laws increase justifiable homicide by private citizens. However, the absolute impact of even the largest and most statistically significant estimates is quite small, given how few homicides are classified in this way. Our estimates suggest the laws cause at most 50 additional justifiable homicides per year across all 23 states that adopted castle doctrine.

More significantly, results indicate that castle doctrine laws increase total homicides by 7 to 9 percent. Put differently, the laws induce an additional 500 to 700 homicides per year across the 23 states in our sample that enacted castle doctrine laws. 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 and policing and incarceration rates. These findings provide evidence that lowering the expected cost of lethal force causes there to be more of it.[28]

The additional homicides induced by castle doctrine could be due to victims practicing self-defense under the terms of the new law, an increased propensity by

[28] Our finding that castle doctrine increases homicide also means our estimates on deterrence are biased *in favor* of finding deterrence effects. The FBI's hierarchy rule states that in multiple-offense situations, agencies should report only the most serious crime. Thus, to the extent that aggravated assaults, robberies, or burglaries are escalated into homicides due to castle doctrine, we overestimate the deterrence effects of the law.

criminals to use lethal force when committing crimes or encountering resistance, the escalation of other conflicts, or some combination of the above. While we would expect different analysts to weight homicides from these situations differently, it is clear that the primary impact of these laws, beyond giving potential victims additional scope to protect themselves, is to increase the loss of human life. Thus, in light of our findings, our view is that an informed debate over these laws will weigh the increased protection offered to law-abiding citizens against the increase in homicide that results from the laws.

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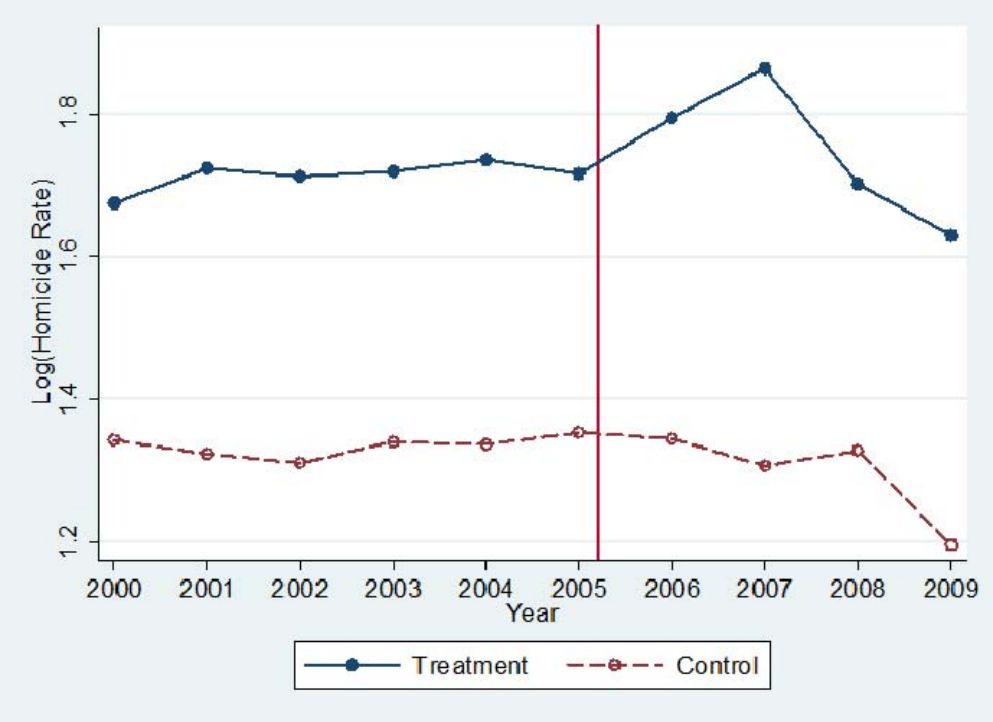

Figure 1: Log Homicide Rate for the 13 States That Enacted Castle Doctrine in 2006 Compared to States that Did Not Enact Castle Doctrine by 2009

Table 1: State Castle Doctrine Laws

State	Effective Year	Removes duty to retreat in any place someone has a legal right to be	Presumption of reasonable fear	Removes civil liability
Alabama	2006	Yes	No	Yes
Alaska	2006	No	Yes	Yes
Arizona	2006	Yes	Yes	Yes
Florida	2005	Yes	Yes	Yes
Georgia	2006	Yes	No	Yes
Idaho	2006	No	No	Yes
Indiana	2006	No	No	Yes
Kansas	2006	Yes	No	Yes
Kentucky	2006	Yes	Yes	Yes
Louisiana	2006	Yes	Yes	Yes
Maine	2007	No	No	Yes
Michigan	2006	Yes	No	Yes
Mississippi	2006	Yes	Yes	Yes
Missouri	2007	No	No	No
Montana	2009	No	Yes	No
North Dakota	2007	No	Yes	Yes
Ohio	2008	No	Yes	Yes
South Carolina	2006	No	Yes	Yes
South Dakota	2006	Yes	No	No
Tennessee	2007	Yes	Yes	Yes
Texas	2007	Yes	Yes	Yes
West Virginia	2008	Yes	No	No
Wyoming	2008	No	Yes	Yes

Table 2: Descriptive Statistics

Variable	Mean
Justifiable Homicide	
Justifiable Homicide by Private Citizens	4.9
	(8.0)
Justifiable Homicide by Police	7.9
	(17.1)
Crime Rates per 100,000 State Population	
Homicide	4.8
	(2.5)
Robbery	108.8
	(60.4)
Aggravated Assault	269
	(133)
Burglary	714
	(240)
Larceny	2,368
	(536)
Motor Theft	343
	(181)
Proportion of Assaults in Which a Gun Was Used	0.03
	(0.02)
Proportion of Robberies in Which a Gun Was Used	0.36
	(0.13)
Control Variables	
Police per 100,000 residents	315
	(65)
Unemployment Rate (%)	5.10
	(1.50)
Poverty Rate (%)	12.3
	(3.0)
Median Household Income ($)	51,812
	(7896)
Prisoners per 100,000 residents	436
	(168)
% Black Male Aged 15-24	0.86
	(0.79)
% White Male Aged 15-24	5.16
	(1.36)
% Black Male Aged 25-44	1.39
	(1.22)
% White Male Aged 25-44	10.02
	(1.96)

Notes: Each cell contains the mean with the standard deviation in parentheses. All variables have 500 observations except for the proportion of assaults in which a gun was used (494) and the proportion of robberies in which a gun was used (494).

Table 3: The Effect of Castle Doctrine Law on Justifiable Homicides

	1	2	3	4	5	6
Panel A: Justifiable Homicide by Private Citizens						
Castle Doctrine Law	0.476***	0.504***	0.440***	0.516***	0.167	0.251
	(0.168)	(0.153)	(0.164)	(0.149)	(0.128)	(0.226)
One Year before Adoption of Castle Doctrine Law			-0.229			
			(0.143)			
Panel B: Justifiable Homicide by Police						
Castle Doctrine Law	0.211	0.240*	0.223*	0.275**	-0.0223	0.290*
	(0.178)	(0.128)	(0.133)	(0.114)	(0.130)	(0.162)
One Year before Adoption of Castle Doctrine Law			-0.0687			
			(0.119)			
Observations	500	500	500	500	500	500
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes
Contemporaneous Crime Rates				Yes		
Region-by-Year Fixed Effects					Yes	
State-Specific Linear Time Trends						Yes
Excludes Texas						

Notes: Each column in each panel represents a separate regression from the fixed-effect Poisson model. The unit of observation is state-year. Standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, median income, poverty 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 4: The Deterrence Effects of Castle Doctrine: Burglary, Robbery, and Aggravated Assault

	FE Poisson						FE OLS					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Burglary	Burglary Rate						Log (Burglary Rate)					
Castle Doctrine Law	0.0584**	0.0416**	0.0352*	0.0334**	0.00402	0.0231	0.0352	0.0242	0.0252	0.0154	-0.00158	0.0180
	(0.0247)	(0.0199)	(0.0212)	(0.0143)	(0.0210)	(0.0156)	(0.0277)	(0.0261)	(0.0297)	(0.0174)	(0.0243)	(0.0210)
One Year Before Adoption of Castle Doctrine Law			-0.0228**						0.00400			
			(0.0114)						(0.0209)			
Panel B: Robbery	Robbery Rate						Log (Robbery Rate)					
Castle Doctrine Law	0.0492*	0.0257	0.0141	0.0218	0.00745	0.0552***	0.0490	0.0567**	0.0662**	0.0484**	0.0285	0.0307
	(0.0298)	(0.0174)	(0.0181)	(0.0134)	(0.0199)	(0.0210)	(0.0294)	(0.0278)	(0.0324)	(0.0236)	(0.0275)	(0.0314)
One Year Before Adoption of Castle Doctrine Law			-0.0394**						0.0351			
			(0.0166)						(0.0283)			
Panel C: Aggravated Assault	Aggravated Assault Rate						Log (Aggravated Assault Rate)					
Castle Doctrine Law	0.0509	0.0255	0.0262	0.0240	0.0178	0.0126	0.0602	0.0717	0.0777	0.0558	0.0399	0.0440
	(0.0404)	(0.0269)	(0.0283)	(0.0251)	(0.0246)	(0.0239)	(0.0568)	(0.0522)	(0.0615)	(0.0456)	(0.0403)	(0.0348)
One Year Before Adoption of Castle Doctrine Law			0.00252						0.0223			
			(0.0168)						(0.0431)			
Observations	500	500	500	500	500	500	500	500	500	500	500	500
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Contemporaneous Crime Rates				Yes						Yes		
Region-by-Year Fixed Effects					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. Standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, median income, poverty 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 Escalation Effects of Castle Doctrine: Homicide, Murder, Ratio of Robberies Involving a Gun, and Ratio of Assaults Involving a Gun

	1	2	3	4	5	6
Panel A: Homicide (Poisson)						
Castle Doctrine Law	0.0853***	0.0926***	0.0850***	0.0892***	0.0729***	0.128***
	(0.0310)	(0.0238)	(0.0279)	(0.0254)	(0.0261)	(0.0328)
One Year Before Adoption of Castle Doctrine Law			-0.0252			
			(0.0216)			
Observations	500	500	500	500	500	500
Panel B: Log Homicide Rate (OLS)						
Castle Doctrine Law	0.108	0.123	0.135	0.106	0.104	0.0721
	(0.0724)	(0.0771)	(0.0844)	(0.0704)	(0.0741)	(0.0509)
One Year Before Adoption of Castle Doctrine Law			0.0448			
			(0.0470)			
Observations	500	500	500	500	500	500
Panel C: Murder (Poisson)						
Castle Doctrine Law	0.0923***	0.0955***	0.0883***	0.105***	0.0630**	0.114***
	(0.0342)	(0.0236)	(0.0250)	(0.0257)	(0.0299)	(0.0401)
One Year Before Adoption of Castle Doctrine Law			-0.0239			
			(0.0205)			
Observations	500	500	500	500	500	500
Panel D: Proportion of Robberies with a Gun (OLS)						
Castle Doctrine Law	0.0148	0.0168	0.0148	0.0150	0.00694	0.0133
	(0.0161)	(0.0169)	(0.0178)	(0.0165)	(0.0179)	(0.0159)
One Year Before Adoption of Castle Doctrine Law			-0.00712			
			(0.0123)			
Observations	494	494	494	494	494	494
Panel E: Proportion of Assaults with a Gun (OLS)						
Castle Doctrine Law	0.00183	0.00292	0.00257	0.00248	0.00149	0.00468**
	(0.00190)	(0.00179)	(0.00193)	(0.00182)	(0.00189)	(0.00194)
One Year Before Adoption of Castle Doctrine Law			-0.00128			
			(0.00105)			
Observations	494	494	494	494	494	494
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes
Contemporaneous Crime Rates				Yes		
Region-by-Year Fixed Effects					Yes	
State-Specific Linear Time Trends						Yes

Notes: Each column in each panel represents a separate regression. The unit of observation is state-year. Standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, median income, poverty rate, and demographics. Contemporaneous crime rates include larceny and motor vehicle theft rates. Homicide data are from the published FBI Uniform Crime Reports, while murder data are from Return A of the FBI Master files.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 6: Falsification Tests: The Effect of Castle Doctrine on Larceny and Motor Vehicle Theft

	FE Poisson						FE OLS					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Larceny	Larceny Rate						Log (Larceny Rate)					
Castle Doctrine Law	0.0109	0.0110	0.00694	0.00730	-0.00635	-0.00449	0.00541	0.000582	-0.000369	-0.0101	0.00129	-0.0117
	(0.0158)	(0.0162)	(0.0174)	(0.0150)	(0.0147)	(0.0143)	(0.0228)	(0.0203)	(0.0223)	(0.0182)	(0.0193)	(0.0173)
One Year Before Adoption of Castle Doctrine Law			-0.0149*						-0.00353			
			(0.00904)						(0.0125)			
Observation	500	500	500	500	500	500	500	500	500	500	500	500
Panel B: Motor Vehicle Theft	Motor Vehicle Theft Rate						Log (Motor Vehicle Thefts)					
Castle Doctrine Law	0.0128	-0.000372	-0.0179	-0.00783	-0.0353	0.0381	0.0687*	0.0571	0.0635	0.0567	0.0176	0.0236
	(0.0563)	(0.0328)	(0.0374)	(0.0304)	(0.0369)	(0.0347)	(0.0392)	(0.0401)	(0.0447)	(0.0367)	(0.0403)	(0.0401)
One Year Before Adoption of Castle Doctrine Law			-0.0626**						0.0236			
			(0.0250)						(0.0276)			
Observation	500	500	500	500	500	500	500	500	500	500	500	500
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Contemporaneous Crime Rates				Yes						Yes		
Region-by-Year Fixed Effects					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. Standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, median income, poverty 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 7: Differential Effects of Castle Doctrine Law by Treatment of Duty to Retreat and Civil Liability

Panel A: Differential Effects by Treatment of Duty to Retreat												
	Justifiable Homicide by Private Citizens		Justifiable Homicide by Police		Burglary		Homicide		Proportion of Robberies with a Gun		Proportion of Assaults with a Gun	
Castle Doctrine Law That Removes	0.500***	0.114	0.232	-0.0429	0.0422**	0.000820	0.0864***	0.0625**	0.0207	0.00974	0.00358	0.00240
Duty to Retreat in Any Place	(0.150)	(0.115)	(0.145)	(0.142)	(0.0212)	(0.0234)	(0.0249)	(0.0272)	(0.0216)	(0.0243)	(0.00224)	(0.00236)
Other Castle Doctrine Law	0.527*	0.452*	0.283	0.0575	0.0387	0.0144	0.122**	0.111**	0.0102	0.00308	0.00182	0.000237
	(0.280)	(0.237)	(0.186)	(0.208)	(0.0264)	(0.0260)	(0.0498)	(0.0466)	(0.0200)	(0.0223)	(0.00266)	(0.00260)
Observations	500	500	500	500	500	500	500	500	494	494	494	494
Panel B: Differential Effects by Whether the Law Includes a Presumption of Reasonableness												
	Justifiable Homicide by Private Citizens		Justifiable Homicide by Police		Burglary		Homicide		Proportion of Robberies with a Gun		Proportion of Assaults with a Gun	
Castle Doctrine Law That Includes	0.597***	0.179	0.298**	0.0659	0.0226	-0.0244	0.0984***	0.0775***	0.00846	-0.000343	0.00265	0.00183
Presumption of Reasonableness	(0.161)	(0.124)	(0.151)	(0.152)	(0.0225)	(0.0241)	(0.0274)	(0.0284)	(0.0183)	(0.0183)	(0.00184)	(0.00189)
Other Castle Doctrine Law	0.234	0.136	0.0657	-0.221	0.0825***	0.0493***	0.0818**	0.0662*	0.0262	0.0144	0.00323	0.00114
	(0.217)	(0.222)	(0.162)	(0.158)	(0.0202)	(0.0178)	(0.0353)	(0.0367)	(0.0239)	(0.0241)	(0.00287)	(0.00262)
Observations	500	500	500	500	500	500	500	500	494	494	494	494
Panel C: Effect of Castle Doctrine Law, Excluding States with Laws That Do Not Remove Civil Liability												
	Justifiable Homicide by Private Citizens		Justifiable Homicide by Police		Burglary		Homicide		Proportion of Robberies with a Gun		Proportion of Assaults with a Gun	
Castle Doctrine Law That Removes	0.498***	0.147	0.224*	-0.0347	0.0421**	0.00470	0.0858***	0.0603**	0.0196	0.0116	0.00194	0.000625
Civil Liability	(0.151)	(0.132)	(0.136)	(0.142)	(0.0199)	(0.0214)	(0.0246)	(0.0262)	(0.0182)	(0.0191)	(0.00192)	(0.00198)
Observations	460	460	460	460	460	460	460	460	454	454	454	454
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes		Yes		Yes		Yes		Yes		Yes

Notes: Each column in each panel represents a separate regression. A fixed-effects Poisson model is used in for all dependent variables except the proportion of robbery using a gun and the proportion of assault with a gun, which are estimated using OLS. The unit of observation is state-year. Standard errors are clustered at the state level. Time-varying controls include policing and incarceration rates, median income, poverty rate, and demographics.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level