Effects of Laws Expanding Civilian Rights to Use Deadly Force in Self-Defense on Violence and Crime: A Systematic Review

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See also Burris, p. 559.

Background. Since 2005, most US states have expanded civilian rights to use deadly force in self-defense outside the home. In most cases, legislation has included removing the duty to retreat anywhere one may legally be, commonly known as stand-your-ground laws. The extent to which these laws affect public health and safety is widely debated in public and policy discourse.

Objectives. To synthesize the available evidence on the impacts and social inequities associated with changing civilian rights to use deadly force in self-defense on violence, injury, crime, and firearm-related outcomes.

Search Methods. We searched MEDLINE, Embase, PsycINFO, Scopus, Web of Science, Sociological Abstracts, National Criminal Justice Reference Service Abstracts, Education Resources Information Center, International Bibliography of the Social Sciences, ProQuest Dissertations and Theses, Google Scholar, National Bureau of Economic Research working papers, and SocArXiv; harvested references of included studies; and consulted with experts to identify studies until April 2020.

Selection Criteria. Eligible studies quantitatively estimated the association between laws that expanded or restricted the right to use deadly force in self-defense and population or subgroup outcomes among civilians with a comparator.

Data Collection and Analysis. Two reviewers extracted study data using a common form. We assessed study quality using the Risk of Bias in Nonrandomized Studies of Interventions tools adapted for (controlled) before–after studies. To account for data dependencies, we conducted graphical syntheses (forest plots and harvest plots) to summarize the evidence on impacts and inequities associated with changing self-defense laws.

Main Results. We identified 25 studies that estimated population-level impacts of laws expanding civilian rights to use deadly force in self-defense, all of which focused on stand-your-ground or other expansions to self-defense laws in the United States. Studies were scored as having serious or critical risk of bias attributable to confounding. Risk of bias was low across most other domains (i.e., selection, missing data, outcome, and reporting biases). Stand-your-ground laws were associated with no change to small increases in violent crime (total and firearm homicide, aggravated assault, robbery) on average across states. Florida-based studies showed robust increases (24% to 45%) in firearm and total homicide while self-defense claims under stand-your-ground law were more often denied when victims were White, especially when claimants were racial minorities.

Author's Conclusions. The existing evidence contradicts claims that expanding self-defense laws deters violent crime across the United States. In at least some contexts, including Florida, stand-your-ground laws are associated with increases in violence, and there are racial inequities in the application of these laws.

Public Health Implications. In some US states, most notably Florida, stand-your-ground laws may have harmed public health and safety and exacerbated social inequities. Our findings highlight the need for scientific evidence on both population and equity impacts of self-defense laws to guide legislative action that promotes public health and safety for all.

Trial Registration. Open Science Framework (https://osf.io/uz68e). (*Am J Public Health*. 2021;111:e1–e14. https://doi.org/10.2105/AJPH.2020.306101)

AJPH April 2021, Vol 111, No. 4

PLAIN-LANGUAGE SUMMARY

Since 2005, most of the United States have adopted stand-your-ground laws. These laws expand people's right to use deadly force in self-defense anywhere they may legally be without first attempting to retreat. To understand how such laws may affect public health and safety, we searched for all evidence on the impacts of laws that expand or restrict the right to use deadly force in self-defense. We identified 25 studies that examined the impacts of stand-your-ground laws and other expansions to self-defense laws on violence, crime, and firearm use and demand in the United States. An additional 7 studies looked at the outcomes of self-defense cases involving stand-your-ground claims in Florida. Evidence from our review suggests that expanding people's right to use deadly force has not reduced crime on average across the United States. In at least some US states, most notably Florida, stand-your-ground laws have been associated with increases in homicides and there has been racial bias in the application of legal protections. More research is needed on how the impacts of these laws on violence, injury, and criminal justice differ by race and gender across states. Our results demonstrate the importance of using scientific evidence on how laws may have an impact on the overall population and social justice in law and policymaking.

The extent to which civilians can justifiably kill or injure others has been the topic of religious, philosophical, and legal discussion for several centuries.[1] Traditional common law allows citizens to use deadly force in self-defense only when safe retreat is impossible—except when in one's home, where there is no duty to retreat (otherwise known as the castle doctrine).[2] Recent amendments to self-defense laws in the United States have reinvigorated this debate.[3] Beginning with Florida in 2005, 26 US states adopted stand-your-ground (SYG) statutes over the past 15 years, which remove civilian duty to retreat anywhere one may legally be (and, in some cases, provide immunity from civil liability and the presumption of reasonable fear).[4] In addition to these states and Utah, which passed a similar law in 1994, 8 states have SYG by case law, and 7 states have expanded castle doctrine laws (sometimes referred to as "limited" SYG laws) that remove the duty to retreat in certain places outside the home (e.g., the workplace; see Appendix Table A [available as a supplement to this article at https://ajph.org] for a summary).[2,5] Advocates maintain that these laws strengthen legal protections for law-abiding citizens to defend themselves and, in some cases, may deter predatory crime.[6] Critics stress that expanding laws to use deadly force threatens public health and safety by encouraging the use of violence and vigilante justice, likely to exacerbate social inequities in violence and criminal justice outcomes.[7]

Changes to self-defense laws create an opportunity to assess how the relaxation (or strengthening) of legal restrictions on the use of deadly force affects violence, injury, crime, and related social inequities. The prevalence of gun ownership and gun violence in the United States amplifies the ability to use deadly force and appears to be a predictor of states adopting SYG laws.[3] However, understanding the consequences of relaxing legal restrictions on civilian use of deadly violence is important to public health and safety beyond the US context: governments worldwide (e.g., Australia, Canada, the United Kingdom) have received petitions for the introduction of US-style relaxations to self-defense laws. We therefore aimed to systematically review all quantitative research available internationally on the impacts of laws altering civilian rights to use deadly force in self-defense on violence, injury, firearm, and criminal justice outcomes and to examine whether there are differences in impacts among sociodemographic groups (e.g., by race or gender).

METHODS

We searched for published and unpublished studies in 10 databases: MEDLINE, Embase, PsycINFO, Scopus, Web of Science, Sociological Abstracts, National Criminal Justice Reference Service Abstracts, Education Resources Information Center, International Bibliography of the Social Sciences, and ProQuest Dissertations and Theses (protocol registered on Open Science Framework, https://osf.io/uz68e). In consultation with an information specialist (University of Oxford Bodleian Libraries), we searched for stand your ground, SYG, shoot first, line in the sand, self-defence, self-defense, deadly force, legal immunity, castle law, castle doctrine, lethal force, or reasonable force (Appendix Box A). We conducted directed searches of Google Scholar, National Bureau of Economic Research working papers, and SocArXiv; harvested references from relevant studies and reviews; set up search alerts; and consulted experts in the field via author networks for any additional studies. Study searching and inclusion proceeded until April 2020.

Five reviewers (A. Y., B. L., G. M. T, A. P., and D. H.) screened titles and abstracts, including all quantitative studies about self-defense laws except studies on state or military violence. All reviewers first independently screened 200 randomly selected records to establish consistency; the remaining records were then randomly divided among the reviewers. A second reviewer (A. Y. or B. L.) double-screened a random 10% of the excluded studies to ensure sensitivity. Three reviewers (A. Y., B. L., and D. H.) screened all potentially relevant full texts; M. D. E. double-screened all decisions. Discrepancies were discussed and resolved with A. Y. and D. H. We included studies that quantitatively estimated the association between laws that expanded or restricted the right to use deadly force in self-defense and population or subgroup outcomes. Studies that had any comparator (including before implementation) and investigated any outcome among civilians were included. There were no language, location, or time restrictions.

Two reviewers (A. Y. and M. D. E.) extracted data on publication information, design, methods, and effect estimates and appraised study quality. When studies provided more than 1 intervention effect estimate for any given outcome, we followed a decision-making algorithm based on previous reviews and guidelines[8–10]:

1. Extract the most adjusted estimate.
2. Extract the model estimate most appropriate for count or rate outcomes as relevant (e.g., Poisson or negative binomial models).
3. Extract the estimate for an immediate, permanent intervention effect (first order) in autoregressive integrated moving average models for comparability with other studies.
4. If multiple estimates equally meet 1 to 3, extract the most- and least-liberal estimates (based on effect size and precision) to determine the range of estimates.

We appraised included studies using the Risk of Bias in Nonrandomized Studies of Interventions (ROBINS-I) tools for before–after and controlled before–after studies.[11] ROBINS-I evaluates bias by confounding, selection of study units, classification of interventions, deviations from intended interventions, missing data, outcome measurement, and results reporting. We evaluated risk of bias in each study based on the highest-risk outcome to be conservative (e.g., justifiable homicides—which are affected by SYG laws, making this analysis more susceptible to bias—over total homicides). Ratings were based on extracted analyses: we did not evaluate analyses presented in figure format without extractable data.

Meta-analysis was not possible as included studies used the same data sources (violating assumptions of data independence). We used graphical synthesis methods and followed reporting guidelines for systematic syntheses without meta-analysis.[12] Most results were available in, or could be transformed to, percent change in outcome rates, which we used as our standardized metric. We graphed these percent changes for each outcome by geographic region wherever there were 2 available estimates by using forest plots. We exponentiated log estimates from log-linked models (producing risk or rate ratios) and estimates from linear models with log-transformed outcomes (producing geometric mean ratios)—both relative effect estimates that could be synthesized in a single plot.

We summarized the results from subgroup analyses using tables and harvest plots.[13] We focused on subgroup characteristics in PROGRESS-plus—a Cochrane framework for the analysis of health inequities.[14] PROGRESS characteristics are place of residence; race/ethnicity, culture, and language; occupation; gender or sex; religion; education; socioeconomic status; and social capital. "Plus" refers to personal characteristics associated with discrimination (e.g., age), relationship features, and time-dependent relationships. Harvest plots highlight where the evidence suggests a positive, negative, or null gradient for inequities in outcomes and where evidence gaps exist. Null associations were defined as small or variable estimates centered around the point of no effect; positive or negative associations were large or consistent estimates in the positive or negative direction, respectively.

RESULTS

After duplicates were removed, we screened 20 987 titles and abstracts, excluding 20 706 records as irrelevant (Appendix Figure A). After title and abstract screening, we identified 19 additional studies through reference harvesting, search alerts, and expert consultation and therefore screened 210 full texts. We included 25 studies that had main effect estimates of expanding civilian rights to use deadly force in self-defense.[15–38] An additional 7 studies only investigated outcome distributions (usually judicial rulings) of Florida self-defense cases by subgroup characteristics—we included these to supplement our overview of the equity impacts of self-defense laws.[39–45]

Study Characteristics

Table 1 summarizes the characteristics of the 25 main effect studies; Appendix Table B summarizes each study's characteristics. All studies were US-based: most investigated SYG laws (84%); 3 studies investigated SYG laws and expanded castle doctrine laws applying to property outside the home. The remaining study investigated mainly castle doctrine laws up to 2005, before most SYG statutes.[6] All but 1 study[25] controlled for covariates in analyses: 8 only accounted for seasonal, secular, or regional or state trends; 16 also accounted for time-varying covariates (e.g., other laws; see Appendix Table C, for summary of covariates). Eighteen studies compared treated versus untreated units across the United States; 9 conducted a before–after analysis of a single geographic unit (e.g., a state). Only 6 of the main effect studies conducted subgroup comparisons (most commonly by race). Some studies only investigated subgroup samples: adolescents,[19] urban counties,[18] states that passed laws within a restricted time frame (2005–2007),[22] or eastern states.[36] These were included as main effect studies because they did not compare subgroups on the sampling characteristic, but noted as potential sources of heterogeneity and evaluated for selection bias as relevant.

Appendix Table D summarizes classifications of states as "treated" versus "untreated" in US-wide studies of laws expanding the right to use deadly force outside the home. Of 32 states that were classified as "treated," only 9 were consistently analyzed as treated across all studies: Arizona, Georgia, Indiana, Kentucky, Louisiana, Michigan, Oklahoma, South Carolina, and Texas. All of these states implemented statutory SYG

TABLE 1— Summary of Characteristics of the 25 Included Studies on the Effects of Laws Expanding Civilian Rights to Use Deadly Force in Self-Defense: 2010–2019

Characteristic	No. (%)
Date of publication	
2010–2014	10 (40)
2015–2019	15 (60)
Type of publication	
Peer-reviewed journal article	20 (80)
Not in a peer-reviewed journal	5 (20)
Working paper	2 (33)
Book	1 (17)
Dissertation	1 (17)
Preprint	1 (17)
Study design[a]	
Controlled before–after study	15 (60)
Controlled interrupted time series study	5 (20)
Interrupted time series study	4 (16)
Case–control study	1 (4)
Comparator	
Preintervention and "untreated" area	12 (48)
Preintervention	7 (28)
Synthetic control	4 (16)
Untreated area	2 (8)
Type of law	
SYG	21 (84)
SYG and expanded castle doctrine laws	3 (12)
Castle doctrine laws	1 (4)
Geographic unit of analysis	
State	22 (88)
County	2 (8)
City	1 (4)
Intervention unit[b]	
Subset of "treated" units	17 (68)
Single unit[c]	10 (40)
Analytic method	
Fixed-effects model	11 (44)
Mixed-effects model	2 (8)
Segmented regression	4 (16)
ARIMA	4 (16)
Synthetic control analysis[d]	1 (4)
Between-group comparison only	2 (8)
Frequency of data intervals[b]	
Annual	16 (64)
Monthly	8 (32)

Continued

TABLE 1— Continued

Characteristic	No. (%)
Quarterly	1 (4)
Daily	1 (4)
Any covariates	
No	1 (4)
Yes	24 (96)
Time-varying covariates	16 (67)
Only seasonal or secular trends	8 (33)
Sensitivity or falsification analysis	
No	8 (32)
Yes[b]	17 (68)
Robustness to model specification	7 (41)
Negative control	6 (35)
Control series	6 (35)
Variation in laws of interest[e]	4 (24)
Subgroup or equity analysis	
No	19 (76)
Yes[b]	6 (24)
Race	4 (67)
Location	2 (33)
Age	1 (17)
Gender	1 (17)
Race*location	1 (17)
Race*gender	1 (17)

Note. ARIMA = autoregressive integrated moving average model; SYG = stand your ground. The sample size was 25 studies.

[a]We defined interrupted time series studies as those that estimated the underlying time trends in the outcome (based on preintervention trends) as part of the counterfactual (e.g., using segmented regression), distinguished from studies that compared pre- versus postimplementation outcome means or only analyzed variation within time or geographic units (as in difference-in-difference designs or fixed effects analyses of panel data).[46]

[b]Does not add to 100% as at least 1 study analyzed multiple categories.

[c]Single states included Florida (n = 5), Alabama (n = 1), Arizona (n = 2), Georgia (n = 1), Indiana (n = 1), Kansas (n = 1), Kentucky (n = 1), Louisiana (n = 1), Michigan (n = 1), Mississippi (n = 1), Oklahoma (n = 2), South Carolina (n = 2), South Dakota (n = 1), and Texas (n = 2).

[d]Comparison of pre– and post–mean square prediction error ratios for the observed outcome of a unit and its synthetic control for all treated and untreated units.

[e]Four studies accounted for variations in state laws that expanded civilian rights to use deadly force. Two studies included dummy control variables in sensitivity analyses representing the inclusion of different legal provisions (i.e., duty to retreat anywhere one may legally be, requirement of imminent fear of bodily harm, removal of civil liability, or presumption of reasonable fear).[34,37] A third study ran sensitivity analyses with different formulations of the intervention variable (i.e., restricted to laws that removed the duty to retreat anywhere one may legally be; restricting to laws that removed civil liability) or that compared different formulations or circumstances of the laws (i.e., expanded castle doctrine laws in states that previously required duty to retreat versus those that did not; expanded castle doctrine laws that include a presumption of reasonable fear versus those that do not).[17] The fourth study adopted a "multiple case study approach" to estimate state-specific effects of SYG laws.[22]

laws between 2006 and 2011. Differences in state classifications were often attributable to variations in study time frames (including whether different states had adopted SYG laws within the study's time frame) and whether authors analyzed only SYG laws or additionally expanded castle doctrine laws. Only 2 studies defined states that adopted SYG in practice (by case law) as treated: Illinois[21] and Oregon.[32] See Appendix Box B, for further discussion.

All studies were at serious or critical risk of bias attributable to confounding, by virtue of being nonrandomized and at risk for influence from simultaneously occurring events (Appendix Table E). Confounding ratings were affected by the likely extent of uncontrolled systematic differences between the intervention and comparator groups, adjusting for postintervention variables that could have been affected by the intervention (e.g., violent crime) and evidence of differences or no information on preintervention trends among intervention and control units. Risk of bias was rated as mostly low across most other domains (i.e., selection, missing data, outcome, and reporting biases). Appendix Table E presents the ratings for each study and Appendix Box C provides further discussion.

Main Effects

Forty outcomes—spanning deaths, injury, crime, unemployment, criminal justice, and firearm demand—were analyzed across the 25 main effect studies (Appendix Table F). The most common outcomes were firearm (n = 9 studies) and total homicides (n = 9). Sixteen outcomes were analyzed as negative controls (i.e., an outcome not hypothesized to change because of the intervention) in at least 1 study.

All study results are presented in the Appendix Tables G and H. We focus here on outcome categories with combinable estimates (i.e., transformable to the standardized metric) from at least 2 studies. Figure 1 shows the percent change in outcome before and

after the intervention for US-wide and Florida-specific studies. Panel A shows that, overall, expanding rights and protections for the use of deadly force in self-defense outside the home had an average null to small positive association with firearm homicide, total homicide, robberies, and aggravated assaults across the United States. A third noncombinable study on aggravated assaults found a negative but variable association with SYG[26]; as the authors noted, these results were not robust to different analyses and were unreliable given their placebo test (traffic fatalities) was also negative. Three studies analyzed nonfirearm homicide, 2 of which were combinable. Overall, associations were null; 1 study found a small, negative average association with nonfirearm homicides[31] (consistently conceptualized as a negative control[18,31,35]). Two combinable studies showed positive but variable associations between expanding self-defense laws and justifiable homicide rates across the United States,[17,37] in line with noncombinable US-[25] and Florida-based[24] evidence. A fifth study showed that homicides were more likely to be ruled justifiable in SYG versus non-SYG states.[30]

Three US-wide studies considered legal variations in expansions of state self-defense laws governing civilian use of lethal force.[17,34,37] All ran analyses accounting for different variants in these laws (e.g., removal of civil liability) and found consistent estimates of the impacts of expanding no duty to retreat outside the home. One study also compared results for states that previously required the duty to retreat and those that did not: the former showed greater increases in homicide rates than the latter after the implementation of expanded castle doctrine laws.[17]

Panel B (Figure 1) demonstrates robust positive associations between SYG in Florida and state firearm (+32% to +45%) and total homicide rates (+24% to +27%). In contrast, firearm suicide (conceptualized as a negative control) showed null or highly variable associations with SYG in 2 Florida-based studies.[19,23] The 2 studies with other state-specific estimates of impacts on homicide rates besides Florida were not conclusive,[15,22] apart from finding strong evidence of increased homicides in Michigan following implementation.[22]

Two studies investigated intermediary variables between laws expanding the right to use deadly force outside the home and violent outcomes—in other words, variables that may be on the causal pathway. These intermediary variables included firearm acquisition or demand and firearm ownership, each of which was measured with a proxy variable. Both studies used federal background checks to approximate firearm acquisition or demand and found that these increased on average across states after implementation.[28,34] One of the studies used the proportion of suicides attributable to firearms to approximate firearm ownership and found that this decreased following implementation.[34]

Equity Effects

Subgroup comparisons from main effect studies. Table 2 shows the subgroup comparisons from the main effect studies. Apart from 1 exception noted subsequently, no main effect study ran interaction analyses, which limits inferences. The results shown in Table 2 are, therefore, estimates of the impacts of expanding the right to use deadly force among each subgroup. SYG was associated with greater firearm and total homicide rate increases across more- versus less-urbanized counties in Florida.[33] Barring a few exceptions, SYG associations with homicide- and firearm-related outcomes tended to be positive among all race groups when intersected by gender or jurisdiction. McClellan and Tekin conducted an interaction analysis (the only study to do so for any outcome) for firearm homicides across the United States, finding consistent associations for all race and gender groups, with postimplementation increases greatest among White males (i.e., homicides in which White males were the victims).[37] For justifiable homicides, McClellan and Tekin also found stronger associations among White males in stratified analyses,[37] whereas Spanbauer, using longer and more frequent data, found stronger associations for cases of Blacks killing Blacks in urban areas.[32] Florida's SYG was associated with increases in firearm homicides for both Black and White people; among adolescents (ages 15–19 years), SYG was associated with greater increases for Black versus White people[19] whereas among adults (age ≥ 20 years), the opposite pattern was observed.[23]

Supplementary studies of the outcomes of stand-your-ground cases in Florida. As discussed previously, 7 supplementary studies only analyzed the characteristics or outcomes of cases (fatal or nonfatal) involving a SYG defense (herein referred to as SYG cases) in Florida—these analyses provide further insight into potential inequities in the application of SYG laws. Appendix Tables J and K detail study characteristics and results, respectively. Figure 2 summarizes the associations between PROGRESS-plus characteristics and conviction rulings: studies analyzed the associations between the odds of a case ending in

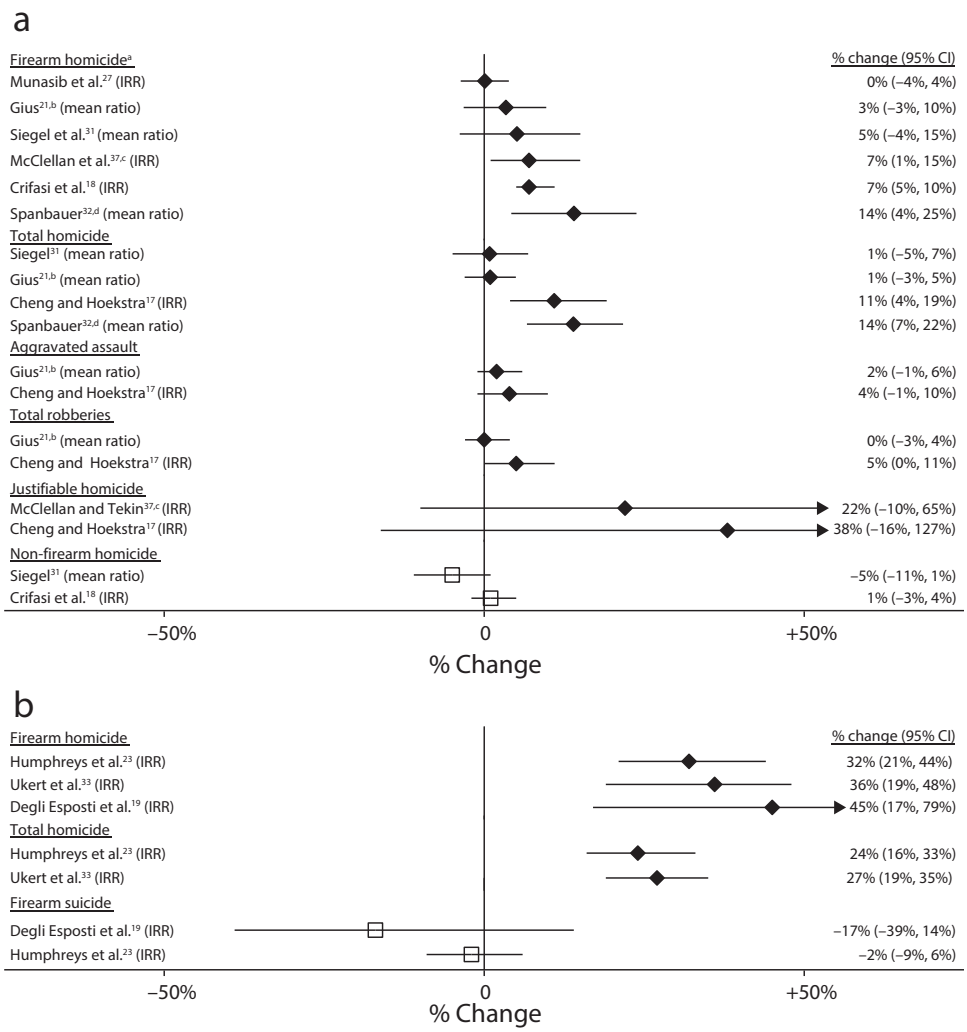

FIGURE 1— Graphical Synthesis Showing Percent Change in Violent Outcomes of Laws Expanding the Right to Use Deadly Force in Self-Defense Outside the Home in (a) United States–Wide Studies and (b) Florida-Specific Studies: 2013–2020

Note. CI = confidence interval; IRR = incidence rate ratio. We multiplied exponentiated coefficients by 100 and subtracted 1 to determine percent change. The type of estimate from which the percent change in outcome was derived is provided in parentheses. Black diamonds indicate associations estimated as intervention effects; white squares indicate associations estimated as a negative control. We excluded Lott[6] from our synthesis as this study analyzed a heterogeneous exposure compared with all other studies (mainly the implementation of castle doctrine laws up until 2005, before most stand-your-ground statutes were implemented). Results are presented in the Appendix Tables G and H (available as a supplement to this article at http://www.ajph.org). Updates to these analyses with data until 2012 were not extractable[47]; however, a later editorial suggests results were close to the null (–1.5%).[48]

[a]Including studies that analyzed gun deaths excluding suicide.

[b]The most conservative estimates are included for Gius.[21] The most liberal estimates are provided in the Appendix Table G—results were consistent, with liberal estimates suggesting null to small positive effects.

[c]There are 3 iterations of these analyses (2 working papers[49,50] and 1 peer-reviewed publication[37]). We included the latter in this review as the most up-to-date analysis.

[d]Study used inverse hyperbolic sine rather than log transformation.[32]

conviction and characteristics of the person claiming self-defense (i.e., the claimant) or the person injured or killed (i.e., the victim). Victim and claimant race as well as claimant gender were most commonly analyzed (n = 5 studies). SYG cases that involved racial minority victims ended in conviction less often than those with White victims, regardless of sample size and whether studies adjusted for other case characteristics (e.g., claimant race).

In addition, Roman[30] found that the proportion of homicides ruled justifiable was higher for all possible race pairs of victims and claimants in SYG versus non-SYG states in unadjusted comparisons. The study also showed similar patterns

TABLE 2— Results From the 6 Main Effect Studies on the Effects of Laws Expanding Civilian Rights to Use Deadly Force in Self-Defense With PROGRESS-Plus Subgroup Analyses: 2010–2019

Author (Year), Characteristic	Location	Firearm Homicide, IRR (95% CI)	Total Homicide, IRR (95% CI)	Justifiable Homicide, IRR (95% CI)	Nonjustifiable Homicide, IRR (95% CI)	Firearm-Related Injuries, IRR (95% CI)
Race/ethnicity						
Humphreys et al.[23]	Florida					
White		1.5 (1.3, 1.6)	1.3 (1.2, 1.4)			
Black		1.2 (1.1, 1.4)	1.2 (1.1, 1.3)			
Degli Esposti et al.[19,a]	Florida					
White and other races		1.3 (0.9, 1.9)				
Black		1.5 (1.2, 3.0)				
Age, y						
Humphreys et al.[23]	Florida					
20–34		1.4 (1.2, 1.5)	1.3 (1.2, 1.5)			
≥35		1.2 (1.0, 1.4)	1.1 (1.0, 1.3)			
Gender						
Humphreys et al. (2017a)[23]	Florida					
Male			1.3 (1.2, 1.4)			
Female			1.1 (1.0, 1.3)			
Race*gender						
McClellan and Tekin[37,b]	United States					
White males		1.3 (1.1, 1.4)		2.7 (1.7, 4.1)	1.2 (1.0, 1.4)	1.8 (1.2, 2.7)
White females		1.1 (1.0, 1.4)			1.1 (0.9, 1.3)	1.3 (0.6, 2.9)
Black males		1.0 (0.8, 1.2)		0.7 (0.4, 1.3)	1.0 (0.9, 1.2)	0.8 (0.3, 2.2)
Black females		1.0 (0.8, 1.1)			1.0 (0.9, 1.2)	1.2 (0.3, 4.6)
McClellan and Tekin[37,b]	Florida					
White males						1.2 (1.1, 1.3)
White females						0.8 (0.4, 1.7)
Black males						1.0 (0.6, 1.5)
Black females						1.8 (1.8, 1.8)
Race*location						
Spanbauer,[32,c] in urban areas	United States					
Blacks killing Blacks				1.2 (1.1, 1.3)		
Whites killing Blacks				1.1 (1.0, 1.1)		
Blacks killing Whites				1.0 (1.0, 1.0)		
Whites killing Whites				1.0 (1.0, 1.1)		
Spanbauer,[32,c] in rural areas	United States					
Blacks killing Blacks				1.1 (1.0, 1.4)		
Whites killing Blacks				1.0 (1.0, 1.1)		
Blacks killing Whites				1.0 (1.0, 1.0)		
Whites killing Whites				1.0 (1.0, 1.1)		
County-level unemployment						
Ukert et al.[33]	Florida					
Quartile 1 (lowest)		1.3 (1.2, 1.4)	1.3 (1.2, 1.4)			
Quartile 2		1.5 (1.2, 1.8)	1.4 (1.1, 1.6)			

Continued

TABLE 2— Continued

Author (Year), Characteristic	Location	Firearm Homicide, IRR (95% CI)	Total Homicide, IRR (95% CI)	Justifiable Homicide, IRR (95% CI)	Nonjustifiable Homicide, IRR (95% CI)	Firearm-Related Injuries, IRR (95% CI)
Quartile 3		1.3 (1.1, 1.6)	1.2 (1.0, 1.3)			
Quartile 4 (highest)		1.3 (1.1, 1.5)	1.4 (1.3, 1.6)			
Location						
Munasib et al.[27]	United States					
Central cities		1.0 (1.0, 1.1)				
Suburbs		1.0 (1.0, 1.1)				
Smaller urban areas		1.0 (1.0, 1.1)				
Rural areas		1.1 (1.0, 1.2)				
Ukert et al.[33]	Florida					
Large metro		1.4 (1.2, 1.6)	1.3 (1.1, 1.5)			
Large fringe metro		1.5 (1.3, 1.8)	1.4 (1.2, 1.6)			
Medium metro		1.2 (0.9, 1.5)	1.2 (1.0, 1.4)			
Small metro		1.5 (0.9, 2.0)	1.3 (1.0, 1.7)			
Micro		1.2 (0.7, 1.8)	1.4 (0.8, 2.1)			
Noncore		0.9 (0.5, 1.3)	1.0 (0.7, 1.4)			

Note. CI = confidence interval; IRR = incidence rate ratio; PROGRESS-Plus = place of residence; race/ethnicity, culture, and language; occupation; gender or sex; religion; education; socioeconomic status; social capital; personal characteristics associated with discrimination (e.g., age); relationship features; and time-dependent relationships. In addition to the results shown here, subgroup differences were also analyzed for 3 outcomes hypothesized as negative controls (firearm suicide, total suicide, and property crime). The full table of results is included in the Appendix Table I (available as a supplement to this article at http://www.ajph.org). All coefficients are incidence rate ratios unless otherwise noted. We only present analyses for which comparisons were made between at least 2 subgroups (i.e., excluding analyses of 1 subgroup only, such as White people). We provide the highest-order interaction results from Spanbauer[32] and McClellan and Tekin[37] as these provided the greatest detail on subgroup differences. Subgroup differences in outcomes that were only ever investigated as negative controls in the included studies are not summarized here.

[a]In contrast to the other included studies, Degli Esposti et al. focused on homicides among adolescents (aged 15–19 years).[19]
[b]There are 3 iterations of these analyses (2 working papers[49,50] and 1 peer-reviewed publication[37]). We include the latter in this review as the most up-to-date analysis.
[c]Results are mean ratios.

in White-on-Black homicides being ruled justifiable more often and Black-on-White homicides being ruled justifiable less often than White-on-White homicides in adjusted analyses. Roman did not run interaction analyses to determine if these differences were exacerbated by SYG laws.

Most other characteristics showed null associations with conviction outcomes across studies. Murphy also conducted adjusted interaction analyses between victim and claimant race and gender.[42] Cases in which the victim was White (vs a racial minority) had lower odds of ending in conviction when the claimant was White or the victim was male (vs female). In a separate interaction model, Murphy further found that cases involving domestic (vs nondomestic) violence had lower odds of resulting in conviction when the claimant was male (vs female). The only study to consider nonconviction outcomes, Isijola found that racial (Black vs non-Black) and age (< 25 vs ≥ 25) concordance between victim and claimant in Florida SYG cases were unrelated to the claimant initiating aggression, proportionality of force, or ability to avoid conflict.[40] Lack of clarity on initial aggressor and ability to avoid conflict were more common in cases where both victim and claimant were Black. The study also analyzed cases involving young Black males, but cell counts were too low (< 5) to be reliable.

DISCUSSION

All available evidence evaluating the quantitative impacts of laws altering civilian rights to use deadly force in self-defense is from the United States, focused primarily on SYG laws. The weight of this evidence suggests that expanding civilian rights to use deadly force in self-defense outside the home is associated with, at most, modest

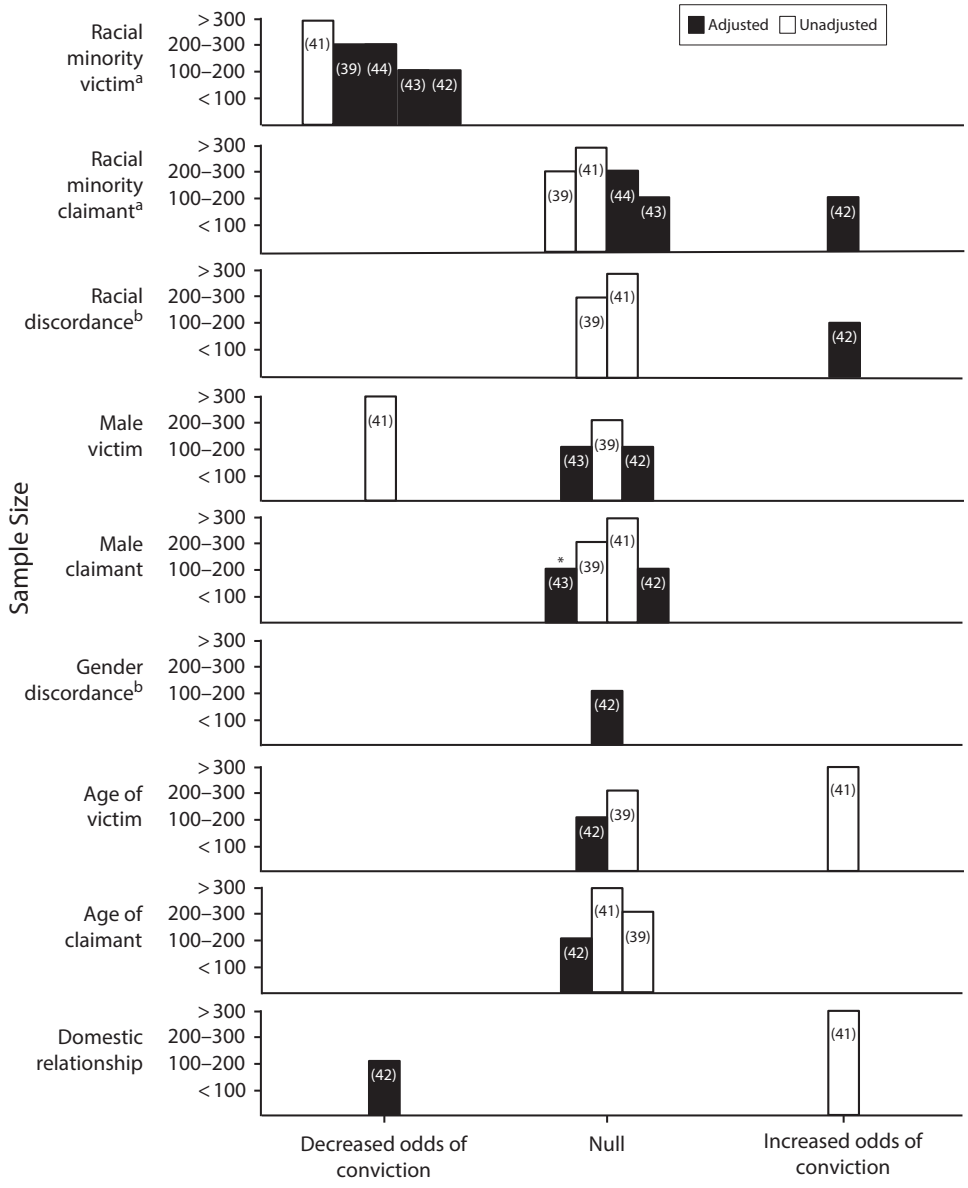

FIGURE 2— Harvest Plots of the Association Between PROGRESS-Plus Characteristics and the Odds of Conviction in Stand-Your-Ground Cases in Florida After Implementation (n = 5 Studies): 2014–2018

Note. PROGRESS-Plus = place of residence; race/ethnicity, culture, and language; occupation; gender or sex; religion; education; socioeconomic status; social capital; personal characteristics associated with discrimination (e.g., age); relationship features; and time-dependent relationships. Each bar represents a study (number = reference number); the position of the bar on the plot indicates whether the characteristic had a negative, null, or positive association with odds of conviction. Null associations were defined as small or variable estimates centered around the point of no effect; positive (increased odds of conviction) or negative (decreased odds of conviction) associations were large or consistent estimates in the positive or negative direction, respectively. Numerical results are provided in the Appendix Table K (available as a supplement to this article at http://www.ajph.org).

[a]Lott also studied victim and claimant race/ethnicity and found that conviction was negatively associated with cases involving Hispanic victims and positively associated, but with extreme variability (SE > 20 000 000 000), with cases involving White or Hispanic claimants.[45] We have not plotted these findings because Lott analyzed dummy variables for all available race/ethnicity categories (White, Black, Hispanic) in the same model (a likely contributor to the model's instability); therefore, the referent for these associations is unclear and the model is unreliable. In addition, McCormick conducted 3 iterations of analyses[41,51,52]; we included only the version with association estimates as per our eligibility criteria.[41]

[b]Discordance refers to discordance between race or gender of victim and claimant (e.g., White victim and racial minority claimant = racially discordant).

*Large point estimate but highly variable association.

increases, on average, in the rates of violent crime (including total and firearm homicide, aggravated assault, and robbery) across the United States. The existing evidence is inconsistent with hypotheses that these laws have an average deterrent effect or lead to large increases (> 25%) in violent crime on average. However, findings across states

were heterogeneous, and Florida-specific evidence demonstrated robust increases (24%–45%) in firearm and total homicide rates following the enactment of its SYG law[19,23,33]—highlighting the need for further studies that account for implementation differences across states.

Four studies accounted for variations in changes to state self-defense laws,[17,22,34,37] 3 of which computed an average (across states) intervention effect.[17,34,37] These 3 studies adjusted for different legal provisions (e.g., removing civil liability) and found that their primary results persisted. There was preliminary evidence that states that previously had a duty to retreat experienced larger increases in homicide rates following expansions to civilian rights to use deadly force outside the home.[17,22] This could, in theory, serve to explain the larger effects observed for Florida, along with its expansive media coverage, partly attributable to Florida being the first state to adopt the "model" SYG law.[3] Nonetheless, the largest estimate of the average intervention effect on homicides (14%) came from a study that excluded Florida, suggesting that the inclusion or exclusion of Florida did not drive the variation observed across study estimates.[32]

Further studies are needed on between- (and within-) state variation and potential explanations (e.g., varying laws, implementation, media coverage, legislative, and societal contexts) as well as the impacts of statistical decisions (e.g., model specifications, time periods covered, and temporal resolution). The common approach to handling interstate variation was to model 2-way fixed effects or conduct state-specific analyses. Alternative methods to explore state-specific deviations from the average effect would be to include a meta-analysis of state-specific effects[20] or the Bacon decomposition for 2-way fixed effects in the presence of varying treatment timing.[53] Authors also differed in their definitions of SYG or expanded castle doctrine laws and tended to focus only on statutory rather than case law. This raises the potential for different impact models (including mechanisms and timing of effects), which future research should investigate by including all states with SYG in practice—perhaps particularly for criminal justice outcomes, discussed later in this section.

Only one quarter of studies considered subgroup differences.[19,23,27,32,33,37] This is notable given concerns that expanding civilian rights to use deadly force outside the home will exacerbate social inequities in violent victimization—particularly for Black people, as the expansion of these rights adds to the history and ongoing context of racism (e.g., via racialized perceptions of threat, stereotypes of criminality, and the increased likelihood of excessive force).[54,55] Comparisons by race showed mixed findings, which are difficult to draw conclusions from without interaction analyses. In Florida, firearm homicide rates increased more dramatically among Black adolescents compared with White adolescents after the implementation of SYG,[19] whereas the reverse pattern was observed among adults.[23] Across states, the associations between expanding self-defense laws and firearm and justifiable homicide rates tended to be small and positive across victim race.[32,37]

Studies examining Florida self-defense cases involving SYG claims help contextualize these findings. Cases ended in conviction (i.e., were not ruled justifiable) more often when the victim was White[39,41–44]—especially when the claimant was a racial minority.[42] These racial inequities were not explained by case characteristics (e.g., victim being armed)[41–44] or dimensions of SYG (e.g., proportionality of force).[40] This initial evidence suggests that there are not dramatic differences in increases in homicide rates among Black versus White people following SYG and expanded castle doctrine laws. However, at least in Florida, there appears to be racial bias in the criminal justice process in rulings on SYG cases.[39,42] This means that even if SYG has increased legal protections for those claiming self-defense, there remains racial bias in the application of these protections that was not explained away by other case characteristics. To draw implications beyond Florida, racial inequities in the outcomes of self-defense claims before and after the implementation of SYG relative to non-SYG states must be analyzed accounting for case characteristics. It is possible that self-defense cases have similar outcome distributions by race in SYG versus non-SYG states but that levels are higher in SYG states (given more self-defense claims).[30] Barriers to such an analysis include the inconsistent reporting of justifiable homicide across states, discussed further in this section.

There has been even less consideration of gender in eligible evaluations, and only 2 included studies examined intersections of race and gender. Scholars have long noted the gendered notions of self-defense underlying castle doctrine laws (i.e., the "true man" empowered to use lethal force in self-defense where he has the legal right to be)[56]—in contrast, for instance, with the battered women syndrome defense, which requires expert testimony to evidence abused women's psychological condition of learned helplessness.[57] Extending the tradition

of castle doctrine, most SYG statutes do not mention domestic violence, and those that do typically only remove the duty to retreat if there is an active protection order.[51] The initial findings of this review and available descriptive evidence[58] demonstrate the importance of further research that examines the outcomes of SYG cases by gender and race across states, with attention to different forms of domestic violence and variations in case characteristics and state laws.

As expected with nonrandomized studies, all studies were rated as having critical or serious risk of bias attributable to confounding.[11] However, reflecting their (overall) high methodological quality for nonrandomized studies, most studies were low-to-moderate risk on most domains of bias. A common problem across studies was not adjusting for time-varying covariates or adjusting for postimplementation variables likely on the causal pathway (e.g., violent crime). Future evaluations of self-defense laws should map hypothesized pathways to impacts (e.g., using directed acyclic graphs) to a priori determine covariates and impact models.[59] Few studies evaluated mechanisms through which these laws may work. Gun ownership and demand have been hypothesized to increase after expanding rights to use deadly force in self-defense outside the home, yet only 2 studies examined this, with mixed findings depending on the outcome.[28,34] Proponents argue that SYG laws empower civilians who need to defend themselves; yet measuring legitimate self-defense is notoriously difficult.[4] The existing studies analyzed justifiable homicides as a proxy. This limits conclusions because self-defense laws change the definition of justifiable homicides, and these data are inconsistently reported across states—in addition to the social disparities in judicial rulings. Future research should triangulate analyses with other available data (e.g., the National Crime Victimization Survey).

Strengths and Limitations

We only synthesized quantitative evaluations—qualitative studies offer important insight of experiences of self-defense laws, which we will consider in future research (see protocol, https://osf.io/uz68e). To create a succinct summary, wherever possible, we focused on a single intervention–outcome effect estimate from each study based on predefined criteria and evaluated risk of bias for the most at-risk outcome. Our risk of bias assessments are thus conservative, and synthesized estimates are not always those highlighted by study authors. In doing so, we maximized consistency among study results and used the highest quality estimates. We focused on outcomes for which there were at least 2 studies to synthesize (all study results are in the Appendix Tables G and H).

Our review placed no restrictions on location—we sought to include all international evidence meeting our inclusion criteria. We only searched in English and our search terms were influenced by the US context (e.g., including SYG), which may mean that we missed relevant studies globally. However, to our knowledge, this review produced the most comprehensive synthesis of the quantitative evidence on the impacts of expanding civilian rights to use deadly force in self-defense in the United States to date.[2,4,7,60] We searched for and included gray literature, which minimizes publication bias. Although all evidence was US-based, our findings may have implications for future reforms to self-defense laws governing civilian use of deadly force internationally and underscore the need for robust and diverse scientific evidence to guide policy decisions.

Conclusions

Self-defense laws have rapidly changed in the United States with the introduction of SYG laws in the past 15 years. Expanding civilian rights to use deadly force in self-defense outside the home has been associated with modest increases in violent crime rates on average across the United States but robust increases in some states, most notably Florida. There are racial inequities in the application of SYG laws to self-defense cases, at least in Florida, with cases involving racial minority victims ruled justifiable more often, accounting for case characteristics like firearm use. Further evaluations are needed on differences in violence, injury, and criminal justice outcomes by state and legal variant, the mechanisms of impacts, and social inequities associated with altering civilian rights to use deadly force, across the United States and internationally. Our findings demonstrate the importance of using scientific evidence on both population and equity impacts of self-defense laws to guide legislative action that promotes public health and safety for all. AJPH

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PUBLICATION INFORMATION

Full Citation: Yakubovich AR, Esposti MD, Lange B, et al. Effects of laws expanding civilian rights to use deadly force in self-defense on violence and crime: a systematic review. *Am J Public Health*. 2021;111(4):e1–e14.

Acceptance Date: November 30, 2020.

DOI: https://doi.org/10.2105/AJPH.2020.306101

CONTRIBUTORS

All authors contributed to the conceptualization and design of this study. D. K. Humphreys originated the study idea and secured funding for the project. A. R. Yakubovich and D. K. Humphreys oversaw all steps of the study process. A. R. Yakubovich and B. C. L. Lange conducted all searches. A. R. Yakubovich, M. Degli Esposti, B. C. L. Lange, G. J. Melendez-Torres, A. Parmar, and D. K. Humphreys screened studies. A. R. Yakubovich, M. Degli Esposti, and B. C. L. Lange extracted data from included studies. A. R. Yakubovich and M. Degli Esposti conducted the risk of bias assessment. A. R. Yakubovich conducted the study syntheses, created all tables and figures, and drafted the appendices. A. R. Yakubovich led and D. K. Humphreys contributed to the writing of the first draft of the article. All authors revised the article critically for important intellectual content and approved the final version.

ACKNOWLEDGMENTS

This study was funded by the University of Oxford Fell Fund (171/007). A. R. Yakubovich is supported by the Canadian Institutes of Health Research (HSI-166388).

We thank Eli Harris for supporting the design of our search strategy and retrieval of study full texts.

CONFLICTS OF INTEREST

M. Degli Esposti, D. K. Humphreys, and D. J. Wiebe were authors on three of the primary studies analyzed as part of this review.

HUMAN PARTICIPANT PROTECTION

Human participant protection is not applicable because this is a systematic review.

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