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 ($p < .01$) 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 ($p < .01$), and I conduct a number of falsification and robustness tests address natural concerns of endogenous policy creation. 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

Affirmative self-defense policies, colloquially referred to as *Stand Your Ground* (*SYG*), are among the more controversial laws in the United States. These policies can mitigate a defendant's culpability in civil or criminal proceedings after a fatal shooting. While proponents insist that *SYG* protects innocent individuals from frivolous prosecution, opponents argue that it lowers the cost of using deadly force and results in increased homicide rates.[1] For example, one recent empirical study finds that *SYG* policies cause approximately 30 additional homicides each month (McClellan and Tekin, 2016; hereafter MT). Opponents also regularly argue that *SYG* policies induce important racial disparities; it is frequently claimed that *SYG* laws "make it easier to kill blacks."[2] These racial disparities are the concern of this paper.

In this paper, I empirically measure racial disparities that are directly attributable to *SYG*. To accomplish this, I examine how enacting *SYG* policies affects the cross-race and own-race killings of "Alleged Perpetrators of Crimes" (APCs).[3] I examine the following categories, hereafter referred to as "race-pairs": blacks killing black APCs, whites killing black APCs, blacks killing white APCs, and whites killing white APCs.[4] By examining this racial disparity, my research directly addresses the frequent media claims that *SYG* laws "make it easier to kill blacks" and the implications that the black community disproportionately pays the costs

[1] I do not further discuss the moral, ethical, and legal arguments for or against these policies 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).

[2] See news stories such as "States Are Quietly Resurrecting a Law That Makes It Easier to Kill Blacks" (accessed on 28 July 2017 from THEROOT.COM/STATES-ARE-QUIETLY-RESURRECTING-A-LAW-THAT-MAKES-IT-EAS-1794633188), "McKnight killing shows how Louisiana's 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 NPR.ORG/SECTIONS/CODESWITCH/2017/02/27/517109271/STAND-YOUR-GROUND-LAWS-COMPLICATE-MATTERS-FOR-BLACK-GUN-OWNERS).

[3] I create the term "Alleged Perpetrators of Crimes" to dispassionately describe the person(s) killed because the deceased cannot be posthumously convicted of a crime related to the incident during which they were killed. As these deceased individuals are unable to explain or defend their actions, I refrain from using terms that imply guilt.

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

of these policies. One example of these costs is the lost economic value of the human life (Conley, 1976; Droman, 2009), which includes the loss of human capital investment that was funded by the community through public schools and other social programs (Schultz, 1961; Glomm and Ravikumar, 1992) and the forfeited lifetime earnings or economic contributions to the community (Rice and Cooper, 1967).

This paper makes several contributions to the existing base of literature. Most importantly, this paper provides the first causal exploration of *SYG*'s effects on "reportedly justified homicides"[5] between race-pairs.[6] I combine a rich set of policy variables with individual-level covariates to create an identification strategy that permits causal interpretation under plausible identifying assumptions. This approach is novel because of my use of justified fatal shooting records to identify the race of both the shooter and the APC killed. The most closely related antecedents, Cheng and Hoekstra (2012; hereafter CH) and MT, isolate the causal effects of *SYG* on total homicides,[7] but do not fully explore the racial disparities due to data limitations that prevent identification of the shooter's race. Other papers, including a well publicized Urban Institute report, identify correlative evidence of racial disparities without exploring causal differences in general or by race.

I construct and analyze a panel dataset by combining detailed *SYG* policy implementation data with police records obtained through the FBI's Supplementary Homicide Report. These data allow me to identify the race of the perpetrator and the race of the victim for all reportedly justified homicides where a private citizen uses a firearm.[8] I then employ a generalized difference-in-differences analysis to calculate the change in the reportedly justified use of lethal force after the implementation of *SYG* policies. The key identifying assumption is that, in the absence of *SYG*, the average change in homicide rates would have been similar

[5] I use the phrase "reportedly justified homicides" rather than the phrase "self-defense homicides" because the former has a more appropriate connotation, for reasons discussed in Footnote 3.

[6] I also explore racial disparities in urban and rural settings to determine if the observed effects are specific to metropolitan regions. These results are presented in the appendix.

[7] I replicate multiple CH and MT results, supporting their conclusions.

[8] 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.

for states that have and have not enacted the policy. Although this assumption is not directly testable, I conduct a number of falsification and robustness tests to support the assumption and address the natural concerns of endogenous policy creation.

The first key finding of this paper is that *SYG* policies significantly increase the number of black APCs killed each month. The second key finding of this paper is that the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the individual purported to shoot in self-defense. The empirical magnitudes are large, both in levels and proportionately. For example, fatal shootings of black APCs increase by 6–14 percent ($p < .05 - p < .01$) while fatal shootings of white APCs increase by only 0–3 percent (not statistically significant). In terms of human lives lost, an average of 2.75 additional black APCs and 0.5 additional white APCs are killed each month nationally. These effects are larger within race than across race ($p < .01$); 2.39 of the 2.75 black APCs killed are killed by black citizens, and 0.49 of the 0.5 white APCs killed are killed by white citizens.

2 Background

2.1 *Stand Your Ground* Institutions and Related Literature

United States law commonly extends strong protections to individuals who defend their person or family while inside their homes. However, individuals 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).

Self defense policy began substantially changing between the years 2005 and 2014, during which twenty-seven states enacted an explicit set of rules enhancing an individual's right to defend their person and their family while outside their home. The first major change during this period began with Florida's Senate Bill 436, passed in October of 2005. 18 more states passed similar policies in 2006 and 2007, and eight more states followed suit over the

following seven years. A graphical depiction of policy changes over time can be found in Figure I.

Figure II illustrates the geographic location of *SYG* states and shows that many south-eastern states have enacted these policies; states that have frequently shared similar political views since the adoption of the "Southern Strategy" by Republicans (Boyd, 1970). However, it can also be seen in Figure III that both bipartisan support and bipartisan opposition exists for *SYG* policies. Figure III plots the political affiliation of *SYG* states' legislative bodies at the time they enact the policies alongside the political affiliation of non-*SYG* states in the year 2010, which is the midpoint of the treatment period. 10 of the 27 states that enacted *SYG* policies during the observed treatment period had either one or both chambers of their state legislature controlled by Democrats. In five other states, no policies were enacted during the observed time period despite both chambers of state legislature being controlled by Republicans.

The *SYG* policies enacted by these states effectively remove the individual's duty to retreat (Boots, Bihari, and Elliott, 2009) and allows them to use deadly force even if they are able to safely retreat and deescalate the situation. In the first systematic analysis of these policies, CH examine police records and find that *SYG* causes an 8 percent increase in reported murders and non-negligent homicides annually. MT extend the analysis by using monthly mortality data, rather than annual police records, and by modifying their definition of *SYG*. MT finds that *SYG* causes 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 and without considering race-pair interactions.

In practice, *SYG* removes the duty to retreat by providing the individual with an "affirmative defense."[9] This affirmative defense requires the government to presume that the citizen reasonably believed that deadly force was necessary and also to presume that the APC had the intention of using violence against the individual. These two presumptions,

[9] An affirmative defense is a legal tool that mitigates a defendant's culpability in civil or criminal proceedings. See LAW.CORNELL.EDU/WEX/AFFIRMATIVE_DEFENSE, accessed 13 June 2017.

taken together, permit the citizen to claim self-defense and grant the individual 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.

By creating this affirmative defense, *SYG* policies "reduce the expected cost of using lethal force" (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 APC. This paper examines how the change in expected punishment induces disparate effects in the cross-race and own-race killings APCs, and mechanisms that can explain these disparities are discussed in the next section.

2.2 Potential Mechanisms for Racial Disparities

Several mechanisms could drive the results found in this analysis. One such explanation could be a simple selection issue; if a percentage of all human interactions degrade into reportedly justified homicides and if this percent varies between race-pairs, then a reduction in a homicide's expected cost would generate the racial disparities observed in this paper. If this is the case, then it could be said that *SYG* itself induces the racially disparate increase in killings. Other plausible mechanisms include, but are not limited to, implicit biases held by the individuals who commit the reportedly justified homicides, public perceptions of law enforcement and prosecutorial behaviors, or the availability of law enforcement officers in a given geographic location.

Implicit biases, or evaluations that are "activated outside of conscious attention" (Bargh, Chaiken, Govender, and Pratto, 1992), can cause racial minorities to appear "more aggressive, even when exhibiting the same behaviors as Caucasians" (American Bar Association, 2015). These biases can also be activated by an object or person and then mis-attributed to a different object (Greenwald and Banaji, 1995); for example, observing a black individual and nonconsciously evaluating a harmless object to be dangerous. Studies show that implicit bias is 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 bias may influence an individual's threat perception during a confrontation with an APC.

An unpublished and non-random study conducted by Project Implicit has identified the existence of individuals who hold an implicit bias associating black persons with dangerous weapons.[10] The existence of such individuals suggests it is plausible for *SYG* policies to increase the killings of minorities, who would be implicitly associate with deadly weapons. If implicit bias can cause a black APC to appear more aggressive than an otherwise identical white APC, then this mechanism could explain the first part of my findings: why the killings of blacks increase significantly more than the killings of whites ($p < .01$). If this bias is held by blacks as well as whites, then this mechanism could also explain the second part of my findings: incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the citizen purported to shoot in self-defense ($p < .01$).

Public perception of law enforcement and prosecutorial behaviors may also drive the results of this analysis. Racial disparities in prosecution and sentencing (Cole, 1999; Mustard, 2001) may influence an individual's belief about how a law will be enforced, causing racial minorities to fear unfair prosecution far more than their white counterparts prior to the passage of *SYG*. If this is the case, the affirmative defense provided by a newly enacted *SYG* policy could sharply change racial minorities' paradigms while affecting white individuals far less; explaining why I find changes in blacks killing black APCs to be far larger in magnitude than whites killing black APCs ($p < .01$). Similarly, if both white and black individuals widely suspect the criminal justice system of bias against racial minorities, then both races may perceive the expected cost of killing black APCs to be lower than the expected cost of killing white APCs; explaining why white APCs are being killed at a lower rate than black

[10]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.

APCs ($p < .01$) and aligning with the aforementioned media claims that *SYG* "makes it easier to kill blacks."

Availability of law enforcement services may also drive the results of this analysis, as the availability of these services have been shown to vary based on neighborhood characteristics and caller demographics. This suggests that black 911-callers and black neighborhoods receive slower police responses than their white counterparts (Lee, Lee, and Hoover, 2016). Anecdotal evidence also suggests that differences exist in how blacks perceive the availability of law enforcement personnel.[11] These differences in law enforcement availability, real or perceived, may incite blacks to assume more responsibility for their own protection. If this is the case, then a change in policy that decreases the expected cost of killing an APC could create larger incentives for blacks to use lethal force, relative to whites. This mechanism only partially explains my findings; it explains the increased use of lethal force by blacks, but fails to fully explain why black APCs are also being killed at a higher rate by whites.

3 Data

3.1 Data Sources

I conduct my analysis using panel data aggregated to the state and month level, which 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 month for each race-pair, including the demographics for both the shooter and the deceased. Second, the month that each *SYG* policy was enacted for each state is needed to distinguish the reportedly justified shootings occurring after the change.

[11] See news stories such as "In New Orleans, call 911 and wait for an hour" (accessed 1 June 2017 from economist.com/blogs/democracyinamerica/2015/12/police-response-times), "Is 911 'still a joke' for African-Americans?" (accessed 1 June 2017 from 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 aclu-il.org/newly-released-data-shows-city-continues-to-deny-equitable-police-services-to-south-and-west-side-neighborhoods).

I take homicide data from the FBI's monthly Uniform Crime Reporting (UCR) program's 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 distinctive for its ability to provide data on both the deceased and the shooter, whereas other data sets, such as the Center for Disease Control's Multiple Cause of Death (MCOD) report, only provide information about the deceased. As a result, the SHR permits a unique investigation into the interactions between racial groups, which is impossible to do with other data sets.

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 facilitates the interpretation of my results, which are presented as *SYG*'s cost to human life.

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.[12] The effective dates of each state's *SYG* policy, along with the

[12]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

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 in the Section 3.2.

3.2 Data Quality

Two data quality issues should be noticed. The first is the availability and quality of homicide records from the state of Florida. The second issue is the potential for incomplete reporting, or measurement error, of homicides nationally.

The first issue arises because the FBI, who compile and publish the SHR data used in this analysis, purposefully exclude Florida data from their report. CH, the *SYG* researchers who also use SHR data, directly contacted the Florida Department of Law Enforcement Office and obtained numbers to use in place of the excluded FBI data. I also obtained the Florida Department of Law Enforcement Office data, but I then contacted the FBI and inquired why Florida is excluded from their reports. I was told Florida does not follow the FBI's data quality guidelines for reporting.[13]

To determine if I should use this data, 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 converge towards zero, 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

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.

[13]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."

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.[14]

The second issue arises because the SHR's reporting requirement is not strictly enforced, implying that the data may not include all perpetrated homicides.[15] 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 verify this by examining reporting behaviors in Section 5.1. I find that reporting behaviors are not correlated with changes in policy, and I show that homicide reporting does not present a risk to my analysis or the analysis of CH.

3.3 Sample Selection and Summary Statistics

I construct an outcome variable which allows my results to be interpreted as *SYG*'s national cost to human life each month. To accomplish this, I first categorize each shooting by the race of the citizen and the APC, 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[16] of all reporting agencies and multiplying by 1,000,000. This construction of a homicide rate is different from previous authors, whose choices of outcome variables do not always yield easily interpretable results. I identify reportedly justified homicides committed after the enactment of *SYG* by using the policy dates listed in Table I. I report the mean and standard deviation of the reportedly justified homicide rates in Table II; full data statistics are listed in column 1, statistics for all observations recorded without

[14] 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."

[15] See Wiersema, Loftin, and McDowall (2000) for a thorough discussion.

[16] Following the example of MT, I use the population of the deceased's race

SYG policies are listed in column 2, and statistics for observations recorded while *SYG* is in effect are listed in column 3.

4 Econometric Methodology

This article empirically measures racial differences that are directly attributable to *SYG* by examining how enacting *SYG* policies affects the cross-race and own-race killings of APCs. To accomplish this, I use variation in state policy as a natural experiment and employ a generalized difference-in-differences model to analyze how these policies influence the reportedly justified homicide rates between race-pairs over time. The outcome variable used is the monthly number of reportedly justified homicides per 1,000,000 citizens in reporting jurisdictions, aggregated to the state level.

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 in their data by adding one to each state's observed homicide count, but I elect to use the Inverse Hyperbolic Sine (IHS) transformation.[17] The IHS 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 $sinh^{-1}(0) = ln(1) = 0$. Whereas a logarithmic transformation would require either

[17] 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).

dropping or altering the zero values, which may cause the model to overestimate causal relationships (Friedline, Masa and Chowa, 2015), the IHS transformation yields precise estimates in the presence of zero values.

I model the IHS of the justified homicide 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. On the right-hand side of the regression 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[18] measured over the sample period and use robust standard errors clustered at the state level to account for spatially correlated errors. With this framework, my estimation of δ is interpreted as the percent change in the monthly homicide rate caused by the implementation of SYG policies.

To demonstrate that my methods yield results similar to the work of published authors, I use my methods to construct the IHS of the total homicide rate and to replicate the primary result of CH: the "8 percent net increase" of total homicides highlighted in their abstract. I match CH's sample time period[19] and treatment classifications,[20] and then I replicate their

[18]Following the example of MT, I use the population of the deceased's race

[19]2000–2010

[20]Thier classification includes Florida in the sample and classifies Oregon as untreated. This required the use of the data that I obtained from the Florida Department of Law Enforcement Office.

estimate in Table III, columns 1 and 2. The similarity of our results provides strong evidence supporting the analysis and findings of CH, while also corroborating the conclusions of MT.[21]

The remainder of Table III presents regression results as I make individual changes to CH's model and data until it more closely resembles the data and model used in this analysis. For brevity, I refer to observations from states which have enacted *SYG* as "the treatment group," and all other observations as "the control group." In specification 3, I broaden my sample to include 2000-2014 data, which moves my estimate closer to 6 percent. In specifications 4 and 5, I modify the treatment group,[22] which moves my estimate closer to 4 percent. I change from measuring homicides per 100,000 to homicides per 1,000,000 in specification 6, which has a negligible effect. 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 to 13 percent. The increase in magnitude is expected, given the timing of *SYG* enactment: since enactment occurs mid-year in all but one instance, aggregating observations to annual levels can reduce the estimated treatment effect by include untreated observations into the treatment group or treated observations into the control group. 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.

4.1 Assumptions

Difference-in-differences models have been employed in several seminal articles, such as Ashenfelter (1978), Ashenfelter and Card (1985), and Card and Krueger (1993), and they

[21] 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, a result that cannot be interpreted in terms of percent-change, 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 A.3 and A.4, and find results similar in sign and magnitude.

[22] I include Oregon and exclude Florida from the treatment group, for the reasons discussed in Section 3.

rely on a key assumption for the estimates to be consistent and unbiased (Bertrand, Duflo, and Mullainathan 2004). The key identifying assumption is that, in the absence of *SYG*, the average change in homicide rates would have been similar for states that have and have not enacted the policy. This assumption permits non-*SYG* states to serve as counterfactuals for what would have happened in *SYG* states if the policy had not been enacted.

Although direct verification of this assumption is unattainable, inspection of the periods prior to passage of *SYG* policies can support the assumption's veracity. Figures IVa and IVb plot the number of justified homicides of black APCs (IVa) and white APCs (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. I repeat these graphs using the IHS transformation of justified homicides, and find identical results; these graphs are presented in the appendix. Given these findings, I consider states that did not change their policies to be good counterfactuals for the states that did.

The key assumption is further corroborated by an event study. I annually aggregate the data annually and then interact five lead and five lag period dummies with *SYG* indicators. I plot the results in Figure V, omitting the 12 months prior to *SYG* enactment so that all interactions are expressed relative to this period. For states that do not change their policies to be good counterfactuals for the states that did, this event study must not show a statistical difference between the two groups in the periods prior to *SYG* enactment; *e.g.*, insignificant results for periods -5 through 0 (Pischke, 2005; Angrist and Pischke, 2008). All results from all periods prior to *SYG* enactment are statistically insignificant. This evidence supports my decision to use these observations as counterfactuals in my analysis, and the significant results following *SYG* enactment in Panel A aligns with the results presented in the next section.

5 Primary Results

I now present the racially disparate results of my primary analysis.Table IVa presents my estimate of δ, the percent change in the monthly reportedly justified homicide rate caused by the enactment of SYG policies.[23] These results show that SYG policies significantly increase the number of black APCs killed each month. Given the timing of SYG enactment, as presented in Table I, these results should be interpreted as the average changes occurring 2–8 years after the policy change. Table IVb tests the joint significance of these estimates, and shows that the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the individual purported to shoot in self-defense. It should be noted that these racially disparate results cannot be attributed to different preexisting crime rates among race-pairs, as the difference-in-differences methodology controls for these baseline trends.

The first two columns of Table IVa show that SYG policies induce a 14.34 percent increase ($p < .01$) in the number of black APCs shot by blacks, and a 5.56 percent increase ($p < .05$) in the number of black APCs shot by whites. These findings superficially support the claims that SYG "makes it easier to kill blacks." However, these results neither support nor disprove the implied claims of institutional racism, as it can be seen that the majority of the APCs are killed during own-race interactions.

In contrast to the clearly defined effect for black APCs, the measured effects of SYG on white APCs is small and statistically insignificant. The final two columns in Table IVa indicate that the increase of white APCs shot by blacks is approximately 0.5 percent, the smallest point estimate of my primary results, but the number of white APCs shot by members of their own race increases by 2.26 percent.

To aid interpretation of my results, I convert[24] the percent changes into the monthly

[23] I repeat my analysis for urban and rural jurisdictions to determine if the observed effects are specific to metropolitan regions. Results for both urban and rural jurisdictions are similar in both sign and magnitude, and do not prove significant differences exist. These results are available in the appendix.

[24] 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 black citizens nationally

number of lives lost in the United states as a result of these policies. My analysis finds that 2.75 additional black APCs are shot and killed nationally each month as a result of *SYG* policy. Of these, 2.39 are killed by blacks and 0.361 are killed by whites. These results are significant at the one percent and five percent levels, respectively. For white APCs, this number is far smaller; my analysis finds that only 0.504 additional white APCs are shot and killed nationally each month. Of these, 0.014 are killed by blacks and 0.490 are killed by whites. Neither of these results are statistically significant.

I now test to confirm that these point estimates are significantly different, following 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. In this test, critical value for significance at the one percent level is 6.63. Table IVb presents the relevant χ^2 test statistics and indicates that each difference is significant at the one percent level. This test confirms a true disparity in how *SYG* policies influence the reportedly justified homicide rates within and between racial groups.

Despite identifying and measuring the racially disparate effects induced by these policies, I cannot fully calculate the welfare implications of *SYG*. As discussed in Section 1, critics of *SYG* claim that these policies impose substantial costs to human life; which could be monetarily calculated in terms of lost human capital or an APC's lost lifetime earnings. My analysis indicates that these costs are disproportionately paid by the black community. Conversely, proponents insist that the policies protect innocent individuals from frivolous prosecution, thereby creating a social benefit. A shortcoming of this analysis is that it can neither identify nor disprove any of these purported benefits, which prevents a complete calculation of the net welfare implications.

(12.48 percent) and 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 CENSUS.GOV/POPCLOCK).

5.1 Placebo, Sensitivity, and Robustness Checks

I now present a series of tests to provide evidence that supports the validity of my analysis. I conduct each test using the same model as described in Section 4. All outcome variables are calculated per 1,000,000 citizens, aggregated to the state and month level, and IHS transformed.

Reporting Behavior

I begin by confirming that total homicide reporting is not systematically correlated with changes in *SYG* policy, as discussed in Section 3. I use UCR program data[25] to examine the total number of homicides reported each month. 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 Va. I find no correlation and conclude that total homicide reporting is not systematically correlated with *SYG* policies.

An additional concern is that police agencies might use policy changes to manipulate their reports or create more favorable numbers. I therefore examine three additional measures of reporting behavior over time. The first is the number of homicides deemed to be unfounded, baseless, or fake during the course of the reported month.[26] The second is the "actual" homicides in a given month, or the difference between the reported and the unfounded cases in a specific month. The third is the "clearance rate," or the number of arrests[27] divided by the number of actual homicides for that month. Tables Vb, Vc and Vd present these results: I find *SYG* is not correlated with any of these reporting behaviors, suggesting widespread misreporting did not begin occurring as a result of *SYG* policies.

[25]Data taken from the UCR's Offenses Known and Clearances by Arrest dataset, rather than the UCR's Supplementary Homicide Report dataset.

[26]According to the reporting manual, reportedly justified homicides should be included in the unfounded category in the month they are determined to be justified.

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

Placebo and Falsification Tests

Next, I examine three events that should be exogenous to changes in *SYG* policy: homicides performed in manners that are unrelated to self-defense, traffic fatalities, and unemployment rates. 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 any of these events. I also separate each analysis by race, to determine if any racial trends are present.

I conduct the first placebo test by restricting my raw data to homicides performed in manners that are unrelated to self-defense,[28] rather than reportedly justified homicides. This test serves to check if an underlying homicide trend exists, which may be spuriously correlated with *SYG* policies. 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 conduct the second placebo test by examining the traffic fatality rates for both black and white individuals. I use publicly available traffic fatality data, obtained from the National Highway Traffic Safety Administration, and attempt to identify correlations between *SYG* policies and trends in national traffic fatalities. I include this test to check for an underlying trend in the general number of fatalities nationwide, which should not be correlated with *SYG*. I again find no significant results, which supports my analysis.

I conduct the last placebo test using Federal Reserve Economic Data. I examine unemployment rates for black and white citizens to determine if unemployment trends are coincidently correlated with *SYG* policies. I include this test because negative economic conditions around the time of the policy changes may cause an increase in criminal activity, which would introduce bias to my analysis. However, I find no evidence of a correlation for either race group, suggesting the results of my primary analysis are indeed driven by *SYG*

[28] Including poison, arson, explosion, or by causing a drug overdose.

rather than underlying economic conditions.

Sensitivity Analysis

I test the sensitivity of my results to my model's specification by including various fixed effects and modifying the sample time period. 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). I report these results in Figure VI, and I include my primary results for comparison. Test 1 removes all fixed effects from my model, Tests 2 adds State×Month fixed effects to the original model, and Test 3 replaces State fixed effects with Regional fixed effects. Tests 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 well within the confidence interval of the original estimates, indicating that my results are robust to different choices in specification and sample periods. This analysis also provides evidence that my sample time period was not selected to accentuate a preconceived set of results.

6 Conclusions

The widespread implementation of *SYG* has created a natural experiment, which I exploit to measure racial disparities attributable to the policies. My approach is novel because I use justified fatal shooting records to identify the race of both the shooter and the APC killed, which allows me to examine the interactions between race groups. By examining these racial disparities, my research directly addresses the frequent media claims that *SYG* laws "make it easier to kill blacks" and the implications that the black community disproportionately pays the costs of these policies. Although this analysis cannot fully calculate *SYG*'s net welfare benefits, I am able to intimate the severity and the disproportionate burden of the policy's cost to human life.

I use a generalized difference-in-differences analysis to measure the effect of *SYG* policies

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

I find that *SYG* policies significantly increase the number of black APCs killed each month, and that the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the individual purported to shoot in self-defense. Fatal shootings of black APCs increase by 6–14 percent ($p < .05 - p < .01$) while fatal shootings of white APCs increase by only 0–3 percent (not statistically significant). This translates to an average of 2.75 additional black APCs killed each month nationally ($p < .05 - p < .01$), 2.39 of whom are killed by black individuals, and an average of 0.5 additional white APCs killed each month nationally, 0.39 of whom are killed by white individuals.

In general, my findings support the claims of those critical of *SYG* by indicating that the policies unequally affect the black community. However, since the majority of all APCs are killed during own-race interactions, my findings can neither support nor disprove critics' claims of institutional racism. Regardless, these significant racial differences provide strong evidence that *SYG* has 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.

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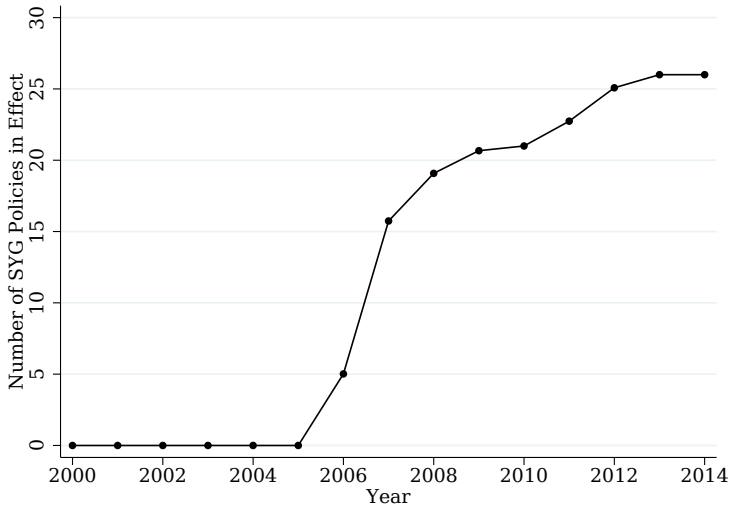


Figure I: Legislation Changes Over Time

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.

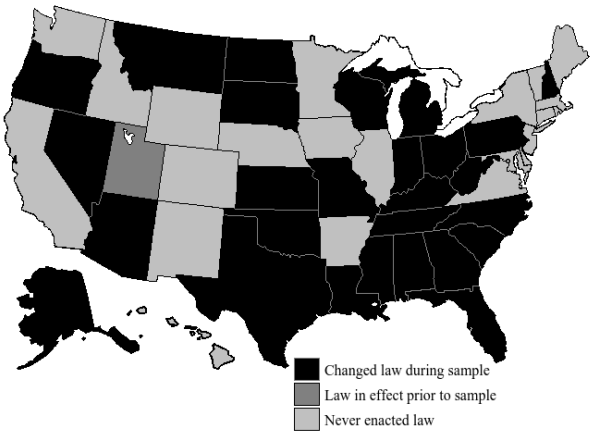


Figure II: Legislation Changes by State

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.

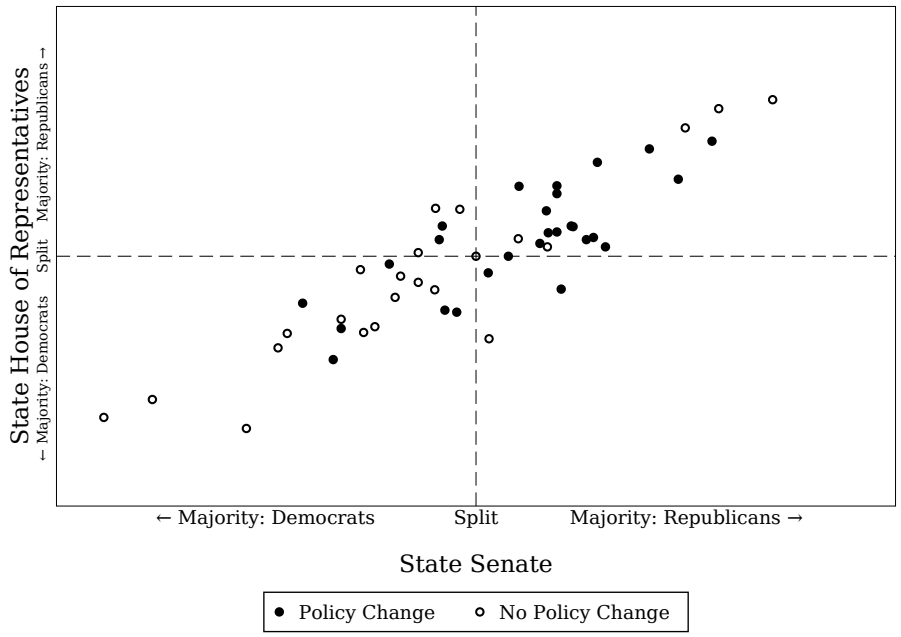


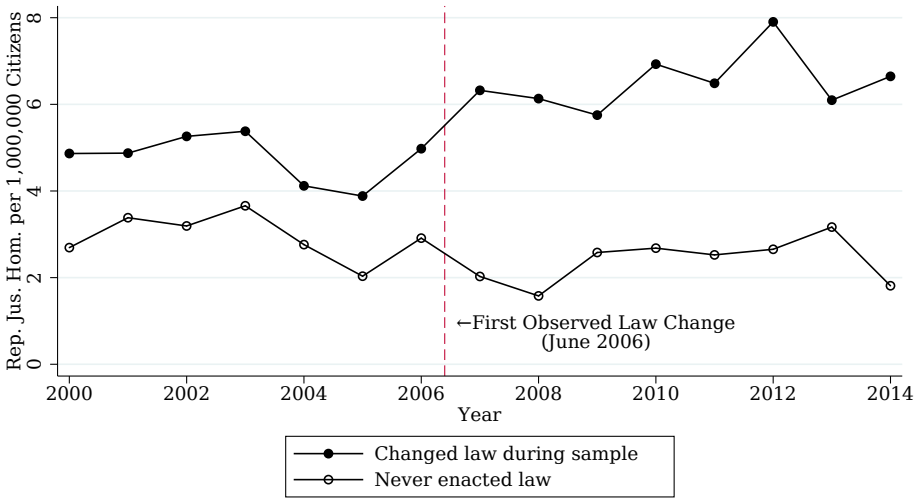
Figure III: Political Composition of Legislative Bodies
at Time of Policy Change

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.

10 of the 27 states that enacted *SYG* policies during the observed treatment period had either one or both chambers of their state legislature controlled by Democrats. In five other states, no policies were enacted during the observed time period despite both chambers of state legislature being controlled by Republicans. This suggests bipartisan support and bipartisan opposition exists for these laws.

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

Panel A: Reportedly Justified Shootings of Black APCs



Panel B: Reportedly Justified Shootings of White APCs

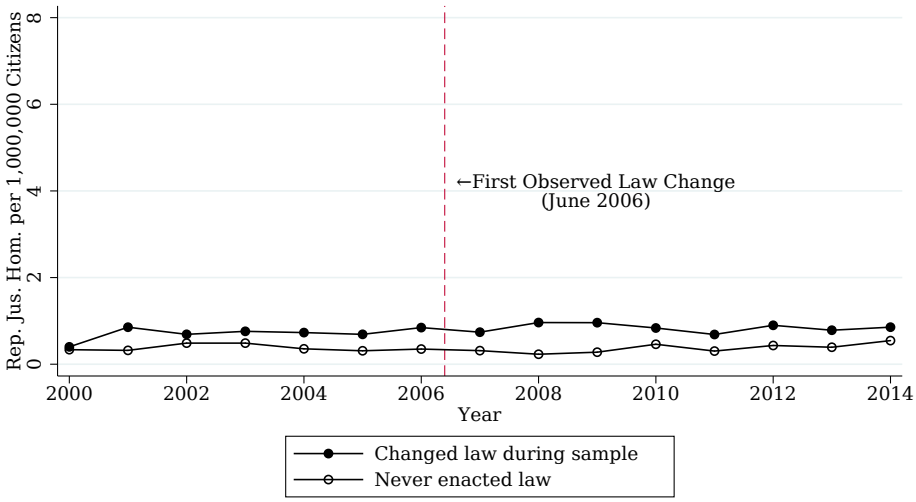


Figure IV: Homicide Trends: Reportedly Justified Shootings of APCs

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.

The impact of time is consistent for all citizens and the impact of residing in a state that eventually did, or did not, enact a *SYG* policy is consistent across time. Given these findings, I consider states that did not change their policies to be good counterfactuals for the states that did.

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

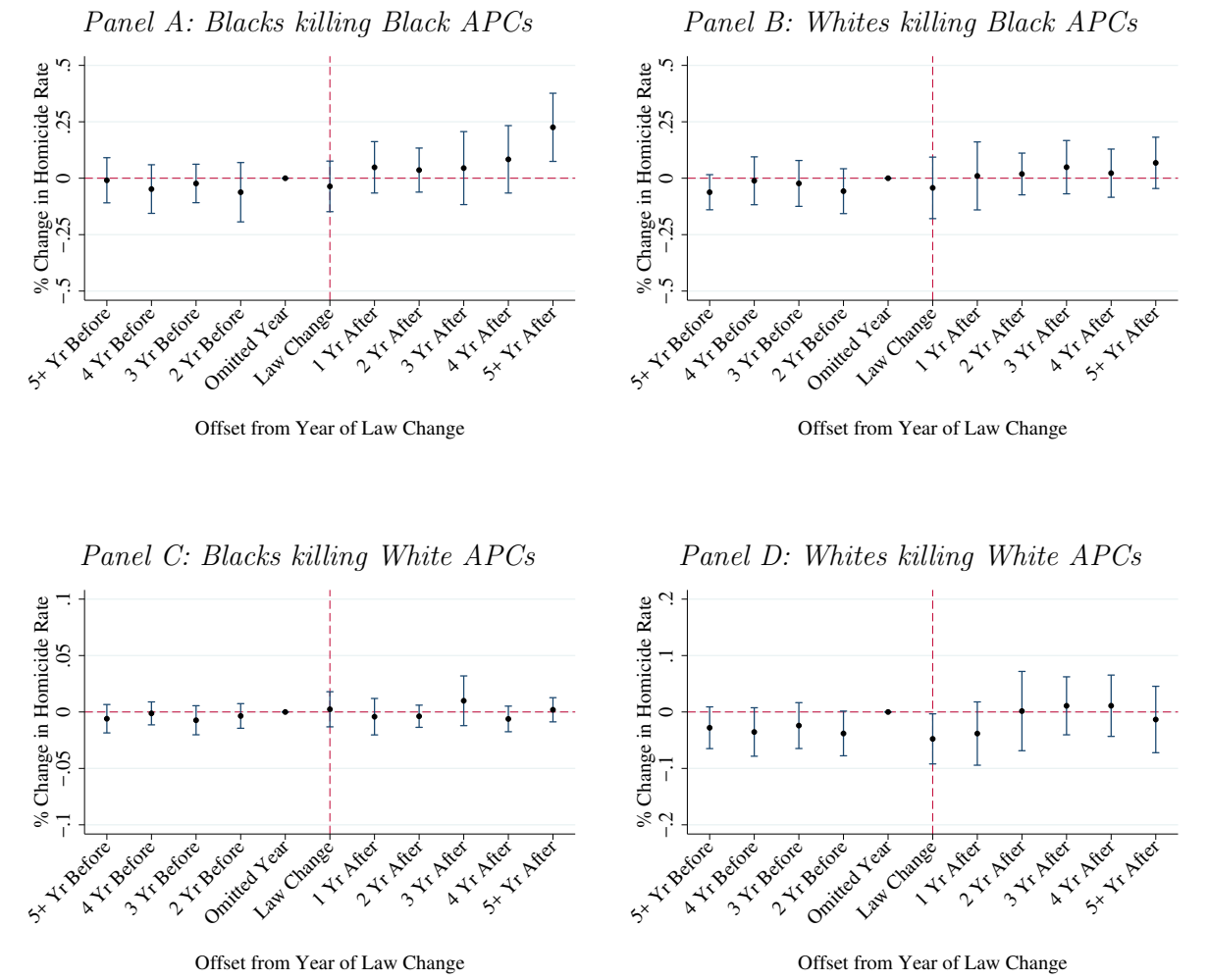
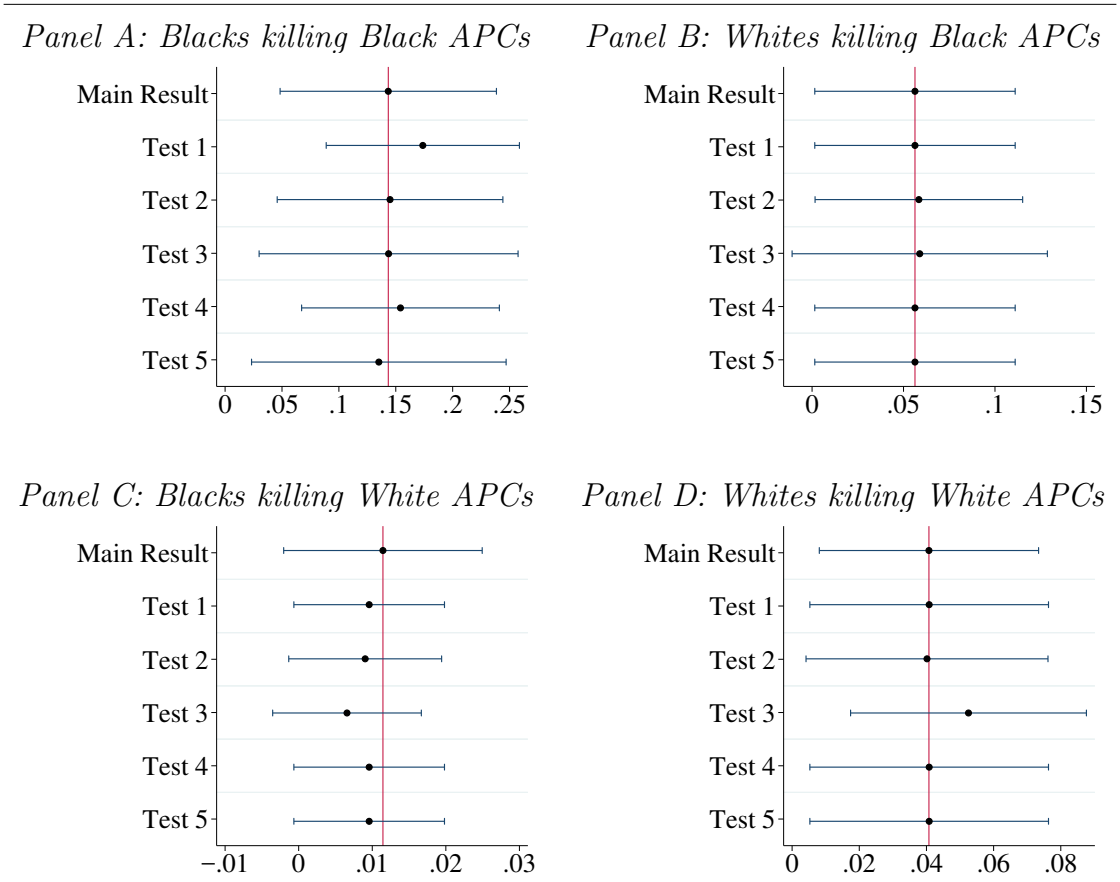


Figure V: Change in Homicide Rate Over Time

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.

Insignificant results for periods -5 through 0 is evidence that non-*SYG* states are good counterfactuals for the states that enact the policy. All results from all periods prior to *SYG* enactment are statistically insignificant, supporting my decision to use these observations as counterfactuals in my analysis.

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



	Original Estimate	Test 1	Test 2	Test 3	Test 4	Test 5
Fixed Effects:						
Year×Month	✓		✓	✓	✓	✓
State	✓		✓		✓	✓
State×Month			✓			
Region				✓		
Restrict Years:					2000–2012	2002–2010

Figure VI: Sensitivity Tests

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 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 3.

[††]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.

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

Mean
(Standard Deviation)

		All States, Full Sample	All States, Before *SYG*	*SYG*-States After *SYG*
Blacks killing...	...Black APCs	0.411	0.293	0.730
		(1.13)	(0.97)	(1.41)
	...White APCs	0.008	0.005	0.016
		(0.08)	(0.07)	(0.11)
Whites killing...	...Black APCs	0.162	0.132	0.241
		(0.73)	(0.64)	(0.92)
	...White APCs	0.084	0.066	0.147
		(0.29)	(0.25)	(0.38)

Notes: Summary of reportedly justified homicide rates committed with a firearm. Monthly statistics are calculated per 1,000,000 citizens of the APCs' 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.

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. 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: Primary Results

Table IVa: Effect of Legislation on Citizens Justifiably Shooting APCs

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
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	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs
Whites killing Black APCs	12.76***	-	-
Blacks killing White APCs	54.99***	32.95***	-
Whites killing White APCs	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 Reporting Behaviors

	Reported Homicides	Unfounded Homicides	Actual Homicides	Clearance Rate
SYG Effect	0.0234	0.0029	0.0209	-0.0000
	(0.0205)	(0.0022)	(0.0209)	(0.0000)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	9,996	9,996	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 VI: Placebo Tests

	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) US DOJ, FBI SHR, 2000-2014 (B) US DoT, NHTSA, Traffic Fatality Data, 2000-2014 (C) Katrina Stierholz, Federal Reserve Bank of St. Louis, State Level Unemployment Rate, 2000-2014.

Appendix A: Supplementary Graphs and Results

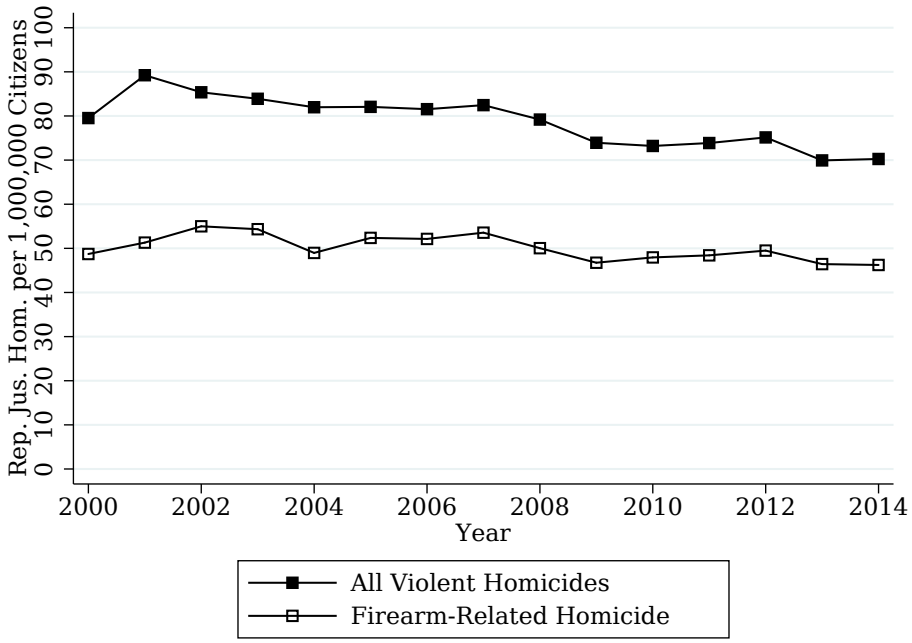


Figure A.1: Homicide Trends: All Homicides and Shootings

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.

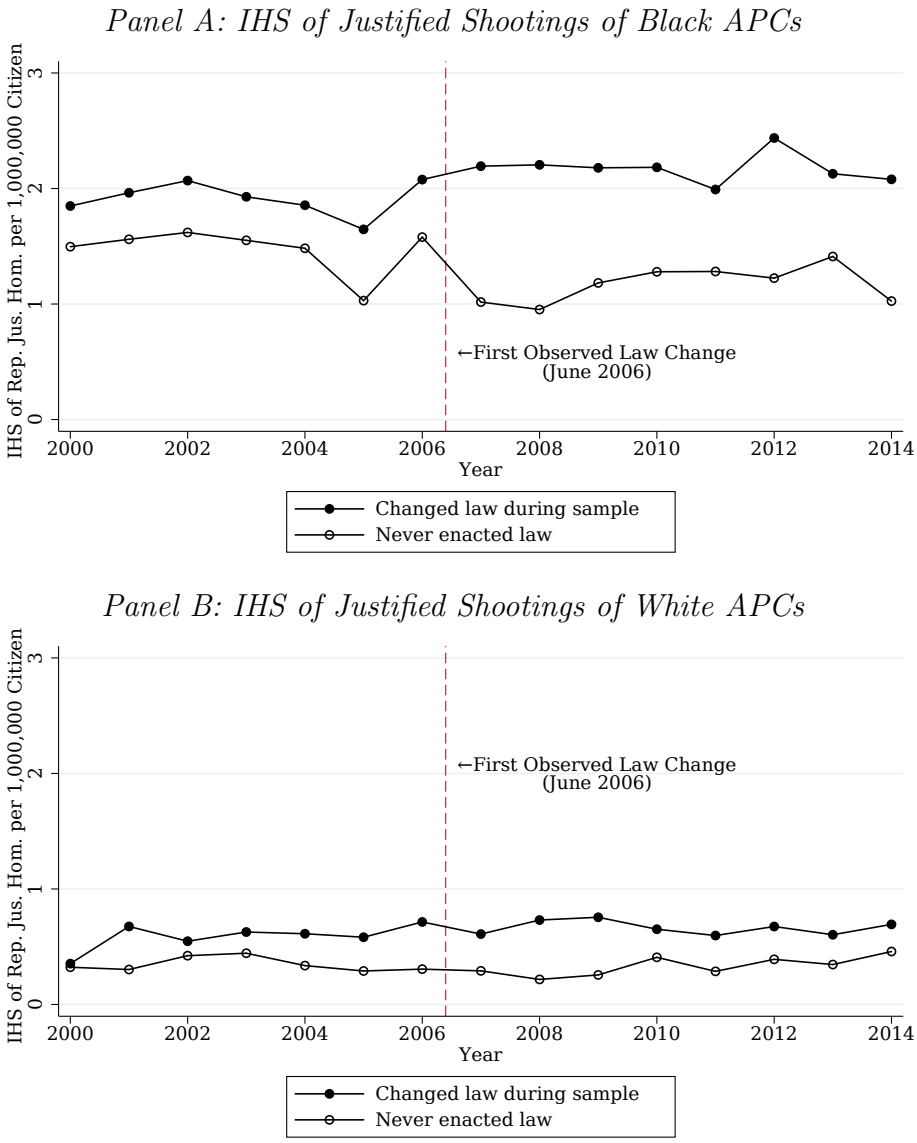


Figure A.2: Inverse Hyperbolic Sine Homicide Trends: Justified Shootings of APCs

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 A.1: Summary Statistics: Average reportedly justified Homicides per 1,000,000 Citizens of APC Race, per Month

	Panel A: Combined		*Panel B: Urban*		*Panel C: Rural*	
Blacks killing...	...Black APCs	0.41	...Black APCs	0.39	...Black APCs	0.60
	...White APCs	0.01	...White APCs	0.01	...White APCs	0.01
Whites killing...	...Black APCs	0.16	...Black APCs	0.16	...Black APCs	0.23
	...White APCs	0.08	...White APCs	0.08	...White APCs	0.30

Notes: Summary of reportedly justified homicide rates committed with a firearm. Monthly statistics are calculated per 1,000,000 citizens of the APCs' 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.

Table A.2: Effect of Legislation on Citizens Justifiably Shooting APCs,

Table A.2a: Urban Jurisdictions

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
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

Table A.2b: Rural Jurisdictions

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
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. (* $p < .10$, ** $p < .05$, *** $p < .01$)

I categorize urban and rural jurisdictions using data from the Center for Disease Control's National Center for Health Statistics (NCHS) and the Urban-Rural Classification Scheme for Counties from 1990, 2006 and 2013. I define "rural" 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.*

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

Table A.3: Effect of Legislation on Justified Shootings

Table A.3a: Justified Shootings of APCs 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 A.3b: Justified Shootings of APCs 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 A.4: Effect of Legislation on Citizens Justifiably Shooting APCs

Table A.4a: Citizens Justifiably Shooting Black APCs

	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 A.4b: Citizens Justifiably Shooting White APCs

	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.