Stand Your Ground Laws, Homicides, and Injuries

Chandler McClellan
Erdal Tekin

ABSTRACT

This paper examines the impact of Stand Your Ground laws on firearm homicides and injuries. Using state-level monthly data and a difference-in-difference identification strategy, we find that these laws result in an increase in homicides. According to our estimates, at least 30 individuals are killed each month as a result of Stand Your Ground laws. Furthermore, we document evidence to suggest that these laws also are associated with an increase in hospitalizations related to firearm-inflicted injuries. Taken together, the findings in this paper raise serious doubts against the argument that Stand Your Ground laws make the public safer.

I. Introduction

The public debate over gun rights and gun control policies in the United States reignites after every high profile shooting tragedy, which are occurring more frequently in recent years (Cohen, Azrael, and Miller 2014). One particular incident happened in February 2012, when Trayvon Martin, a 17-year-old teenager, was fatally shot by George Zimmerman, a community watch coordinator for a gated community in Florida in the United States.[1] The circumstances that triggered the shooting, the public

1. Based on preliminary physical evidence and the testimony provided by the teenager's shooter that he had acted in self-defense, the police department did not pursue criminal prosecution against Zimmerman, at least

Chandler McClellan is an adjunct professor at American University and a post-doctoral researcher at the National Bureau of Economic Research (NBER). Erdal Tekin is a professor of public policy in the School of Public Affairs at American University, a research fellow at the Institute for the Study of Labor (IZA), and a research associate at the NBER. *They would like to thank Chris Herbst, Mark Hoekstra, Dinand Webbink, and participants at the 4th Transatlantic Workshop on the Economics of Crime in the Netherlands, 2013 Society of Labor Economics Meetings, and 2013 European Society for Population Economics Meetings for helpful comments and suggestions. The data used in this article can be obtained beginning January 2018 through December 2020 from Chandler McClellan, School of Public Affairs, American University, 4400 Massachusetts Ave, NW, Washington, DC 20016-8070, mcclella@american.edu.*
[Submitted June 2013; accepted March 2016]; doi:10.3368/jhr.52.3.0613-5723R2
ISSN 0022-166X E-ISSN 1548-8004
Supplementary materials are freely available online at: http://uwpress.wisc.edu/journals/journals/jhr-supplementary.html

outcry caused by the initial decision not to charge Zimmerman, and the not-guilty verdict at his trial have prompted tremendous media coverage and public attention, and moved a wave of self-defense statutes introduced by states in recent years to the forefront of jurisprudence and policy debates. Between 2005 and 2010, 18 states passed legislation—also known as Stand Your Ground (SYG) laws—to allow their citizens to use force, including lethal force, in self-defense when there is reasonable belief of a threat, without having any duty to retreat first.[2]

Although the Martin–Zimmerman case has ended, the controversy surrounding SYG laws remains very much alive, and the public remains sharply divided between advocates of these laws and others who would like to see them repealed. Despite the tremendous publicity generated by the SYG laws and the significant ramifications that these laws may have on public health and safety, there has been only one rigorous evaluation of them. In a recent study, Cheng and Hoekstra (2013) conduct a thorough examination of the effect of expanded self-defense laws on homicides and violent crime using data from the FBI's Uniform Crime Reports (UCR). To account for the influence of confounding factors, the authors exploit the within-state variation in the adoption of these laws using a difference-in-differences identification strategy. They find that this new wave of self-defense laws does not deter burglary, robbery, or aggravated assault. On the contrary, the authors show strong evidence to indicate that these laws lead to a significant increase in the number of reported homicides and nonnegligent manslaughters.

With the exception of Cheng and Hoekstra (2013), most of what is currently known on the potential consequences of these laws are descriptive and come from media reports. For example, according to the *Tampa Bay Times*, "justifiable homicides" in Florida steadily increased from an annual average of 34 during the first half of the 2000s to 105 in 2009 (Montgomery and Jenkins 2010). In fact, this newspaper identified about 200 cases in which the SYG laws were invoked, and then investigated their outcomes through media reports, court records, and interviews with prosecutors and defense attorneys in Florida (Hundley, Taylor, and Humburg 2012). This investigation indicated that 70 percent of the cases where the SYG law was invoked in an attempt to avoid prosecution, individuals have gone free. The report also cites numerous examples that support the perception that these laws might have encouraged individuals to be aggressive even in situations where retreat was possible.[3] Furthermore, an independent review panel, which examined Florida's SYG law, cites documentation of the law's

initially, under the Florida's statute on justifiable use of force. Although he was eventually arrested and charged with second-degree murder and manslaughter, his trial ended in acquittal from both charges in July 2013.

2. According to Florida Statute 776.013, which took effect on October 1, 2005, "(3) A person who is not engaged in an unlawful activity and who is attacked in any other place where he or she has a right to be has no duty to retreat and has the right to stand his or her ground and meet force with force, including deadly force if he or she reasonably believes it is necessary to do so to prevent death or great bodily harm to himself or herself or another or to prevent the commission of a forcible felony." See http://www.leg.state.fl.us/statutes/index.cfm?App_mode=Display_Statute&Search_String=&URL=0700-0799/0776/Sections/0776.013.html.

3. For example, the report states that "During an argument at a 2009 party in Fort Myers, Omar Bonilla fired his gun into the ground and beat Demarro Battle, then went inside and gave the gun to a friend. If Battle feared for his life, he had time to flee. Instead, he got a gun from his car and returned to shoot Bonilla three times, including once in the back. Battle was not charged in the slaying."

application to excuse killings in neighborhoods, bar brawls, gang shootouts, and road-rage incidents (Florida Stand Your Ground Task Force 2012). Contrary to these concerns, there are also those who argue that the high profile examples highlighted in the media draw a misleading picture of the actual impact of these laws and that the laws largely have been successful in protecting citizens against wrongful attacks and intrusions. Until now, these laws have been the subject of intense debate that has been largely driven by anecdotes and subjective assessments. We believe there is a need for accumulating scientific evidence on their impact on public safety measures, regardless of whether that evidence corroborates the views of those in favor of these laws or those who oppose them.

In this paper, we provide a fresh analysis of the impact of these laws on the outcomes of firearm-related homicides and injuries. Whereas Cheng and Hoekstra (2013) is an important first step that illuminates the consequences of expanded self-defense laws on public safety, additional research with alternative data sources and using a similarly credible identification strategy is warranted in order to fully inform the ongoing controversy concerning these laws. If, for example, additional analysis produces evidence confirming the findings of Cheng and Hoekstra (2013), this also would help enhance the validity of their conclusions and serve as an important step toward building a consensus in the debate. Of course, if this analysis yields results that dispute the findings of Cheng and Hoekstra (2013), this would indicate that we are far from reaching a consensus and highlight the need for even further investigation.

Aside from representing a validation attempt, our analysis also differs from Cheng and Hoekstra (2013) in a number of ways that render the two analyses to be complementary to each other. First, our paper explicitly focuses on the impact of SYG laws that have extended the right to self-defense with no duty to retreat in places people have a legal right to be, while the main focus of Cheng and Hoekstra (2013) is the self-defense laws that removed the duty to retreat from somewhere outside the home. In other words, the coding of the expanded self-defense laws in Cheng and Hoekstra (2013) is slightly different from the one in this paper in the sense that their indicator captures states that removed duty to retreat from some places outside the home, including the person's own vehicle or place of employment, but not necessarily at other public places. According to our definition, both sets of states fall under the group that expanded self-defense laws, but with slightly different scopes.

Second, we use data on firearm-related homicides from the U.S. Vital Statistics in our main analysis, while Cheng and Hoekstra (2013) primarily rely on data from the UCR. Each of these two data sources have unique advantages. For example, the UCR data provide information on both homicides and crime, which allows researchers to study both outcomes. It is also possible with the UCR to break down homicides into subcategories of justifiable and nonjustifiable homicides, which is particularly important for understanding the implications of the SYG laws. On the other hand, the homicide data from the UCR are not considered as complete as those from Vital Statistics. (See Rokaw, Mercy, and Smith 1990; Wiersema, Loftin, and McDowall 2000; Harris et al. 2002; Loftin, McDowall, and Fetzer 2008.) [4] In addition to being a more accurate source

4. Federal law enforcement agencies in certain jurisdictions do not participate in the UCR program. Therefore, homicides occurring in places like prisons, national parks, and Indian reservations, which are not reported by state or local law enforcement agencies, do not appear in in the UCR system.

of firearm-related homicides, the data from Vital Statistics also allow us to conduct our analysis at the monthly level.[5] Third, we also employ data on hospitalizations to examine the impact the SYG law on injuries. For each homicide death, there are likely many more nonfatal injuries that result in hospitalizations. Therefore, studying hospitalizations as an additional outcome would not only serve as a validation and robustness analysis, but also enable us to reach a more complete understanding of the overall consequences of these laws.

Finally, we consider the possibility that the impact of SYG laws can vary across racial and gender dimensions. There are a number of factors motivating a race and gender-specific analysis. For example, there is widespread criminological evidence to highlight the presence of racial disparities in the criminal justice system (see Cole 1999; Mustard 2001; Bushway and Reuter 2008). If there are indeed sharp differences in the way criminal justice laws are applied to different racial groups, then it is natural to ask if there are also disparate impacts of SYG laws between whites and blacks.[6] In a recent analysis, Roman (2013) documents substantial evidence of racial disparities in justifiable homicide determinations. More importantly, he shows that the recent expansion of SYG laws has exacerbated that disparity.[7] Furthermore, whites are more than twice as likely to own guns as blacks (Morin 2014). Although only 32 percent of Americans are white men, they make up 61 percent of gun owners (Lind 2015). To the extent that white males primarily interact with other white males, this also may be another reason why any SYG impact on homicides or injuries might be stronger among white males than the other groups.

Finally, another argument often presented in support of SYG laws is that these laws would result in a reduction in the number of violent crimes against women. In fact, calls to repeal SYG laws have faced stiff opposition by certain political groups and the NRA, which argue that these laws help make women safer.[8] Thus, it is important to assess whether SYG laws are associated with a decrease in the female homicides as suggested by the deterrence argument.

5. With the exception of three states, all Castle Doctrine laws were passed in a period of three years between 2006 and 2008. In particular, 12 states passed these laws in 2006 and two states passed them in 2007. The number of states that passed a SYG law in 2005 and 2008 is only one. Clustering of laws around such a narrow time window would reduce variation over time in the spread of these laws, which can be important for identification in the difference-in-differences framework. Given the monthly frequency of our data, we are able to utilize variation in homicides across months within a year. This also helps us capture seasonality in the homicides.

6. If there are indeed racial and gender differences in the way these laws influence homicide rates, then an analysis combining all those groups together could potentially mask some interesting dynamics in the effect of these laws across races. An extreme case of example is one where the effects are large in magnitude and opposite in sign between blacks and whites. Under this scenario, it is quite possible to obtain an effect that is zero or small in magnitude and statistically insignificant, which would lead one to incorrectly conclude that these laws have no impact on homicides.

7. However, it is important to keep in mind that the finding of such disparities in and of itself would not be a proof of racial discrimination in the application of these laws. Such evidence would require more detailed data with information on the context and circumstances of each case (Roman 2013).

8. Recently, two Florida politicians declared calls to repeal these laws as antiwoman, arguing that imposing a duty-to-flee places the safety of the rapist above a woman's own life (See http://www.saintpetersblog.com /archives/57261)

II. Background

Historically, the right to defend one's home against intruders without a duty to retreat is well protected by the principle of "Castle Doctrine" in the United States. Rooted in the notion that "a man's home is his castle," this principle, recognized by common law, held an exception to the duty to retreat when an individual faced an attack in his home.[9] The key difference in this new wave of laws is that they have extended the Castle Doctrine to apply to places outside one's home, such as a vehicle, workplace, or anywhere else, an individual has a legal right to be. Between 2005 and 2010, 18 states passed some version of a SYG law that contains language granting immunity from criminal prosecution to individuals using defensive or deadly force to venues beyond homes.[10]

With strong support by the National Rifle Association (NRA), the proponents of the SYG laws argue that they would deter crime. They also contend that law-abiding citizens must be able to protect themselves from intruders and attackers without having to worry about criminal or civil penalties before taking action in self-defense. Reflected in this argument is a diminished sense of confidence in the criminal justice system's ability to protect victims and the perceived discrepancy in the judicial system that emphasizes the due process rights of defendants over the rights of victims (Jansen and Nugent-Borakove 2007).[11] It also has been suggested that in the aftermath of the terrorist attacks in the United States in September 2001 and Hurricane Katrina in 2005, increased concerns about public safety have played a role in the spread of SYG laws (Jansen and Nugent-Borakove 2007).

The opponents, on the other hand, claim that these laws give too much freedom to private citizens to use deadly force, almost making them a license to kill rather than a protective measure (Kleindienst 2005; Megale 2010). It also has been argued that these laws are open to abuses by those engaged in illegal activities or those with criminal records and might lead to an increased number of people carrying guns and willing to use them (Formby 2006; Weaver 2008; Jansen and Nugent-Borakove 2007). Along similar lines, it has been suggested that these laws could embolden individuals to stand their ground rather than simply walk away and could lead to individuals resorting to the use of deadly weapons even during situations posing no imminent danger (Weaver 2008; Florida Stand Your Ground Task Force 2012). Another concern raised by critics over the expansion of "no duty to retreat" to public areas is the amplified risk to innocent bystanders and public safety personnel and the possibility of increased violence due to retaliation (Jansen and Nugent-Borakove 2007; Rios 2012).

Another motivating factor for studying SYG laws is the rise in the number of justified homicides nationally since 2005 (Federal Bureau of Investigation 2010).[12] According to

9. See Catalfamo (2007) and Levin (2010) for a summary of the historical origins of the Castle Doctrine.

10. These states include Alabama, Arizona, Florida, Georgia, Indiana, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Montana, New Hampshire, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, and West Virginia. See Table 1 for more information.

11. Following the passage of Florida's law, the Institute for Legislative Action (ILA), the lobbying arm of the NRA, wrote, "Without doubt, Florida's recently enacted "Castle Doctrine" law is good law, casting a common-sense light onto the debate over the right of self-defense. It reverses the pendulum that for too long has swung in the direction of protecting the rights of criminals over the rights of their victims." (NRA-ILA 2006).

12. According to the Federal Bureau of Investigation, justifiable homicide is defined as, and limited to, the killing of a felon by a peace officer in the line of duty or the killing of a felon during commission of a felony by a private citizen.

data from the UCR, the total number of justifiable homicides has increased steadily from 196 in 2005 to 278 in 2010.[13] This is in contrast to the total number of overall homicides, which has continued to decline during the same period. However, it is not straightforward how to interpret the rise in justifiable homicides. On the one hand, it might be that more civilians are killing each other and claiming self-defense (Palazzolo and Barry 2012). In other words, the increase in justifiable homicide might reflect deaths that would not have occurred otherwise. This might support the concerns raised by the critics over the SYG laws. On the other hand, it also might reflect a mechanical shift as the new laws now result in more deaths being labeled justifiable homicides. Nevertheless, the coincidence in trend for justifiable homicides and the expansion of the new SYG laws is suggestive of a possible causal link, which deserves closer investigation. Unfortunately, the official data sources from the Federal Bureau of Investigation do not allow detailed information on the nature of circumstance in each incident. While the Supplemental Homicide Reports provide a breakdown of justifiable and nonjustifiable homicides, these data have been shown to be problematic due to underreporting and differences in interpretation and administrative practices in the timing of reporting across jurisdictions (Kleck 1998; Cheng and Hoekstra 2013). Despite these problems, we will show results from auxiliary analyses that examine the extent to which the SYG law is associated with justifiable versus nonjustifiable homicides.

III. Data

Our primary data source is firearm-related homicide victimization between 2000 and 2010 drawn from the U.S. Vital Statistics available from the National Center for Health Statistics (NCHS). The Vital Statistics records each instance of death based on information from death certificates filed with the vital statistics offices of each state and the District of Columbia. Each record contains pertinent demographic information as well as the locality, date, and cause of death. The cause of death is then recoded by the NCHS based on the Tenth Revision of the International Classification of Disease (ICD-10). For our purposes, we focus on firearm-related homicides committed by private citizens.[14] The individual records are aggregated to the state and month in which they occur. In order to construct homicide rates, we obtained population data from the U.S. Census' Intercensus County Population Estimates for each state and each year.

Our main explanatory variable is defined as a binary variable and takes on the value of one if a state has a SYG law in effect in a particular month of the year and zero otherwise. The month the SYG law has become effective is determined by information provided by the state legislature of each state in which a SYG law has been enacted.[15] In

13. The corresponding figures are 238 for 2006, 257 for 2007, 265 for 2008, and 266 for 2009. It is notable that this upward trend is solely due to an increase in the homicides committed with firearms and not accounted for by other dangerous weapons and knives or cutting instruments.

14. We exclude justifiable killings committed by police or other law enforcement officers and suicides by firearms from all our analyses.

15. For some states, the effective date is the day the legislation is signed into law by the governor, while for others, it is stipulated in the text of the legislation or set as a specific day of the year on which all passed laws become effective.

the month the law became effective, we coded the SYG as the proportion of days within the particular month in which the law was in effect as the treatment variable. The first state to pass a SYG law that has extended the right to self-defense with no duty to retreat in venues outside homes is Florida. By the end of 2010, a total of 18 states had similar laws in their criminal code.[16] A complete list of these states and the exact dates that their SYG laws have become effective are listed in Table 1. Note that a number of states have expanded their self-defense laws in other ways, including "removing duty to retreat somewhere outside home, but not everywhere a person can legally be present," "removed civil liability for those not found criminally liable," or "added the reasonableness" provision for those using self-defense measures in their own home. These states are listed in Column 3 of Table 1. In our interpretation, the expansion of no duty to retreat in public venues is the most dramatic aspect of these laws with the most significant ramifications for public safety. In the results section, however, we present evidence from a series of robustness analyses, which indicate that our results are not influenced by the presence of these other provisions.

We control for several time-varying characteristics of states in our analysis. These include the racial composition of state (percentage black, white, and Hispanic), the age distribution of state population (percentage aged 15–19 and aged 20–24), and the proportion of state population living in urban areas. Data on age distribution of state population, racial and ethnic composition, and urbanization are obtained from the Bureau of the Census. We also control for the state's unemployment rate, the poverty rate, and the political party that controls the governorship in the state. These measures are compiled from the University of Kentucky's Center for Poverty Research. Note that controlling for these variables should not affect the estimate on the SYG indicator as long as they are orthogonal to variation in SYG laws once we account for fixed effects and state-specific time trends. Our results are indeed robust to these variables.

Next, we supplement our models with data on the number of law enforcement agents and the number of state prisoners obtained from the annual Justice Expenditure and Employment Abstract of the Bureau of Justice statistics.[17] It can be argued that these variables might be endogenous to SYG laws. For example, if law enforcement officers anticipate that the expanded self-defense laws may lead to a rise in gun prevalence and escalation of violent conflicts, then they may react by intensifying their policing efforts. Alternatively, if the new laws result in an increase in the number of regular citizens taking up arms, for example, to serve as community watch volunteers, then the law enforcement officers actually may reduce their patrolling activities in some neighborhoods. These two variables are intended to capture the influence of these factors. To investigate this issue further, we regress each of these variables on the SYG indicator along with state and month*year fixed effects. The estimate on the SYG indicator from this exercise is small and imprecisely estimated in both regressions. This result lends support to the notion that law enforcement effort, as proxied by prisoner population and the number of law enforcement agents, is orthogonal to within state variation in SYG laws.

16. Utah had a law that allowed the right of self-defense without a duty to retreat outside the home since 1994.
17. The data on number of law enforcement agents includes private security personnel. Controlling for this variable separately did not change the results.

Table 1
List of States with Stand Your Ground Laws

State	No Duty to Retreat in Any Place Someone Has a Legal Right To Be	Any Expansion to Self-defense Laws	Effective Date for SYG Law	Notes/ Sources
Alabama	1	1	6/1/2006	a
Alaska	0	1	9/13/2006	b
Arizona	1	1	4/24/2006	b
Arkansas	0	0		
California	0	0		
Colorado	0	0		
Connecticut	0	0		
Delaware	0	0		
District of Columbia	0	0		
Florida	1	1	10/1/2005	a
Georgia	1	1	7/1/2006	b
Hawaii	0	0		
Idaho	0	1	7/1/2006	b
Illinois	0	1	7/28/2004	b
Indiana	1	1	7/1/2006	b
Iowa	0	0		
Kansas	1	1	7/1/2006	b
Kentucky	1	1	7/12/2006	b
Louisiana	1	1	8/15/2006	c
Maine	0	0		
Maryland	0	0		
Massachusetts	0	0		
Michigan	1	1	10/1/2006	a
Minnesota	0	0		
Mississippi	1	1	7/1/2006	b
Missouri	0	1	8/28/2007	b
Montana	1	1	4/27/2009	a
Nebraska	0	0		
Nevada	0	0		
New Hampshire	1	1	11/13/2011	b
New Jersey	0	0		
New Mexico	0	0		
New York	0	0		
North Carolina	0	0		
North Dakota	0	1	8/1/2007	b
Ohio	0	1	9/9/2008	b
Oklahoma	1	1	11/1/2006	a

(*continued*)

Table 1 (*continued*)

State	No Duty to Retreat in Any Place Someone Has a Legal Right To Be	Any Expansion to Self-defense Laws	Effective Date for SYG Law	Notes/ Sources
Oregon	0	0		
Pennsylvania	0	0		
Rhode Island	0	0		
South Carolina	1	1	6/9/2006	b
South Dakota	1	1	7/1/2006	b
Tennessee	1	1	5/22/2007	b
Texas	1	1	9/1/2007	d
Utah	1	1	3/2/1994	d
Vermont	0	0		
Virginia	0	0		
Washington	0	0		
West Virginia	1	1	2/28/2008	b
Wisconsin	0	0		
Wyoming	0	1	7/1/2008	b

Notes/Sources: a NRA-Institute for Legislative Action, http://nraila.org/news-issues/news-from-nra-ila.aspx
b State Legislators: Alaska, http://www.legis.state.ak.us/basis/get_bill.asp?bill=SB%20200&session=24; Arizona, http://www.supreme.state.az.us/opin/pdf2007/cv060320pr.pdf; Georgia, http://www1.legis.ga.gov/legis/2005_06/search/sb396.htm; Idaho, http://legislature.idaho.gov/legislation/2006/S1441.html; Illinois, http://www.ilga.gov/legislation/publicacts/fulltext.asp?Name=093-0832&GA=93; Indiana, http://www.nraila.org/search.aspx?s=%22Indiana%22&sort=date&pageNum=9; Kansas, http://www.kslegislature.org/li/b2011_12/measures/sb366/; Kentucky, http://www.lrc.ky.gov/krs/503-00/085.PDF; Michigan, http://www.legislature.mi.gov/documents/2005-2006/publicact/pdf/2006-PA-0311.pdf; Mississippi, http://billstatus.ls.state.ms.us/documents/2006/html/SB/2400-2499/SB2426PS.htm; Missouri, http://www.senate.mo.gov/07info/BTS_Web/Bill.aspx?SessionType=R&BillID=108; Montana, http://data.opi.mt.gov/bills/2009/billhtml/HB0228.htm; New Hampshire, http://www.nhliberty.org/bills/view/2011/SB88; North Dakota, http://www.legis.nd.gov/assembly/60-2007/docs/pdf/2007effectivedatesoflegislation.pdf; Ohio, http://lsc.state.oh.us/coderev/sen127.nsf/Senate+Bill+Number/0184?OpenDocument; Oklahoma, http://webserver1.lsb.state.ok.us/2005-06bills/HB/hb2615_engr.rtf; South Carolina, http://www.schouse.gov/sess116_2005-2006/bills/4301.htm; South Dakota, http://legis.state.sd.us/sessions/2006/bills/HB1134enr.htm; Tennessee, http://wapp.capitol.tn.gov/apps/Billinfo/default.aspx?BillNumber=HB1907&ga=105; West Virginia, http://www.legis.state.wv.us/Bill_Text_HTML/2008_SESSIONS/RS/Bills/SB145%20SUB1%20enr.htm; Wyoming, http://legisweb.state.wy.us/2008/Summaries/HB0137.htm.
c Louisiana Association for Justice, http://www.lafj.org/index.cfm?pg=06LegSummaryCriminal.
d Association for Prosecuting Attorneys (APA), http://www.apainc.org/%28S%28fcxbywqqevm5y3yplrtrvb45%29%29/documentdownload.aspx?documentid=27&getdocnum=1

Table 2 shows the means and standard deviations for our control variables. The first column shows the descriptive statistics for the full sample. The descriptive statistics for the subsample of states that has never passed a SYG law are shown in Column 2 and for those states that have passed a SYG law at some point during the analysis period are shown in Column 3. Finally, Columns 4 and 5 show descriptive statistics for all state–

Table 2
Summary Statistics

Variable	Full Sample	Ever Implemented SYG = 0	Ever Implemented SYG = 1	Observations with SYG = 0	Observations with SYG = 1
SYG	0.144	0.000	0.409	0.000	1.000
	(0.351)	(0)	(0.491)	(0)	(0)
Homicides per month	19.391	16.771	24.194	18.580	24.171
	(25.261)	(26.538)	(21.94)	(25.582)	(22.71)
Monthly homicide rate per 100,000	0.321	0.292	0.375	0.314	0.366
	(0.351)	(0.402)	(0.221)	(0.367)	(0.2261)
Percent Hispanic	8.679	9.206	7.713	8.670	8.735
	(9.155)	(9.095)	(9.189)	(9.188)	(8.9643)
Percent white	81.699	81.970	81.202	10.857	81.260
	(13.58)	(15.091)	(10.23)	(11.494)	(10.4267)
Percent black	11.270	9.797	13.971	8.774	13.703
	(11.559)	(11.298)	(11.546)	(14.044)	(11.647)
Percent aged 15–19	7.275	7.218	7.380	7.258	7.374
	(0.549)	(0.51)	(0.6)	(0.514)	(0.7135)
Percent aged 20–24	7.139	7.007	7.380	7.051	7.654
	(0.809)	(0.772)	(0.819)	(0.715)	(1.0895)
Percent aged 25–34	35.785	36.148	35.118	36.046	34.246
	(1.849)	(1.904)	(1.537)	(1.808)	(1.2448)
Percent living in urban areas	56.506	56.994	55.611	59.013	41.740
	(33.787)	(35.386)	(30.626)	(32.245)	(38.5673)
Unemployment rate	5.520	5.419	5.706	5.323	6.680
	(1.997)	(1.964)	(2.043)	(1.794)	(2.637)
Poverty rate	12.197	11.119	14.174	11.789	14.602
	(3.27)	(2.932)	(2.917)	(3.069)	(3.3835)
Democratic governor	0.497	0.540	0.419	0.523	0.349
	(0.5)	(0.498)	(0.494)	(0.5)	(0.4769)
Prisoners per 100,000 residents	432.633	386.484	517.240	418.729	514.539
	(165.084)	(145.742)	(164.988)	(160.02)	(170.6801)
Number of law enforcement agents	19,304.960	19,269.880	19,369.280	19,221.870	19794.380
	(22,430.71)	(24,456.27)	(18,143.65)	(22,965.39)	(18980.84)
Number of observations	6,732	4,356	2,376	5,755	977

Note: Standard Deviations are in parentheses. Columns 1 refers to states that never implemented stand your ground laws, and Column 3 refers to those that passed these laws during our analysis period. Columns 3 and 4 display means for state*month*year observations in which Stand Your Ground indicator is 0 and 1, respectively.

month–year observations without a SYG law and with a SYG law in effect, respectively. The average monthly homicide counts are 16.8 in non-SYG states and 24.2 in SYG states. The homicide rate is also higher among SYG states with 0.375 deaths per month per 100,000 residents deaths compared to non-SYG states, which have an average homicide rate of 0.292 per month per 100,000 residents. Of the 6,732 state–month–year observations, 729 (10.8 percent) are zeros due to no homicides committed during those months. The largest homicide count is observed in California, a non-SYG state, with 198 deaths in September 2002. The largest number of homicides among our SYG states was observed in Texas in May 2009 with 106 homicides. If we consider homicide rates per population rather than homicide counts, then the largest homicide rate was observed in District of Columbia, again a non-SYG state, in July 2002 with 2.27 homicides per 100,000 residents. Among the SYG states, the largest homicide rate was in Louisiana in July 2009 with 0.66 homicides per 100,000 residents. Table 2 also shows that SYG and non-SYG states differ in several observable characteristics. In particular, SYG states have a higher percentage of black population, are more likely to have a Republican governor, have a higher incarceration rate, and have a larger number of law enforcement agents. These states also tend to be more urban and have a higher poverty rate.[18]

IV. Empirical Strategy

Our goal is to estimate the impact of the SYG law on firearm-related homicides. The key empirical challenge in answering this question stems from the fact that there may be confounding factors that are correlated with both the passage of these laws and homicides or crime in general. In order to account for the influence of these factors, we follow Cheng and Hoekstra (2013) and employ a difference-in-differences strategy, exploiting the variation in the timing of these laws across states. A key identification assumption in this strategy is that, in the absence of the SYG laws, homicide rates would have trended similarly between states that enacted these laws and those which did not. One potential threat to this assumption is that the decision to pass a SYG law might reflect differences in preexisting trends in the crime rates between treatment and control states. While pro-gun activists have intensified their campaigns both at the legislative and grassroots levels in the last decade, it is not exactly clear what specifically prompted these particular states to extend the Castle Doctrine in a controversial way (Jansen and Nugent-Borakove 2007). However, these laws are unlikely to have been introduced as a reaction to a wave of crime epidemic in those states because crime rates including homicides declined virtually everywhere in the United States during the analysis period. As illustrated in Figure 1, trends in homicide rates are fairly similar across states that passed SYG laws and those that did not pass such legislation prior to the passage of these laws. The lines with solid circles in Figure 1 reflect the smoothed average log homicide rate weighted by the relevant population in a given month and a year among the treatment states, that is, the states that passed a SYG law during our analysis period. The vertical line presents the month and the year in which the SYG law became effective in each of the treatment states. Because these laws became effective at

18. A similar picture is obtained if we rather focus on the differences between state-month-year observations with SYG law in effect and without a SYG law as presented in Columns 4 and 5 of Table 2.

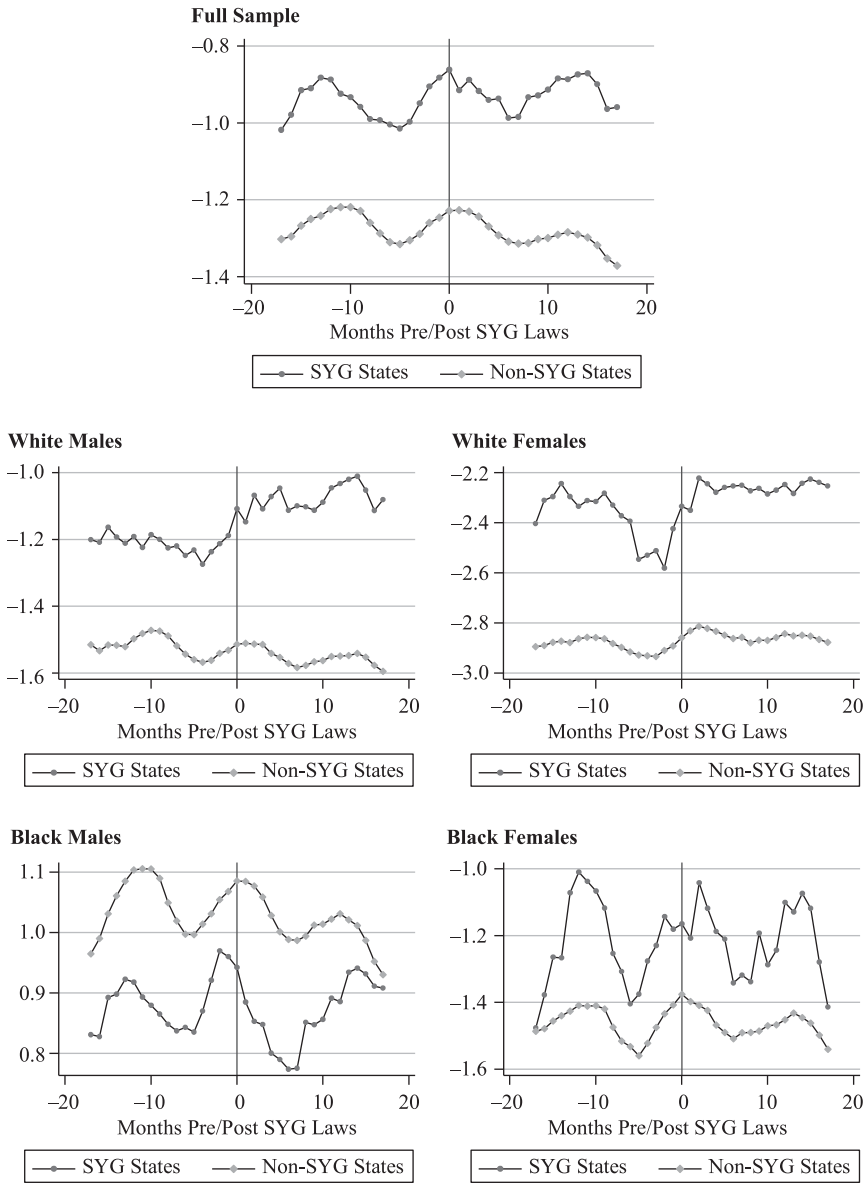

Figure 1

Trends in Homicides Before and After Stand Your Ground Laws

different points in time, the graph is centered in the month and year in which the SYG law became effective in each of the SYG states (time 0) and tracks homicide rates in the months leading up to and following this point for 18 months (for example, time zero is 10/2005 for Florida and 6/2006 for Georgia). Similarly, the line with solid diamonds in Figure 1 displays the smoothed average homicide rate across states that fall into the control group, that is, those states that did not pass a SYG law.[19]

As shown in Figure 1, the homicide rates are higher in treatment states than the control states prior to the effective dates of SYG laws with the exception of black males. However, the differences in levels of homicide between the two types of states are not a threat to our identification strategy. What is important for identification is that the SYG and non-SYG observations generally follow a similar trend in homicides prior to the passage of SYG laws, suggesting little evidence for systematic differences between the two groups of states other than differences in levels. Note that when we focus on the combinations of gender and race, the picture becomes more blurred, especially for homicides in treatment states, because of volatility in the monthly rates of homicides.[20] Following the enactment of SYG laws, the homicide rates start trending upward in the treatment states, especially for white males and white females. Even for black males, who appear to have a higher homicide rate in non-SYG states for both before and after the implementation date of these laws, the trend appears to be upward for the SYG states and downward for the non-SYG states in the postimplementation period. The overall evidence illustrated in Figure 1 provides suggestive evidence that in the absence of SYG laws, the treatment states might have followed a similarly flat or downward trend. Despite this visual evidence in support of the parallel trends assumption, we still relax this restriction by including state-specific trends in addition to state and month*year fixed effects in our analysis.

The difference-in-differences method can be formalized in a regression framework as follows:

$$(1) \quad H_{smy} = \alpha\, SYG_{smy} + \boldsymbol{X}_{sy}\beta + \mu_s + \lambda_{my} + \varepsilon_{smy},$$

where H_{smy} is the homicide rate in state s in month m in calendar year y. SYG_{smy} is our key explanatory variable, which equals one if state s has a SYG law in effect in month m in calendar year y, and zero otherwise. The $\boldsymbol{X}_{sy}$ is a vector of determinants of crime described above. The μ_s are state fixed effects and would capture any time-invariant, state-specific factors, which may influence homicides. The λ_{my} are month*year fixed effects that would account any seasonality-related trends as well as national changes in homicides that are common to all states. The ε_{smy} is the usual error term. The coefficient of interest is α, the impact of SYG laws on homicides. The standard errors are clustered

19. To illustrate this point more clearly, assume we have three states with the SYG laws, which became effective on 10/2005, 6/2006, and 12/2007, and three control states (X, Y, and Z) that did not pass a SYG law during the analysis period. Then the average homicide rate among the control states at time 0 is based on the homicide rates in states X, Y, and Z in 10/2005, 6/2006, and 12/2007. Similarly, the homicide rates in control states at time 1 is constructed as the average of homicide rates in states X, Y, and Z in 11/2005, 7/2006, and 1/2008. The average homicide rates for the months prior to time 0 are constructed in a similar fashion.

20. Note that the volatility in homicides in SYG states is particularly higher because the number of states with a SYG law in effect decreases as one moves away from time 0. In other words, the homicide rate is averaged over a decreasing number of states as we move away from time 0.

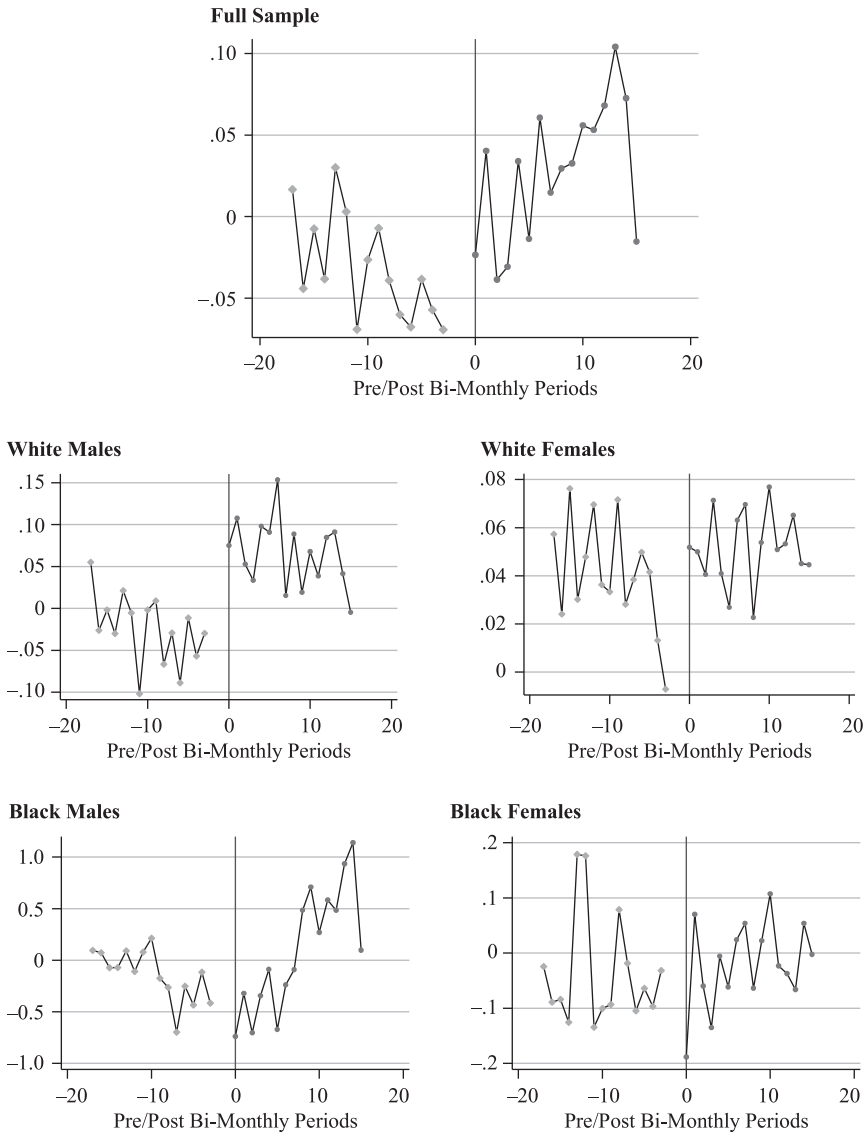

Figure 2

Event Study Estimates of the Impact of Stand Your Ground Laws on Homicides

Notes: For ease of exposition, the specifications are based on data aggregated at the bimonthly level. The general pattern with monthly data is similar, though more volatile. The reference category is one period before the treatment.

at the state level to account for serial correlation within states over time (Bertrand, Duflo, and Mullainathan 2004).

We further assess the credibility of our research design by examining whether there are any systematic changes in homicide rates prior to states passing a SYG law. This can be done by performing an event study analysis, which would allow us to trace out the trend in the homicide rate for the periods leading up to and following the passage of a SYG law.[21] In practice, we implement this by estimating a regression of homicide rates on a set of indicators for the periods leading up to and from the time of the passage of the SYG law up to 32 months. The estimates from this analysis, which are shown in Figure 2, generally suggest that our results are unlikely to reflect a continuation of some preexisting long-term trends. Furthermore, there appears to be an upward trend in the homicide rate in the post-SYG months. Although the visual evidence illustrated in Figures 1 and 2 suggests that there is little evidence of systematic differences in pre-existing trends between the treatment and control states, we extend our baseline specification in Equation 1 by including state-specific linear trends in addition to state and month*year fixed effects. These additional controls would account for state-specific unobserved factors that trend linearly over time and are correlated with homicides.[22]

We begin our empirical analysis with OLS models estimated in the logarithm of homicide rates per 100,000 state residents.[23] Additionally, we also present results from fixed-effects Poisson models.[24] These models are particularly well suited for our purpose given the count nature of our data and the fact that homicides are rare events, especially at the monthly level and in some smaller states. In fixed effects Poisson models, we also control for the logarithm of state population for the relevant race and gender group.[25]

V. Results

Table 3A presents OLS estimates of the impact of the SYG laws on the logarithm of homicides per 100,000 residents. Each cell in the table presents the coefficient on the indicator for the SYG law and its standard error (adjusted for clustering

21. A typical event-study is modeled by using a vector of binary indicators, each of which takes on the value of one when an observation is a certain number of periods before or after some discrete policy event. These event time indicators then replace the treatment variable in the difference-in-difference regression model.

22. We also experiment with specifications that control for state-specific quadratic trends as well as region*year fixed effects. These results did not cause any appreciable changes to the results presented here.

23. Our results are similar when we estimate these models using rates of homicide instead. These results are presented in Appendix Table 1. Note that all appendix tables are available online at http://uwpress.wisc.edu/journals/journals/jhr-supplementary.html. Estimating a proportional effects model is not straightforward in our case because of zero homicides observed in some states in certain months. To overcome this complication, we replaced zeros with ones, and estimated log-linear models with the resulting data. Our results do not change when we only use the nonzero observations. These results are available from the authors upon request.

24. One advantage of Poisson models is that observed homicide rates of zero present no problem because Poisson regression does not require taking the logarithm of the dependent variable. However, one limitation of these models is the assumption that variance equals the mean. We use the robust option to obtain robust standard errors as recommended by Cameron and Trivedi (2010), which should mitigate concerns over the equivariance assumption.

25. These models are frequently used in the crime literature. (See Sampson et al. 1997; Osgood 2000; Weiner et al. 2009.) By adding the logarithm of state population, Poisson regression becomes an analysis of rates of homicides per population, rather than an analysis of counts of homicides.

Table 3A
OLS Estimates of the Impact of the Stand Your Ground Law on log(Homicides)

Full Sample			
Stand Your Ground	0.067	0.088**	0.075*
	(0.054)	(0.043)	(0.038)
R^2	0.748	0.765	0.767
Whites			
Stand Your Ground	0.086	0.134***	0.125**
	(0.062)	(0.049)	(0.053)
R^2	0.664	0.675	0.677
Blacks			
Stand Your Ground	0.047	–0.003	–0.033
	(0.058)	(0.100)	(0.098)
R^2	0.606	0.632	0.636
White Males			
Stand Your Ground	0.100	0.156***	0.153***
	(0.065)	(0.053)	(0.055)
R^2	0.663	0.675	0.677
White Females			
Stand Your Ground	0.063	0.085	0.084
	(0.056)	(0.059)	(0.080)
R^2	0.517	0.523	0.524
Black Males			
Stand Your Ground	0.057	–0.001	–0.032
	(0.059)	(0.105)	(0.099)
R^2	0.607	0.630	0.635
Black Females			
Stand Your Ground	0.029	0.005	–0.017
	(0.062)	(0.073)	(0.091)
R^2	0.551	0.558	0.560
State Fixed Effects	Yes	Yes	Yes
Month*year fixed effects	Yes	Yes	Yes
State-specific linear time trends	No	Yes	Yes
Time-varying state characteristics	No	No	Yes
Number of observations	6,732	6,732	6,732

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors that are clustered by state are in parentheses. The unit of observation is state-month-year. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively.

at the state level). We present results for the full sample in the top panel and then separately by race and combinations of race and gender in panels two to seven. Regressions are weighted by the state population of relevant race and gender group.[26] In each panel, Column 1 shows the SYG estimates from a specification expressed in Equation 1 that controls for state, month, and year fixed effects; Column 2 shows the estimates from the specification that adds state-specific time trends to the list in Column 1. If our identification strategy is valid, then adding time-varying determinants of crime should have little impact on the SYG estimate. This is illustrated in Column 3, which contains time-varying state characteristics in addition to all other controls in Columns 1 and 2.

As shown in the first column of Table 3A, the estimates are positive for all groups, but they are imprecisely estimated. The same pattern still is present when we account for state-specific linear trends in Column 2, except that the estimates are now statistically significant for the full sample, as well as whites and white males. Finally, controlling for time-varying state characteristics does not change the estimates in any meaningful way. Based on the point estimates, the SYG law is associated with a 7.5 percent increase in the overall homicide rate. Given the average homicide rate of 0.44 per 100,000 persons in the month prior to the passage of SYG law, the estimate translates into an effect size of 0.033 additional deaths per 100,000. Multiplying this with the population in SYG states results in about 35 additional deaths.

The effect on whites and white males are 12.5 and 15.3 percent, respectively. Focusing on white males, the average homicide rate is 0.425 per 100,000 among this group in the month prior to the passage of SYG laws. Then the estimate of 15.3 percent translates into 0.065 additional homicides per 100,000 per month among this group. Given a white male population of 45.4 million in these states, this would indicate an additional 30 deaths per month among this demographic group. The estimate on white females is not statistically significant. Consistent with the results presented in the third panel, the estimates on blacks are small in size and statistically insignificant for both males and females.

Next we present results from the fixed effects Poisson regressions in Table 3B. As shown in the table, these results are largely consistent with those from the OLS. Furthermore, the estimates in Columns 2 and 3 are similar, suggesting that our results are not sensitive to time-varying state characteristics once state and year*month fixed effects as well as state-specific linear time trends are accounted for. The estimate for the full sample is 7.7 percent ($e^{0.074} - 1$). Using the average homicide rate in SYG states in the month prior to the legislation, this translates into 36 additional homicides. Note that this figure is very close to 35 obtained from the OLS. The point estimate for whites and white males are 0.200 and 0.221, and they are both statistically significant.

The model specified in Equation 1 produces the average impact of SYG law on homicides as measured by the average change in homicides between post- and pre-treatment periods for the SYG states to the average change over the same period for the control states. Therefore, it is not clear whether the effect of SYG laws on homicides becomes present immediately or rather gradually. To investigate this issue further, we

26. The unweighted fixed effects Poisson estimates for our most comprehensive specification are 0.06 for the full sample, 0.13 for whites, and 0.15 for white males, and all these estimates are statistically significant at conventional levels. None of the estimates for other groups are estimated with precision. The corresponding figures for the OLS models are 0.07, 0.10, and 0.13.

Table 3B
Fixed Effects Poisson Estimates of the Impact of the Stand Your Ground Law on Homicides

Full Sample			
Stand Your Ground	0.130**	0.100*	0.074**
	(0.060)	(0.052)	(0.035)
R^2	0.371	0.384	0.389
Whites			
Stand Your Ground	0.169***	0.212***	0.200***
	(0.052)	(0.044)	(0.047)
R^2	0.458	0.465	0.467
Blacks			
Stand Your Ground	0.058	–0.006	–0.021
	(0.063)	(0.103)	(0.084)
R^2	0.342	0.350	0.353
White Males			
Stand Your Ground	0.179***	0.230***	0.221***
	(0.054)	(0.047)	(0.044)
R^2	0.467	0.474	0.476
White Females			
Stand Your Ground	0.127***	0.126***	0.135
	(0.049)	(0.039)	(0.083)
R^2	0.145	0.148	0.149
Black Males			
Stand Your Ground	0.066	–0.004	–0.019
	(0.066)	(0.109)	(0.085)
R^2	0.334	0.341	0.345
Black Females			
Stand Your Ground	0.029	–0.023	–0.043
	(0.071)	(0.081)	(0.087)
R^2	0.0954	0.0992	0.101
State fixed effects	Yes	Yes	Yes
Month*year fixed effects	Yes	Yes	Yes
State-specific linear time trends	No	Yes	Yes
Time-varying state characteristics	No	No	Yes
Number of observations	6,732	6,732	6,732

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors that are clustered by state are in parentheses. The unit of observation is state-month-year. All models include the logarithm of state population for the relevant demographic group as an additional control variable. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

Table 4
Fixed Effects Poisson Estimates of the Impact of Stand Your Ground Laws on Homicides, Years since Implementation

Years Since Implementation	Full Sample	Whites	Blacks	White Males	White Females	Black Males	Black Females
First year	0.042	0.205***	–0.093	0.221***	0.151*	–0.090	–0.121
	(0.028)	(0.049)	(0.100)	(0.051)	(0.084)	(0.100)	(0.112)
Second year	0.137***	0.226***	0.075	0.256***	0.114	0.070	0.117
	(0.050)	(0.059)	(0.076)	(0.058)	(0.082)	(0.075)	(0.120)
Third year	0.193***	0.246***	0.159**	0.268***	0.141	0.165**	0.102
	(0.059)	(0.077)	(0.079)	(0.080)	(0.094)	(0.082)	(0.122)
Fourth year	0.171**	0.229***	0.096	0.224***	0.185**	0.086	0.172
	(0.068)	(0.075)	(0.107)	(0.083)	(0.089)	(0.110)	(0.169)
Fifth year	0.174*	0.314***	–0.001	0.333***	0.158	–0.004	0.017
	(0.099)	(0.093)	(0.146)	(0.104)	(0.113)	(0.148)	(0.176)
R^2	0.388	0.468	0.356	0.477	0.150	0.347	0.102

Notes: Standard errors that are clustered by state are in parentheses. The unit of observation is state–month–year. All models include state fixed effects, year*month fixed effects, state-specific linear trends, time varying state characteristics and the logarithm of state population for the relevant demographic group as an additional control variable. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

next consider a more nonparametric specification. In particular, we estimate our most comprehensive specification for the fixed effects Poisson regression with five binary SYG indicators, each representing the number of years that the laws has been in effect (the first year indicator refers to the period between effective date and the first year anniversary, second year indicator refers to the period between the first and second year anniversaries, etc.) along with time-varying characteristics, state and year*month fixed effects, and state-specific linear time trends. As presented in Table 4, the estimates are much more precisely estimated and larger in magnitude for white males than other groups. Furthermore, the impact size is fairly homogenous across the five indicators for whites, supporting a more immediate and persistent pattern instead of a gradual one.

The results presented above are obtained from monthly records of homicides drawn from the Vital Statistics. In order to assess the sensitivity of our results to the way the unit of analysis is determined and also to make our analysis comparable to Cheng and Hoekstra (2013), we convert our monthly data to annual data and reestimate models in Tables 3A and 3B. As shown in Appendix Table 2, these results are very much in line with those obtained from the analysis using monthly data. Furthermore, the estimates are again precisely estimated only for the full sample and the subsamples of whites and white males. It is important to note that the OLS estimate for the full sample in the most comprehensive specification in Appendix Table 2 is 0.103, which is very close to the estimate of 0.0937 in Cheng and Hoekstra (2013).[27]

27. See Column 3 of Table 5 on page 840 in Cheng and Hoekstra (2013).

Our main focus has been the impact of SYG laws that have explicit language extending no duty to retreat to places an individual has a legal right to be. However, some states have passed narrower versions of self-defense laws that limit the no duty to retreat to only an individual's home, business, or vehicle; include provisions that create a presumption of reasonableness; or remove civil liability for individuals using deadly force in self-defense. In our view, it is the provision of "no duty to retreat in any place an individual has a legal right to be" that has been the most controversial aspect of the new laws. Nevertheless, we perform a series of robustness analyses to determine the extent to which our results are confounded by these other provisions. In the first analysis, we create an additional treatment indicator to reflect whether a state has passed some type of Castle Doctrine law during the analysis period, but one without removing duty to retreat in any place a person has a legal right to be. We call this indicator "Other Self-Defense Provisions." Then we estimate all our models with this variable as well as our original treatment indicator. The omitted category in our models is states that did not pass any self-defense laws during this period.

We present the SYG estimates from OLS and fixed effects Poisson models in Columns 1 and 2 in Appendix Table 3. As shown in the table, the results from this analysis are hardly different from those reported in Tables 3A and 3B. The estimates on SYG indicator still points to a robust impact of SYG laws on homicides among whites and white males. Moreover, the estimates on blacks are again small in size and statistically insignificant. Regarding the coefficients on the indicator for other self-defense provisions, they are estimated with precision only significant for white males. Surprisingly, however, the estimates are opposite in sign, suggesting that these other provisions as a group are associated with a decrease in the homicide rate among this group. The estimates from the fixed effects Poisson results presented in Column 2 of Appendix Table 3 again corroborate those shown in Table 3B. We interpret this as strong evidence to indicate that the estimated SYG law impacts obtained earlier are not confounded by these other provisions. Focusing on gender- and race-specific estimates, we see that the impact of SYG laws on the homicide rate among white males is 24.2 ($e^{0.217} - 1$) percent, which is almost identical to the effect size in Table 3B. Furthermore, all of the SYG estimates on blacks are again small in size, and none are statistically significant. Regarding the estimates on the indicator for other self-defense provisions, we obtain negative and statistically significant impacts for blacks, white males, and black males, while the estimate on white females is positive and significant.

As stated above, identifying the impacts of various attributes of self-defense laws is complicated due to high-level collinearity among them. Although it is beyond the purpose of this paper to uncover the impact of these specific attributes, we offer another preliminary look at the potential impact of individual provisions of self-defense laws by estimating our preferred specification with three indicators, each of which represents a different attribute. These three laws include the presence of (i) a stand your ground law that removes the duty to retreat any place one has a legal right to be, (ii) a provision of a presumption of reasonable fear for the person using lethal force, and (iii) a provision that removes civil liability for those acting under the law. The estimates on these three attributes from OLS and fixed effects Poisson regressions are presented in Appendix Table 4. As shown at the top panel, there is a strong and statistically significant impact of SYG law on overall homicide rate, independent of any of the other attributes of self-

defense laws. This pattern of statistical significance also holds for whites and white males and to some extent blacks, for whom only the estimate from the fixed effects Poisson regression is estimated with some precision. Again, the impact on whites is driven almost entirely by homicides on white males at levels similar to those from our primary analysis. Other individual provisions associated with expanded self-defense laws have little impact and are imprecisely estimated.

Our final attempt to assess the sensitivity of our results to the way our treatment variable is coded follows closely the approach taken by Cheng and Hoekstra (2013). In particular, we estimate our models classifying 21 states as having passed some form of expanded self-defense laws, namely removing duty to retreat "somewhere" outside home. (see Table 1 on page 826 in Cheng and Hoekstra 2013). In Appendix Table 5, we present estimates from our most comprehensive specification from both the OLS and fixed effects Poisson models. As shown in the table, these estimates are not out of line with Cheng and Hoekstra (2013). The OLS estimates presented in Column 1 show that the effect of expanded self-defense laws is positive and statistically significant for whites. The estimates are positive and sizeable for both males and females, but somewhat surprisingly, only significant for females.[28] The fixed effects Poisson estimates reported in Column 2 lend even stronger support for our main findings. These estimates are positive and significant for the full sample as well as for both white males and white females. As far as the estimates for blacks are concerned, all of them are small in magnitude and none of them are estimated with precision.

Note that the SYG coefficients displayed in Tables 3A and 3B are estimated most precisely for among whites and white males in particular. However, this should not be interpreted as indicating that SYG laws affect only the homicides among white males and no other group. The estimates are also positive and significant for the full sample indicating that these laws are associated with an increase in the total number of homicides regardless of race and gender. In order to provide further insights into race- and gender-specific disparities in the impact of SYG on homicides, we estimate pooled regressions using OLS and fixed effects Poisson in which we allow the effect of SYG to differ by combinations of race and gender. The results from this analysis are presented in Appendix Table 6. As shown by the *p-value*s at the bottom of the table, the effect of SYG appears to be different between white males and black males in the fixed effects Poisson model, but not in the OLS model, in which the *p-value* is 0.11. A similar pattern is observed for the difference between white males and white females. According to the test statistics, we fail to reject the hypothesis that the SYG effects are equal to each other between white males and black females.

A. Justifiable versus Nonjustifiable Homicides

Some may argue that the increase in homicides could have been driven largely by killings that are justified under the SYG laws. In other words, those deaths that occur while law-abiding citizens are protecting themselves from intruders or attackers should not necessarily be viewed as an undesirable outcome.[29] It is indeed possible that

28. Note that the estimate on white males is just outside the conventional range of statistical significance with a *p-value* of 0.11.

29. Note that justifiable killings by police or other law enforcement officers are already excluded from our analyses. If the deterrence argument were valid, then we should have seen a decrease in justifiable homicides by

additional homicides associated with the SYG law may partly be driven by the killings of assailants. However, note that the net rise in homicides cannot be accounted by a one-to-one substitution between the deaths of assailants and the deaths of victims unless multiple assailants are killed in some instances (Cheng and Hoekstra 2013). If at least some of the additional homicides are due to individuals resorting to the use of deadly force in situations where the threat of death or serious bodily injury is not imminent to either party, this could suggest that these laws may impose serious costs not only on criminals but also on private citizens as well. It is also possible that potential criminals, who, in the absence of SYG laws, could have committed their illegal acts without physically hurting anyone, are now being killed themselves as a result of these laws. Whether all of these killings should be considered justifiable in this circumstance is a question that is beyond the purpose of this paper. However, it cannot be argued in that case that the SYG laws are saving the lives of innocent people because these individuals would not have been killed in the first place.

Unfortunately, our main data source, the U.S. Vital Statistics, does not allow us to distinguish between justifiable and nonjustifiable homicides. As a matter of fact, the only nationally representative source of information on homicides is the FBI's Supplemental Homicide Reports (SHR). However, the problem with the SHR is that it has been known to suffer from significant underreporting, especially for justifiable homicides (Kleck 1998; Cheng and Hoekstra 2013). Despite this problem, we draw upon data from the SHR to investigate the relationship between the SYG laws and justifiable versus nonjustifiable homicides for the period between 2000 and 2009. The means for homicide counts in SHR are shown in Appendix Table 7. Consistent with the earlier discussion, the homicide numbers in the SHR data are an undercount of the total number of homicides. Furthermore, the overwhelming majority of homicides are classified as nonjustifiable. For example, there are only four justifiable homicides on average compared to 198.2 nonjustifiable homicides in our full sample. The number of justifiable homicides is 2.6 for the non-SYG states and 6.5 and for the SYG states, while the corresponding figures for nonjustifiable homicides are 173.5 and 245.5.

Next, we run separate regressions for nonjustifiable and justifiable homicides aggregated at the annual level. In Table 5, we report the SYG estimates for the models of justifiable homicides estimated by OLS and fixed effects Poisson regressions in Columns 1 and 2, respectively.[30] In both columns, we use a specification that includes state and year fixed effects along with time-varying state characteristics and state-specific trends. As shown in the table, the evidence points to a strong and positive association between SYG laws and homicides. The point estimates are 0.378 in the OLS and 0.22

private citizens, but we find the opposite to be the case. Furthermore, let's assume that these laws cause a one-to-one replacement in the deaths between victims and assailants. In other words, those who would have been killed by assailants prior to the stand your ground law are now able to shoot and kill their so-called assailants because they do not have to retreat before using deadly force. But this scenario only would shift the burden of these laws from "good guys" to "bad guys" without changing the number of total deaths, unless multiple "bad guys" are killed in some instances.

30. Given that the justifiable homicide figures for white females and black females are so low and coupled with the already-mentioned problems associated with justifiable homicide records, we believe that the estimates for females are not reliable, so are not presented in the tables. For example, there are 0.04 deaths per state per year among white females that are classified as justifiable. The corresponding figure for black females is 0.02. Nevertheless, they are available from the authors upon request.

Table 5
Estimates of the Impact of the Stand Your Ground Law on Justifiable Homicides Using Supplemental Homicide Reports

	OLS	Fixed Effects Poisson
Full Sample		
Stand Your Ground	0.378*	0.198
	(0.189)	(0.155)
R^2	0.787	0.169
Whites		
Stand Your Ground	0.421**	0.929***
	(0.189)	(0.211)
R^2	0.832	0.178
Blacks		
Stand Your Ground	0.118	–0.308
	(0.267)	(0.289)
R^2	0.770	0.249
White Males		
Stand Your Ground	0.402**	0.973***
	(0.193)	(0.225)
R^2	0.825	0.207
Black Males		
Stand Your Ground	0.112	–0.308
	(0.266)	(0.300)
R^2	0.766	0.309
Number of observations	500	500

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors that are clustered by state are in parentheses. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. Both specifications include state and year fixed effects, state-specific trends, and time-varying characteristics of states.

($e^{0.198} - 1$) in the fixed effects Poisson, although only the former is statistically significant at conventional levels. Moreover, the estimates are large and significant for whites and white males. Such large effects are consistent with the voluminous anecdotal and descriptive evidence pointing to a sharp rise in the number of justifiable homicides following SYG laws. As explained above, however, this may represent a rather mechanical shift simply because of a change in the way the incidents are now categorized into justifiable and nonjustifiable homicides.

In light of the widespread evidence that justifiable homicides in SHR are severely underreported, we submit our estimates in Table 5 to further scrutiny following Cheng and Hoekstra (2013). According to Kleck (1988) and as highlighted in Cheng and Hoekstra (2013), only about 20 percent of the actual number of justifiable homicides are reported accurately. Note that our estimate for justifiable homicides in the full sample is 0.38. This estimate translates into an effect size of 1.55 justifiable homicides every month given the baseline monthly average of 4.08. If we assume that only 20 percent of all justifiable homicides are reported as such, then the estimate of 0.38 represents an increase in monthly justifiable homicides of 7.75, of which 6.2 would be misreported as nonjustifiable. Our effect size for the SYG indicator from Table 3B is about 36 additional killings. Therefore, even with an assumption of 80 percent underreporting, the majority of the additional deaths caused by the SYG laws would come from the deaths of presumably innocent victims, not the killings of assailants. If we were to change the assumption about the extent of underreporting to 90 percent, then that still would indicate that more than half of the additional homicides caused by the SYG laws were deaths of innocent individuals instead of criminals. Of course, these are back-of-the-envelope calculations, and without more accurate estimates on the degree of underreporting and information on any changes in the way reporting practices for justifiable homicides have been changed under the SYG, one needs to exercise caution in interpreting these figures.

The results for the nonjustifiable homicides are reported in Table 6. The OLS estimates shown in Column 1 suggest that the SYG law has a large and statistically significant positive effect in the nonjustified homicides among white males. The point estimate indicates a 20.8 percent increase in these homicides associated with the SYG laws. The results from the fixed effects Poisson regressions shown in Column 2 suggest that the SYG law is associated with a significant increase in the nonjustifiable homicides of not only white males, but also white females, while no other statistically significant effect is detected for other groups.

Despite the aforementioned concerns about the reliability of the SHR data, the pattern obtained from the results in Tables 5 and 6 is largely supportive of our earlier finding that SYG laws do in fact cause an increase in the total homicides including those that fall under the nonjustifiable category. Also, if these laws do not increase overall homicides, but simply result in a substitution away from nonjustifiable homicides toward justifiable homicides, then we should have seen a clear reduction in nonjustifiable homicides. If anything, the evidence from Table 6 shows strong evidence that the opposite is true. Overall, our results on justifiable homicides corroborates the findings of Cheng and Hoekstra (2013).

According to data from the Uniform Crime Reports, 67.5 percent of all homicides are committed by firearms in the United States in 2010. The anecdotal evidence describing cases in which SYG laws have been invoked mostly refer to the involvement of firearms in these situations. However, the law is not weapon-specific, and therefore applies to all incidents, regardless of the type of weapon used. To assess the sensitivity of our results to the type of weapon, we estimated all of our models using homicides committed with all types of weapons including firearms and nonfirearms. This variable is also more in line with the measure of total homicides used in Cheng and Hoekstra (2013). This analysis produced results that are remarkably similar to those presented in Tables 3A

Table 6
Estimates of the Impact of the Stand Your Ground Law on Non-Justifiable Homicides Using Supplemental Homicide Reports

	OLS	Fixed Effects Poisson
Full Sample		
Stand Your Ground	0.098	0.091*
	(0.071)	(0.051)
R^2	0.937	0.408
Whites		
Stand Your Ground	0.152	0.155**
	(0.096)	(0.074)
R^2	0.935	0.291
Blacks		
Stand Your Ground	0.070	0.040
	(0.077)	(0.066)
R^2	0.873	0.653
White Males		
Stand Your Ground	0.208*	0.187**
	(0.106)	(0.081)
R^2	0.936	0.342
White Females		
Stand Your Ground	0.085	0.057
	(0.113)	(0.094)
R^2	0.854	0.267
Black Males		
Stand Your Ground	0.080	0.044
	(0.078)	(0.069)
R^2	0.872	0.744
Black Females		
Stand Your Ground	−0.038	0.000
	(0.094)	(0.081)
R^2	0.750	0.409
Number of observations	500	500

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors that are clustered by state are in parentheses. * and ** indicate that the estimate is statistically significant at the 0.10 and 0.05 levels, respectively. Both specifications include state and year fixed effects, state-specific trends, and time-varying characteristics of states.

and 3B. In particular, the marginal effects for white males are 0.148 for the OLS model and 0.186 for the fixed effects Poisson model and are both statistically significant. Consistent with our earlier results, none of the estimates for other race and gender categories are estimated with statistical precision.

B. Placebo Analysis

Next we perform a placebo analysis by assessing the relationship between SYG laws and deaths that should not be related to self-defense laws, including deaths from nonhomicide and nonsuicide related causes. These deaths primarily include those caused by cardiovascular events, cancer, and motor vehicle accidents, which arguably should not be affected by gun laws. To implement this analysis, we grouped all nonhomicide and nonsuicide related deaths together and estimated our models using this variable as the outcome measure. The results from this placebo analysis are presented in Table 7. As expected, the estimates are small in magnitude and imprecisely estimated. These findings suggest that our findings on homicides are unlikely to reflect a general increase in overall deaths.

Additionally, we also estimate our models with the indicator of SYG defined as if the laws became effective two years prior to their actual date of passage. As expected, the estimates from this exercise are much smaller, and the statistical significance is largely wiped out. We also perform this analysis using a one-year lag instead of two and reached a similar conclusion. Finally, following Cheng and Hoekstra (2013), we include in the model specified in Equation 1 an indicator for two years prior to the effective date of the SYG laws. The estimates on this indicator are again small and imprecisely estimated. For example, the estimate in the panel for white males is –0.034 (p-*value* = 0.521) in Table 3A and –0.049 (p-*value* = 0.193) in Table 3B.

C. SYG Laws and Firearm Related Injuries

If the SYG laws are causing an increase in homicides, it is plausible that they might cause an even larger increase in firearm-related injuries. According to data from the NCHS, for each death by injury in 2009, there were about 11 times as many hospitalizations and 182 times as many emergency room department visits. Accordingly, an additional way to assess the validity of our findings is to examine whether the SYG laws are also related to firearm-related injuries that result in hospitalizations. This is the first study to consider the effect of SYG laws on firearm-related injuries.

Some basic insights into the relationship between SYG laws and injuries can be gained by utilizing two databases collected by the Healthcare Cost and Utilization Project (HCUP). The HCUP services a family of healthcare databases developed through a federal–state–industry partnership, which is sponsored by the U.S. Agency for Healthcare Research and Quality (AHRQ). Specifically, for emergency room department visits, we use the State Emergency Department Databases (SEDD), which provides discharge information on all emergency department visits that do not result in admission to a hospital. For hospital discharges, we rely upon the State Inpatient Databases (SID), which hold the universe of inpatient discharge abstracts from individual states. In order for a discharge record to exist, the patient must have been admitted

Table 7
Estimates of the Impact of the Stand Your Ground Law on All Non-Homicide Deaths—Placebo Analysis

	OLS	Fixed Effects Poisson
Full Sample		
Stand Your Ground	–0.009	–0.003
	(0.006)	(0.003)
R^2	0.974	0.892
Whites		
Stand Your Ground	–0.006	–0.001
	(0.006)	(0.004)
R^2	0.971	0.878
Blacks		
Stand Your Ground	–0.017	–0.012
	(0.011)	(0.008)
R^2	0.902	0.608
White Males		
Stand Your Ground	–0.005	–0.001
	(0.007)	(0.004)
R^2	0.962	0.809
White Females		
Stand Your Ground	–0.007	–0.001
	(0.007)	(0.004)
R^2	0.959	0.808
Black Males		
Stand Your Ground	–0.022*	–0.017
	(0.013)	(0.011)
R^2	0.852	0.481
Black Females		
Stand Your Ground	–0.012	–0.007
	(0.010)	(0.009)
R^2	0.847	0.459
Number of observations	6,732	6,732

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors that are clustered by state are in parentheses. * indicates that the estimate is statistically significant at the 0.10 level. Both specifications include state and month*year fixed effects, state-specific time trends, and time-varying state characteristics.

to the hospital. Thus, the SID typically contains records of patients who require more intensive treatment or for whom treatment cannot be provided on an out-patient basis. Note that our SEDD and SID data only cover the states of California, Florida, Hawaii, Iowa, Maryland, New Jersey, and Wisconsin and for the period between 2005 and 2009.[31] Note that among these states, Florida is the only one which has passed a SYG law during our analysis period. Because Florida passed its SYG law in October 2005, we only have three pretreatment quarters of data for Florida as opposed to 17 quarters of posttreatment data. Despite these limitations, we believe that useful insights still can be gained by considering a state that comprises a significant proportion of all of the justifiable homicides and one that has been at the heart of the controversy surrounding these laws, yet the analysis with HCUP data should be viewed with caution due to data limitations.

The injury categories in the HCUP data are based on ICD-9 codes, and we focus on external injuries that are inflicted purposefully, excluding firearm injuries that are inflicted accidently or by police officers. We identified the firearm-related injuries in the SEDD and SID databases for each of our sample states and aggregated the counts at the quarterly level. Then we computed the quarterly total counts of emergency room visits (from SEDD) and hospital discharges (from SID) to generate our measure of injuries.[32] The rate of firearm injuries is higher in Florida than the average of the six control states. This descriptive evidence is suggestive that the SYG law might have indeed played a role in increasing injuries in Florida.

In order to gain additional insights on the impact of SYG laws on injuries, we also draw data from the National Inpatient Sample (NIS) database. Like SEDD/SID, the NIS is part of the family of databases developed by the HCUP. Furthermore, it is the largest publicly available inpatient healthcare database in the United States, yielding a sample of approximately 20 percent of U.S. community hospitals covering around 8 million inpatient stays. Although the NIS covers over 97 percent of the U.S. population, the included states are limited to those voluntarily participating in the project and not all of these states report data on race, limiting the data for our purposes to 12 states. Of these 12 states, six passed SYG laws and six did not. The SYG states are Florida, Kansas, Missouri, South Carolina, Tennessee, and Texas. The non-SYG states are Hawaii, Iowa, Maryland, Massachusetts, New Jersey, and New York. Similar to the SEDD/SID analysis, injuries are identified using ICD-9 codes indicating a purposely inflicted firearm injury not caused by a law enforcement official. State-Quarterly totals accounting for the complex survey design of the NIS are then calculated for 2003 to 2010.[33]

The results from our empirical analysis for the impact of SYG laws on firearm-related injuries are presented in Table 8. The estimates on the SYG law indicator are shown in Column 1 for the SEDD/SID analysis and in Column 2 for the NIS analysis. Because we

31. There are a few reasons for limiting the injury analysis to these states. First, individual states voluntarily subscribe to the HCUP project, so not every state provides these data. Second, some states do not provide information on patient race, which is crucial for our analyses. Third, there is a financial cost, which makes it prohibitively expensive to acquire data for all available states for all years.

32. Because the SEDD records include cases that did not result in a hospital admission, adding counts from SEDD and SID would not result in double-counting.

33. We limit the analysis period to post-2002 because of a change in the sampling and weighting of the NIS in 2002.

Table 8
OLS Estimates of the Impact of Stand Your Ground Law on Firearm-Related Injuries

	SEDD/SID	NIS
Full Sample		
Stand Your Ground	0.022	0.173
	(0.515)	(0.662)
R^2	0.972	0.664
Whites		
Stand Your Ground	0.118**	0.361
	(0.048)	(0.434)
R^2	0.919	0.599
Blacks		
Stand Your Ground	0.017	0.299
	(0.448)	(0.440)
R^2	0.864	0.607
White Males		
Stand Your Ground	0.187**	0.577
	(0.047)	(0.220)
R^2	0.924	0.578
White Females		
Stand Your Ground	–0.225	0.288
	(0.392)	(0.404)
R^2	0.749	0.607
Black Males		
Stand Your Ground	–0.037	–0.256
	(0.230)	(0.534)
	0.876	0.427
Black Females		
Stand Your Ground	0.607***	0.201
	(0.000)	(0.674)
R^2	0.500	0.430
Number of observations	136	384

Notes: Each cell presents the coefficient on the indicator for Stand Your Ground Law. Standard errors are calculated using wild bootstrap procedure suggested by Cameron et al. (2008) and Cameron and Miller (2015). *P*-values obtained this procedure are presented in parentheses. **, and *** indicate that the estimate is statistically significant at the 0.05, and 0.01 levels, respectively. Both specifications include state and year*quarter fixed effects, state-specific time trends, and time-varying state characteristics.

have a small number of clusters in both of these data sets, we perform the wild cluster bootstrap method proposed by Cameron et al. (2008) and Cameron and Miller (2015). Both specifications include state and year*quarter fixed effects, state-specific time trends, and time-varying state characteristics. As shown in Table 8, the coefficients are not estimated with a lot of precision, which is not surprising given the small sample sizes due to limited number of states and the fact that these data are quarterly instead of monthly. Yet the estimates in Column 1 indicate that SYG laws are associated with an increase in inpatient hospitalizations for firearm-related injuries among whites and this increase is largely driven by injuries among white males. There also appears to be a positive impact of SYG laws on the injuries among black females in Florida based on the analysis with SEDD/SID data set.[34] The estimates using the NIS data set are indicative of a positive impact of SYG laws on injuries with a particularly large effect size on white males, although none of the estimates are statistically significant, again possibly due to a small sample size.

We also perform the injury analysis using fixed effects Poisson. However, note that we cannot implement the wild bootstrap method because Cameron and Miller (2015) advise against using it for nonlinear models. Instead, we estimate these models clustering standard errors at the state level. Keeping this limitation in mind, the estimates from the fixed effects Poisson are very much in line with those in Table 8. In particular, there are positive and statistically significant effects of SYG laws on injuries for the full sample, whites, and white males in the SEDD/SID analysis. Furthermore, the estimates for whites and white males are positive and statistically significant in the NIS analysis.

Taken together, the results from the injury analysis summarized above generally support our findings from the homicide analysis. Based on the injury analysis, we argue that the passage of SYG laws has likely contributed to a general increase in gun violence not only resulted in firearm-related homicides, but also injuries.

VI. Conclusions

The controversies surrounding a recent wave of self-defense laws introduced by a large number of states in the United States have captured the nation's attention recently. These laws allow citizens to take measures of self-defense, including the use of lethal weapons, without having to retreat first. Despite significant implications of these laws for public safety, there has been only one comprehensive analysis of them. Specifically, a recent paper by Cheng and Hoekstra (2013) provides a rigorous evaluation of the impact of expanded self-defense laws on violent crime and homicides using data from the UCR and exploiting the variation in the passage of these laws across states and over time. Their findings indicate that not only do these laws not provide any deterrence against crime measured by burglary, robbery, or aggravated assault, they lead to a significant increase in the number of reported homicides and nonnegligent manslaughters.

34. The large estimate on black females is consistent with the hypothesis that the SYG laws are associated with an increase in firearm-related injuries for this demographic group. However, this finding is not supported with our earlier analysis on homicides and is therefore likely to be specific to Florida.

In this paper, we perform a fresh analysis of these expanded self-defense laws on homicides and injuries. While our empirical methodology is similar to the one employed by Cheng and Hoekstra (2013), the two analyses differ from each other in several respects. Most importantly, we use data from the Vital Statistics for the homicide analysis. Accordingly, we are able to conduct our analysis at the monthly level. Our measure of expanded self-defense law is also slightly different from the one adopted by Cheng and Hoekstra (2013). Specifically, our treatment states include those that removed the duty to retreat in any place a person has a legal right to be, while Cheng and Hoekstra (2013) takes a broader approach by considering states that removed the duty to retreat somewhere outside home. We also pay particular attention to whether there are differences in the relationship between these laws and homicides by race and gender. Finally, we perform an auxiliary analysis to assess the impact of SYG laws on firearm-related injuries using data from the SEDD/SID and NIS.

Taken together, our results lend strong support to the findings of Cheng and Hoekstra (2013). Specifically, we find consistent evidence to indicate that SYG laws are associated with an increase in homicides by about 30 per month. We also find evidence to suggest that the SYG laws are associated with increases in both justifiable and nonjustifiable homicides, using data from the Supplemental Homicide Reports. We obtain no evidence that these laws are related to nonhomicide deaths, which makes sense given that one would expect no association between gun laws and these types of deaths. Finally, despite its limitations, our analysis on injuries using two data sets produce results that are largely consistent with notion that the SYG laws have caused an increase in violence captured by both homicide and injuries. In summary, our results provide no evidence to support the view argued by the supporters of the SYG laws that these laws would deter crime. On the contrary, we find consistent evidence to support the opposite view; these laws would cause more violence that results in an increased number of homicides and injuries.

Those who support the expanded self-defense laws argue that they would make the public safer by deterring crime and providing law-abiding citizens with greater freedom and scope to use justifiable lethal force for self-defense. These laws indeed lower the expected cost of using lethal force to protect oneself. Accordingly, there may be many situations in which these laws work in the way they are intended for and save lives. Unfortunately, they also reduce the cost of escalating violence in general, which might lead to situations in which individuals are quick to resort to the use of lethal force even when there is no imminent danger. Our findings support the latter view indicating that there is a net increase in the number of presumably innocent individuals being killed as well as injured as a result of these laws.

References

Bedard, Kelly, and H.E. Frech III. 2009. "Prison Health Care: Is Contracting Out Healthy?" *Health Economics* 18(11):1248–60.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1):249–75.

Bushway, Shawn, and Peter Reuter. 2008. "Economists' Contribution to the Study of Crime and the Criminal Justice System." *Crime and Justice* (37)1:389–451.

Cameron, Colin, Jonah Gelbach, and Douglas Miller. 2008. "Bootstrap-based Improvements for Inference with Clustered Errors." *The Review of Economics and Statistics* 90(3):414–27.
Cameron, Colin, and Douglas Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50(2):317–72.
Cameron, Colin, and Pravin Trivedi. 2005. *Microeconometrics: Methods and Applications.* Cambridge: Cambridge University Press.
Catalfamo, Cristine. 2006. "Stand Your Ground: Florida's Castle Doctrine for the Twenty-First Century." *Rutgers Journal of Law & Public Policy* 4(3):504–45.
Cheng, Cheng, and Mark Hoekstra. 2013. "Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine." *Journal of Human Resources* 48 (3):821–54.
Cohen, Amy, Deborah Azrael, and Matthew Miller. 2014. "Mass Public Shootings Are Increasing in Frequency." *Mother Jones*, October 15. http://www.motherjones.com/politics/2014/10/mass-shootings-increasing-harvard-research.
Cole, David. 1999. *No Equal Justice: Race and Class in the American Criminal Justice System.* New York City: New Press.
Duggan, Mark. 2001. "More Guns, More Crime." *Journal of Political Economy* 109(5):1086–114.
Federal Bureau of Investigation. 2010. *Uniform Crime Reports: Crime in the United States.* Washington, D.C: The Federal Bureau of Investigation.
Taskforce on Citizen Safety and Protection. 2012. *Report of the Governor's Task Force on Citizen Safety and Protection FINAL.* Tallahassee, Fla.: Executive Office of Governor Rick Scott.
Formby, Brandon. 2006. Bill to Seek "Castle Doctrine" Crime Protection. *Dallas Morning News,* October 13.
Government Accountability Office. 2012. "Gun Control: States' Laws and Requirements for Concealed Carry Permits Vary across the Nation." GAO-12-717, July. Washington, D.C.: GAO.
Harris, Anthony, Stephen Thomas, Gene Fisher, and David Hirsch. 2002. "Murder and Medicine: The Lethality of Criminal Assault 1960–1999." *Homicide Studies* 6(2):128–66.
Hundley, Kris, Susan Martin, and Connie Humburg. 2012. "Florida 'Stand Your Ground' Yields Some Shocking Outcomes Depending on How Law Is Applied." *Tampa Bay Times,* June 1. Accessed on June 3, 2012 at http://www.tampabay.com/news/publicsafety/crime/article1233133.ece.
Jansen, Steven, and Elaine Nugent-Borakove. 2007. *Expansions to the Castle Doctrine: Implications for Policy and Practice.* Washington, D.C.: Association of Prosecuting Attorneys.
Kleck, Gary. 1988. "Crime Control Through the Private Use of Armed Force." *Social Problems* 35 (1):1–21.
Kleindienst, Linda. 2005. "Welcome to Florida, but Look Out: Groups Alert Guests to Law, Deadly Force." *Sun Sentinel*, September 26. Accessed on May 20, 2012 at http://articles.sun-sentinel.com/2005-09-26/news/0509250378_1_deadly-force-abseals-prosecutors-and-courts.
Levin, Benjamin. 2010. "A Defensible Defense?: Reexamining Castle Doctrine Statutes." *Harvard Journal on Legislation* 47(2):523.
Lind, Dara. 2015. "Who Owns Guns in America? White Men, Mostly." *Vox Policy and Politics*, December 4. http://www.vox.com/2015/12/4/9849524/gun-race-statistics
Loftin, Colin, David McDowall, and Matthew D Fetzer. 2008. "A Comparison of SHR and Vital Statistics Homicide Estimates for US Cities." *Journal of Contemporary Criminal Justice* 24 (1):4–17.
Megale, Elizabeth. 2010. "Making Murder Legal: How Laws Expanding Self-Defense Allow Criminals to 'Get Away with Murder.'" Working Paper. Accessed May 20, 2012 from http://works.bepress.com/elizabeth_megale/1.
Montgomery, Ben, and Colleen Jenkins. 2010. "Five Years Since Florida Enacted "Stand-Your-Ground" Law, Justifiable Homicides are Up." *Tampa Bay Times*, October 15. Accessed on May 15, 2012 at http://www.tampabay.com/news/publicsafety/crime/article1128317.ece.

Morin, Rich. 2014. "The Demographics and Politics of Gun-Owning Households." *Fact Tank.* Pew Research Center, July 15. http://www.pewresearch.org/fact-tank/2014/07/15/the-demographics-and-politics-of-gun-owning-households/

Mustard, David B. 2001. "Racial, Ethnic, and Gender Disparities in Sentencing: Evidence from the US Federal Courts." *Journal of Law and Economics* 44.1:285–314.

NRA-ILA. 2006. "Fortifying the Right to Self-Defense." *News & Issues*, February 6. Accessed on May 12, 2011 at http://nraila.org/news-issues/fact-sheets/2006/fortifying-the-right-to-self-defense.aspx.

Osgood, Wayne. 2000. "Poisson-based Regression Analysis of Aggregate Crime Rates." *Journal of Quantitative Criminology* 16(1):21–43.

Roman, John. 2013. "Race, Justifiable Homicides, and Stand Your Ground Laws: Analysis of FBI Supplementary Homicide Report Data." Research Report for The Urban Institute, July 26.

Sampson, Robert, Stephen Raudenbush, and Felton Earls. 1997. "Neighbourhoods and Violent Crime: A Multilevel Study of Collective Efficacy." *Science* 277(5328):918–24.

Palazzolo, Joe, and Rob Barry. 2012. "More Killings Called Self Defense." *Wall Street Journal,* April 2.

Project America. 2008. "Firearms: Ownership: Race." Accessed on May 20, 2012 at http://www.project.org/info.php?recordID=272.

Rios, Victor. 2012. "The Need to Repeal 'Stand Your Ground' Laws." *The Opinion Pages, The New York Times*, March 21.

Rokaw, William, James Mercy, and Jack Smith. 1990. "Comparing Death Certificate Data with FBI Crime Reporting Statistics on US Homicides." *Public Health Reports* 105(5):447.

Weaver, Zachary. 2008. "Florida's Stand Your Ground Law: The Actual Effects and the Need for Clarification." *Miami Law Review* 63(1):395–430.

Weiner, David, Byron Lutz, and Jens Ludwig. 2009. "The Effects of School Desegregation on Crime." National Bureau of Economic Research Working Paper No. 15380, September.

Wiersema, Brian, Colin Loftin, and David McDowall. 2000. "A Comparison of Supplementary Homicide Reports and National Vital Statistics System Homicide Estimates for US Counties." *Homicide Studies* 4(4):317–40.