Web table 1. All Included Studies Assessing the Association between Firearm-Related Laws and Homicides, Suicides and Unintentional firearm deaths, 1950-2014.

Reference - Country	Law	Unit of analysis/ sample size/years	Study design/type of analysis[a]	Measurement of exposure (laws) and outcome firearm (death)	Potential limitations	Association measure	Results
Lott and Mustard, 1997. United States (1)	Shall issue/Right to carry concealed weapons	U.S. counties/states 1977-1992, follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: For SI laws: Based on Cramer and Kopel (2), with corrections made by authors. For waiting periods: Bureau of Alcohol, Tobacco, and Firearms ordinances Outcome: Firearm deaths: FBI UCR	Sampling: County level covariates with missing data exclude counties from analysis Exposure measurement: Coding errors in exposure variable (as described by Ayres and Donohue 2003 (3)) Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders; Risk of collinearity due to adjustment for a vast number of confounders	Change in log of homicide rates	SI laws were associated with a reduction in the number of homicides at the county level (beta=-7.65%, $p<0.05$). SI laws were associated with a reduction in the number of homicides using the aggregated state level data (beta=-8.62%, $p<0.05$)

Bronars and Lott, 1998. United States (6)	Shall issue/Right to carry concealed weapons	U.S. counties/states No details on the number of counties 1977-1992, Follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: As in Lott and Mustard 1977 (1) Outcome: FBI UCR	Outcome: Not reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders	Change in log of homicide rates	SI laws were associated with a 6.57% reduction in homicide rates ($p<0.05$). In neighboring counties without SI laws there was a non-significant increment in homicides (4.5%, $p<0.05$). Allowing for trend-rate effects of own county laws showed that SI laws were associated with 16.1% reduction in counties with the laws, and with 9.4% increase in homicides in neighboring counties without SI laws. Models allowing for trends in neighbor's SI laws showed similar results
Bartley and Cohen, 1998. United States (7)	Shall issue/Right to carry concealed weapons	U.S. counties/states 1977-1992, follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares, Extreme bound analyses	Exposure and outcome data: As in Lott and Mustard 1977 (1)	Sampling: County level covariates with missing data exclude counties from analysis Outcome: Not reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Exposure: coding errors as suggested by Ayres and Donohue 2003 (3) and 2009 (8)	Change in log of homicide rates	Extreme bound analyses shows that the implementation of SI laws were associated with significant reductions in homicide rates when including (or not) arrest rates, adding trends in models and when models included only larger counties, or all counties (however, using all counties, no significant associations were observed after excluding the arrests rate variable)

Mustard, 2001. United States (9)	Shall issue/Right to carry concealed weapons	U.S. states. 50 states and D.C. 1984-1996, follow-up length = 13 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: From Cramer and Kopel 1995 (2), data on SI laws from Lott 1998 (10), Outcome data: FBI UCR	Other: Disaggregated results for single units are not provided Confounders: No adjustment for other potential confounders	Change in log of homicide rates	SI laws were not associated with reductions in the likelihood of a state having a felonious police death in logit modes (trend in post-SI laws period= -3.11, $p<0.05$; F-statistic for differences between pre/post-law trends = 5.37, $p<0.05$), and with total death rates per police officer in tobit models (trend in post-SI laws period = -1.48, $p>0.05$; F-statistic= 5.20, $p<0.05$). No significant associations were found for SI laws with handgun death rates per police officer. Waiting periods laws were not associated with reductions in felonious police deaths, deaths or handgun deaths per police officer. Poisson models showed similar results overall.
Olson and Maltz, 2001. United States (11)	Shall issue/Right to carry concealed weapons	U.S. counties with population of 100,000 or more/U.S. states. 1977-1992, follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: As in Lott and Mustard 1997 (1) Outcome data: FBI UCR and Supplementary Homicide Reports (SHRs)	Sampling: Convenience sample Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in log of homicide rates	In dummy models SI laws were not associated with a reduction of homicides (-6.52%, $p>0.05$), but were associated with reductions in firearm homicides (-20.9%, $p<0.05$). In trend models, trends in pre and post-law periods were significantly different (F statistic= 5.42, p=0 .05)

Plassmann and Tideman, 2001. United States (12)	Shall issue/Right to carry concealed weapons	U.S. counties/states. 1977-1992, Follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares, Tobit, Probit and Poisson regression models	Exposure and outcome data: Described as in Lott and Mustard 1997 (1)	Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders	Change in log of homicide rates	SI laws were associated with reduction in the rate of homicide in Poisson models (10.55% reduction, $p<0.05$). In weighted least square models, there was no association between the laws and homicides when models included all observations, or when included fewer independent variables, or counties with at least one crime (1.83% to -1.98%; $p>0.05$). At the state level, 3 states showed increments and 1 state showed reductions in homicide rates associated with SI laws
Plassmann and Whitley, 2003. United States (13)	Shall issue/Right to carry concealed weapons	U.S. counties. No details on the number of counties 1977-2000, follow-up length = 24 years	Greatest: Cross-sectional time-series/Weighted least squares and Poisson models	Exposure data: As in Lott and Mustard 1977 (1) Outcome data: FBI UCR	Exposure measurement: Coding errors in exposure variable (as described in Ayres and Donohue 2003 (3)) Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: Risk of collinearity due to adjustment for a vast number of confounder	Change in log of homicide rates	The law was associated with changes in homicides at the county level in dummy models (dummy law exposure variable) (beta=-6.2%, $p<0.05$), but not associated in pre vs. post-law trends/spline models (differences between trends=-0.02, $p>0.05$), and in hybrid models (both dummy and spline models) (differences in trends=-0.02, $p>0.05$). Results from Poisson regression showed SI laws were associated with reductions in homicides in dummy models (beta= -4.5%, $p<0.01$), spline models (beta=-0.01, $p<0.01$), and hybrid models (beta=-0.01, $p<0.01$)

Helland and Tabarrok, 2004. United States (14)	Shall issue/Right to carry concealed weapons	U.S. counties/states. 1977-1992 and 1977-1997, follow-up length = 16 years and 21 years	Greatest: Cross-sectional time-series/ empirical standard error function randomly generated from "placebo" laws - Weighted least squares	Exposure and outcome data: As in Lott and Mustard 1997 (1)	Sampling: County level covariates with missing data exclude counties from analysis Exposure measurement: Coding errors in exposure variable (as described in Donohue 2004 (15)) Outcome measurement: No reliable county data (as described in Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders	Change in log of homicide rates	In dummy models SI laws were not significantly associated with reductions in homicide rates (-7.8% reduction, $p>0.05$, Placebo SE= 4.8%). In spline/trends models, SI laws were associated with significant reductions in homicide rates (4.7% reduction, $p<0.05$, Placebo SE= 1.6%)
Lott, 2000. United States (16)	Shall issue/Right to carry concealed weapons	U.S. counties/states 1977-1994, follow-up length = 17 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: For SI laws: Based on Cramer and Kopel (2), with corrections made by authors. Outcome: Firearm deaths: FBI UCR	Sampling: County level covariates with missing data exclude counties from analysis Exposure measurement: Coding errors in exposure variable (as described in Donohue 2004 (15)) Outcome measurement: No reliable county data (as described in Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders; Risk of collinearity due to adjustment for a vast number of confounder	Change in log of homicide rates	SI laws were associated with a reduction in the number of homicides at the county level (dummy model: beta=-7.7%, $p<0.05$; trend model= -3%, $p<0.05$). Using the aggregated to state-level data SI laws were associated with an 8.62% reduction in homicides ($p<0.05$).

Duggan, 2001. United States (17)	Shall issue/Right to carry concealed weapons	U.S. counties 1977-1992, Follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: As described in Lott and Mustard 1997 (1)	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders	Change in log of homicide rates	Initial reductions in homicides associated with SI laws were no longer observed when including only year and county fixed effects variables, or county-year observations with non-missing crime data (0.9% to 5.3%, reductions in homicides, $p>0.05$)
Duwe, 2002. United States (18)	Shall issue/Right to carry concealed weapons	U.S. counties 1976-1999, follow-up length = 24 years	Greatest: Cross-sectional time-series/Poisson regression, negative binomial regression	Exposure: As in Marvell 1999 (19) Outcome data: Mass public shooting data collected from newspapers databases	Other: Disaggregated results for single units are not provided	Change in log of homicide rates, Incident rate ratio	Results from negative binomial (dummy) models indicate that SI laws were not significantly associated with mass public shootings (IRR ranging from 0.57 to 0.85, $p>0.05$) or the number of people killed in these (IRR ranging from 0.66 to 0.72, $p<0.05$). The laws were also not associated with these outcomes in trend models.
Ludwig, 1998. United States (20)	Shall issue/Right to carry concealed weapons	U.S. States. 1977-1994, follow-up length = 18 years	Greatest: Cross-sectional time-series/Weighted least squares - difference-in-difference-in-difference approach	Exposure data: As in Lott and Mustard 1997 (1) with corrections made by the author Outcome data: Vital statistics reports compiled by the U.S. Department of Health and Human Services	Confounders: No adjustment for other potential confounders	Change in log of homicide rates	SI laws were not associated with significant changes in adult homicide rates (1.4% increment, $p>0.05$). There was no significant association between the shall-issue laws and adult homicide rates relative to juvenile homicide rates (beta=0.16, $p>0.05$).

Moody, 2001. United States (21)	Shall issue/Right to carry concealed weapons	U.S. counties/states 1977-1992, Follow-up length = 16 years state data: 1971-1998, follow-up length = 28 years	Greatest: Cross-sectional time-series/semi-log weighted models and log log models	Exposure and outcome data: As in Lott and Mustard 1977 (1)	For models using the County data: Exposure measurement: Coding errors in exposure variable (as described by Ayres and Donohue 2003 (3)) Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	In semi-log weighted models using county data, SI laws were associated with reductions in homicides (beta=-0.04, $p<0.05$) (models included the prison population variable); however, results were not robust to the inclusion/exclusion of the arrest rate variable ($p>0.05$ in 2 of 3 models) and other model specifications. Similar analyses using aggregated state-level data showed significant reductions in 3 out of 8 models.
Black and Nagin, 1998 United States. (22)	Shall issue/Right to carry concealed weapons	U.S. counties population of 100,000 or more. 1977-1992 Follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: As in Lott and Mustard 1997 (1)	Sampling: Convenience sample Exposure measurement: Coding errors in exposure variable (as described by Ayres and Donohue 2003 (3)) Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	SI laws were associated with a reduction in homicides (beta=-0.09, $p<0.05$); however at the state level, Florida was the only state with a significant reduction. When Florida was removed the association was no longer significant (beta=-0.013, $p>0.05$). Adjusting models for state specific trends also resulted in no significant results (beta=0.038, $p>0.05$)
Kovandzic and Marvell, 2003. United States (23)	Shall issue/Right to carry concealed weapons	58 Florida counties. 1980-2000, follow-up length = 21 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: Florida Department of State, Department of Licensing Outcome data: FBI UCR and CDC vital statistics data	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data	Change in log of homicide rates	The concealed permit rate was not significantly associated with a reduction in homicides (0.4%, $p>0.05$). Similar results were obtained using CDC data and Poisson models controlling for simultaneity bias and other model specifications.

McDowall et al. 1995, United States (24)	Shall issue/Right to carry concealed weapons	Miami, Jacksonville, Tampa, Portland area and Jackson. 1977-1992, follow-up length = different across the three states	Moderate: Cross-sectional time-series without control group/Interrupted time series analysis - ARIMA models	Exposure data: Florida. Stat. ch. 790.06. Oregon. Rev. Stat.166.29 166.295. Mississippi Code Ann. 45-9-101 Outcome data: NCHS	Sampling: Convenience sample Confounders: No adjustment for other potential confounders	Change in number of monthly homicides and rate of homicides	Results from aggregated analyses indicate that SI laws were significantly associated with increments in homicide rates (beta=4.5, $p<0.05$). Background checks and waiting periods were not significantly associated with changes in homicide rates (beta=-3.25, $p>0.05$). Three of the five areas examined had significant increments in the number of homicides per month after SI laws were enacted.
Rubin and Dezhbakhsh. 2003, United States (25)	Shall issue/Right to carry concealed weapons	U.S. counties 1977-1992, Follow-up length = 16 years	Greatest: Cross-sectional time-series/2 stage least squares regressions	Exposure and outcome data: As in Lott and Mustard 1997 (1)	Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: No adjustment for other potential confounders	Change in log of homicide rates	Results show that for counties in 27 states, SI laws were not associated with variations in homicide rates. Only counties in 6 states had some evidence of SI being associated with homicide reductions.
Martin and Legault 2005, United States (4)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-1992, follow-up length = 16 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: As described in Lott and Mustard 1997 (1), and Ludwig 1998 (20). Outcome data: Data used in Lott and Mustard 1997 (1); FBI UCR	Data analysis: No additional models to account to time trends specifications Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in log of homicide rates	SI laws were not significantly associated with homicide rates using the state aggregated data (Model including arrest rate covariate: beta= -0.0554%, $p>0.05$)

Ayres and Donohue 2003. United States (3)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-1999, Follow-up length = 23 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: As in Vernick and Hepburn 2003 (26), and as in Lott and Mustard 1997 (1)	County data analyses: Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	SI laws were not associated with homicide rates at the state level in dummy models (beta=3.5%, p>0.05) or hybrid models (dummy: beta=4.8%, p>0.05; trend/spline: beta=-1.5%, p>0.05) (models using data from 1977-1999, Vernick's coding, dummies for years data, and controlling for incarceration rates and state specific trends). Results were inconsistent across multiple model specifications.
Ayres and Donohue, 2003. United States. (27)	Shall issue/Right to carry concealed weapons	U.S. counties/states. 1977-2000, follow-up length = 24 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: As in Vernick and Hepburn 2003 (26); corrections to errors in data used in Plassmann and Whitley 2003 (13) Outcome data: FBI UCR in Plassmann and Whitley 2003 (13)	County data analyses: Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	SI laws were not associated with changes in homicide rates in dummy models (beta=-4.2%, p>0.05) or hybrid models (dummy: beta= -3.9%, p>0.05; trend/spline: beta= -0.6%, p>0.05) (models using county data, Vernick's coding and region-year effects). Models using state-level data showed that SI laws were associated with increments in homicides in the dummy model (beta= 7.7%, p<0.05) but not in the hybrid model (dummy: beta= 6.8%, p>0.05; trend/spline: beta= 0.1%, p>0.05) (models with Vernick's coding, dropping early legalizers and controlling for incarceration rates and state-specific trends).

Donohue, 2004. United States (15)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-1999, follow-up length = 23 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: As in Lott and Mustard 1997 (1) with corrections made by authors	Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	The association between SI laws and homicides was not robust to model specifications. Results from state-level data analyses adjusting for state specific trends showed an association only in the hybrid-trend model (beta=-3.5%, $p<0.01$); no significant associations were observed in other models (dummy: beta=-1.9, $p>0.05$; hybrid-dummy: beta=2.7%, $p>0.05$).
Wellford et al. 2005. United States (28)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-2000 Follow-up length = 24 years	Greatest: Cross-sectional time-series/ Weighted least squares	Exposure and outcome data: Lott's revised new data covering the period 1977-2000	Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Other: Disaggregated results for single units are not provided	Change in log of homicide rates	In models using data from 1977-1992, the committee was able to replicate Lott and Mustard's 1977 study results; however, the association between SI laws and homicides was not robust to model specifications in dummy or trend models (e.g. dummy model with socioeconomic and demographic covariates: beta=-8.33, $p<0.05$; dummy model without covariates: beta=-1.95, $p>0.05$).
Grambsch, 2008. United States (29)	Shall issue/Right to carry concealed weapons	U.S. states. 1976-2001, Follow-up length = 26 years	Greatest: Cross-sectional time-series. Poisson random variable with log-linear mean specification	Exposure data: As in Lott 2000 (16) and Vernik and Hepburn 2003 (26) Outcome data: FBI UCR	Confounders: Adjustment for other potential confounders (e.g. incarceration rate) Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Controlling for regression to the mean showed that the relative homicide rate increased more rapidly in the post-law period than in the pre-law period in both random (percent change= 0.5%, $p>0.05$) and fix effects model (percent change= 6%, $p<0.05$).

Rosengart et al., 2005. United States (30)	Shall issue laws, minimum age of 21 years for handgun purchase, minimum age of 21 years for private handgun possession, one gun a month laws, and junk gun ban laws	U.S. States 1979 to 1998, Follow-up length = 20 years	Greatest: Cross-sectional time-series/Poisson regression	Exposure data: review of criminal statutes and codes, the Bureau of Justice Statistics and the Open Society Institute Outcome data: National Center for Health Statistics	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Rate ratios for homicide rates	There was no association between SI laws and firearm homicides (RR =1.11; 95% CI=0.99; 1.24), or all homicides (RR =1.07; 95% CI =0.98 to 1.17). Bans of junk guns were associated with a decrease in total suicide rates (RR= 0.86; 95% CI= 0.77;0.96). Minimum age for purchase/possession and "one gun a month" laws were not associated with significant variations in the rates of firearm homicides or total homicides. Results were similar after varying model specifications: removing the state-law interaction and the state-year interaction terms
Hepburn et al, 2004. United States (31)	Shall issue/Right to carry concealed weapons	U.S. States 1979-1998, Follow-up length = 20 years	Greatest: Cross-sectional time-series/Negative binomial regression and generalized estimating equations	Exposure data: State legislative code and session law text Outcome data: WONDER	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratio (IRR) of homicide rates	No significant association between SI laws and homicide rates was observed for SI laws enacted between 1985 and 1991 only (IRR= 0.95, 95%CI= 0.85;1.07) or SI laws enacted between 1992 and 1997 only (IRR= 1.07, 95%CI= 0.96;1.19) Analyses by gender and age groups produced similar results. Changing the implementation of the law to 1 next year after enactment, also showed similar results

Kovandzic et al, 2005. United States (32)	Shall issue/Right to carry concealed weapons	189 U.S. cities with population of 100,000 or more. 1980-2000, follow-up length = 21 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: Authors provide a description on the implementation of the laws Outcome data: FBI reports	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data.	Change in log of homicide rates	SI laws were not significantly associated with variations in homicide rates at the city level (beta=0.011, p>0.05). Individual analysis for states showed that the implementation of the law is associated with increments and reductions in homicides (5 and 2 states, respectively).
La Valle, 2007. United States (33)	Shall issue/Right to carry concealed weapons Brady gun law	20 of the U.S. largest cities in the U.S. 1990-2000, follow-up = 11 years	Greatest: Cross-sectional time-series/Generalized least squares time series, generalized estimating equations	Exposure data: Author provides a description on the implementation of the laws Outcome data: FBI UCR	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Shall issue laws were not associate with a reduction in the rate of total or firearm homicides (beta=-0.02 for both overall and firearm homicides; p >0.05, one-tailed). States that implemented the Brady law showed a significant reduction in the rate of overall homicides (beta=-0.120, p <0.05, one-tailed) and of firearm homicides (beta=-0.156, p <0.05, one-tailed).
La Valle and Glover, 2012. United States (34)	Shall issue/Right to carry concealed weapons	57 of the largest cities in the U.S. 1980-2006, Follow-up length = 27 years	Greatest: Cross-sectional time-series/Generalized least squares time series, generalized estimating equations	Exposure data: WESLAW Outcome data: FBI UCR, Crime in the United States, and National Archives of Criminal Justice Data	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in log of homicide and firearm homicide rates	Results from weighted estimates with interpolated data shows that SI laws were associated with an increment in firearm homicides (beta=0.274, p<0.05) and homicides (beta=0.206, p<0.05); results were not consistent in alternative models (no significant association with either firearm or homicides). May issue laws were associated with a reduction in the rate of homicides (b=-0.214, p<0.05) and firearm homicides (b=-0.263, p<0.05).

Ginwalla, 2013. United States (35)	Shall issue/Right to carry concealed weapons	Southern Arizona. August 1, 2008, through July 31, follow-up length = 48 months	Moderate: Cross-sectional pre/post-law observations without control group/Relative risk analysis	Exposure data: Authors provide a description on the implementation of the laws Outcome data: University of Arizona Medical Center's trauma registry, and the Pima County Medical Examiner's (ME's) Office	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data; No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders	Firearm injuries/deaths relative risk	The law was associated with an increment in the proportion of firearm related injuries/deaths (of total events potentially involving guns) (RR=1.11; 95%CI= 1.01,1.22). The law was also associated with an increment in the proportion of firearm homicides (RR=1.27; 95%CI=1.02,1.58). The proportion of firearm suicides was not different between the pre/post law periods
Strnad. 2012, United States (36)	Shall issue/Right to carry concealed weapons	U.S. states 1977-1999, follow-up length = 23 years	Greatest: Cross-sectional time-series/Bayesian hierarchical methods	Exposure and outcome data: As in Donohue 2004 (15)	None	Change in log of homicide rates	The model average approach suggest a modest reduction in homicides associated with SI laws: the model averaged coefficient of the SI laws spline dummy = -0.0146, and 0.009% of the posterior distribution for the coefficient was above zero (SD= 0.0039)
Moody and Marvell, 2008. United States (37)	Shall issue/Right to carry concealed weapons	U.S. counties 1977-2000, Follow-up length = 24 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: Described as in Lott and Mustard 1997 (1) with additional coding for new years included in models	Exposure measurement: Coding errors in exposure variable (as indicated by Ayres and Donohue 2009 (49)) Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5) Confounders: Risk of collinearity due to adjustment for a vast number of confounders	Change in log of homicide rates	SI laws were not associated with changes in homicides (US weighted average dummy model= 0.6%; however, the US weighted average trend was significantly associated with homicides= -1.7%, F-test= 4.74). At the state level, (dummy models) 8 states showed significant reductions, and 4 states increments in homicide rates (at a 90% confidence level). In trend models, 7 states showed

							reductions and 8 states increments in homicides (90% confidence)
Moody and Marvell, 2009 (38)	Shall issue/Right to carry concealed weapons	U.S. counties 1977-2000, Follow-up length = 24 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: with corrections as in Ayres and Donohue 2009 (8)	Outcome measurement: No reliable county data (as described by Martin and Legault 2005 (4) and Maltz 2006 (5)	Change in log of homicide rates	The population-weighted average estimate across all states showed that SI laws were not associated with changes in homicide rates (US weighted average dummy model= -0.9%, p>0.1; US weighted average trend model: beta= -1.3%, p>0.1)
Lott, 2010. United States (39)	Shall issue/Right to carry concealed weapons Castle Doctrine laws	U.S. states 1977-2005, Follow-up length = 29 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: Detailed description on SI laws enacted in all states Outcome: Firearm deaths: FBI UCR	Exposure measurement: Coding errors in exposure variable (as described by Ayres and Donohue 2003 (3)) Confounders: Risk of collinearity due to adjustment for a vast number of confounders *Castle doctrine law analysis: Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data; No information on statistical strategies used in analyses Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units not provided	Change in log of homicide rates	At the state level, shall issue (SI) laws were associated with reductions (2%) in the homicide rate in the trend (p<0.01), and hybrid models (trend model= 2% reduction, p<0.01), but was only marginally associated in the dummy model (20.3% reduction, p<0.05). The Castle Doctrine laws were associated with a 9% (p<0.01) reduction in the rate of homicides The Brady law was not associated with a reduction in the rate of homicides (% change after the law = 3.6%, p>0.05)

Gius, 2014. United States (40)	Shall issue/Right to carry concealed weapons, Federal assault weapons ban, State assault weapons laws	U.S. States. 1980 to 2009, Follow-up length = 30 years	Greatest: Cross-sectional time-series/not described	Exposure data: From Ludwig and Cook 2003 (41), Legal Community Against Violence, the National Rifle Association Outcome data: Supplementary Homicide Reports from Department of Justice	Exposure measurement: No clear description of the units (e.g. states) included in analyses Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in homicide rates	More restrictive laws regarding carrying concealed firearms (prohibited or "May issue" laws) were associated with higher rates of firearm homicides (beta=0.365, $p<0.05$). The Federal assault weapons ban law was associated with higher rates of firearm homicides (beta=0.66, $p<0.05$) State assault weapons laws were not associated with higher rates of firearm homicides (beta=0.29, $p>0.05$).
Ayres and Donohue, 2009. United States (8)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-2006, Follow-up length = 30 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: as in Moody and Marvell 2008 (37) with corrections	Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Results from dummy (0.4% reduction), spline (0.34%) and hybrid models (dummy= 0.18%, trend= 0.33%) showed that SI laws were not associated with changes in homicide rates ($p>0.1$) (models adjusted by state trends and an extending crack variable)
Ayres and Donohue, 2009. United States (42)	Shall issue/Right to carry concealed weapons	U.S. states. 1977-2000, Follow-up length = 24 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure and outcome data: as in Moody and Marvell 2008 (37) with corrections in exposure and outcome variables	Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Results from dummy (1.7% reduction), spline (0.2%) and hybrid models (dummy= 1.5%, trend= 0.0%) showed that SI laws were not associated with changes in homicide rates ($p>0.1$)

Aneja et al., 2014. United States (43)	Shall issue/Right to carry concealed weapons	U.S. states. 1979-2010, follow-up length = 31 years	Greatest: Cross-sectional time-series/Weighted least squares	Exposure data: As in Lott and Mustard 1977 (1) with corrections; the authors also provide a description on the latest laws implemented in recent years Outcome data: Michigan's Interuniversity Consortium for Political and Social Research - FBI UCR data	Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Models using county data from 1993-2000, including lagged arrest rates, robust and clustered standard errors, showed no association between SI laws and homicide rates (dummy= -1.13%, hybrid dummy= -1.11% and spline= -0.00%, p>0.05). Models adding more years of data and using different specifications showed no significant associations between SI laws and homicide rates. Similarly, models using state-level data (up to 2010) showed no association between SI laws and homicide rates (dummy= 0.23%, hybrid dummy= -0.06% and spline= 0.48%, p>0.05).
Villaveces et al., 2000. Colombia (44)	Shall issue/Right to carry concealed weapons	Bogota and Cali, Colombia. In Cali form 1993-1994; in Bogota from 1995-1997, follow-up length = 2 and 3 years, respectively	Greatest: Cross-sectional time-series/Negative binomial regression	Exposure data: Authors provide a description on the implementation of the laws Outcome data: National Institute of legal medicine, the police, the district attorney's office ad the department of transportation	Data analysis: No alternative strategies to test for robustness of findings given other model specifications.	Rate ratio of homicides, standardized mortality ratio	The law was associated with reductions in the incidence of homicides in Bogota (rate ratio= 0.87, 95%CI= 0.76;0.99) and in Cali (rate ratio=0.86, 95%CI= 0.76;0.97). In Bogota the law was associated with reductions in the incidence of firearm-related homicides (rate ratio= 0.85, 95%CI= 0.75;0.97). In Cali the association was marginally significant (rate ratio= 0.90, 95%CI= 0.79;1.03) .

Cheng and Hoekstra, 2013. United States (45)	Castle doctrine	U.S. states. 2000-2010, follow-up length = 11 years	Greatest: Cross-sectional time-series/Linear and Negative binomial models	Exposure data: Institute for Legislative Action of the National Rifle Association Outcome data: FBI UCR	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Change in log of homicide rates	In adjusted models, the Castle doctrine laws (states removing the duty to retreat in some place outside the home) were associated with a 6-11% increment in homicides ($p<0.05$)
McClellan and Tekin, 2012. United States (46)	Castle doctrine/Stand your ground laws	U.S. states. 2000-2010, follow-up length = 11 years	Greatest: Cross-sectional time-series/Linear and Negative binomial models	Exposure data: Institute for Legislative Action of the National Rifle Association, and State Legislators Outcome data: U.S. Vital Statistics	Confounders: No adjustment for other potential confounders Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Change in log of monthly homicide rates	Results showed that "stand your ground" laws were associated with a 6-6.8% increase in homicide rates, mainly driven by increments (14-16%) in homicide rates among White males ($p<0.05$). Other self-defense provisions (e.g. castle doctrine laws) were associated with 26% increments in White female homicides, and with 13% in White males homicides.

Kleck and Patterson, 1993. United States (47)	Variety of laws	170 U.S. cities with population of 100,000 or more. 1979-1981	Least: Cross sectional comparison/Ordinary least square regressions	Exposure data: U.S. Bureau of Alcohol, Tabaco, and Firearms, and as listed in other prior research publications Outcome data: NCSH	Confounders: No adjustment for other potential confounders	Log of homicide rates	Laws requiring permits to purchase or acquire a firearm were associated with lower homicide rates (beta=-0.150, $p<0.05$) and firearm suicides rates (beta=-0.146, $p<0.05$) Restrictions for possession of firearms for those with mental conditions were associated with lower firearm homicide rates (b=-0.177, $p<0.01$) Laws mandating additional discretionary penalties for felons committing crimes with gun were associated with lower firearm homicide rates (b=-0.115, $p<0.01$) State or local licenses requirements for gun dealers were associated with lower firearm suicide rates (beta=-0.140, $p<0.05$)
Ruddell and Mays, 2005. United States (48)	Background checks	U.S. states. 1999-2001	Least: Cross sectional comparison/ Ordinary least squares	Exposure data: Americans for Gun Safety Foundation. Outcome data: WONDER	Exposure measurement: Not validated scale for exposure classification Data analysis: No alternative strategies to test for robustness of findings given other model specifications. Confounders: No adjustment for other potential confounders	Homicide rate	The implementation of the law was associated with a reduction in the rate of state firearm homicides (beta ranging from -0.016 to -0.019, $p<0.05$, in models adjusted for different sets of covariates)

Sumner et al., 2008. United States (49)	Background checks	U.S. states. 2002-2004	Least: Cross sectional comparison/Negative binomial regression	Exposure data: Bureau of Justice Statistics Outcome data: WISQARS	Data analysis: No alternative strategies to test for robustness of findings given other model specifications. Confounders: No adjustment for other potential confounders	Incidence rate ratio of firearm homicides and suicides	Performing local-level background checks, compared with federal checks was associated lower firearm homicide rates (IRR= 0.78, 95% CI=0.61;1.01) and lower firearm suicide rates (IRR=0.73, 95%CI=0.60;0.89) in adults >20 years.
Ludwig and Cook, 2000. United States (50)	Brady Handgun violence prevention act (background checks and waiting periods)	U.S. states. 1985-1997, follow-up length = 13 years	Greatest: Cross-sectional time-series/Weighted least squares and Negative binomial regression	Exposure data: Bureau of Alcohol, Tabaco, and Firearms Outcome data: Vital statistics NCHS	Confounders: No adjustment for other potential confounders Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Incidence rate ratio of total and firearm Homicides/suicides and percentage of firearm homicides/suicides	Results showed there were no significant associations between the Brady law and homicides, firearm homicides, suicides and firearm suicides among those aged 21 or older (p>0.05). In negative binomial models, among those aged 55 or older, the Brady law was associated with lower firearm suicides (IRR=0.94, 95%CI=0.90,0.98) and lower percentage of firearm suicides (IRR=0.97, 95%CI=0.94,0.99). In weighted least squares models, waiting periods along with Brady law showed stronger reductions in firearm suicides among those aged 55 or older (beta=-1.03, 95%CI=-1.58, -0.47)

Vigdor and Mercy, 2003. United States (51)	Background checks	U.S. states. 1982-1998, follow-up length = 17 years	Greatest: Cross-sectional time-series/Negative binomial regression	Exposure data: Bureau of Alcohol, Tabaco, and Firearms, Lexis-Nexis databases Outcome data: FBI supplementary homicide report	Outcome measurement: outcome variable with some percentage of missing data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of intimate partner homicide (IPH) and firearm IPH	States with restraining order laws had significantly lower rates of total intimate partner homicides IPH (IRR=0.91, 95%CI= 0.83;0.99), female IPH (IRR=0.89, 95%CI= 0.82;0.97) and female firearm IPH (IRR=0.88, 95%CI= 0.78;0.99), with the magnitude being stronger if states had greater ability to check for background checks. Laws restricting firearm possession/purchase of firearms for those with domestic misdemeanor laws were found not associated with these outcomes ($p<0.05$)
Vigdor and Mercy, 2006. United States (52)	Background checks	U.S. states. 1982-2002, follow-up length = 21 years	Greatest: Cross-sectional time-series/Negative binomial regressions	Exposure data: Bureau of Alcohol, Tabaco, and Firearms, Lexis-Nexis databases Outcome data: FBI supplementary homicide report	Outcome measurement: outcome variable with some percentage of missing data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of intimate partner homicide (IPH) and firearm IPH	States with restraining order laws had significantly lower rates of total intimate partner homicides (IPH) (IRR= 0.92, 95%CI=0.86-0.98), female IPH (IRR= 0.92, 95%CI= 0.86;0.98) and female firearm IPHs (IRR=0.90, 95%CI=0.83-0.97), with the magnitude of the association being stronger in states with greater ability to check for background checks. Laws restricting firearm possession/purchase of firearms for those with domestic misdemeanor were not associated with these outcomes ($p<0.05$)

Zeoli and Webster, 2010. United States (53)	State laws reducing access to firearms for those under domestic violence restraining order (DVRO) and with convicted of a domestic violence misdemeanor; laws allowing police officers to confiscate firearms from the scene	46 of largest cities in the U.S. 1979-2003, follow-up length = 25 years	Greatest: Cross-sectional time-series/Generalized estimating equations - Poisson distribution	Exposure data: As in Dugan et al. 2003 (54), Vigdor and Mercy 2006 (52) and updated using Lexis-Nexis databases Outcome data: FBI supplementary homicide report	Sampling: Convenience sample Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Outcome measurement: Outcome variable with some percentage of missing data Other: Disaggregated results for single units are not provided	Incidence rate ratios of intimate partner homicide (IPH) and firearm IPH	States with restraining order laws had significantly lower rates of total IP Homicides (IPH) (IRR= 0.81, 95%CI= 0.68-0.95) and firearm IPHs (IRR= 0.75, 95%CI= 0.62-0.92). Laws allowing for firearm confiscation and those restricting firearm possession/purchase of firearms for those with domestic misdemeanor were not associated with these outcomes ($p<0.05$)

Rodriguez and Hempstead, 2011. United States (55)	General prohibitions (permit requirements on possession and purchases by minors), behavioral problems (backgrounds checks for "high risk" individuals without criminal history), and restrictions for those with criminal history	U.S. states. 1995-2004, follow-up length = 10 years	Least: Cross-sectional time-series without pre-post observations/Negative binomial models	Exposure data: Bureau of Justice Statistics Outcome data: NCHS data	Exposure measurement: Not validated scale for exposure classification Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of suicides	General prohibitions (permits and minimum age requirements to purchase firearms) (IRR=0.94, $p < 0.01$) and behavioral prohibitions (mental health, alcohol and drug problems, domestic violence or misdemeanor conviction) (IRR=0.99, $p<0.01$) were associated with lower male suicide rates. Permit requirements were associated with lower rates of suicides among those aged 45 or older (IRR=0.86, $p<0.05$), but higher suicides rates among males aged 15-24 (IRR=1.2, $p<0.01$). Restrictions for under age purchases, and individuals with mental health conditions were associated with lower rates of suicides among males 25-44 years old (IRR=0.86 and IRR=0.97, respectively, $p<0.05$)

Sen and Panjamapirom, 2012. United States (56)	Background checks	U.S. states. 1996-2005, follow-up length = 10 years	Least: Cross-sectional time-series without pre-post observations/Negative binomial models	Exposure data: State Procedures Related to Firearm Sales from the Bureau of Justice Statistics Outcome data: NCHS	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of all and firearm homicides /suicides	An additional background check was associated with fewer firearm homicides (IRR= 0.93, 95% CI= 0.91;0.96) and only marginally with firearm suicides (IRR= 0.98, 95%CI= 0.96;1.00). Firearm suicide deaths are lower in states with checks for mental illness (IRR= 0.96, 95% CI= 0.92;0.99), and fugitive status (IRR=0.95, 95% CI= 0.90;0.99) (compared with having only criminal background checks) Firearm homicides were lower when states with checks on restraining orders (IRR= 0.87, 95% CI= 0.79;0.95), and fugitive status (IRR= 0.79, 95% CI= 0.72;0.88) (compared with having only criminal background checks)
Irvin et al., 2014. United States (57)	Restrictions regarding licensing for dealers, record-keeping, inspections ad theft reporting	U.S. states 1995-2010, 16 years	Moderate: Cross-sectional time-series without pre-post observations/Poisson regression	Exposure data: As in Vernick et al. 2006 (58), and Lexis Nexis databases Outcome data: NCHS	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data; Inappropriate or not clear operationalization of variables Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratio of firearm homicides	Licensing requirements for dealers (IRR= 0.74, 05%CI= 0.67, 0.81) and allowing inspections (0.64, 05%CI= 0.59, 0.69) were associated with lower rates of firearm homicides. Record keeping of sales was associated with higher rates of firearm homicide (IRR= 1.45, 95%CI= 1.30, 1.61)

Medoff and Magaddino, 1983. United States (59)	Restrictions on license to purchase and waiting periods	U.S. states. 1970	Least: Cross-sectional comparison/Ordinary least squares	Exposure data: State reports Outcome data: Vital statistics	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Suicide rate of White males	States with restrictions on license purchases and waiting periods had lower suicide rates (beta= -3.02, p<0.01) that states without these laws. States with more strict restrictions had lower rates of suicides compared with states with less restrictions (beta= -2.9, p<0.01)
Marvell, 2001. United States (60)	Laws targeting juvenile firearm possession: Laws passed before 1994 Laws passed in 1994 Federal 1994 law	U.S. states. 1980-1998, follow-up light=19 years (analyses for all ages 1970-1998 = 29 years)	Greatest: Cross-sectional time-series/Multiple time-series regression	Exposure data: State reports Outcome data: NCHS Vital statistics and Bureau of Justice Statistics	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data	Firearm homicide rates	States affected by the federal law only had a significant reduction in the rate of firearm homicides when all age groups were combined (beta=-0.084, p<0.05), with the association being driven by trends in New York; there were no associations between the law and firearm homicides in specific age groups. In addition, there was no association between any of the laws and homicide rates, or suicide rates (among individuals aged 15-19).

Webster et al., 2004. United States (61)	Laws targeting juvenile firearm possession/purchases CAP laws	U.S. states. 1976-2001, follow-up length = 26 years	Greatest: Cross-sectional time-series/Negative binomial regression and generalized estimating equations	Exposure data: Bureau of Alcohol, Tobacco and Firearms Outcome data: NCHS	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Rate ratio of total, firearm, and non-firearm suicides	Among individuals aged 14-17 minimum age restrictions for possession or purchase were not associated with changes in firearm or all suicides ($p<0.05$). However, among those aged 18-20, these restrictions were associated with increments in suicides (restrictions on possession: RR=1.13, 95%CI=1.01;1.27; permits to purchase: RR= 1.18, 95%CI=1.04;1.34) and firearm suicides (permit to purchase only: RR=1.22, 95%CI =1.04;1.43) State CAP laws were associated with both firearm and all suicides among those aged 14-17 and those aged 18-20 (RR ranging from 0.87 to 0.92, $p<0.05$)
Webster et al., 2014. United States (62)	Licensing for firearm purchases	U.S. states/counties. 1999-2010/2012, follow-up length = 12/14 years	Greatest: Cross-sectional time-series/Generalized least squares regression models	Exposure data: Description on when the law was implemented Outcome data: WISQARS, WONDER and FBI UCR	Data analysis: No alterative strategies to test for robustness of findings given other model specifications.	Change in homicide rates	The repeal of the law was associated with an increment in the rate of firearm homicides (beta=1.18, 95%CI=0.92;1.43), total homicides (beta=1.08, 95%CI=0.77;1.40) and Murder and non-negligent manslaughter rates (FBI data) (beta=0.81, 95%CI=0.26;1.35), but not with rates of non-firearm homicides.

Reisch et al., 2013. Switzerland (63)	National Army XXI reform	Switzerland. March 1995-December 2008, follow-up length= 154 months	Greatest: Cross-sectional time-series/Interrupted time series, Ordinary least squares, Poisson regressions	Exposure data: Authors provide a description on the implementation of the laws Outcome data: Federal Statistical Office	Confounders: No adjustment for other potential confounders	Changes in rate of total, firearm, and non-firearm suicides	Among men aged 18-43, the reform was associated with a reduction of suicide rates (beta=2.16, 95% CI=1.29; 3.03) and firearm suicides (beta= 2.64, 95% CI=2.19;3.08). No similar reductions were observed for control groups (women aged 18-43, and men aged 44-53). Alternative models also showed a significant association with the intervention.
Gjertsen et al., 2014. Norway (64)	Norwegian gun control laws: Hunter's examination, shotgun acquisition (police permit), weapons cabinet, and Home Guard firearm	Norway. 1969-2009, follow-up length= 41 years	Moderate: Cross-sectional time-series without control group/Piecewise regression, Poisson regression and negative binomial regression	Exposure data: Authors provide a description on the implementation of the laws Outcome data: Norwegian Institute of Public Health	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Rate ratio of suicides, homicides and unintentional death	The laws were not associated with reductions in unintentional firearm death rates. Results suggested that requiring a police permit before acquiring a shotgun was possibly associated with reductions in male suicide rates among those aged 15-34 (trend after the 1990: beta=-0.078, $p<0.05$) Results suggested that the home guard weapons law was associated with reductions in male firearm homicides (trend after the 2003: beta=-0.37, $p<0.05$)

Cummings et al., 1997. United States (65)	Child access prevention (CAP) laws	U.S. states 1979-1994, follow-up length, 16 years	Greatest: Cross-sectional time-series/Poisson and negative binomial regressions	Exposure data: State reports Outcome data: NCHS	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratio of firearms unintentional deaths, suicides, and homicides	CAP laws were associated with a reduction in unintentional firearm deaths among children aged 14 or younger (IRR= 0.77, 95%CI= 0.63;0.94), but only marginally associated with firearm suicides (IRR= 0.81, 95%CI= 0.66;1.01) and firearm homicides (IRR= 0.89, 95%CI= 0.76;1.05). CAP laws were also not associated with reductions in unintentional firearm deaths among individuals 15 or older ($p<0.05$) Stronger reductions were observed among states allowing for a felony prosecution (IRR= 0.59, 95% CI=0.45;0.77)
Webster et al., 2000. United States (66)	Child access prevention (CAP) laws	U.S. states 1979-1997, follow-up length, 19 years	Greatest: Cross-sectional time-series/Negative binomial regressions	Exposure data: As in Cummings et al. 1997 (65) Outcome data: NCHS	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data	Incidence rate ratios of firearm unintentional deaths	CAP laws were associated with a 17% reduction in firearm unintentional deaths (95%CI= -29;-3%). These results were mainly driven by Florida (55% reduction, 95%CI= -75%, -31%); when Florida was excluded from analyses, there were no significant associations in the remaining 14 states. In states allowing felony prosecution of law offenders there was a 31% reduction (95%CI= -44%;-15%) in these deaths.

Hepburn et al, 2006. United States (67)	Child access prevention (CAP) laws	U.S. states 1979-2000, follow-up length, 22 years	Greatest: Cross-sectional time-series/Poisson and negative binomial regressions	Exposure data: As in Vernick and Hepburn 2003 (26), Outcome data: NCHS WONDER	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data	Rate ratio of firearm unintentional deaths	CAP laws were associated with a reduction in unintentional firearm rates among individuals under age 15 (RR= 0.78, 95%CI= 0.61;0.99), but not among adults aged 55-74. The association was stronger in states allowing felony prosecution of law offenders (RR= 0.64, 95CI= 0.46;0.89).
Lott and Whitle, 2001. United States (68)	Safe storage Laws	U.S. states 1979-1996 follow-up length, 18 years	Greatest: Cross-sectional time-series/Weighted tobits and Poisson regressions	Exposure data: As in Cummings et al. 1997 (65) Outcome data: FBI UCR	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: Risk of collinearity due to adjustment for a vast number of confounders Other: Disaggregated results for single units are not provided	Change in log of homicide rates	Safe storage laws were not associated with changes in firearm unintentional deaths across any of the aged groups analyzed (under age 5, 5-9, 10-14, and 15-19) ($p<0.05$) Safe storage laws were not associated with changes in firearm suicides among individuals under age 15 or those aged 15-19 ($p<0.05$). The authors suggest that varying model specifications, including whether the penalty was a felony or misdemeanors, showed some significant reductions in firearm suicides

Lee et al. 2013. United States (69)	Stand your graound laws and CAP Laws	44 U.S. states 2006 and 2009	Least: Cross-sectional comparison/Logistic regression	Exposure data: Not source described Outcome data: Healthcare Cost and Utilization Project Kids' Inpatient Database	Sampling: Convenience sample Exposure: No clear details on source of exposure variable Outcome measurement: Outcome variable with some percentage of missing data Confounders: No adjustment for other potential confounders	Odds ratio of firearm injuries	Among those aged 20 or younger states with "Stand your ground" laws were associated with greater odds of overall firearm injuries (OR=1.15, $p<0.001$) and unintentional injuries (OR= 1.28, $p<0.001$). Among those 12 or younger, CAP laws were associated with lower odds of unintentional firearm injuries (OR=0.74, $p<0.05$) and of suicidal injuries (OR=0.23, $p<0.05$). In multivariate analysis both laws were associated with greater odds of firearm injuries ($p<0.001$)
DeSimone et al. 2013. United States (70)	Child access prevention (CAP) laws	U.S. states 1988-2003, follow-up length, 16 years	Greatest: Cross-sectional time-series/Poisson regression	Exposure data: As in Webster et al. 2004 (61); Legal Community Against Violence Outcome data: Agency for healthcare research and quality's nationwide impatient sample	Outcome measurement: Outcome variable with some percentage of missing data Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Rate ratio of firearms deaths	CAP laws based on negligent storage only were associated with a reduction of 26% of self-inflicted injuries and 5% reduction in non-self-inflicted firearm injuries per hospital annually, in children 17 or younger (results for 11 states with data for the entire period). Results were similar for self-inflicted injuries but not for non-self-inflicted injuries when using data from 26 states with limited data. Significant associations between the law and a reduction in unintentional firearm injuries among individuals 18 and older. No association was observed for self-inflicted injuries in this

							group
Koper CS and Roth JA, 2001. United States (71)	1994 Federal Assault Weapon (ban on semiautomatic assault weapons and large capacity magazine)	U.S. states 1980-1995, follow-up length=16 years	Moderate: Cross-sectional time-series without control group/ Weighted least squares	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Sampling: Limited year data (period studied) to identify the effects of the intervention Outcome measurement: Outcome variable with some percentage of missing data Data analysis: No alterative strategies to test for robustness of findings given other model specifications	Change in the log of firearm homicide rates	The Federal law was associated with a non-significant reduction (6.7%) in firearm homicide rates in 15 states after considering states with own assault guns and high capacity magazine bans, juvenile restriction laws, and trends in California and New York ($p<0.05$)
Webster et al., 2002. United States (72)	Ban on Saturday night specials Maryland Gun Violence Act of 1996	Maryland. 1975–1998, follow-up length = 24 years	Moderate: Cross-sectional time-series without control group/ARIMA regression	Exposure data: MD Code Ann. 1988 Outcome data: NCHS	Confounders: No adjustment for other potential confounders	Change in homicide rates	In models assuming a delayed with constant/gradual effect, the ban on Saturday night specials was associated with reductions in firearm homicides (estimated percent change= from 6.8 to 11.5% reduction, $p<0.05$). Only the model assuming a immediate and constant effect was not significantly associated. The 1996 Maryland gun violence act was associated with reductions in firearm homicides (estimated percent change = from 10.3 to 11.4% reduction, $p<0.05$)
Ozanne-Smith et al., 2004. Australia (73)	1996 National Firearm Agreement (NFA), 1988 Victoria law	Australia and Victoria. 1979-2000, follow-up length= 21 years	Greatest: Cross-sectional time series/Poisson regression and Wilcoxon signed ranks tests	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Percentage change in firearm death rates	The Victorian legislation was associated with a 17.3% reduction in the rate of firearm deaths relative to Australia (beta=-0.1903; p=0.0001). The NFA was associated with a 14.0% reduction in the rate of firearm deaths in all other states in Australia relative to Victoria (beta=-0.1511; p = 0.0372)

Deutsch and Alt, 1977. United States (74)	The Bartley-Fox Gun law	Boston. January 1966 to October 1975, follow-up length = 118 months	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Sampling: Limited year data (period studied) to identify the effects of the intervention Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data	Change in monthly homicides	The implementation of the law was not associated with significant changes in homicides in Boston in the months following the implementation of the law ($p<0.05$)
Berk et al. 1979. United States (75)	The Bartley-Fox Gun law	Boston. January 1966 to October 1975, follow-up length = 118 months	Moderate: Cross-sectional time-series without control group/Ordinary least squares, Generalized least squares autoregressive model, ARIMA models	Exposure and outcome data: As in Deutsch and Alt 1977 (74)	Sampling: Limited year data (period studied) to identify the effects of the intervention	Change in monthly homicides	The implementation of the law was not associated with significant changes in homicides in Boston in any of the models used in analyses (Ordinary least squares models: estimate for change=0.049, $p>0.05$; GLS AR-1: estimate for change=0.043, $p>0.05$)
Hay and McCleary, 1979. United States (76)	The Bartley-Fox Gun law	Boston. 1974-1976, follow-up length = 132 months	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure and outcome data: as in Deutsch and Alt 1977 (74)	Sampling: Limited year data (period studied) to identify the effects of the intervention	Change in monthly homicides	The implementation of the law was not associated with significant changes in homicides in Boston (estimate for change in homicides =1.86, $p<0.05$, $p<0.05$)
Pierce and Bowers, 1981. United States (77)	The Bartley-Fox Gun law	U.S. cities. 1974-1976, follow-up length = 3 years	Greatest: Cross-sectional time series/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Other: Results from statistical tests are not presented	Percentage change in homicides and firearm homicides	The implementation of the law was found associated with a steeper reduction (55.7%) in the rate of homicides in Boston in the first 2 years of the implementations of the law, a reduction not observed in any of the cities used as controls

Deutsch, 1981. United States (78)	The Bartley-Fox Gun law	Boston. January 1966-September 1977, follow-up length = 141 months	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Confounders: No adjustment for potential confounders	Change in monthly homicides	After adding more years of data, in the post-law period there was a significant average reduction (29.21%) in homicides (over all months).
Loftin and Mcdowall. 1981. United States (79)	Michigan Felony firearms law	Detroit. 1969-1978, follow-up length= 10 years	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: Office of Vital and Health Statistics of the Michigan Department of Public Health.	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders	Change in monthly firearm homicides	The law was associated with an abrupt and permanent reduction in the number of firearm homicides (percent change=-10.86, 95%CI=-17.11;-4.61), but it was not associated with a decline in the number of non-firearm homicides (percent change=-0.008, 95%CI=-0.16 to 0.16)
Loftin and Mcdowall. 1984. United States (80)	Florida felony firearm law	Florida cities: Miami, Tampa, and Jacksonville. 1968-1978, follow-up length= 11 years	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; Confounders: No adjustment for potential confounders	Change in firearm homicides	In Tampa, the law was associated with a reduction in the number of firearm homicides (percent change = -1.21, 95%CI=-1.96;-0.46) but not with non-firearm homicides (percent change=0.03, 95CI=-0.50 to 0.57). In Miami and Jacksonville no significant reductions were observed ($p<0.05$)

Mcdowall et al., 1992. United States (81)	Mandatory sentencing laws	Detroit, Miami, Tampa, Jacksonville, Pittsburgh and Philadelphia. 1968-1978, 1969-1978, 1970-1984 follow-up length= 11, 10 and 15 years	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR, Office of Vital and Health Statistics of the Michigan Department of Public Health, and Pennsylvania Department of Health	Data analysis: No alternative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders	Change in Firearm homicides	The law was associated with an abrupt and permanent reduction in the number of firearm homicides in Tampa (estimate change in homicides =-1.2), Detroit (estimate for change in homicides =-10.57), Pittsburgh (beta=-1.07, $p<0.05$) and Philadelphia (beta=-6.83, $p<0.05$). The aggregated overall association for the six cities showed a significant reduction in firearm homicides associated with the law (estimate change in homicides=-0.69, $p<0.05$).
Fife and Abrams, 1989. United States (82)	New Jersey's Grave amendment	New Jersey. 1974-1986, follow-up length= 13 years	Moderate: Cross-sectional time-series without control group/differences of slopes t-test	Exposure data: Authors provide a description on the implementation of the law Outcome data: Department of Health's center for health statistics and NCHS	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the proportion of firearm homicides and firearm suicides	The law was associated with a reduction in the proportion of firearm homicides (difference in pre/post-law slopes = -3.2 ± 1.85%), a reduction not observed in the rest of the United States. The law was also associated with changes in the percentage of firearm suicides (difference in pre/post-law slopes = -0.98 ± 0.98%).

O'Carroll et al. 1991. United States (83)	Detroit's 1986 law (mandatory jail sentence for unlawfully carrying a firearm in public)	Detroit. 1980-1987, follow-up length= 8 years	Moderate: Cross-sectional time-series without control group/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: Detroit City police department	Sampling: Limited year data (period studied) to identify the effects of the intervention Data analysis: No alternative strategies to test for robustness of findings given other model specifications	Change in monthly firearm homicides	The law was not associated with variations in the number of monthly firearm indoor homicides ($p>0.05$) but was associated with an increment in homicides committed outside (22% increment, $p>0.05$). The law was also not associated with reductions in firearm homicides, and only marginally with an increment (16%, $p=0.07$) in non-firearm homicides. According to authors, these findings are what would be expected if the law had an effect on firearm homicides and outside homicides (given the tendency of crime in Detroit during the observation period)
Marvell and Moody, 1995. United States (84)	Firearm sentence enhancement laws	U.S. states. 1971-1993, follow-up length= 13 years	Greatest: Cross-sectional time series/Multiple time series regression	Exposure data: Authors provide a description on the implementation of the laws Outcome data: FBI UCR	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders	Change in log rate of homicides and firearm homicide	The laws (aggregated) were not associated with variation in firearm homicide rates with the usable data (beta= 0.2, $p>0.05$) or the best available data (beta=-0.2, $p>0.05$) In disaggregated models, the Massachusetts law was associated with reductions in firearm homicides (beta=-2.0, $p<0.05$)

La Valle, 2008. United States (85)	Firearm sentence enhancement laws, mandatory minimum, additional jail time for gun-crimes	20 of the largest cities in the U.S. 1970-2005, follow-up length= 36 years	Greatest: Cross-sectional time series/Generalized least squares, generalized estimating equations, generalized estimating equations	Exposure data: As in Marvell and Moody 1995 (84) Outcome data: FBI UCR	Sampling: Convenience sample Data analysis: No alternative strategies to test for robustness of findings given other model specifications. Confounders: No adjustment for other potential confounders	Change in log rate of homicides and firearm homicide	Additional jail time was associated with a reduction in the rate of firearm homicides (beta= -0.59 and -0.23 ($p<0.05$) in models with and without interpolated observations, respectively). The enhancements for firearm only laws were associated with an increase in the rate of firearm homicides (beta=0.14, $p<0.05$, in models with interpolated observations)
Raphael and Ludwig, 2003. United States (86)	Project Exile from Richmond, Virginia	Richmond, Virginia. 1994-1999, follow-up length= 6 years	Greatest: Cross-sectional time series/Linear regression models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Sampling: Convenience sample Confounders: No adjustment for other potential confounders	Change in homicide rates	The law was not associated with significant reductions in the homicides (beta=-1.85, $p>0.05$). The authors indicate that reductions in homicides were likely to be explained by the high rates in the pre-law period and regression to the mean effects.
Rosenfeld et al., 2005. United States (87)	Project Exile from Richmond, Virginia	95 U.S. cities with population of 175,000 or more 1992-2001, follow-up length= 10 years	Greatest: Cross-sectional time series/Growth-curve analysis, Poisson models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Sampling: Convenience sample Data analysis: No alternative strategies to test for robustness of findings given other model specifications	Change in firearm homicide rates	The implementation of the project was associated with a 22% reduction in firearm homicide rates ($p<0.05$). After replacing the 1997 value with the average of the 1996 and 1998 values the reduction was only marginal ($p<0.10$)

Rosenfeld, 1996. United States (88)	St. Louis gun buy-back programs	St. Louis 1980-1994, follow-up length= 15 years	Moderate: Moderate: Cross-sectional time-series without control group/ARIMA	Exposure data: Authors provide a description on the enactment/implementation of the program Outcome data: St. Louis Metropolitan Police Department	Sampling: Limited year data (period studied) to identify the effects of the intervention Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data; No alternative strategies to test for robustness of findings given other model specifications. Confounders: no adjustment for potential confounders	Change in monthly firearm homicides	The 1991 and 1994 gun buy-back programs were not associated with changes in firearm homicides ($p<0.05$). A marginal association was observed (beta=-10.85, $p=0.07$) only during the period of operation of the 1994 program.
Phillips et al., 2013. United States (89)	City of Buffalo's gun buy-back programmed	City of Buffalo 2001/2006-2012, follow-up length= 6/12 years	Moderate: Moderate: Cross-sectional time-series without control group/ARIMA	Exposure data: Authors provide a description on the implementation of the program Outcome data: Erie County Crime Analysis Center	Data analysis: No alternative strategies to test for robustness of findings given other model specifications. Confounders: No adjustment for other potential confounders	Changes in firearm homicides	The gun buyback programs in Buffalo were not associated with changes in firearm homicide rates (estimate for change in firearm homicides= -0.25, $p>0.05$) (2006-2012 data, abrupt temporary model, with June 2007 as the intervention date). In models (data 2001-2012, with September 2008 as the intervention date), a significant association was found with an increment of firearm homicides (change in firearm homicides=7.68, $p<0.05$)
Leigh and Neill, 2010. Australia (90)	Australia's gun buyback program, number of firearm bought back	Australian states. 1968-2006, follow-up length= 39 years	Greatest: Cross-sectional time series/Stripped-down regression-Panel models	Exposure data: Authors provide a description on the implementation of the program; Reuter and Mouzos 2003 (91). Outcome data: Erie County Crime Analysis Center	None	Changes in overall, firearm and non-firearm homicides and suicides	The number of guns bought back was significantly associated with reductions in the rate of firearm suicides (beta=-0.49, $p<0.05$), and all homicides (beta=-0.26, $p<0.05$), but only marginally with firearm homicides (beta=-0.18, $p<0.1$). Results were consistent in models with

							different specifications
Magaddino and Medoff, 1984. United States (92)	1868 gun control act	U.S. states. 1947-1977, follow-up length= 31 years	Greatest: Cross-sectional time series/Two stage Cochrane - Orcutt model	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI UCR	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in rate of homicides	The law was not associated with changes in the rate of homicides (beta=0.003, $p>0.05$)
Loftin et al. 1991. United States (93)	1976 District of Columbia Handgun Ban	District of Columbia, and surrounding areas of Maryland and Virginia. 1968-1987, follow-up length= 20 years	Moderate: Cross-sectional time-series without control group/ARIMA models and t-tests	Exposure data: Authors provide a description on the implementation of the law Outcome data: NCHS	Sampling: Convenience sample Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders	Change in yearly and monthly firearm homicides and suicides	The implementation of the law was associated with significant reductions in monthly rates of firearm homicides in D.C. (25% reduction, $p<0.05$) and firearm suicides (23% reduction, $p<0.05$) that were not observed in control areas.

McDowall et al. 1996. United States (94)	1976 District of Columbia Handgun Ban	District of Columbia, Memphis and Boston. 1968-1987/90, follow-up length= 20/23 years	Greatest: Cross-sectional time series/ARIMA models	Exposure and outcome data: as in Loftin et al. 1991 (93)	Sampling: Convenience sample Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data. Confounders: No adjustment for potential confounders	Change in monthly firearm homicides	The implementation of the law was not associated with reductions in firearm homicides in D.C. (using data up to 1990) (estimate for change in homicides= 2.08, $p<0.05$). However, the law was associated with reductions in firearm suicides (estimate for change in suicides= -0.47, $p<0.05$), that were not observed in Memphis, Boston or Baltimore.
Britt et al., 1996. United States (95)	1976 District of Columbia Handgun Ban	District of Columbia and Baltimore. 1968-1987/89, follow-up length= 20/22 years	Greatest: Cross-sectional time series/ARIMA models	Exposure data: Authors provide a description on the implementation of the law Outcome data: FBI Supplementary Homicide Reports and NCHS	Sampling: Convenience sample Confounders: No adjustment for potential confounders	Change in monthly firearm homicides	The implementation of the law was associated with significant reductions in firearm homicides in D.C. (estimate for change in homicides=-3.23, $p<0.05$) as well as in Baltimore (estimate for change in homicides= -2.81, $p<0.05$) (FBI data). Similar results were observed with NCHS data Results were similar after moving the intervention date. Adding two more years of data resulted in no significant variations in firearm homicides in D.C. but still significant reductions in Baltimore.

Leenaars and Lester, 1999. Canada (96)	Bill C-51	Canada. 1969-1985, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/time series regressions	Exposure and outcome data: as in Leenaars and Lester 1996 (97)	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of overall and firearm homicides, suicides and unintentional deaths	The bill was associated with significant reductions in the rates of all (beta=−0.32, $p<0.05$) and firearm homicides (beta=−0.24, $p<0.05$), and also unintentional deaths (beta=−0.08, $p<0.05$), but was not associated with reductions in the rate of all and firearm suicides (beta=-0.237, $p<0.05$)
Carrington, 1999. Canada (98)	Bill C-51	Canada. 1969-1985, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/differences of slopes t-test	Exposure and outcome data: as in Leenaars and Lester 1996 (97)	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Differences in pre-post trends in overall and firearm homicides and suicides	Among males, the bill was associated with a significant reduction in firearm homicides (difference in slopes= -0.09, $p<0.05$), and in firearm suicides (difference in slopes= -0.52, $p>0.05$). Reductions in firearm suicides and homicides were also observed among females ($p<0.05$). Among females and males, the bill was associated with reductions in firearm deaths (difference in slopes=-0.36, $p<0.05$).

Leenaars and Lester, 2001. Canada (99)	Bill C-51	Canada. 1969-1985, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/Linear time series regressions	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of homicides and firearm homicides	The bill was associated with reductions in the rate of overall homicides (beta=-0.35, $p<0.05$) but not with firearm homicides (beta=-0.16, $p>0.05$). Among males the bill was associated with a significant reduction in the rates of all homicides (beta=−0.52, $p<0.05$) but not with reductions in firearm homicides (beta=−0.32, $p>0.05$). Among females the bill was not associated with changes in all or firearm homicides
Mauser and Holmes, 1992. Canada (100)	Bill C-51	Canada. 1968-1988, follow-up length= 21 years	Moderate: Cross-sectional time-series without control group/Generalized least squares	Exposure data: Authors provide a description on the implementation of the law Outcome data: Center for Justice Statistics	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of homicides	The association between the bill and homicide rates (beta=−0.35, $p<0.01$) became non-significant after adjusting for a linear trend term (beta=−0.02, $p>0.01$).
Blais et al., 2011. Canada (101)	Bill C-51 Bill C-17 Bill C-68	Canada. 1974-2004, follow-up length= 31 years	Moderate: Cross-sectional time-series without control group/Least generalized squares	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of homicides	Bill C-51 was associated with a 13.68% reduction in firearm homicides (beta=-0.16, t=1.96). Similarly bill C-68 (beta=-0.17, t=3.05) was associated with change in the rate of firearm homicides.

Langmann, 2012. Canada (102)	Bill C-51 Bill C-17 Bill C-68	Canada. 1974-2008, follow-up length= 35 years	Moderate: Cross-sectional time-series without control group/Poisson, negative binomial, ARIMA, and Joinpoint regressions	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada and CANSIM	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data Other: Disaggregated results for single units are not provided	Change in the rate of homicides , spousal homicide	None of the bills were associated with reductions in firearm homicide or spousal homicide rates (in interrupted-regression models and ARIMA adjusted models). A joinpoint was generated at 2002 (closer to the C-68 law implementation date), with an increment in the baseline rate of firearm homicide from an annual percentage change of –2.7% (95%CI=–3.2; –2.1) to 2.3% (95%CI=–4.2;9.2).
McPhedran and Mauser, 2013. Canada (103)	Bill C-68	Canada. 1974-2009, follow-up length= 36 years	Moderate: Cross-sectional time-series without control group/ARIMA and Zivot–Andrews structural breakpoint tests	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada and Department of Justice	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of homicides and domestic homicides	The implementation of the law was not associated with a reduction of the firearm spouse homicide (mean predicted = 0.11, mean observed = 0.11, $p>0.05$). No breakpoints were found in 1995-1997 structural breakpoint tests. In Zivot-Andrews analyses significant breaks were observed between 1979-1982 suggesting an association between bill C-51 and firearm female homicides

Rich et al., 1990. Canada (104)	Bill C-51	Toronto and Ontario. 1973-1977 and 1979 - 1983, follow-up length= 10 years	Moderate: Cross-sectional time-series without control group/Linear time series analyses	Exposure data: Authors provide a description on the implementation of the law Outcome data: Chief Coroner for Ontario and Toronto	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders	Change in the rate of suicides and firearm suicides	The implementation of the law was not associated with a reduction in firearm suicides in Toronto or in Ontario ($p>0.05$) when 1977 (the year of the implementation of the law) was excluded from analyses. Comparisons of slopes in pre/post-law periods showed no changes in trends of suicides ($t=1.51$, $p=0.13$). There was a significant decrease in the mean proportion of firearms suicides among men in Toronto (differences in means=7.0%, $p<0.001$).
Carrington and Moyer, 1994. Canada (105)	Bill C-51	Ontario. 1965-1977 and 1979 - 1989, follow-up length= 24 years	Moderate: Cross-sectional time-series without control group/pseudo-generalized least squares	Exposure data: As in Rich et al. 1990 (104) Outcome data: Statistics Canada	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of homicides and age-standardized homicides	The implementation of the law was associated with a reduction in firearm suicides in Ontario (change in slopes in pre/post-law periods=-0.2, $p<000.1$) and total suicides (change in slopes in pre/post-law periods=-0.6, $p<0.001$) even after 1977 (the year of the implementation of the law) was excluded from analyses. Similar findings were observed for age standardized rates (change in mean -1.1, $p=0.02$)

Leenaars et al., 2003. Canada (106)	Bill C-51	Canada. 1969-1985, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/Interrupted time-series, multivariate linear regression	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada	Data analysis: No alterative strategies to test for robustness of findings given other model specifications (strategies from the two models were not integrated in a single model)	Change in the rate of suicides and firearm suicides	In unadjusted models the bill was associated with a long-term reduction in the all suicide rate (beta= -0.48; $p<0.01$) and firearm suicides (beta= -0.33; $p<0.01$); reductions were also significant for males and females. The law was not associated with short-term outcomes for firearm suicide, total suicides by all methods or other than firearms. In models adjusted for confounders, the law was associated with reductions in firearm suicide rates (beta= -0.72; $p<0.05$) but not in all suicides. Reductions were also significant for males and females.
Caron et al., 2008. Canada (107)	Bill C-17	Quebec. 1986-2001, follow-up length= 16 years	Moderate: Cross-sectional time-series without control group/Ordinary least squares	Exposure data: Authors provide a description on the implementation of the law Outcome data: Quebec Coroner's office	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for other potential confounders	Change in the rate of suicides and firearm suicides	Among males the bill was not associated with a short-term (beta= 0.28; $p=0.45$) or long-term (beta= -0.040; $p=0.70$) significant reduction in firearm suicide rates. Similar results were observed for females. Among females, there was a significant increment in hanging suicide rates. In adjusted models the law was associated with an overall increase in all suicide rates among males and females (beta= 4.204; $p<0.05$ and beta=1.368, $p<0.05$)

Gagne et al., 2010. Canada (108)	Bill C-17	Quebec. 1986-2001/1981-2006, follow-up length= 16/26 years	Moderate: Cross-sectional time-series without control group/Joinpoint and Poisson and negative binomial regressions	Exposure data: Authors provide a description on the implementation of the law Outcome data: Quebec's death database	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for other potential confounders	Change in the rate of suicides and firearm suicides	Results from Jointpoint analyses indicated that among men aged 15-34, the rate of firearm suicides decreased between 1981 and 1996 (annual percent change (APC) of -2.7%) but decreased after 1996 (APC of -11.1%). Among men aged 35-64 slower declines in firearm suicides were also observed. Results from Poisson models showed a significant reduction in firearm suicides associated with the law, after adding more years of data (Change of trend after the law = -3.7, 95%CI=-5.8;-1.5).
Cheung and Dewa, 2005. Canada (109)	Bill C-17	Canada. 1979-1999, follow-up length= 21 years	Moderate: Cross-sectional time-series without control group/Linear time series analyses	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of suicides and firearm suicides	Among individuals aged 15-19, the bill was associated with a reduction in the rate of firearm suicides (beta=-0.30, 95%CI = -0.44;-0.15), but also with a significant increment in the rate of hanging suicides (beta=0.19, 95%CI = 0.37;0.35).

Leenaars and Lester, 1997. Canada (110)	Bill C-51	Canada. 1969-1985, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/Linear time series analyses, differences of slopes t-test	Exposure data: Authors provide a description on the implementation of the law Outcome data: Statistics Canada	Data analysis: No alterative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units (e.g. provinces, cities) are not provided	Change in the rate of unintentional firearm deaths	The law was only marginally associated with reductions in the rate of unintentional firearm deaths among males (beta=-1.38, $p<0.08$) but not females (beta=-0.32, $p<0.30$).
Chapman et al. 2006. Australia (111)	1996 National Firearm Agreement (NFA)	Australia. 1979–2003, follow-up length= 25 years	Moderate: Cross-sectional time-series without control group/Negative binomial regression	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Rate ratio of all and firearm suicides and homicides / ratio of pre/post-law slopes	The law was associated with significant faster reductions in total firearm deaths (RR= 0.97;$p = 0.03$), all homicides (RR=0.97, $p = 0.01$); the steeper reduction in firearm homicides was not significant (according to authors due to low power given low numbers); there were also reductions in all suicides, firearm suicides and non-firearm suicides (RR ranging from 0.94 to 0.95, $p<0.01$), and an increment in unintentional firearm deaths (RR=1.17; $p = 0.001$). Additionally, in the 18 years before the gun law reforms, there were 13 firearm mass shootings in Australia, and zero in the 10.5 years afterwards

Baker and McPhedran, 2007. Australia (112)	1996 National Firearm Agreement (NFA)	Australia. 1979–2004, follow-up length= 26 years	Moderate: Cross-sectional time-series without control group/ARIMA	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Data analysis: No alterative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Difference between predicted vs. observed rates of homicides and suicides	The law was associated with a significant reduction in the rate of firearm suicides (mean rates: predicted= 1.85 vs. observed = 1.22, $p<0.05$) but not with a change in the rate of firearm homicides ($p=0.14$). The law was also associated with an increment in the rate of unintentional firearm death rates (mean rates: observed= 0.06 vs. estimated = 0.15, $p<0.02$)
Neil and Leigh, 2007. Australia (113)	1996 National Firearm Agreement (NFA)	Australia. 1979–2004, 1915–2004, 1969–2004, follow-up length= 26, 90, 36 years	Moderate: Cross-sectional time-series without control group/ARIMA	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Difference between predicted vs. observed rates of homicides and suicides and logarithm of these rates	In models including more years of data (1969-2004) the 1996 law was associated with reductions in the rate of firearm homicides (mean rates: predicted= 0.4 vs. observed = 0.27, $p=0.001$) and firearm suicides (mean rates: predicted= 2.02 vs. observed = 1.23, $p<0.001$). Results were similar when using the log of the rate of firearm homicides/suicides and when using additional years of data.
Lee and Suardi, 2010. Australia (114)	1996 National Firearm Agreement (NFA)	Australia 1915-2004, follow-up length= 90 years	Moderate: Cross-sectional time-series without control group/ARIMA - Quandt, Bai, Bai and Perron tests	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of firearm suicides and homicides	Results from structural tests showed no evidence of significant reductions in firearm suicides or firearm homicides that could be attributable to the NFA.

McPhedran and Baker, 2012. Australia (115)	1996 National Firearm Agreement (NFA)	Australia 1907–2007 and 1915–2004, follow-up length= 101 and 90 years	Moderate: Cross-sectional time-series without control group/Dickey-Fuller, Zivot and Andrews structural breakpoint test, Quandt test, ARIMA, Linear, Poisson and negative binomial models	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Institute of Health and Welfare	Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of firearm suicides and homicides	Results from structural tests showed no evidence of significant reductions in firearm suicides for any of the age groups examined, except in the case of ZA tests, which showed a break in 1997 for those aged 35-44 when using the 1979–2007 data only; further analyses using linear, Poisson and negative binomial models showed no association between the law and suicides in this age group
Klieve et al., 2009. Australia (116)	1996 National Firearm Agreement (NFA)	Queensland and Australia 1988-2004, follow-up length= 17 years	Moderate: Cross-sectional time-series without control group/Negative binomial models	Exposure data: Authors provide a description on the implementation of the law Outcome data: Queensland suicide register and Australian Bureau of Statistics	Data analysis: No alternative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders	Rate ratio of all and firearm suicide	In Queensland, which had previously introduced the Weapons Act, 1990, there were no significant reductions in firearm suicide rates associated with the NFA (ratio of pre/post-law slopes: RR= 1.01, p=0.78). In Australia, the NFA law was associated with a reduction in the rate of firearm suicides (trend in post-law period: RR= 0.93, $p<0.05$; ratio of pre/post-law slopes: RR= 0.97, p= 0.01).

Snowdon and Harris, 1992. Australia (117)	South Australia Firearms act 1977	Australian states. 1968-1989, follow-up length= 22 years	Greatest: Cross-sectional time series/Multiple linear regression	Exposure data: Authors provide a description on the implementation of the law Outcome data: Australian Bureau of Statistics	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of firearm suicides	The law was associated with a reduction in the rate of firearm suicides in South Australia from 3.62 per 100,000 in the pre-law period to 3.11 per 100,000 in the post-law period. All other states had increments in the rate of firearm suicides in the post-law period. In addition, there was an increment in the rate of suicides by other methods in South Australia.
Marinho de Souza et al., 2007. Brazil (118)	Brazil's Estatuto do Desarmamento	Brazil. 1996-2004, follow-up length= 9 years	Moderate: Cross-sectional time-series without control group/Linear time series regression	Exposure data: Authors provide a description on the implementation of the law Outcome data: Brazilian Ministry of Health's vital statistics	Data analysis: No alternative strategies to test for robustness of findings given other model specifications; No use of alternative analytical strategies to account for dynamic trends of time series data Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of firearm homicides	The number of firearm homicides decreased by 8.2% from 2003 to 2004, which was significantly lower that the predicted homicides.
Kapusta et al., 2007. Austria (119)	1997 Austrian firearm legislation	Austria. 1985-2005, follow-up length=21 years	Moderate: Cross-sectional time-series without control group/Linear, Poisson and negative binomial regressions	Exposure data: Commission of the European Communities, 2000 Outcome data: Statistics Austria	Data analysis: No alternative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of all suicides, firearm suicides and firearm homicides	The law was associated with reductions in firearm homicide (percent change in trends in pre/post-law periods = -4.8) and firearm suicide rates (percent change = -9.9) in models adjusted for unemployment and alcohol consumption.

Niederkrotenthaler et al., 2009. Austria (120)	1997 Austrian firearm legislation	Austria. 1986-2006, follow-up length=21 years	Moderate: Cross-sectional time-series without control group/Poisson regression	Exposure data: As in Kapusta et al. 2007 (119) Outcome data: Statistics Austria	Data analysis: No alternative strategies to test for robustness of findings given other model specifications Other: Disaggregated results for single units are not provided	Change in the rate of firearm suicides	The law was associated with an abrupt increment (dummy model: beta= 1.1, $p<0.05$) and a gradual reduction in firearm suicides among adolescents (trend model: beta= -0.2, $p<0.05$). The law was also associated with a reduction in the percentage of firearm suicides (trend model: beta = -0.22, $p<0.05$). The law was not associated with changes in overall suicides and non-firearm suicides in dummy and trend models ($p>0.05$)
Beautrais et al., 2006. New Zealand (121)	1992 Amendment to the Arms Act	New Zealand. 1985-2002, follow-up length=18 years	Moderate: Cross-sectional time-series without control group/Poisson regression, auto-regressive and time series models	Exposure data: Authors provide a description on the implementation of the law Outcome data: New Zealand Health Information Service	Data analysis: No alternative strategies to test for robustness of findings given other model specifications. Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Change in the rate of firearm suicide rates	The law was associated with reductions in the rate of firearm suicides among youth (aged 15-24) in the implementation (beta= -0.50; $p<0.05$) and post implementation periods (beta= -1.09; $p<0.001$) compared with the pre-law period. This association was also observed among adults (aged 25 or older) (implementation period: beta= -0.29; $p<0.05$; post-implementation period: beta= -0.49, $p<0.0001$); similar results were observed among those 15 and older. The law was not associated with reductions in all suicides in the post implementation period ($p>0.05$). Alternative models showed no association between the law and suicides

Matzopoulos et al., 2014. South Africa (122)	South Africa's Firearm Control Act	5 cities in South Africa. 2001-2005, follow-up length=5 years	Least: Cross-sectional time-series without pre-post observations and without control group /Generalized linear models	Exposure data: Authors provide a description on the implementation of the law Outcome data: NIMSS	Data analysis: No use of alternative analytical strategies to account for dynamic trends of time series data; No alternative strategies to test for robustness of findings given other model specifications Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Rate ratio of firearm and non-firearm homicides	The law was associated with a significant reduction in firearm homicides year-on-year (RR= 0.864, 95%CI= 0.848;0.880), and a less steep reduction in non-firearm homicides (RR=0.976, 95%CI = 0.95;0.997).
Geisel et al., 1969. United States (123)	Variety of firearm related laws	U.S. states/129 cities with population of 100,000 or more/ 1960 and 1965	Least: Cross-sectional comparison/Linear regression	Exposure data: Different sources for national and state laws are presented by the authors Outcome data: Not provided	Confounders: No adjustment for other potential confounder Outcome: No clear details on source of the outcome variable Other: Disaggregated results for single units are not provided	Difference in homicide, suicide and unintentional firearm death rates given firearm laws index	At the state level the increment in one unit of gun control index was marginally associated with significant reductions in firearm homicides in 1960 or 1965 (beta=-0.18, beta= -0.23, respectively, $p>0.05$); however an increment in one unit in the gun control index was associated with a reduction in firearm suicides in 1960 or 1965 (beta= -0.49, beta= -0.47, respectively, $p<0.05$) and firearm unintentional deaths 1960 or 1965 (beta=-0.19, beta=-0.17, respectively, $p<0.05$)
Murray, 1975. United States (124)	Variety of firearm related laws	U.S. states. 1969	Least: Cross-sectional comparison/Linear regression	Exposure data: As in Bakal 1966 (125) Outcome data: Vital Statistics	Confounders: No information on covariates used in analyses Other: Results of some analyses described in methods are not provided in the text; Disaggregated results for single units are not provided	Difference in rates of homicides and suicides	The laws (individually or grouped as a single variable) were not associated with reductions in the rates of homicides and suicides.

Lester, 1988. United States (126)	Variety of firearm related laws	U.S. regions. 1970	Least: Cross-sectional comparison/Linear regression	Exposure data: As in Lester 1984 (127) Outcome data: NCHS	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of homicides and suicides	In models adjusted for gun ownership and socio-demographic characteristics and divorce rate, firearm related laws were not associated with lower rates of firearm suicides or homicides ($p<0.05$).
Lester and Murrell, 1986. United States (128)	Variety of firearm related laws	U.S. States. 1960 and 1970	Least: Cross-sectional comparison/Linear regression	Exposure data: As in Bakal 1968 (129) Outcome data: Vital and Health Statistics	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of homicides, suicides and firearm unintentional deaths	Stricter firearm related laws were associated with lower firearm suicide rates in 1960 and 1970 (beta=-0.46 and beta= -0.52, $p<0.05$), and unintentional firearm deaths (beta=-0.54 and beta= -0.42, $p<0.05$), but not with firearm homicide rates (beta=-0.08 and beta= 0.07, $p>0.05$)
Seitz, 1972. United States (130)	Variety of firearm related laws	U.S. States. 1967	Least: Cross-sectional comparison/Linear regression models - factor analysis	Exposure data: American Bar Foundation and Newton and Zimring 1970 (131) Outcome data: Vital statistics and the FBI UCR	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of homicides	Firearm related laws were associated with lower rates of firearm homicides among Whites (beta=-0.19, $p<0.05$) but not among non-White individuals ($p>0.05$)
Sloan et al., 1988. United States-Canada (132)	Variety of firearm related laws	Seattle and Vancouver. 1980-1986	Least: Cross-sectional comparison/Maximum likelihood-Mantel-Haenszel summary odds ratio	Exposure data: Codes of Canada and Washington state Outcome data: Medical examiners or coroner reports from each city	Sampling: Convenience sample Confounders: No adjustment for other potential confounders	Difference in rate of homicides	A higher rate of homicides (RR= 1.63, 95%CI= 1.28;2.08) and firearm homicide (RR= 5.08, 95%CI= 3.54-7.27) was observed in Seattle compared with Vancouver

Kwon et al., 1997. United States (133)	Variety of firearm related laws	U.S. states. 1990	Least: Cross-sectional comparison/Multivariate linear regression	Exposure data: Time Magazine Outcome data: Vital and Health Statistics	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rate of homicides	States with more extensive laws were not associated with lower rates of firearm deaths (beta=-2.84, p>0.05)
Kwon et al., 2005. United States (134)	Variety of firearm related laws	U.S. states. 2000	Least: Cross-sectional comparison/Multivariate linear regression	Exposure data: Open Society Institute, New York Outcome data: Vital and Health Statistics	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rate of homicides	States with more extensive laws were associated with lower rates of firearm deaths (beta=-3.33, 95%CI=-5.66,-1.02)
Lanza, 2014. United States (135)	Variety of firearm related laws	U.S. states. 2007-2010	Least: Cross-sectional time-series without pre-post observations/Linear regression	Exposure data: Brady Campaign to Prevent gun violence Outcome data: NCHS	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Differences in rates of firearm homicides	The Brady score (stricter restrictions) was associated with reductions in firearm homicide rates in simple regression models (beta=-0.033, p<0.01) and in the random effects models (beta=-0.013, p<0.05). However, in models with a lagged dependent variable and models with fixed effects the Brady score was not associated with firearm homicides.
Safavi et al., 2014. United States (136)	Variety of firearm related laws	U.S. states. 2009	Least: Cross-sectional comparison/Linear regression	Exposure data: Brady Campaign to Prevent Gun Violence and the Brady Center to Prevent Gun Violence Outcome data: National Inpatient Sample database	Exposure measurement: Not validated scale for exposure classification Outcome measurement: Outcome variable with some percentage of missing data Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of firearm homicides	States with less restrictive laws had higher rates of firearm injuries (beta=3.75, 95%CI=0.25,7.25)

Fleegler et al., 2013. United States (137)	Variety of firearm related laws	U.S. states. 2007-2010	Least: Cross-sectional time-series without pre-post observations/Poisson regression	Exposure data: Brady Campaign to Prevent gun violence and the Brady Center to Prevent Gun Violence Outcome data: WISQARS	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of firearm homicides and suicides	States in the highest quartile of legislative strength (scores of ≥9) compared with those in the lowest quartile (scores of ≤2) had lower overall firearm fatality rates (IRR=0.58, 95 CI= 0.37,0.92), firearm homicide rates (IRR=0.60, 95%CI=0.38,0.95) and suicide rates (IRR= 0.63, 95%CI= 0.48-0.83)
Lester and Murrell, 1980. United States (138)	Variety of firearm related laws	U.S. states. 1959-1961 and 1969-1971	Least: Cross-sectional comparison/Linear regression	Exposure data: No clear description in manuscript Outcome data: Vital Statistics	Exposure: No clear details on source of exposure variable Data analysis: No information on statistical strategies used in analyses Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Differences in rates of suicides	The Guttman score (degree of law strictness) was associated with an absolute increase in suicide rates from 1959-1961 to 1969-1971 (beta=-0.35, p<.01) which was also significant among males and females (p<0.05)
Lester and Murrell, 1982. United States (139)	Variety of firearm related laws	U.S. states. 1960 and 1970	Least: Cross-sectional comparison/Component analysis	Exposure data: As in Bakal 1968 (129) Outcome data: Vital Statistics	Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Differences in rates of firearm homicides	Only firearm restrictions on sellers and buyers were related to firearms homicides (coefficients ranged from -0.35 to -0.40 p<0.01).
Lester, 1990. United States (140)	Variety of firearm related laws	U.S. states. 1965	Least: Cross-sectional comparison/Linear regression	Exposure data: As in Bakal 1968 (129) Outcome data: No clear description	Outcome measurement: No clear details on source of the outcome variable Confounders: No adjustment for potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of homicides and suicides	The Guttmann scale (degree of law strictness) was associated with changes in suicide rates (beta=-0.28, p<0.05), but not with homicides rates (p>0.05)

Sommers, 1984. United States (141)	Variety of firearm related laws	U.S. states. 1970	Least: Cross-sectional comparison/Linear regression	Exposure data: As in Magaddino 1972[b] Outcome data: Vital Statistics	Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Difference in rates of suicides	License to carry laws were associated with reductions in firearm suicides (beta=-1.34, $p<0.05$), and also among Whites, females and males ($p<0.05$). Firearm licenses were associated with lower firearm suicides among females (beta=-0.57, $p<0.05$). Dealer licenses were associated with increments in firearm suicides among females (beta=0.57,$p<0.05$). Waiting periods were not associated with suicide rates
Boor and Bair, 1990. United States (142)	Variety of firearm related laws	U.S. states. 1985	Least: Cross-sectional comparison/Linear regression	Exposure data: National Rifle Association Outcome data: Statistical Abstract of the United States (Bureau of the Census, 1987)	Other: Disaggregated results for single units are not provided	Difference in rates of firearm suicides	Restriction laws on seller including waiting periods and licenses to carry firearms (beta=-0.67, $p<0.05$) and on buyer including license or permit for purchases, registration of firearms, and ownership license (beta=-1.07, $p<0.01$) were negatively associated with suicide rates
Sloan et al., 1990. Unites States-Canada (143)	Variety of firearm related laws	Vancouver and King County 1985-1987	Least: Cross-sectional comparison/Maximum likelihood-Mantel-Haenszel summary odds ratio	Exposure data: Codes of Canada and Washington state Outcome data: Medical examiners records	Sampling: Convenience sample Confounders: No adjustment for other potential confounders	Difference in rates of all and firearm suicides	Vancouver compared with King County had a lower risk for firearm suicides in the overall population (RR=2.34, 95%CI=1.90,2.88), and also among those aged 15-24 (RR=3.14, 95%CI=1.81,5.46). There were no significant differences in all suicide rates.

Conner and Zhong, 2003. United States (144)	Variety of firearm related laws	U.S. states. 1999-2000	Least: Cross-sectional comparison/Poisson ad negative binomial models	Exposure data: Open Society Institute Outcome data: WISQARS	Exposure measurement: Not validated scale for exposure classification Confounders: No adjustment for other potential confounders Other: Disaggregated results for single units are not provided	Incidence rate ratios of suicides	Results among females: Compared with states with restrictive firearm laws, there were higher rates of suicides in states with modest (IRR=1.64; 95% CI 1.34–2.01) and unrestrictive laws (IRR=1.55; 95% CI, 1.23–1.95). Results among males: Compared with states with restrictive firearm laws, there were higher rates of suicides in states with modest firearm laws (IRR=1.51; 95% CI, 1.27–1.79) and unrestrictive firearm laws (IRR=1.49; 95% CI, 1.23–1.82)

Abbreviations: SI, Shall Issue laws; WISQARS, Web-based Injury Statistics Query and Reporting System; RR, rate ratio; IRR, Incidence rate ratio; ARIMA, autoregressive integrated moving average; CAP, Child Access Prevention; NCHS, National Center for Health; UCR, Uniform Crime Reports

[a] Longitudinal prospective or retrospective cohort studies with a concurrent comparison group and multiple pre/post intervention measurements were classified as having "greatest" design suitability; longitudinal studies without a concurrent comparison group but with multiple pre/post intervention measurements were classified as "moderate"; and cross sectional studies or longitudinal studies without a concurrent comparison group and with only single pre/post intervention measurements or with only post intervention measurements were classified as "least" design suitability. Potential limitations in study execution are described in table 3. We acknowledge that a single limitation may seriously threaten the internal validity of studies, and also that limitations described here may, or may not, have impacted the internal validity of studies.

[b] J.P. Magaddino, Virginia Polytechnic Institute, Unpublished dissertation, 1972.

References:

1. Lott JR, Mustard DB. Crime, deterrence, and right-to-carry concealed handguns. *J Legal Stud.* 1997;26(1):1-68.
2. Cramer CE, Kopel DB. "Shall Issue": The new wave of concealed handgun permit laws, *Tenn L Rev.* 1995; 62(3).
3. Ayres I, Donohue JJ. Shooting down the "more guns, less crime" hypothesis. *Stanford Law Rev.* 2003;55(4):1193-1312.

4. Martin RA, Legault RL. Systematic measurement error with state-level crime data: Evidence from the "more guns, less crime" debate. *J Res Crime Delinq.* 2005;42(2):187-210.

5. Maltz M. analysis of missingness in UCR crime data. Washington, D.C. U.S. Department of Justice. 2006

6. Bronars SG, Lott JR. Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *Am Econ Rev.* 1998;88(2):475-479.

7. Bartley WA, Cohen MA. The effect of concealed weapons laws: An extreme bound analysis. *Econ Inq.* 1998;36(2):258-265.

8. Ayres I, Donohue JJ. Yet Another Refutation of the More Guns, Less Crime Hypothesis - With Some Help From Moody and Marvell. *Econ J Watch.* 2009;6(1):35-59.

9. Mustard DB. The impact of gun laws on police deaths. *J Law Econ.* 2001;44(2):635-657.

10. Lott JR. More guns, less crime: understanding crime and gun control laws. Chicago, IL: University of Chicago Press. 1998

11. Olson D, Maltz M. Right-to-carry concealed weapons laws and homicide in large U.S. counties: the effect on weapons types, victim characteristics, and victim-offender relationships. *J Law Econ.* 2001;44:747-770.

12. Plassmann F, Tideman TN. Does the right to carry concealed handguns deter countable crimes? Only a count analysis can say. *J Law Econ.* 2001;44(2):771-798.

13. Plassmann F, Whitley J. Confirming "more guns, less crime". *Stanford Law Rev.* 2003;55(4):1313-1369.

14. Helland E, Tabarrok A. Using Placebo Laws to Test "More Guns, Less Crime". *Advances in Economic Analysis & Policy 4.* 2004;4(1):1-11.

15. Donohue J. Guns, crime, and the impact of state right-to-carry laws. *Fordham Law Review.* 2004;73(2):623-652.

16. Lott J. *More Guns, Less Crime: Understanding Crime and Gun Control Laws*. Second ed. Chicago, IL: University of Chicago Press; 2000.

17. Duggan M. More guns, more crime. *J Polit Econ.* 2001;109(5):1086-1114.

18. Duwe G, Kovandzic T, Moody C. The Impact of Right-to-Carry Concealed Firearm laws on Mass Public Shootings. *Homicide Stud.* 2002;6(4):271-296.

19. Marvell TB. Outline of remarks concerning Lott and Mustard evaluation of ten “shall-issue” handgun permit laws. Paper presented at the annual meeting of the American Society of Criminology, Toronto, Canada. 1999.

20. Ludwig J. Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *Int Rev Law Econ.* 1998;18(3):239-254.

21. Moody CE. Testing for the effects of concealed weapons laws: Specification errors and robustness. *J Law Econ.* 2001;44(2):799-813.

22. Black DA, Nagin DS. Do right-to-carry laws deter violent crime? *J Legal Stud.* 1998;27(1):209-219.

23. Kovandzic T, Marvell T. Right-to-Carry Concealed handguns and Violent Crime: Crime Control Through Gun Decontrol? *Criminol Public Policy.* 2003;2(3):363-396.

24. McDowall D, Loftin C, Wiersema B. Easing Concealed Firearms Laws: Effects on Homicide in Three States. *J. Crim. L. & Criminology.* 1995;86(1):193-206.

25. Rubin PH, Dezhbakhsh H. The effect of concealed handgun laws on crime: beyond the dummy variables. *Int Rev Law Econ.* 2003;23(2):199-216.

26. Vernick JS, Hepburn LM. State and federal gun laws: trends for 1970-99. In: Ludwig J, Cook P, eds. *Evaluating gun policy: Effects on crime and violence.* Washington, DC: Brookings Institution Press. 2003:345-402.

27. Ayres I, Donohue JJ. The latest misfires in support of the "more guns, less crime" hypothesis. *Stanford Law Rev.* 2003;55(4):1371-1398.

28. Wellford C, Pepper J, Petrie C. Right to Carry laws. In: *Firearms and Violence: A Critical Review. Committee to Improve Research Information and Data on Firearms. Committee on Law and Justice, Division of Behavioral and Social Sciences and Education*. Washington, DC: The National Academies Press; 2005:120-151.

29. Grambsch P. Regression to the Mean, Murder Rates, and Shall-Issue Laws. *Am Stat.* 2008;62(4):289-295.

30. Rosengart M, Cummings P, Nathens A, et al. An evaluation of state firearm regulations and homicide and suicide death rates. *Inj Prev.* 2005;11(2):77-83.

31. Hepburn L, Miller M, Azrael D, et al. The effect of nondiscretionary concealed weapon carrying laws on homicide. *J Trauma.* 2004;56(3):676-681.

32. Kovandzic TV, Marvell TB, Vieraitis LM. The impact of "shall-issue" concealed handgun laws on violent crime rates - Evidence from panel data for large urban cities. *Homicide Stud.* 2005;9(4):292-323.

33. La Valle J. Rebuilding at Gunpoint: A City-Level Re-Estimation of the Brady Law and RTC Laws in the Wake of Hurricane Katrina. *Crim Justice Policy Rev.* 2007;18(4):451-465.

34. La Valle J, Glover T. Revisiting Licensed Handgun Carrying: Personal Protection or Interpersonal Liability? *Am J Crim Just.* 2012;37:580–601.

35. Ginwalla R, Rhee P, Friese R, et al. Repeal of the concealed weapons law and its impact on gun- related injuries and deaths. *J Trauma Acute Care.* 2014;76(3):569-574.

36. Strnad J. Should Legal Empiricists Go Bayesian? *Am Law Econ Rev.* 2007;9(1):1-109.

37. Moody CE, Marvell TB. The Debate on Shall-Issue Laws. *Econ J Watch.* 2008;5(3):269-293.

38. Moody C, Marvell TB. The Debate on Shall Issue Laws, Continued. *Econ J Watch.* 2009;6(2):203-217.

39. Lott J. *More Guns, Less Crime: Understanding Crime and Gun Control Laws*. Third ed. Chicago, IL: University of Chicago Press; 2010.

40. Gius M. An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates. *Appl Econ Lett.* 2014;21(4):265-267.

41. Ludwig J, Cook PJ. Evaluating gun policy: effects on crime and violence. Washington, DC: Brookings Institution Press. 2003

42. Ayres I, Donohue JJ. More Guns, Less Crime Fails Again: The Latest Evidence from 1977-2006. *Econ J Watch.* 2009;6(2):218-238.

43. Aneja A, Donohue III J, Zhang J. The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy. NBER Working Paper No. 18294. *National Bureau of Economic Research* (Cambridge, Mass.). 2012. http://www.nber.org/papers/w18294. Published August 2012. Updated November 7, 2014. Accessed December 12, 2014.

44. Villaveces A, Cummings P, Espitia VE, et al. Effect of a ban on carrying firearms on homicide rates in 2 Colombian cities. *JAMA- J Am Med Assoc.* 2000;283(9):1205-1209.

45. Cheng C, Hoekstra M. Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine. *J Hum Resour.* 2013;48(3):821-853.

46. McClellan C, Tekin E. Stand Your Ground Laws and Homicides. Working Paper 18187. *National Bureau of Economic Research.* Cambridge, MA. 2012;1-55. http://nber.org/papers/w18187 Accessed December 13, 2014

47. Kleck G, Patterson EB. The impact of gun control and gun ownership levels on violence rates. *J Quant Criminol.* 1993;9(3):249-287.

48. Ruddell R, Mays GL. State background checks and firearrns homicides. *J Crim Just.* 2005;33(2):127-136.

49. Sumner SA, Layde PM, Guse CE. Firearm death rates and association with level of firearm purchase background check. *Am J Prev Med.* 2008;35(1):1-6.

50. Ludwig J, Cook PJ. Homicide and suicide rates associated with implementation of the Brady Handgun Violence Prevention Act. *Jama-J Am Med Assoc.* 2000;284(5):585-591.

51. Vigdor E, Mercy J. Disarming batterers: The impact of laws restricting access to firearms by domestic violence offenders. In: Ludwig J, Cook P, eds. *Evaluating gun policy: Effects on crime and violence*. Washington, DC: Brookings Institution; 2003:157-215.

52. Vigdor ER, Mercy JA. Do laws restricting access to firearms by domestic violence offenders prevent intimate partner homicide? *Evaluation Rev.* 2006;30(3):313-346.

53. Zeoli AM, Webster DW. Effects of domestic violence policies, alcohol taxes and police staffing levels on intimate partner homicide in large US cities. *Inj Prev.* 2010;16(2):90-95.

54. Dugan L, Nagin DS, Rosenfeld R. Exposure reduction or retaliation? The effects of domestic violence resources on intimate-partner homicide. *Law Soc Rev* 2003;37:169-198.

55. Rodriguez Andres A, Hempstead K. Gun control and suicide: the impact of state firearm regulations in the United States, 1995-2004. *Health Policy.* 2011;101(1):95-103.

56. Sen B, Panjamapirom A. State background checks for gun purchase and firearm deaths: An exploratory study. *Prev Med.* 2012;55(4):346-350.

57. Irvin N, Rhodes K, Cheney R, et al. Evaluating the effect of state regulation of federally licensed firearm dealers on firearm homicide. *Am J Public Health.* 2014;104(8):1384-1386.

58. Vernick JS, Webster DW, Bulzacchelli MT, Mair JS. Regulation of firearm dealers in the United States: an analysis of state law and opportunities for improvement. *J Law Med Ethics.* 2006;34(4):765-775.

59. Medoff MH, Magaddino JP. Suicides and Firearm Control Laws. *Evaluation Rev.* 1983;7(3):357-372.

60. Marvell TB. The impact of banning juvenile gun possession. *J Law Econ.* 2001;44(2):691-713.

61. Webster DW, Vernick JS, Zeoli AM, et al. Association between youth-focused firearm laws and youth suicides. *JAMA - J Am Med Assoc.* 2004;292(5):594-601.

62. Webster D, Crifasi CK, Vernick JS. Effects of the repeal of Missouri's handgun purchaser licensing law on homicides. *J Urban Health.* 2014;91(2):293-302. Erratum in: *J Urban Health.* 2014;91(3):598-601; doi:10.1007/s11524-014-9882-7

63. Reisch T, Steffen T, Habenstein A, et al. Change in suicide rates in Switzerland before and after firearm restriction resulting from the 2003 "Army XXI" reform. *Am J Psychiatry.* 2013;170(9):977-984.

64. Gjertsen F, Leenaars A, Vollrath ME. Mixed impact of firearms restrictions on fatal firearm injuries in males: a national observational study. *Int J Environ Res Public Health.* 2014;11(1):487-506.

65. Cummings P, Grossman DC, Rivara FP, et al. State gun safe storage laws and child mortality due to firearms. *Jama-J Am Med Assoc.* 1997;278(13):1084-1086.

66. Webster DW, Starnes M. Reexamining the association between child access prevention gun laws and unintentional shooting deaths of children. *Pediatrics.* 2000;106(6):1466-1469.

67. Hepburn L, Azrael D, Miller M, et al. The effect of child access prevention laws on unintentional child firearm fatalities, 1979-2000. *J Trauma.* 2006;61(2):423-428.

68. Lott JR, Whitley JE. Safe-storage gun laws: Accidental deaths, suicides, and crime. *J Law Econ.* 2001;44(2):659-689.

69. Lee J, Moriarty K, Tashjian D, et al. Guns and states: Pediatric firearm injury. *J Trauma Acute Care Surg.* 2013;75(1):50-53.

70. DeSimone J, Markowitz S, Xu J. Child Access Prevention Laws and Nonfatal Gun Injuries. *South Econ J.* 2013;80(1):5-25.

71. Koper CS, Roth JA. The impact of the 1994 federal assault weapon ban on gun violence outcomes: An assessment of multiple outcome measures and some lessons for policy evaluation. *J Quant Criminol.* 2001;17(1):33-74.

72. Webster DW, Vernick JS, Hepburn LM. Effects of Maryland's law banning "Saturday night special" handguns on homicides. *Am J Epidemiol.* 2002;155(5):406-412.

73. Ozanne-Smith J, Ashby K, Newstead S, et al. Firearm related deaths: the impact of regulatory reform. *Inj Prev.* 2004;10(5):280-286.

74. Deutsch SJ, Alt FB. The Effect of Massachusetts Gun-Control Law on Gun-Related Crimes in City of Boston. *Evaluation Quart.* 1977;1(4):543-568.

75. Berk R, Hoffman D, Maki J, et al. Estimation procedures for pooled cross-sectional and time-series data. *Evaluation Quart.* 1979;3(2):385-411.

76. Hay RA, Mccleary R. Box-Tiao Time-Series Models for Impact Assessment - Comment. *Evaluation Quart.* 1979;3(2):277-314.

77. Pierce GL, Bowers WJ. The Bartley-Fox Gun Laws Short-Term Impact on Crime in Boston. *Ann Am Acad Polit Ss.* 1981;455(May):120-137.

78. Deutsch S. Intervention modeling: Analysis of changes in crime rates. In: Fox J, ed. *Methods in Quantitative Criminology*. New York, NY: Academic Press; 1981:171-193.

79. Loftin C, Mcdowall D. One with a Gun Gets You 2 - Mandatory Sentencing and Firearms Violence in Detroit. *Ann Am Acad Polit Ss.* 1981;455(May):150-167.

80. Loftin C, Mcdowall D. The Deterrent Effects of the Florida Felony Firearm Law. *J Crim Law Crim.* 1984;75(1):250-259.

81. Mcdowall D, Loftin C, Wiersema B. A Comparative-Study of the Preventive Effects of Mandatory Sentencing Laws for Gun Crimes. *J Crim Law Crim.* 1992;83(2):378-394.

82. Fife D, Abrams WR. Firearms' decreased role in New Jersey homicides after a mandatory sentencing law. *J Trauma.* 1989;29(11):1548-1551.

83. O'Carroll PW, Loftin C, Waller JB, Jr., et al. Preventing homicide: an evaluation of the efficacy of a Detroit gun ordinance. *Am J Public Health.* 1991;81(5):576-581.

84. Marvell TB, Moody CE. The Impact of Enhanced Prison Terms for Felonies Committed with Guns. *Criminology.* 1995;33(2):247-281.

85. La Valle J. Guns and homicide: Is the instrument-focused approach to deterrence efficacious? . *Justice Policy Journal.* 2008;5(2):1-30. http://www.cjcj.org/uploads/cjcj/documents/guns_and.pdf. Published ecember 18, 2008. Accessed December 12, 2014

86. Raphael S, Ludwig J. Prison Sentence Enhancements: The Case of Project Exile In: Cook P, Ludwig J, eds. *Evaluating Gun Policy: Effects on Crime and Violence*. Washington, DC: Brookings Institution; 2003:251-286.

87. Rosenfeld R, Fornango R, Baumer E. Did Ceasefire, Compstat, and Exile reduce homicide? *Criminol Public Policy.* 2005;4(3):419-449.

88. Rosenfeld R. Gun buybacks: crime control or community mobilization. In: Plotkin M, ed. *Under fire: Gun buy- backs, exchanges and amnesty programs* Washington, DC: Police Executive Research Forum.; 1996:1-28.

89. Phillips S, Kim D, Sobol J. An evaluation of a multiyear gun buy-back programme: re-examining the impact on violent crimes. *International Journal of Police Science & Management.* 2013;15(3): 246-261.

90. Leigh A, Neill C. Do Gun Buybacks Save Lives? Evidence from Panel Data. *Am Law Econ Rev.* 2010;12(2):509-557.

91. Reuter P, Mouzos J. Australia: A Massive Buyback of Low-Risk Guns. In: Ludwig J, Cook PJ, eds. *Evaluating Gun Policy: Effects on Crime and Violence*. Brookings Institute Press. 2003:121-155.

92. Magaddino J, Medoff M. State firearm control laws and violent crimes - An empirical analysis of federal and state firearm control laws. In: Kates D, ed. *Firearms and violence*. Cambridge MA: Ballinger; 1984:229-241.

93. Loftin C, McDowall D, Wiersema B, et al. Effects of restrictive licensing of handguns on homicide and suicide in the District of Columbia. *N Engl J Med.* 1991;325(23):1615-1620.

94. McDowall D, Loftin C, Wiersema B. Using quasi-experiments to evaluate firearm laws: Comment on Britt et al's reassessment of the DC gun law. *Law Soc Rev.* 1996;30(2):381-391.

95. Britt CL, Kleck G, Bordua DJ. A reassessment of the DC Gun Law: Some cautionary notes on the use of interrupted time series designs for policy impact assessment. *Law Soc Rev.* 1996;30(2):361-380.

96. Leenaars AA, Lester D. Gender, gun control, suicide and homicide: A reply. *Arch Suicide Res.* 1999;5(1):77-79.

97. Leenaars A, Lester D. Gender and the impact of gun control on suicide and homicide. *Arch Suicide Res*. 1996;(2):223-234.

98. Carrington PJ. Gender, gun control, suicide and homicide in Canada. *Arch Suicide Res.* 1999;5(1):71-75.

99. Leenaars AA, Lester D. The impact of gun control (Bill C-51) on homicide in Canada. *J Crim Just.* 2001;29(4):287-294.

100. Mauser GA, Holmes RA. An Evaluation of the 1977 Canadian Firearms Legislation. *Evaluation Rev.* 1992;16(6):603-617.

101. Blais E, Gagne MP, Linteau I. The effect of laws in relation to firearm control on homicides in Canada, 1974-2004. *Can J Criminol Crim.* 2011;53(1):27-61.

102. Langmann C. Canadian firearms legislation and effects on homicide 1974 to 2008. *J Interpers Violence.* 2012;27(12):2303-2321.

103. McPhedran S, Mauser G. Lethal Firearm-Related Violence Against Canadian Women: Did Tightening Gun Laws Have an Impact on Women's Health and Safety? *Violence Vict.* 2013;28(5):875-883.

104. Rich CL, Young JG, Fowler RC, et al. Guns and suicide: possible effects of some specific legislation. *Am J Psychiatry.* 1990;147(3):342-346.

105. Carrington PJ, Moyer S. Gun control and suicide in Ontario. *Am J Psychiatry.* 1994;151(4):606-608.

106. Leenaars AA, Moksony F, Lester D, et al. The impact of gun control (Bill C-51) on suicide in Canada. *Death Stud.* 2003;27(2):103-124.

107. Caron J, Julien M, Huang JH. Changes in suicide methods in Quebec between 1987 and 2000: the possible impact of bill C-17 requiring safe storage of firearms. *Suicide Life-Threat Behav.* 2008;38(2):195-208.

108. Gagne M, Robitaille Y, Hamel D, et al. Firearms regulation and declining rates of male suicide in Quebec. *Inj Prev.* 2010;16(4):247-253.

109. Cheung AH, Dewa CS. Current trends in youth suicide and firearms regulations. *Canadian journal of public health = Revue canadienne de sante publique.* 2005;96(2):131-135.

110. Leenaars AA, Lester D. Effects of gun control on the accidental death rate from firearms in Canada. *J Safety Res.* 1997;28(3):119-122.

111. Chapman S, Alpers P, Agho K, et al. Australia's 1996 gun law reforms: faster falls in firearm deaths, firearm suicides, and a decade without mass shootings. *Inj Prev.* 2006;12(6):365-372.

112. Baker J, McPhedran S. Gun laws and sudden death - Did the Australian firearms legislation of 1996 make a difference? *Brit J Criminol.* 2007;47(3):455-469.

113. Neill C, Leigh A. Weak tests and strong conclusions: a re-analysis of gun deaths and the Australian firearms buyback. 2007. http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1011519. Published September 5, 2007. Accessed December 12, 2014.

114. Lee WS, Suardi S. The Australian Firearms Buyback and Its Effect on Gun Deaths. *Contemp Econ Policy.* 2010;28(1):65-79.

115. McPhedran S, Baker J. Suicide Prevention and Method Restriction: Evaluating the Impact of Limiting Access to Lethal Means Among Young Australians. *Arch Suicide Res.* 2012;16(2):135-146.

116. Klieve H, Barnes M, De Leo D. Controlling firearms use in Australia: has the 1996 gun law reform produced the decrease in rates of suicide with this method? *Soc Psych Psych Epid.* 2009;44(4):285-292.

117. Snowdon J, Harris L. Firearm suicides in Australia. *Med J Australia.* 1992;156:79-83.

118. Marinho de Souza Mde F, Macinko J, Alencar AP, et al. Reductions in firearm-related mortality and hospitalizations in Brazil after gun control. *Health Aff.* 2007;26(2):575-584.

119. Kapusta ND, Etzersdorfer E, Krall C, et al. Firearm legislation reform in the European Union: impact on firearm availability, firearm suicide and homicide rates in Austria. *Br J Psychiatry.* 2007;191:253-257.

120. Niederkrotenthaler T, Till B, Herberth A, et al. Can media effects counteract legislation reforms? The case of adolescent firearm suicides in the wake of the Austrian firearm legislation. *J Adolesc Health.* 2009;44(1):90-93.

121. Beautrais AL, Fergusson DM, Horwood LJ. Firearms legislation and reductions in firearm-related suicide deaths in New Zealand. *Aust NZ J Psychiat.* 2006;40(3):253-259.

122. Matzopoulos RG, Thompson ML, Myers JE. Firearm and Nonfirearm Homicide in 5 South African Cities: A Retrospective Population-Based Study. *Am J Public Health.* 2014;104(3):455-460.

123. Geisel MS, Roll R, Wettick RS. Effectiveness of State and Local Regulation of Handguns - Statistical Analysis. *Duke Law J.* 1969.(4):647-676.

124. Murray DR. Handguns, Gun-Control Laws and Firearm Violence. *Soc Probl.* 1975;23(1):81-93.

125. Bakal C. The right to bear arms. McGraw-Hill, New York. 1966

126. Lester D. Gun control, gun ownership, and suicide prevention. *Suicide Life-Threat Behav.* 1988;18(2):176-180.

127. Lester D. Gun Control. Springfield, IL: Charles C Thomas Pub. 1984

128. Lester D, Murrell ME. The Influence of Gun-Control Laws on Personal Violence. *J Community Psychol.* 1986;14(3):315-318.

129. Bakal C. No right to bear arms. New York, NY: Paperback Books. 1968

130. Seitz S. Firearms, homicides, and gun control effectiveness. *Law Soc Rev.* 1972;6(4):595-614.

131. Newton GD, Zimring FE. Firearm and violence in American life. Washington D.C.: U.S. Government Printing Office. 1970

132. Sloan JH, Kellermann AL, Reay DT, et al. Handgun regulations, crime, assaults, and homicide. A tale of two cities. *New Engl J Med.* 1988;319(19):1256-1262.

133. Kwon IWG, Scott B, Safranski SR, et al. The effectiveness of gun control laws: Multivariate statistic analysis. *Am J Econ Sociol.* 1997;56(1):41-50.

134. Kwon IWG, Baack DW. The effectiveness of legislation controlling gun usage - A holistic measure of gun control legislation. *Am J Econ Sociol.* 2005;64(2):533-547.

135. Lanza S. The effect of firearm restrictions on gun-related homicides across US states. *Appl Econ Lett.* 2014;21(13):902-905.

136. Safavi A, Rhee P, Pandit V, et al. Children are safer in states with strict firearm laws: A National Inpatient Sample study. *J Trauma Acute Care.* 2014;76(1):146-150.

137. Fleegler EW, Lee LK, Monuteaux MC, et al. Firearm legislation and firearm-related fatalities in the United States. *JAMA Inter Med.* 2013;173(9):732-740.

138. Lester C, Murrell M. The influence of gun control laws on suicidal behavior. *Am J Psych.* 1980;137(1):121-122.

139. Lester D, Murrell ME. The Preventive Effect of Strict Gun-Control Laws on Suicide and Homicide. *Suicide Life-Threat Behav.* 1982;12(3):131-140.

140. Lester D. Capital punishment, gun control, and personal violence (suicide and homicide). *Psychol Rep.* 1990;66(1):122.

141. Sommers P. The effect of gun control laws on suicide rates. *Atlantic Econ J.* 1984;12:67-69.

142. Boor M, Bair JH. Suicide Rates, Handgun Control Laws, and Sociodemographic Variables. *Psychol Rep.* 1990;66(3):923-930.

143. Sloan JH, Rivara FP, Reay DT, et al. Firearm regulations and rates of suicide. A comparison of two metropolitan areas. *New Engl J of Med.* 1990;322(6):369-373.

144. Conner KR, Zhong YY. State firearm laws and rates of suicide in men and women. *Am J Prev Med.* 2003;25(4):320-324.