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Self-defense Policy, Justified Homicides, and Race

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Abstract

The self-defense policies known as Stand Your Ground reduce the expected cost of using lethal force. I use detailed police records to examine whether these policies had differential effects on the reportedly-justified homicide rates of people across races. I find that the implementation of Stand Your Ground policies lead to an average of 2.75 additional black Alleged Perpetrators of Crimes being killed each month, 2.39 of whom are killed by black citizens. Additionally, I find 0.5 additional white Alleged Perpetrators are killed each month, 0.49 of whom are killed by white citizens. I test the differences between race groups and find that they are strongly significant in all cases. I then use event studies to confirm that my results are not caused by pre-existing trends. My results provide strong evidence that Stand Your Ground policies cause unequal outcomes across racial groups, and I postulate several mechanisms that may be the cause of these racially disparate effects.

Keywords: Crime; self-defense; Stand Your Ground; criminal policy; discrimination
JEL codes: K42, Z18.

SELF-DEFENSE POLICY, JUSTIFIED HOMICIDES, AND RACE

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July 31, 2017

Abstract

The self-defense policies known as *Stand Your Ground* reduce the expected cost of using lethal force. I use detailed police records to examine whether these policies had differential effects on the reportedly-justified homicide rates of people across races. I find that the implementation of *Stand Your Ground* policies lead to an average of 2.75 ($p < .01$) additional black Alleged Perpetrators of Crimes being killed each month, 2.39 ($p < .01$) of whom are killed by black citizens. Additionally, I find 0.5 additional white Alleged Perpetrators are killed each month, 0.49 of whom are killed by white citizens. I test the differences between race groups and find that they are strongly significant in all cases ($p < .01$). I then use event studies to confirm that my results are not caused by pre-existing trends. My results provide strong evidence that *Stand Your Ground* policies cause unequal outcomes across racial groups, and I postulate several mechanisms that may be the cause of these racially disparate effects.

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1 Introduction

It is common for laws in the United States to extend strong protections to citizens who must defend their person or family while inside their homes. However, citizens in public venues have historically been obligated by law to attempt a safe retreat prior to using force in self-defense, a requirement known as one's "duty to retreat" (Levin, 2010). Between the years 2005 and 2014, twenty-seven states enacted an explicit set of rules enhancing a private citizen's right to defend their person and their family while outside their home; these rules effectively remove the duty to retreat (Boots, Bihari, and Elliott, 2009). The primary goal of this article is to measure the effect of these policies on citizens' propensity to use lethal force, and to explore whether these policies had differential effects on the "reportedly justified homicide"[1] rates of people across races. I find that the effects of these policies vary significantly across race groups, with killings of black persons sharply increasing while killings of white persons are nearly unaffected.

These policies are colloquially known as *Stand Your Ground* (*SYG*). Although subtle variations exist between each state's policy, they typically remove the duty to retreat by creating an "affirmative defense."[2] This affirmative defense requires the government to presume that the citizen reasonably believed that deadly force was necessary and also to presume that the Alleged Perpetrator of a Crime (APOC)[3] had the intention of using violence against the citizen. These two presumptions, taken together, permit the citizen to claim self-defense and grant the citizen immunity to prosecution. If these presumptions are disproved throughout the course of the police investigation, then the protections granted by the *SYG* policy are revoked.

[1] I use the phrase "reportedly-justified homicides" rather than the phrase "self-defense homicides" because the former has a more appropriate connotation: although the police reported the homicide as "justified," scholars cannot positively conclude that each killing was truly an act of self-defense.

[2] An affirmative defense is a legal tool that mitigates a defendant's culpability in civil or criminal proceedings. See `www.law.cornell.edu/wex/affirmative_defense`, accessed 13 June 2017.

[3] I create the term "Alleged Perpetrator of a Crime" to dispassionately describe the person killed during a reportedly justified homicide. I do this because the deceased was never convicted, by a jury of their peers, of any crime related to the incident during which they were killed. As these APOCs are unable to explain their actions or present a cogent defense, I refrain from using terms that confer a sense of guilt.

By creating an affirmative defense, *SYG* policies "reduce the expected cost of using lethal force" (Cheng and Hoekstra, 2012; hereafter *CH*). Becker (1968) explains that a reduction in expected punishment will increase a citizen's propensity to perform the punishable action, suggesting that *SYG* policies will increase the likelihood that a citizen will kill an APOC. The moral, ethical, and legal arguments for or against these policies are not discussed in this paper because they are thoroughly examined by many authors in journals of law and policy, including Catalfamo (2006), Ross (2007), Megale (2010), Lawson (2012), and Lave (2012).

In the first systematic analysis of *SYG* policies, CH examine police records and find that *SYG* policies cause an 8 percent increase in reported murders and non-negligent homicides annually. McClellan and Tekin (2016; hereafter *MT*) extend the analysis by using monthly mortality data, rather than annual police records, and by slightly modifying their definition of *SYG*. MT find that *SYG* policies cause approximately 30 additional murders or non-negligent homicides each month. Both sets of authors briefly examine the policies' effect on reportedly-justified homicides, but only incidentally. I am able to replicate many of the CH and MT results in this paper, supporting their conclusions and findings.

The primary contribution of my work is to examine the racially disparate effects of these policies on the reportedly-justified use of lethal force by citizens. By examining this racial disparity, my research directly addresses the frequent media claims that SYG laws "make it easier to kill blacks"[4] and the implications that the costs of these policies are disproportionately paid by the black community. These costs include the economic value of the human life (Conley, 1976; Droman, 2009) that is lost when a citizen commits a homicide, as well as the substantial loss of human capital investment that was funded by the community through public schools and other social programs (Schultz, 1961; Glomm and Ravikumar, 1992). Each

[4]See news stories such as "States Are Quietly Resurrecting a Law That Makes It Easier to Kill Blacks" (accessed on 28 July 2017 from `www.theroot.com/states-are-quietly-resurrecting-a-law-that-makes-it-eas-1794633188`), "McKnight killing shows how Louisianas stand your ground law codifies bigotry" (accessed on 28 July 2017 from `thelensnola.org/2017/01/06/mcknight-killing-shows-how-louisianas-stand-your-ground-law-codifies-bigotry`), "Stand Your Ground Laws Complicate Matters For Black Gun Owners" (accessed on 28 July 2017 from `www.npr.org/ sections/codeswitch/2017/02/27/517109271/stand-your-ground-laws-complicate-matters-for-black-gun-owners`).

APOC killed also forfeits the remainder of their lifetime earnings and all associated economic contributions to their community, as measured by Rice and Cooper (1967).

I am unable to measure these monetary costs directly, because the monetary costs strongly depend on the mechanisms that drive my results. These mechanisms are difficult to precisely identify. However, I am able to quantify the cost of these policies in terms of lives lost each month and separate this cost by the race of the citizen who used the lethal force. I then present a list of potential mechanisms that can explain the observed differential effects in Section 5.1, and discuss how these can influence the monetary cost of *SYG* policies in Section 5.2.

To measure the cost of these policies in terms of lives lost per month, I examine four different combinations of reportedly-justified homicides: black APOCs killed by black citizens, black APOCs killed by white citizens, white APOCS killed by black citizens, and white APOCs killed by white citizens. These combinations are hereafter referred to as *race-pairs*.[5] I measure the reportedly-justified use of lethal force by using detailed police records of incidents where a private citizen uses a firearm[6] to kill an APOC. I then employ a generalized difference-in-differences analysis to calculate the change in the reportedly-justified use of lethal force before and after the implementation of *SYG* policies.

I find that reportedly-justified homicides of black APOCs increase by 6–14 percent ($p < .05 - p < .01$) while reportedly-justified homicides of white APOCs increase by only 0–3 percent (not statistically significant). In terms of the cost to human life, these results imply an average of 2.75 additional black APOCs and 0.5 additional white APOCs are killed each month. These effects are larger within race than across race; 2.39 of the 2.75 black APOCs killed are killed by black citizens, and 0.49 of the 0.5 white APOCs killed are killed by white citizens.

[5] I examine other races, but the small number of observations yield imprecise estimates; these results are available upon request.

[6] I follow the work of MT and use firearm-related homicides so my results can be accurately referenced in future discussions of gun policy. Results using all reportedly-justified homicides are similar in sign and magnitude; these are available upon request.

I test the significance of these differential effects, and I find that all differences are strongly significant ($p < .01$). I perform an event study for each race-pair to confirm that my results are being driven by enactment of *SYG* policies rather than by preexisting homicide trends. I also perform a series of placebo tests to rule out spurious correlations with mortality trends over time. Finally, I conduct sensitivity tests to illustrate that I do not select a specification or time period to accentuate my results.

2 Data

2.1 Data Collection

This analysis requires two key components. First, detailed information pertaining to each homicide is needed to calculate the per capita reportedly-justified homicide rate in each observed year for each race-pair, including the demographics for both the shooter and the deceased. Second, the date that each *SYG* policy was enacted for each state is required to distinguish the reportedly-justified shootings occurring before and after the change.

Homicide Data

Homicide data are taken from the FBI's monthly Uniform Crime Reporting Program (UCR), the Supplementary Homicide Report (SHR), 2000–2014. The program records details of each homicide "incident," as defined by the UCR Program. The FBI's SHR counts all reported homicides and categorizes them by the method of – and the reason for – death, making it possible to identify homicides performed as acts of self-defense. Each observation also includes information on the victims, the offenders, the weapons used, and the circumstances surrounding the homicide. This makes the SHR unique in its ability to provide data on both the deceased and the shooter, whereas other data sets only provide information about the deceased. This allows me to conduct a unique investigation into the interactions between racial groups, which are impossible to do with other data sets, such as the Center for Disease Control's Multiple Cause of Death (MCOD) report.

The primary criticism of the SHR is that the reporting requirement is not strictly enforced, and therefore the data may not include all perpetrated homicides.[7] This would only present a problem for my analysis if the reporting behavior covaried with changes in *SYG* policies. However, CH postulate that "there is no reason to believe that any total homicide reporting issue at any state level should be systematically correlated with changes in *SYG* law." I confirm their belief by examining reporting behaviors in Section 4.3. I find that reporting behaviors are not correlated with changes in policy. By verifying the claims of CH, I show that homicide reporting does not present a risk to my analysis or to the analysis of CH.

Policy Data

I identify *SYG* policies by reviewing existing publications on the topic. I create a list of states commonly reported to have a *SYG* policy, and then locate the public records of each original legislative action.[8] The effective dates of each state's *SYG* policy, along with the name of the bill creating the protections, can be found in Table I. It can be seen that 27 states changed their laws during the observed period, one state had a *SYG* policy in place prior to the observed period, and the remaining states never enacted these expanded self-defense rules. Of these 27 states, Florida is excluded from my analysis for reasons discussed later. A graphical depiction of policy changes over time can be found in Figure I. It can be seen in this figure that a majority of states enacted a *SYG* policy between the years of 2005–2007. Figure II illustrates the geographic location of the states that changed their laws during the observed time period.

[7]Testing the number of reported homicides against the MCOD report, as seen in Figure VIII, shows that the SHR tends to under-report homicides. See Wiersema, Loftin, and McDowall (2000) for a thorough discussion.

[8]To verify each law on my list, I utilized each state's public directory of statutes. After verifying the existence and content of each law on my list, I searched for mentions of any other state laws or policies in non-academic sources, such as websites hosted by politically motivated lobbying groups and websites intended to provide information to firearm enthusiasts. Through these sources, I discovered a 2007 Oregon State Supreme Court ruling regarding enforcement practices of the existing self-defense statute, ORS 161.219. The court's decision on the case, *State of Oregon v. Sandoval*, included the following statement: "On a purely textual level, ORS 161.219 contains no specific reference to 'retreat,' 'escape,' or 'other means of avoiding' a deadly confrontation. Neither, in our view, does it contain any other wording that would suggest a duty of that kind." After this decision, the law in Oregon was enforced in the same manner as a state that passed new *SYG* legislation. Therefore, for the purpose of analyzing the changing self-defense rules, the effect of the court ruling is identical to the effect of a legislative action.

As noted, I make a substantial change from the existing literature's methods in how I handle the state of Florida, which is excluded from the FBI's SHR data. CH contacted the Florida Department of Law Enforcement Office and obtained numbers to use in their analysis in place of the missing FBI data. I obtained the same data from the Florida Department of Law Enforcement Office, but I then contacted the FBI and inquired why they exclude Florida from their reports. I was told Florida does not follow the FBI's guidelines for reporting.[9]

I test my model with and without the data from the Florida Department of Law Enforcement Office. I find that excluding the Florida data causes my results to diminish, but does not alter their practical interpretation. Based on this test and my conversation with the FBI, I elect to exclude the Florida data from my analysis. If the Florida data truly merit exclusion, then my results represent the true treatment effect. If the Florida data should have been included, then my results represent the lower bound for the true treatment effect and maintain their validity. I consider this to be the most conservative solution to the problem at hand, since the FBI did not clarify which of their data reporting guidelines was violated.[10]

To check if any clear political trends exist in the passage of SYG policies, I examine the composition of each state's legislative chambers at the time a SYG policy was enacted. Using public election records, I plot the political affiliation of each state's legislative body at the time the law was passed; see Figure III. I use a spectrum ranging from full control by Democrats, through an even split of power, to full control by Republicans. I then plot the composition of the remaining states, which did not enact a SYG policy, using data from the midpoint of the treatment period, or 2010. Despite the Republican political platform commonly supporting gun rights and citizen autonomy,[11] it can be seen that 10 of the 27

[9]The UCR Program guidelines are published at `ucr.fbi.gov/data-quality-guidelines-new`. Some published requirements could affect the data's quality if they are violated, such as the requirements for "logical consistency," "reasonableness," and "adherence to sound estimation methodologies." Other published requirements would not affect the data's quality if they are violated, such as the requirement to "allow adequate time for reviews" or "provide methodologies, origins of data."

[10]I spoke with an FBI representative and inquired why Florida was excluded from the SHR data. When I requested a quotable statement for this paper, the representative provided me with the following written statement: "The SHR data reported by the state of Florida does not follow UCR Program guidelines and are not used."

[11]See the full Republican Platform at `www.gop.com/platform`, accessed 12 June 2017.

states that enacted *SYG* policies during the observed treatment period had either one or both chambers of their state legislature controlled by Democrats. Furthermore, in 5 states, both chambers of state legislature are controlled by Republicans, yet no policies were enacted during the observed time period. This evidence suggests that both bipartisan support and bipartisan opposition exists for *SYG* policies.

2.2 Creating Homicide Measurements

The SHR data are available in two forms: summary files providing total numbers of homicides in each state but a limited number of other identifying variables, and raw files containing details of every individual reported homicide event. Unlike previous studies, I elect to use the raw files, and then separate each event into unique observations for each victim. Through this process, I am able to obtain an accurate count of the total homicides in each state, as is available in the summary files, while also maintaining access to the rich set of covariates. Other authors who use the raw SHR data files employ a binary variable to indicate when a homicide event involves multiple victims (Roman, 2013), which makes interpreting the results difficult. My process allows my results to be interpreted as *SYG* policies' cost to human life. Furthermore, this choice in data set and outcome variable provide me with full data on the citizen using lethal force and on the APOC who was killed; past studies using other data or outcome variables only had access to the race of the deceased.

I categorize each shooting by the race of the citizen and the APOC, and then tally the total number of reportedly-justified shootings for each race-pair at the state level. I make these state-totals comparable across state lines by dividing by the population of all reporting agencies and multiplying by 1,000,000. This construction of a homicide rate is different from previous authors, who use counts of total homicides in one, or all, of their specifications (Cheng and Hoekstra, 2013; Roman, 2013; McClellan and Tekin, 2016). In Table II, I report the mean reportedly-justified homicide rate for the full data set, along with the averages for urban and rural populations.

Finally, I follow convention and transform my outcome variable so as to interpret my results in terms of a percent-change. This transformation is commonly performed using the natural logarithm of the outcome variable, but my data set contains many zeros at which the logarithmic transformation would be undefined. Cheng and Hoekstra (2013) solve this problem by adding 1 to each state's observed homicide count, but I elect to use the Inverse Hyperbolic Sine transformation.[12] The Inverse Hyperbolic Sine transformation has the same interpretation as the logarithmic transformation, but has the benefit of being defined at zero. As discussed by Pence (2006), the transformation an outcome variable, X, is defined as

$$sinh^{-1}(X) = ln(X + \sqrt{(X^2 + 1)}\ )$$

The transformation of large values of X becomes $sinh^{-1}(X) \approx ln(2) + ln(X)$, a vertical displacement of the logarithmic transformation of X, while the transformation of $X = 0$ is simply $sinh^{-1}(0) = ln(1) = 0$.

3 Econometric Methodology

I employ a generalized difference-in-differences model to analyze the mechanisms by which *SYG* policies influence the behavior of private citizens. This method has been employed in several seminal articles, such as Ashenfelter (1978), Ashenfelter and Card (1985), and Card and Krueger (1993). However, difference-in-differences models rely on key assumptions for the estimates to be consistent and unbiased (Bertrand, Duflo, and Mullainathan 2004). First, I must assume that the impact of time is consistent for all citizens; that is, if the passage of time causes justified homicide rates to fall in states that eventually enact a *SYG* policy, then the same passage of time causes the same fall in states that never enact a policy. Second, I must assume that the impact of residing in a particular state is consistent across time; that

[12]This transformation was first proposed by Johnson (1949), discussed in economic applications by Burbidge, Magee, and Robb (1988), MacKinnon and Magee (1990) and Pence (2006), and also has been used Card and DellaVigna (2013).

is, if the states that eventually enact a *SYG* policy have higher justified homicide rates than states that never enact a policy, then this difference will remain constant over time. These assumptions allow me to isolate the change in justified homicide rates caused by enactment of *SYG* policies.

These assumptions can be verified by examining a graph of the data during the period before the enactment of *SYG* policies. Figures IVa and IVb can be utilized for this verification. These figures plot the number of justified homicides of black APOCs (IVa) and white APOCs (IVb). It can be seen that the impact of time is consistent for all citizens: reportedly-justified homicide trends generally rise and fall together during the period before these policy changes begin. It can also be seen that the impact of residing in a state that eventually did, or did not, enact a *SYG* policy is consistent across time: the difference between reportedly-justified homicide rates is generally constant during the period before these policy changes begin. Given these assumptions, I can consider states that did not change their policies to be the counterfactuals for the states that did and thereby calculate the change in justifiable homicides caused by enacting *Stand Your Ground* policies.

I now create the equation used to calculate the changes for each race-pair. On the left-hand side, I use the Inverse Hyperbolic Sine of the monthly homicide rate per 1,000,000 citizens as the outcome variable for each race-pair. On the right-hand side of the equation, I incorporate the changing policies with a binary variable ($P_{s,t}$) equal to 1 if the state (s) has already enacted a *SYG* policy in the observed month and year (t). I also include λ_s, a vector of fixed effects controlling for variations caused by the state in which the homicide event occurred, and μ_t, a vector of fixed effects controlling for variations caused by the month and year in which the homicide event occurred. The inclusion of λ_s and μ_t prevent bias from being introduced into my calculations by spurious correlations between the enactment of *SYG* policies and prominent events at the month- or state-level. Finally, following the example of CH and the suggestions of Solon, Haider and Wooldridge (2015), I weight my observations by the average population measured over the sample period and use robust standard errors

clustered at the state level.

With this framework, I model the Inverse Hyperbolic Sine of the justified homicides rate ($Y_{s,t}$) for each race-pair as:

$$Y_{s,t} = \alpha + \delta(P_{s,t}) + \lambda_s + \mu_t + \varepsilon_{s,t} \tag{1}$$

where the coefficients α and δ are unknown parameters and $\varepsilon_{s,t}$ is an idiosyncratic error term. Since the outcome ($Y_{s,t}$) is the Inverse Hyperbolic Sine transformed homicide rates, my estimation of δ is interpreted as the percent change in the monthly homicide rate caused by the implementation of SYG policies.

4 Results

4.1 Consistency with Previous Literature

Before presenting the results of my analysis, I demonstrate that my methods for cleaning, organizing, and analyzing the data create results consistent with the work of published authors. I begin by replicating the primary result of CH: the "8 percent net increase" of homicides highlighted in their abstract.

I restrict my data to the years 2000–2010, their sample time period, use the same regression specification, and use the same treatment classifications; *i.e.*, including Florida in the sample and classifying Oregon as untreated.[13] This required the use of the data that I obtained from the Florida Department of Law Enforcement Office. Table III, columns 1 and 2, show that my methods yield similar results despite the differences in how I cleaned, organized, and transformed my data. This replication provides strong evidence supporting the analysis and findings of CH, while also corroborating the conclusions of MT.

The remainder of Table III presents regression results as I make individual changes to

[13]Refer to Footnote 8

CH's model, culminating in the model used for the remainder of my analysis. In specification 3, I broaden my sample to include 2000-2014 data, which moves my estimate closer to zero. In specification 4, I include Oregon in the treatment group, which slightly increases the estimated treatment effect. In specification 5, I drop Florida from the sample for the reasons discussed in Section 2, and I employ my preferred measurement of homicide rate in specification 6. I switch from annual to monthly data measurements in specification 7, increasing my sample size from 829 to 8,149 and increasing the magnitude of the treatment effect. Finally, in specification 8, I follow the example of MT and narrow my sample to include only firearm-related homicides so that my results can be discussed in the context of firearm policy. This has a negligible impact on the estimated treatment effect.

I also check if my methods can corroborate the results reported by CH and MT regarding reportedly-justified shootings committed by private citizens. CH employed an unweighted OLS regression using a simple count of justified homicides as the outcome variable as well as a negative binomial regression. Their OLS model estimates an increase of 3.2 justified homicides per state,[14] and their negative binomial model estimates an increase of 28 to 57 percent per state. MT employ OLS and Poisson regressions on simple counts of reportedly-justified homicides across and also found statistically significant coefficients, but these coefficients do not have a practical interpretation. I examine moderately similar outcomes in Tables IX and X, and find results similar in sign and magnitude.

4.2 Effects of Implementing *SYG* Policies, by Race-Pair

I now turn to the racially disparate results of my primary analysis. Table IV presents my estimate of δ, the percent change in the monthly homicide rate caused by the enactment of *SYG* policies, for each race-pair. It can be seen that the increase of white APOCs shot by black citizens is approximately 0.5 percent, the smallest point estimate of my primary results. In contrast, white APOCs shot by members of their own race increases by 2.26 percent. These

[14]These OLS results cannot be interpreted in terms of percent-change.

percentages can be converted[15] to reflect the number of lives lost as a result of these policies: 0.014 additional white APOCs shot by black citizens each month and 0.490 additional white APOCs shot by white citizens each month. Neither of these results is statistically significant.

The estimated effect for black APOCs is far larger in magnitude. *SYG* policies can be seen to increase the number of black APOCs shot by white citizens by 5.56 percent ($p < .05$), while increasing the number of black APOCs shot by black citizens by 14.34 percent ($p < .01$). These percentages can also be converted[16] to reflect the number of lives lost as a result of these policies: 2.39 additional black APOCs shot by black citizens each month and .361 additional black APOCs shot by white citizens each month.

To test if these point estimates are significantly different, I follow the methods discussed by Cameron and Trivedi (2009). For each race-pair combination, I jointly estimate a system of Seemingly Unrelated Regression (SUR) equations and test for equality between coefficients. Table IVb presents the relevant test statistics. It can be seen that there is a strong statistical difference between each estimate ($p < .01$). For example, the χ^2 test statistic for the difference between "white APOCs shot by black citizens" and "black APOCs shot by black citizens" is 54.99, greatly exceeding the critical value necessary for confidence at the 1 percent level, which is 6.63. These differences indicate a true disparity in how *SYG* policies influence the homicide rates within and between racial groups.

In short, *SYG* policies increase the homicide rates among race-pairs in the following order of descending magnitude:

(Black APOCs shot by Black citizens) $\gg$ *(Black APOCs shot by White citizens)*

$\gg$ *(White APOCs shot by White citizens)* $\gg$ *(White APOCs shot by Black citizens)*

[15]Calculation performed using changes in reportedly-justified rates from Table IV, average number of reportedly-justified shootings from Table II, the average historical percentage of white citizens nationally (83.29 percent) as recorded in SEER data (accessed on 30 June 2015 from `seer.cancer.gov`), and current U.S. population (325,340,715) as estimated by U.S. Census Bureau at the time of writing (accessed on 30 June 2017 from `www.census.gov/popclock`).

[16]Calculation performed using the average historical percentage of black citizens nationally (12.48 percent) as recorded in SEER data (accessed on 30 June 2015 from `seer.cancer.gov`) in conjunction with the data described in Footnote 15.

Next, I repeat my analysis for urban and rural jurisdictions to check if citizens in these areas respond differently to the change in policy. To accomplish this, I utilize data from the Center for Disease Control's National Center for Health Statistics (NCHS). Specifically, I utilize the Urban-Rural Classification Scheme for Counties from 1990, 2006 and 2013. I consider the six categories of classifications utilized by the NCHS. I define "rural" citizens to include *population clusters of less than 50,000* and all smaller counties. I define "urban" to include the categories of *small metro populations between 50,000 and 250,000* up through *large metropolitan centers of 1 million or more*.

In Table V, I present results using only urban (Panel A) and rural (Panel B) reportedly-justified shootings. In urban environments, the estimated effects of a policy change are similar to the full-sample estimates: white APOCs shot by black citizens yielding the smallest result, a 0.5 percent increase in justified homicides, and black APOCs shot by black citizens yielding the largest result, a 15.3 percent increase in justified homicides ($p < .01$). In rural environments the results are again similar, but none of the race-pair categories yield statistically significant results.

My full-sample race-specific results are further corroborated by my event study, graphed in Figures V and VI. Figure V graphs the effect of these policies against black APOCs over time and in relationship to the year each *SYG* policy was enacted. Panel A shows a sharp increase in the rate of black citizens shooting black APOCs following each *SYG* change and a relatively flat trend in the years preceding each change. Panel B shows a mild increase in the rate of white citizens shooting black APOCs following the enactment of each policy, while also showing a flat trend in the years prior. Figure VI, Panels A and B, examine the policies' effects on white APOCs: unequivocally flat trends can be seen both before and after each change. In addition to providing strong evidence of a racially disparate policy effect, these event studies also provide evidence that my results are not driven by preexisting trends in citizens' use of lethal force.

4.3 Placebo, Sensitivity, and Robustness Checks

I now provide evidence to support the validity of my analysis by confirming CH's claim that "there is no reason to believe that any total homicide reporting issue at any state level should be systematically correlated with changes in *SYG* law," as discussed in Section 2. Using the Offenses Known and Clearances by Arrest data, provided through the Uniform Crime Reporting program, I examine the total number of homicides reported each month across the UCR program per 1,000,000 citizens. This report records every murder or nonnegligent manslaughter event regardless of whether the event was eventually deemed to be fake, baseless, unfounded, or only an "attempted" murder. If reporting activity was systematically correlated with changes in *SYG* law, this correlation would be observed in Table VIa. I find no correlation and therefore verify the claims of CH. This evidence supports my results, as well as the results of CH.

Although I have already verified CH's claim, I continue my investigation into reporting behaviors by examining three additional measures of reporting behavior over time, all of which I analyze per 1,000,000 citizens. The first is the number of homicides deemed to be unfounded, baseless, or fake during the course of the reported month. This category includes both the current month's unfounded incidents as well as old homicide cases that were originally investigated as a homicide but more recently deemed baseless. According to the reporting manual, reportedly-justified homicides should be included in the unfounded category in the month they are determined to be justified. The second is the "actual" homicides in a given month. This is recorded as the difference between the reported and the unfounded cases in a specific month, implying that a prior month's homicide that has recently been deemed to be baseless would be subtracted from the current month's homicide count. The last category is the "clearance rate," which is the number of arrests[17] each month divided by the "actual" number of homicides calculated for that month. I examine these behaviors to

[17]This also includes clearance by "exceptional means," such as when a murderer commits suicide and cannot be arrested

determine if police agencies manipulate their reports in an attempt to make crime in their jurisdiction appear better or worse; *e.g.*, if an agency began recording regular homicides as self-defense in an attempt to improve their clearance rate.

If police agencies manipulated their reports to accomplish a given goal, it would be observed through some combination of the three reporting measures in Tables VIb, c and d; *e.g.*, widespread misreporting to improve clearance rates would be observed in Table VId, which reports the correlation between clearance rates and the passage of *SYG* policies. I find no significant results in any category of reporting behavior, suggesting widespread misreporting did not begin occurring as a result of *SYG* policies.

Next, I conduct placebo, or falsification, tests on events that should be exogenous to changes in *SYG* policy: traffic fatalities, unemployment rates, and homicides performed in manners that are unrelated to self-defense. These tests are reported in Table VII. If my analysis is truly capturing the effect of enacting a *SYG* policy, rather than some other unobserved trend in fatalities, then I expect to find no correlation between the enactment of *SYG* policies and these events.

I begin by recreating my data set using only homicides performed in manners that are unrelated to self-defense, such as poison, arson, explosion, or by causing a drug overdose. I cannot examine full racial interactions here, as I do in my primary analysis, because only reportedly-justified homicides have reliable data on the race of both the perpetrator and the deceased. Using the same treatment assignments and the same data window, I repeat my analysis with these placebo homicides as the outcome variable. All results are small in magnitude and statistically insignificant, which provides evidence to support my analysis.

I then obtain traffic fatality data from the National Highway Traffic Safety Administration and examine deaths of both black and white citizens. Using the same treatment assignments and the same data window, I find no significant results, which again provides evidence to support my analysis.

For my last falsification test, I utilize the Federal Reserve Economic Data to repeat my

falsification test using unemployment data for black and white citizens. I again find no significant results.

I now test the sensitivity of my results to my model's specification. I include various fixed effects and modify the sample time period to verify my results are robust to changes in the model; I report these results in Table VIII. Specification 1 removes all fixed effects from my model, while 2 and 3 add State×Month and Region fixed effects to the original model, respectively. Specifications 4 and 5 restrict the sample time period around the years 2005–2007, when the bulk of the states enacted a *SYG* policy. It can be seen that all results are either similar to my primary results or larger in magnitude, indicating that my results are robust to different choices in specification and sample periods, and also confirming that my sample period was not selected to accentuate a preconceived set of results.

Lastly, I test to see if controlling for *Carrying a Concealed Weapon* (*CCW*) legislation improves the precision of my estimates. I repeat my entire analysis[18] including indicator variables for *CCW* legislation, but I find that my estimates do not substantially change. I do not explore the effect of including other controls, such as police presence or incarceration rates, as these have been shown to be of little importance by the existing literature (Cheng and Hoekstra, 2013; Roman, 2013; McClellan and Tekin, 2016).

5 Mechanisms and Implications

I have presented evidence that the enactment of *SYG* policies produces measurably different effects across race-pairs, and I have performed a series of checks to support my results. My analysis determines that the cost imposed upon the black community, quantified in the number of human lives lost, is 5.5 times larger than the cost imposed upon the white community. However, I am not able to determine the monetary cost of these racially disparate effects, as the monetary cost strongly depends on the mechanisms that drive my results. Therefore, I alphabetically present a number of possible mechanisms in Section 5.1, followed

[18] I omit these results for brevity; they are available upon request.

by a discussion of how these mechanisms can influence the monetary cost of these policies in Section 5.2.

5.1 Potential Mechanisms

Availability of Law Enforcement Officers

Police response times have been shown to vary based on neighborhood characteristics and caller demographics, suggesting that black 911-callers and black neighborhoods receive slower police responses than their white or Hispanic counterparts (Lee, Lee, and Hoover, 2016). Anecdotal evidence also suggests that differences exist in how black citizens perceive the availability of law enforcement personnel.[19] These differences in law enforcement availability, real or perceived, may incite black citizens to assume more responsibility for their own protection. If this is the case, then a change in policy that increases crime[20] or that decreases the expected cost of killing an APOC could create larger incentives for black citizens to use lethal force than for white citizens.

Black-Crime Hypothesis

The *Black-Crime Hypothesis* of Eitle, D'Alessio, and Stolzenberg (2002) states that an increase in black-on-white crime causes an increase in black arrest rates, while an increase in black-on-black crime yields no changes. This implies that an increase in criminal activity caused by *SYG* policies will lead to a decrease in the stock of criminals acting against white citizens while the stock of criminals acting against black citizens is not diminished. Relative to black citizens, white citizens would therefore be faced with fewer encounters over time where using lethal force in self-defense is allowed. If this is the case, then the racially disparate

[19]See news stories such as "In New Orleans, call 911 and wait for an hour" (accessed 1 June 2017 from `www.economist.com/blogs/democracyinamerica/2015/12/police-response-times`), "Is 911 'still a joke' for African-Americans?" (accessed 1 June 2017 from `www.thegrio.com/2014/04/23/is-911-still-a-joke-for-african-americans`) or "Newly-released data shows City continues to deny equitable police services to South and West Side neighborhoods" (accessed 1 June 2017 from `www.aclu-il.org/newly-released-data-shows-city-continues-to-deny-equitable-police-services-to-south-and-west-side-neighborhoods`).

[20]Increased criminal activity causally related to *SYG* policies were observed by CH and MT.

results of my analysis could provide evidence supporting Eitle, D'Alessio, and Stolzenberg's hypothesis.

Implicit Attitudes and Threat Perception

Implicit attitudes are evaluations that are "activated outside of conscious attention" (Bargh, Chaiken, Govender, and Pratto, 1992). Implicit attitudes have the potential to be activated by one object and mis-attributed to a different object (Greenwald and Banaji, 1995), such as observing a black individual and mis-attributing the fear of a deadly weapon onto a harmless object. Studies show that implicit attitudes are more influential when making quick decisions under pressure (Dovidio, Gaertner, and Kawakami, 2002). Considering that the average self-defense shooting occurs in 3–5 seconds (Beretta, 2014), it is reasonable to suspect that implicit attitudes may influence an average citizen's perception during a confrontation with an APOC.

A legal claim to self-defense requires a reasonable belief that an APOC was threatening a citizen with harm, but if these implicit attitudes cause a citizen to associate an APOC with a dangerous weapon, then the citizen may mistakenly believe their unnecessarily lethal behavior to be "reasonable." An unpublished and non-random study conducted by Project Implicit has identified individuals who hold implicit attitudes associating black persons with dangerous weapons,[21] suggesting that the determination of a belief as "reasonable" may be dependent on the race of the citizen and the APOC. Policies that increase a citizen's propensity to kill could therefore cause more citizens to kill the black APOCs whom they implicitly associate with deadly weapons, thus producing disparate effects across race-pairs. If this mechanism is present, then the results of my analysis could provide evidence of widely-held implicit attitudes associating black APOCs with deadly weapons, particularly among black citizens.

[21] The Race-Weapons Task, available at `implicit.harvard.edu/implicit/Launch?study=/user/demo.us/demo.weapons.0002/weaponsdemo.expt.xml` (accessed 13 May 2017) has been completed by 530,817 website visitors between the years 2004–2015 and found that 73% had either a slight, moderate or strong implicit attitude that associates black persons with dangerous weapons.

Perception of Enforcement Behaviors

Despite intentions to uniformly enforce the law, the racial disparities in prosecution and sentencing (Cole, 1999; Mustard, 2001) may influence a citizen's belief about how any law will be practically enforced. Citizens of racial minorities may fear unfair prosecution in the criminal justice system, increasing the expected cost of using lethal force and decreasing the amount of force they use to defend their person or family. It is therefore possible that, prior to the passage of *SYG* policies, black citizens may calculate the expected cost of not resisting a criminal's actions to be less than the expected cost of resisting the criminal and subsequently facing unfair prosecution by the government. If this is the case, then a *SYG* policy that creates an affirmative defense would sharply lower black citizens' expected cost of using lethal force relative to the white citizens' expected cost, producing the disparate effects across race-pairs as observed in my analysis.

Protection Hypothesis

The expected cost of criminal activity increases if the potential victim possesses a strong network of associates willing to seek revenge upon the criminal. This concept of familial or social protection can be found both in modern times and throughout history. Greek prose places great importance on the act of showing hospitality to travelers who have no local family to protect them (Regenos, 1956; Roth, 1993), and similar concepts can be seen in the protection offered to Mafia family or to gang members (Skaperdas, 2001).

According to Fomby, Mollborn, and Sennott (2010), black citizens have a "broader network of kin and kinlike figures" than their white counterparts, and their *Protection Hypothesis* states that this strong family network offers assistance during times of structural family change. Black citizens also have a higher propensity to participate in organized crime gangs than white citizens (National Gang Center, 2017). It is therefore possible for violence perpetrated against black citizens to have a higher propensity to incite retaliatory crimes than violence perpetrated against white citizens, creating more opportunities for black citizens to use lethal force in self-defense. As a result, a policy change that increases a citizen's propensity

to use violence could cause significantly larger effects in the black community relative to communities where theses strong social or family ties are not as prevalent.

These racially disparate effects could then be exaggerated if this mechanism is combined with the aforementioned *Black-Crime Hypothesis* mechanism. Taken together, these suggest that an increase in black-on-black retaliatory crimes would not cause an increase in arrest rates, allowing the cycle of retaliation to continue. Conversely, an increase in black-on-white crimes would increase arrest rates, slowing the potential cycle of retaliatory crimes and exaggerating the differences between race-pairs caused by these policies.

5.2 Implications

As discussed in Section 1, the monetary cost of *SYG* policies can include the economic value of the human life, the loss of human capital, and the loss of the APOC's lifetime earnings. Although the number of lives lost will remain the same, each of the mechanisms discussed in Section 5.1 has different implications for the monetary cost of these policies. To illustrate this point, I present two cases of black APOCs who were shot by black citizens and I hypothesize what each APOCs' lifetime earning potential would have been, had they not been killed. For simplicity, I consider no costs other than the loss of lifetime earnings.

The first case occurred on on 6 July 2008, when an APOC "yelled" at a delivery man, who proceeded to fatally shoot the yelling APOC. Police reported the APOC was unarmed, and video evidence confirmed that the APOC did not attack the citizen, yet the citizen was still able to invoke protection under the state's *SYG* policy.[22] Given that the video evidence confirms that this APOC did not attack the citizen, it is unlikely that the APOC would have been arrested or incarcerated as a result of this encounter. If this is the case, then the cost of the state's *SYG* policy would be the entirety of the APOC's lost lifetime earnings. This case appears to be driven by the *Implicit Attitudes and Threat Perception* mechanism.

The second case occurred 22 May 2015, when an APOC began shooting customers in a

[22]See "Florida's Stand Your Ground Law, Case 128" (accessed 29 June 2017 from `www.tampabay.com/stand-your-ground-law/cases/case_128`).

Philadelphia barbershop, some of whom were young children. A citizen entered the shop and fatally shot the APOC, invoking protection under the state's *SYG* policy.[23] If this APOC had not been killed by the citizen, it is likely that he would have been arrested and incarcerated, thereby greatly reducing his lifetime earning potential. If this is the case, then the loss of earnings and the cost of these policies would be similarly reduced. This scenario appears to be driven by the *Availability of Law Enforcement* mechanism.

Taken together, these two cases imply that it is possible for the circumstance of the reportedly-justified homicide to determine the monetary cost of *SYG* policies. Therefore, we must make assumptions about what circumstances are most prevalent in the data set before we are able to estimate the monetary cost of these policies. If future research can accurately identify which mechanism, or combination thereof, drives the racially disparate results I observe in my analysis, then we should be able to make an accurate assumption about the prevailing circumstances. Continuing from the previous examples, if the *Implicit Attitudes and Threat Perception* mechanism is driving my results, then we might assume that the first case is more prevalent and the monetary cost of these policies may be greater than if my results were driven by the *Availability of Law Enforcement* mechanism.

6 Conclusions

In this paper, I use the widespread implementation of *SYG* policies as a natural experiment to highlight their racially disparate effect on reportedly-justified homicide trends. I accomplish this by using a generalized difference-in-differences analysis to measure the effect of these policies on four categories of race-pair interactions, and I use systems of SUR equations to determine that each difference is strongly significant ($p < .01$). I present event studies to show that these effects are not caused by pre-existing trends, and I conduct a number of placebo and sensitivity tests to rule out spurious correlations in reporting behaviors or

[23] See "Gunman Shot, Killed Inside West Philly Barbershop" (accessed 29 June 2017 from `www.nbcphiladelphia.com/news/local/Man-Shot-in-the-Chest-Inside-West-Philly-Barbershop-297176271`).

mortality trends. I also replicate and corroborate many of the results published by CH and MT, which, in the words of MT, is "an important step towards building a consensus on the debate."

The primary contribution of this article is to examine the racially disparate effects of *SYG* policies on citizens' reportedly-justified use of lethal force. Implementation of *SYG* policies increases reportedly-justified homicides of black APOCs by 6–14 percent, resulting in an average of 2.75 additional black APOCs killed each month nationally ($p < .05 - p < .01$), 2.39 of whom are killed by black citizens. Reportedly-justified homicides of white APOCs increased by 0–3 percent, or 0.5 additional white APOCS killed each month nationally, but this is not a statistically significant result. These significant racial differences provide strong evidence of public policies that have imposed unequal costs, measured in terms of lives lost each month, across racial groups. These costs are 5.5 times larger for the black community than for the white community.

The monetary costs of these policies depend on the circumstances surrounding the reportedly-justified homicide; *e.g.*, whether the APOC was malicious or misunderstood. Knowledge of the mechanisms behind my results can help to establish reasonable assumptions about these circumstances. Therefore, the secondary contribution of this article is my list of potential mechanisms that can explain these differential effects across race-pairs. Assumptions based on these mechanisms may shape future discussions of *SYG* policies either as policies akin to capital punishment, or as policies that increases the homicide rate of misunderstood persons. Unfortunately, neither situation mitigates the fact that the cost of these policies is disproportionately levied against the black community.

America's criminal justice system is based strongly on *Blackstone's ratio*, which states that "it is better 100 guilty persons should escape, than that one innocent person should suffer" (Franklin, 1774). For this reason, if *SYG* policies are causing the deaths of APOCs who are victims of bias or misunderstanding, then these policies should face immediate and thorough reconsideration. Since knowledge of APOCs' intentions may be gained through

an understanding of the aforementioned mechanisms, additional research is necessary to determine which one, or combination thereof, is driving the racially disparate effects I observe through my analysis.

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Table I: Changes by State: Sources of Change

State	Source	Date
Alabama	2006 AL. SB 283	06/2006
Alaska	2005 AK. SB 200	09/2006
Arizona	2006 ARIZ. SB 1145	04/2006
Florida†	2005 FLA. SB 436	10/2005
Georgia	2005 GA. SB 396	07/2006
Indiana	2006 IND. HEA 1028	07/2006
Kansas	2005 KAN. SB 366	07/2006
Kentucky	2006 KY. SB 38	07/2006
Louisiana	2006 LA. HB 89	08/2006
Michigan	2005 MI. HB 5143	10/2006
Mississippi	2006 MISS. S.B. 2426	07/2006
Missouri	2007 MO. SBs 62 and 41	08/2007
Montana	2009 MT. HB 228	04/2009
North Carolina	2011 N.C. HB 650	05/2011
North Dakota	2007 N.D. HB 1319	02/2007
New Hampshire	2011 N.H. SB 88	11/2011
Nevada	2011 NEV. AB 321	05/2011
Ohio	2008 OH. SB 184	09/2008
Oklahoma	2005 OK. HB 2615	11/2006
Oregon††	State of Oregon v. Sandoval	03/2007
Pennsylvania	2011 PA. HB 40	06/2011
South Carolina	2005 S.C. HB 4301	06/2006
South Dakota	2006 S.D. HB 1134	07/2006
Tennessee	2007 TENN. HB 1907	05/2007
Texas	2007 TX. SB 378	09/2007
Utah†††	Utah Code 76-2-(402-404)	03/1994
West Virginia	2008 W.V. SB 145	02/2008
Wisconsin	2011 WISCONSIN ACT 94	12/2011

Notes: A list of states that enacted a *Stand Your Ground* policy.
†Excluded from sample for reasons discussed in Section 2.
††Oregon's law did not change, but the 2007 Supreme Court case *State of Oregon v. Sandoval* ruled that the existing law effectively does not require a victim to retreat before using deadly force, thereby causing a change in prosecutorial behavior in the same manner as new legislation.
†††Because the law changed *prior* to observed sample period, Utah is included in the control group. This allows the results to be interpreted as the effects of a change in *SYG* policy.

Source: Original legislation and court documents as listed in this table.

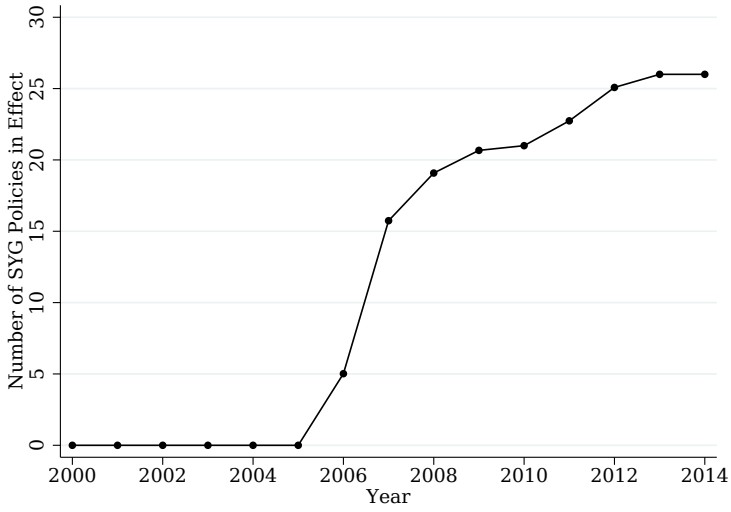


Figure I: Legislation Changes Over Time

Note: Graphical depiction of the number of states changing *Stand Your Ground* policies over the observed time period, based on legislation changes and court rulings. Observed period: 2000-2014.

Source: See original legislation and court documents listed in Table I.

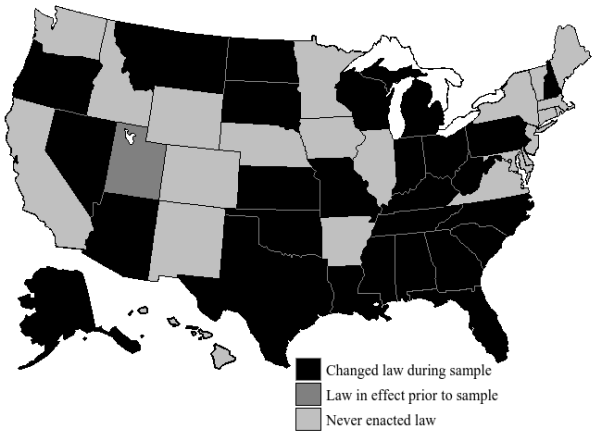


Figure II: Legislation Changes by State

Note: Graphical depiction of states that enacted *Stand Your Ground* policies over the observed time period. States without *SYG* policy changes and states enacting policies prior to the observed time period are selected into the control group. States enacting new *SYG* policies during observed period are selected into treatment group. Observed period: 2000-2014.

Source: See original legislation and court documents listed in Table I.

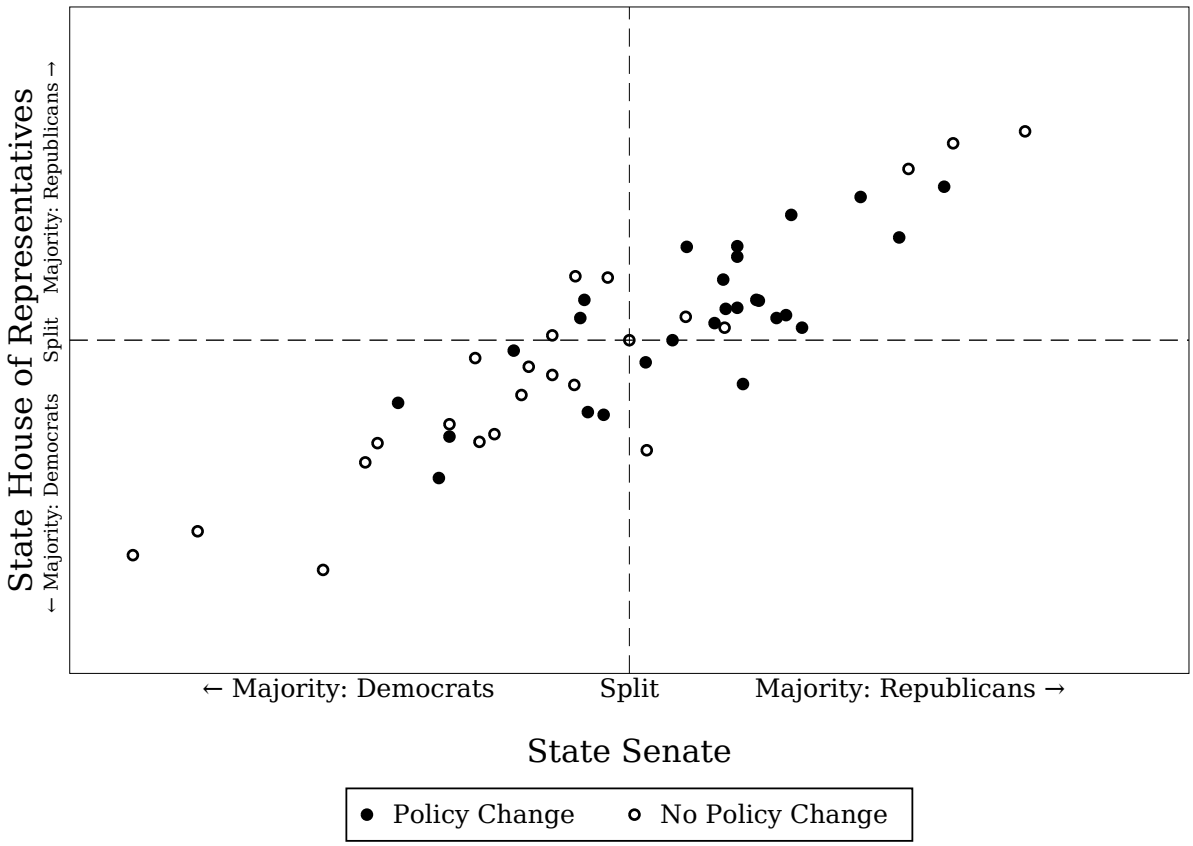


Figure III: Political Composition of Legislative Bodies
at Time of Policy Change

Note: Political composition of the State House and the State Senate for each state. Composition reported at time of policy change for states that enact *Stand Your Ground* policies. Composition reported at midpoint of treatment period (year 2010) for states that did not enact *Stand Your Ground* policies. Axes extend from the center lines (Split), which indicate an even split of political affiliation, out to 100% political composition by either party.

Source: POLIDATA Demographic & Political Guides, Party Control Tables 2004-2012.

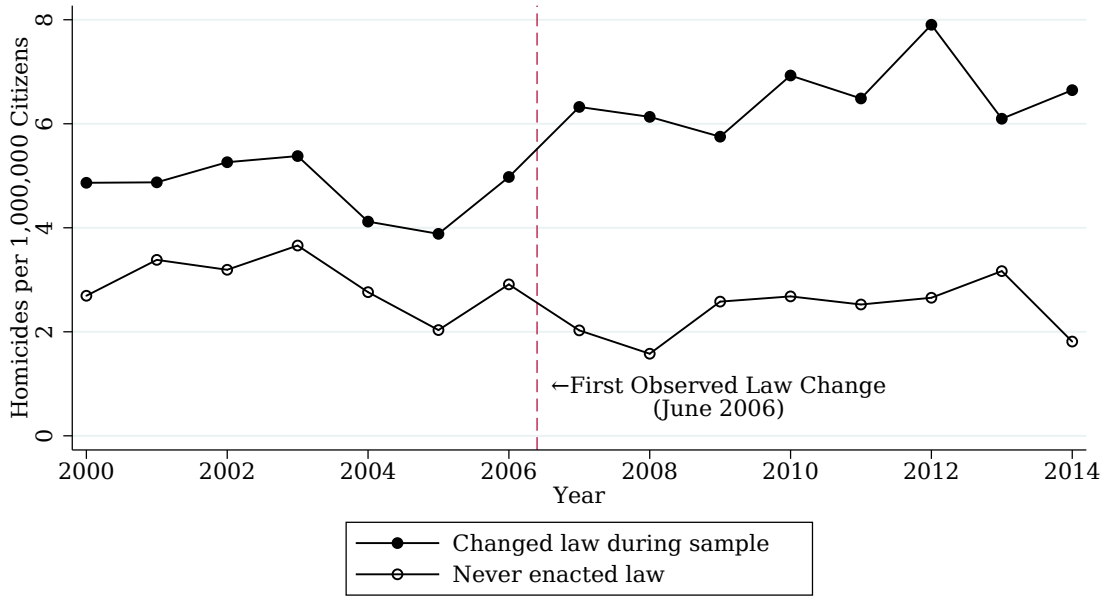
Table II: Summary Statistics: Average Reportedly-Justified Homicides
per 1,000,000 Citizens of APOC Race, per Month

	Panel A: Combined Jurisdictions		*Panel B: Urban Jurisdictions*		*Panel C: Rural Jurisdictions*	
Black APOCs Shot	...by Black Citizens	0.41	...by Black Citizens	0.39	...by Black Citizens	0.60
	...by White Citizens	0.16	...by White Citizens	0.16	...by White Citizens	0.23
White APOCs Shot	...by Black Citizens	0.01	...by Black Citizens	0.01	...by Black Citizens	0.01
	...by White Citizens	0.08	...by White Citizens	0.08	...by White Citizens	0.30

Notes: Summary of reportedly-justified homicide rates committed with a firearm. Monthly statistics are calculated per 1,000,000 citizens of the APOCs' race in a law enforcement agency's jurisdiction.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Panel A: Justified Shootings of Black APOCs



Panel B: Justified Shootings of White APOCs

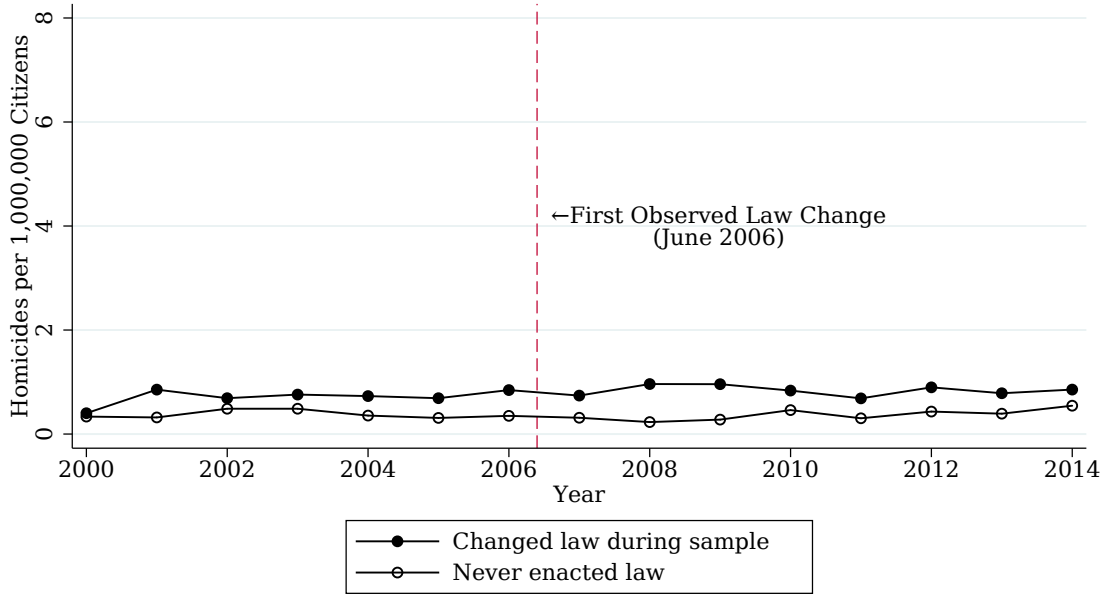


Figure IV: Homicide Trends: Justified Shootings of APOCs

Note: Homicide trends by type and category of homicide. All results are measured per 1,000,000 citizens in the reporting jurisdiction and calculated using race-specific population weights.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table III: Replication of Cheng and Hoekstra

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SYG Effect	0.0801**	0.0841**	0.0612*	0.0652*	0.0466	0.0479	0.1310***	0.1312***
	(0.0342)	(0.0347)	(0.0352)	(0.0344)	(0.0329)	(0.0333)	(0.0336)	(0.0487)
Cheng and Hoekstra's original result	✓							
Log Transformation	✓							
Replication of C&H's original result		✓						
IHS Transformation		✓	✓	✓	✓	✓	✓	✓
Use 2000-2014 data			✓	✓	✓	✓	✓	✓
OR. in treatment				✓	✓	✓	✓	✓
Drop FL.					✓	✓	✓	✓
Rate: Per 1 Million						✓	✓	✓
Use monthly data							✓	✓
Outcome: Firearm Homicide								✓
Fixed Effects:								
Year	✓	✓	✓	✓	✓	✓		
Year×Month							✓	✓
State	✓	✓	✓	✓	✓	✓	✓	✓
Observations	550	550	846	846	829	829	8,149	8,149

Notes: A replication attempt of Cheng and Hoekstra's (2013) primary result using Supplementary Homicide Report raw data files. Column 1 lists Cheng and Hoekstra's original result and column 2 reports the result of my replication – I successfully replicate their result. The minor difference is due to in part to (a) the difference between the raw data files and the modified summary totals, (b) the method of transformation, and (c) updates to old homicide cases that are recorded in the new data files. Results should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: (1) United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014. (2) Florida Department of Law Enforcement, Crime in Florida Report Abstract, 2000-2014

Table IV: Effect of Legislation on Citizens Justifiably Shooting APOCs

	Black APOCs Shot by Black Citizen	Black APOCs Shot by White Citizen	White APOCs Shot by Black Citizen	White APOCs Shot by White Citizen
SYG Effect†	0.1434***	0.0556**	0.0052	0.0226
	(0.0473)	(0.0239)	(0.0041)	(0.0189)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	8,149	8,149	8,149	8,149
†*Additional lives lost each month due to policy change*	*2.39*	*0.361*	*0.014*	*0.490*

Table IVb: Differences in Point Estimates, by Race
$\chi^2(1)$ *Test Statistics:*

race-pairs	Black Shot by Black	Black Shot by White	White Shot by Black
Black Shot by White	12.76***		
White Shot by Black	54.99***	32.95***	
White Shot by White	36.95***	17.38***	4.55***

Notes: Results from difference-in-differences analysis of Justified Firearm-Related Homicides using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Each combination of equations is jointly estimated and the coefficients are tested for equality. A statistically significant result indicates the null hypothesis of equality is rejected, and the increased use of lethal force measured by the equations is statistically different. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table V: Effect of Legislation on Citizens Justifiably Shooting APOCs,
Urban and Rural

Panel A: Urban Jurisdictions

	Black APOCs Shot by Black Citizen	Black APOCs Shot by White Citizen	White APOCs Shot by Black Citizen	White APOCs Shot by White Citizen
SYG Effect	0.1530***	0.0583**	0.0056	0.0233
	(0.0486)	(0.0248)	(0.0043)	(0.0170)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	7,735	7,735	7,735	7,735

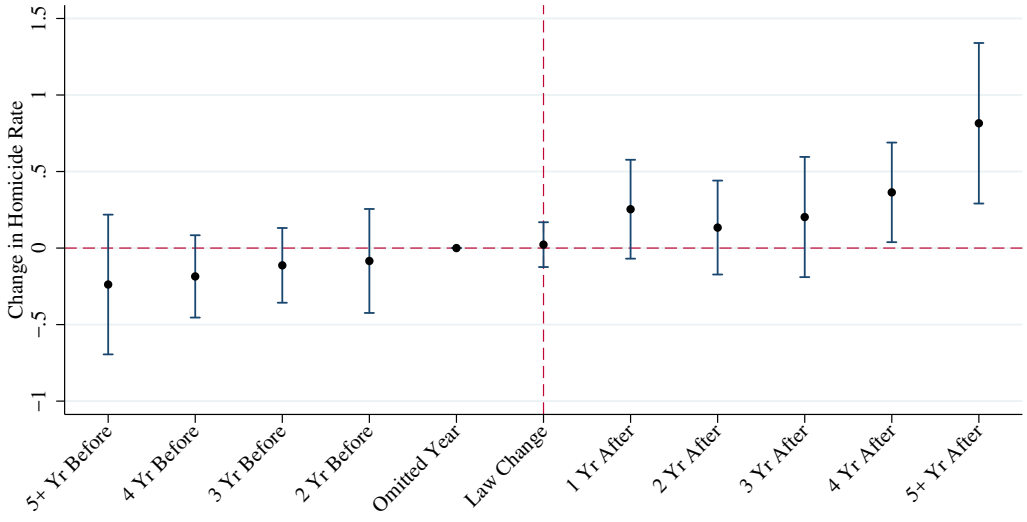
Panel B: Rural Jurisdictions

	Black APOCs Shot by Black Citizen	Black APOCs Shot by White Citizen	White APOCs Shot by Black Citizen	White APOCs Shot by White Citizen
SYG Effect	0.1347	0.0049	0.0037	0.0129
	(0.0835)	(0.0227)	(0.0045)	(0.0274)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	5,685	5,685	5,685	5,685

Notes: Results from difference-in-differences analysis of Urban (Panel A) and Rural (Panel B) Justified Firearm-Related Homicides using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Panel A: Black APOCs Shot by Black Citizen



Panel B: Black APOCs Shot by White Citizens

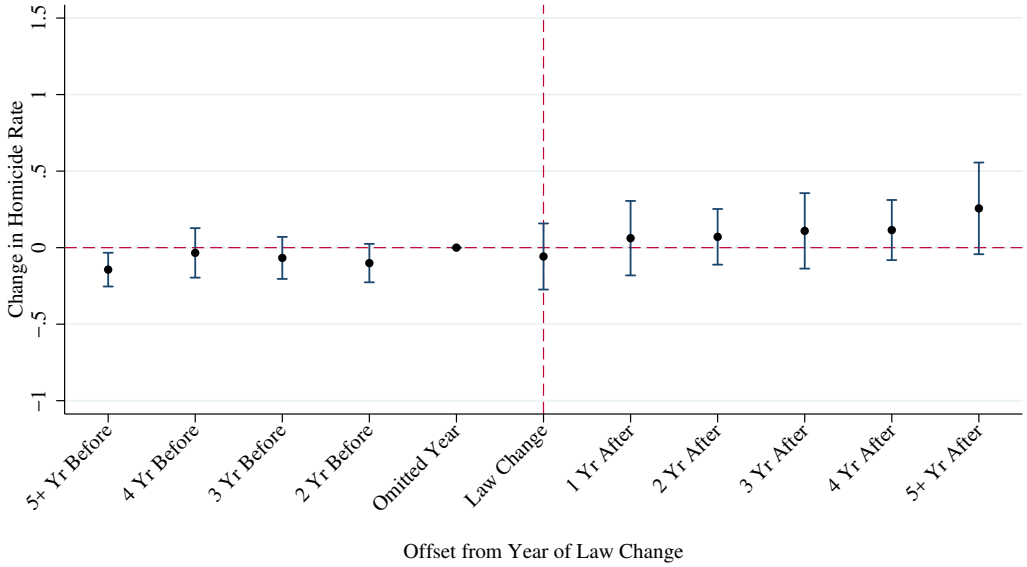
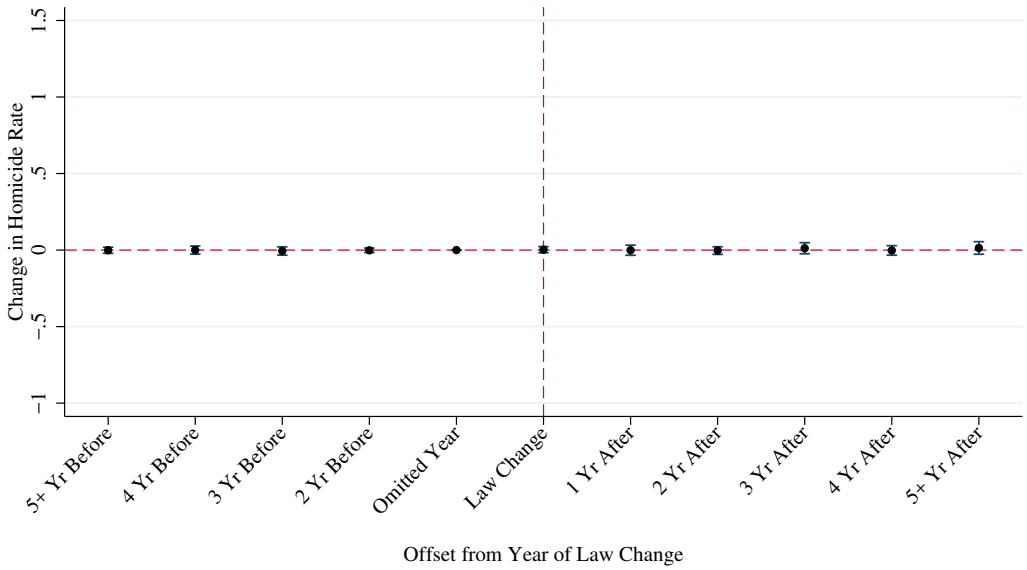


Figure V: Change in Homicide Rate Over Time

Note: Results from event study analysis of Justified Firearm-Related Homicides by Black Citizens (Panel A) and White Citizens (Panel B) using State and Year fixed effects. Coefficients of annual indicator variables and their 95% confidence intervals illustrating the percent change in homicides for states enacting *Stand Your Ground* policies during observed time period. Confidence intervals utilize robust standard errors clustered at the state level. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by exposure to treatment over time. Effects are normalized to zero in the year prior to treatment.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014

Panel A: White APOCs Shot by Black Citizens



Panel B: White APOCs Shot by White Citizens

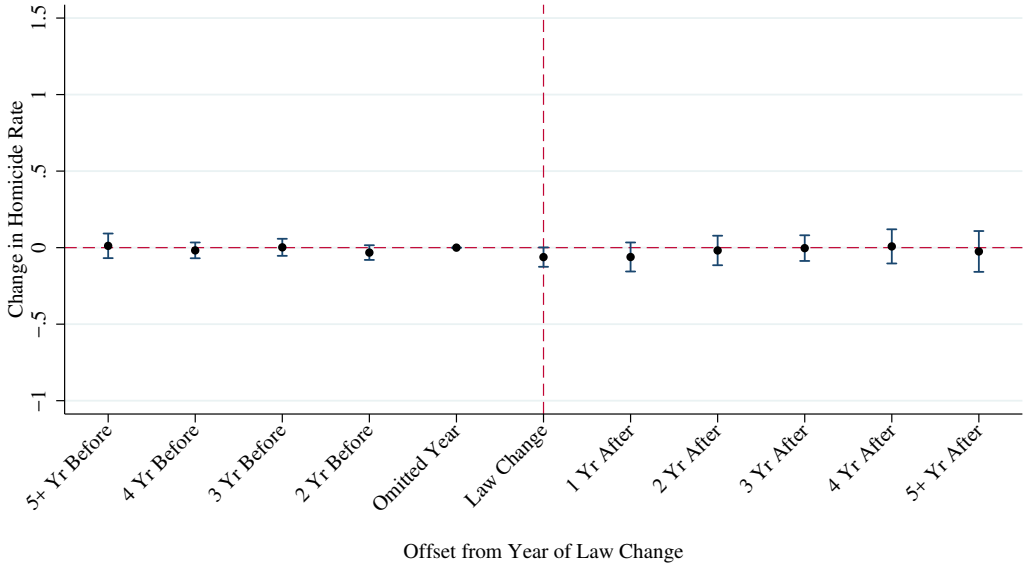


Figure VI: Change in Homicide Rate Over Time

Note: Results from event study analysis of Justified Firearm-Related Homicides by Black Citizens (Panel A) and White Citizens (Panel B) using State and Year fixed effects. Coefficients of annual indicator variables and their 95% confidence intervals illustrating the percent change in homicides for states enacting *Stand Your Ground* policies during observed time period. Confidence intervals utilize robust standard errors clustered at the state level. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by exposure to treatment over time. Effects are normalized to zero in the year prior to treatment.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014

Table VI: Effect of Legislation on Reporting Behaviors

Table VIa: Number of Homicides Reported

	(1)	(2)
SYG Effect	-0.0096	0.0234
	(0.0194)	(0.0205)
Fixed Effects:		
Year×Month		✓
State		✓
Observations	9,996	9,996

Table VIb: Number of Homicides Classified as "Unfounded"

	(1)	(2)
SYG Effect	0.0020	0.0029
	(0.0022)	(0.0022)
Fixed Effects:		
Year×Month		✓
State		✓
Observations	9,996	9,996

Table VIc: Number of Homicides Classified as "Actual"

	(1)	(2)
SYG Effect	-0.0116	0.0209
	(0.0202)	(0.0209)
Fixed Effects:		
Year×Month		✓
State		✓
Observations	9,996	9,996

Table VId: Police Agency's Clearance Rate

	(1)	(2)
SYG Effect	-0.0000	-0.0000
	(0.0000)	(0.0000)
Fixed Effects:		
Year×Month		✓
State		✓
Observations	9,996	9,996

Notes: Results from difference-in-differences analysis of Offenses Known and Clearances by Arrest records using population weights. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the reporting or classification of homicides caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest, 2000-2014.

Table VII: Placebo Test,
Effect of SYG Legislation on Various Rates

	Placebo Homicide		Traffic Fatality		Unemployment	
	Black	White	Black	White	Black	White
SYG Effect	-0.0192	-0.0479	-0.1186	-0.2704	-0.0679	-0.0364
	(0.0798)	(0.0975)	(0.2024)	(0.2243)	(0.1185)	(0.0555)
Fixed Effects:						
Year×Month	✓	✓	✓	✓	✓	✓
State	✓	✓	✓	✓	✓	✓
Observations	1,914	1,914	8,817	8,817	8,776	8,820

Notes: Results from difference-in-differences analysis of placebo outcomes. It can be seen that no specification is significant at any level, suggesting that the mechanism causing the change in behavior is correct. Tests use population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: (A) United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014 (B) United States Department of Transportation, National Highway Traffic Safety Administration, Traffic Fatality Data, 2000-2014 (C) Katrina Stierholz, Federal Reserve Bank of St. Louis, State Level Unemployment Rate, 2000-2014.

Table VIII: Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)
Panel A: Black APOCs Shot by Black Citizen					
SYG Effect	0.1738***	0.1450***	0.1434***	0.1541***	0.1351**
	(0.0422)	(0.0493)	(0.0473)	(0.0432)	(0.0556)
Panel B: Black APOCs Shot by White Citizen					
SYG Effect	0.0556**	0.0560**	0.0556**	0.0564**	0.0563**
	(0.0239)	(0.0243)	(0.0239)	(0.0276)	(0.0272)
Panel C: White APOCs Shot by Black Citizen					
SYG Effect	0.0052	0.0051	0.0052	0.0063	0.0096*
	(0.0041)	(0.0041)	(0.0041)	(0.0044)	(0.0051)
Panel D: White APOCs Shot by White Citizen					
SYG Effect	0.0226	0.0229	0.0226	0.0283*	0.0408**
	(0.0189)	(0.0192)	(0.0189)	(0.0148)	(0.0177)
Fixed Effects:					
Year×Month		✓	✓	✓	✓
State		✓	✓	✓	✓
State×Month		✓			
Region			✓		
Years Dropped:				['98-'00) ('12-'14]	['98-'02) ('10-'14]
Observations	8,149	8,149	8,149	7,069	4,923

Notes: Sensitivity analysis for difference-in-differences results. Models use population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014

Table IX: Effect of Legislation on Justified Shootings

Table IXa: Justified Shootings of APOCs Committed by Law Enforcement

	Combined	Urban	Rural
SYG Effect	0.0563*	0.0563*	0.0250
	(0.0324)	(0.0322)	(0.0291)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Table IXb: Justified Shootings of APOCs Committed by Citizens

	Combined	Urban	Rural
SYG Effect	0.0786***	0.0833***	0.0501*
	(0.0248)	(0.0242)	(0.0288)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Notes: Results from difference-in-differences analysis of Law Enforcement and Citizen IHS[Justified Firearm-Related Homicides] using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table X: Effect of Legislation on Citizens Justifiably Shooting APOCs

Table Xa: Citizens Justifiably Shooting Black APOCs

	Combined	Urban	Rural
SYG Effect	0.1809***	0.1908***	0.1643
	(0.0580)	(0.0602)	(0.1037)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Table Xb: Citizens Justifiably Shooting White APOCs

	Combined	Urban	Rural
SYG Effect	0.0298	0.0307	0.0188
	(0.0201)	(0.0184)	(0.0272)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Notes: Results from difference-in-differences analysis of Law Enforcement and Citizen IHS[Justified Firearm-Related Homicides] using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < .10$, ** $p < .05$, *** $p < .01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

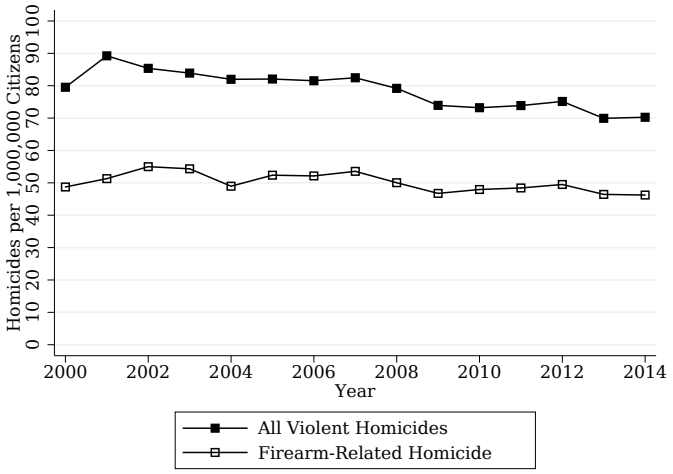


Figure VII: Homicide Trends: All Homicides and Shootings

Note: Homicide trends by type and category of homicide. All results are measured per 1,000,000 citizens in the reporting jurisdiction.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

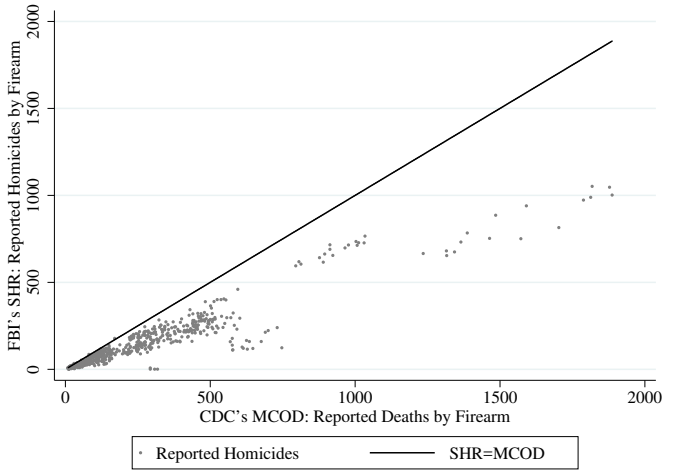


Figure VIII: Comparison of Supplementary Homicide Report to Multiple Cause of Death Data

Note: Comparison of reported firearm-related deaths in the FBI's Supplementary Homicide Report with an alternative data source, the CDC's Multiple Cause of Death report. Each point represents a state's reported annual homicides in the two data sets.

Source: (1) United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014. (2) Centers for Disease Control and Prevention, National Center for Health Statistics Multiple Cause of Death, 1999-2014.