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Laws Facilitating Gun Carrying and Homicide

See also Siegel, et al., p. 1923.

In "Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States," Siegel et al. (p. 1923) estimate the impact of right-to-carry (RTC) laws on murders over the period from 1991 to 2015. They advance the current literature in a number of ways and bring new data to bear on this important issue. Siegel et al. conclude that RTC laws lead to a substantial increase in murders, almost all of which come through increased firearms killings—specifically from handguns.

VITAL STATISTICS

Although earlier studies on this topic have typically relied on crime data from the Uniform Crime Reports, the authors used murder data from the National Vital Statistics and from the Supplemental Homicide Reports, which enabled them not only to get a different set of estimates on overall homicides but also to explore subcategories of crime that can illuminate our understanding of the impact of RTC laws. The Vital Statistics data are particularly attractive because they are collected under mandatory obligations (as opposed to police data, which are submitted on a voluntary basis) and are gathered pursuant to public health norms that place a high value on measurement and science.[1] A potential pitfall is that the Vital Statistics include justifiable homicide by citizens in their intentional homicide counts (homicides by police in the line of duty are separately classified as "legal intervention" cases). Although this could be problematic if RTC laws increased justifiable homicides in a way that reduced other criminal victimizations, the evidence is now quite strong that RTC laws have led to an increase in overall violent crime.[2] Thus, the value of having the best count of intentional killings is worth the cost of having some justifiable homicides in the measure. After all, as Phil Cook noted in an e-mail to me, "Any reasonable social objective function would say we want to reduce the number of intentional killings, whether criminal or resulting from legal intervention or self-defense."

REPRODUCING

The first row of Table 1 simply reproduces the first line of Table 3 in the Siegel et al. article (while presenting standard errors in parentheses rather than confidence intervals). The first row depicts their estimated incident rate ratios for the impact of RTC laws on five homicide measures, using a negative binomial model for data from 1991 to 2015 and controlling for year and state fixed effects and an array of time-varying, state-level factors. The story emerging from the first row is that RTC laws increase murders, particularly firearm and handgun murders, but seem to have virtually no effect on nongun murders or long-gun murders. This story buttresses the fears of those who think that removing constraints on who can carry handguns will increase intentional killings and criminal use of guns.

REPLICATING

Although Siegel et al. show in their Table 4 that the basic results shown in the first row of my Table 1 are relatively unaffected by a number of permutations, I was interested in further probing some of the econometric choices underlying these estimates. Accordingly, I tried to replicate the first row using the data I had available (I had one less year of data, so I used 1991–2014), making a number of data choices that I thought were used by the authors in creating their own data set and analysis (for more complete details of all the models in Table 1, see the version of this comment on my bepress Web page, http://works.bepress.com/john_donohue). The result is shown in the first replication row of Table 1, which is fairly close to the first row values in that it again shows that RTC laws increase total, firearm, and handgun homicides, while showing no statistically significant effect on nonfirearm homicides and long-gun homicides. The first replication row attempt at replication adjusts its standard errors via clustering by state, which elevates the standard errors, as is typically the case, and which I have found to be necessary.[3]

CRACK COCAINE EPIDEMIC

One potential concern in estimating the impact of RTC laws on crime is that the period from about 1985 through 1992 was one of substantially increasing crime in certain (usually non-RTC) states owing to the crack cocaine epidemic, which tended to improperly make RTC laws seem beneficial even though this simply reflected a serious problem of omitted variable bias. One might fear that by using the data from 1991 to 2015, the authors might be causing the reverse problem: making RTC states look worse than in fact they were as the crack problem

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TABLE 1—The Impact of Right-to-Carry Laws on Intentional Homicides Using Variations in Specification and Time Period: United States, 1991–2015

	CDC, IRR (SE)			SHR, IRR (SE)	
	Total	Firearm	Nonfirearm	Handgun	Long Gun
Original results, 1991–2015	1.065* (0.017)	1.086* (0.020)	1.014 (0.027)	1.106* (0.035)	0.999 (0.045)
My replications					
Original model, 1991–2014	1.078* (0.036)	1.087* (0.043)	1.049 (0.035)	1.164* (0.083)	1.110 (0.102)
Original model, 2000–2014	1.077* (0.038)	1.085* (0.044)	1.068 (0.051)	1.169* (0.078)	1.100 (0.131)
Modified model, 1991–2014	1.092* (0.046)	1.133* (0.061)	1.041 (0.041)	1.177* (0.089)	1.204* (0.108)
Modified model, 2000–2014	1.051 (0.042)	1.089* (0.045)	1.020 (0.053)	1.171* (0.084)	1.103 (0.156)
DAW model, 1991–2014	1.094* (0.047)	1.133* (0.062)	1.049 (0.040)	1.157 (0.091)	1.204* (0.108)
DAW model, 2000–2014	1.060 (0.043)	1.095* (0.045)	1.037 (0.052)	1.158* (0.085)	1.095 (0.153)

Note. CDC = Centers for Disease Control and Prevention; DAW = the basic model that I used in my own study of RTC laws[2]; IRR = incident rate ratio; SHR = Federal Bureau of Investigation's Uniform Crime Reports, Supplemental Homicide Reports. Standard errors clustered in the replication rows. I calculated standard errors for the first row based on authors' confidence intervals, using the delta rule.

*$P < .05$.

subsided and differentially lowered the rate of murder.

To address this problem, I ran the authors' model on data from 2000 to 2014 (a period past the influence of the crack epidemic). The results are shown in the "Original model, 2000–2014" row of Table 1 and again show the almost identical pattern that RTC laws increase murders—specifically by firearms and handguns with no statistically significant impact on nonfirearm and long-gun homicides. The "Original model, 2000–2014" row results importantly show that the authors' results remain robust even when the influence of crack cocaine is controlled for and a different set of 10 states that adopted RTC laws after 2000 is analyzed (as opposed to the full set of 26 states that adopted after 1991).

Siegel et al. made a number of modeling choices that one could handle differently, so I made a few plausible modifications to see if the results would be influenced be these changes. Specifically, I dropped other gun control policies (background check, permit for sale, waiting periods), which might be highly correlated with RTC laws, and dropped all other crime rates (e.g., violent crime), which might be endogenous. I included the proportions of the population that are male; either Black, White, or other race; and either aged 15 to 19 years or 20 to 39 years (rather than the proportions aged 18-29 years and the male proportion of that age group that Siegel et al. used). Alabama data was excluded because of possible missing data, and I re-coded the RTC laws so that the year in which they take effect is counted as the proportion of the year for which the law was in effect. Rather than using both disposable and median household income as controls, only disposable income was included to reduce overfitting. The Siegel et al. model used controls for population density, total population, and urbanization, while my modified model only includes population and percentage of population in a metropolitan statistical area (as a measure of urbanization). I further dropped the gun prevalence measure because of possible confounding with the treatment effect. When I made these changes in a "modified model," they turned out to somewhat strengthen the overall results over the 1991 to 2014 period ("Modified model, 1991–2014" row) and had little impact on four of the five estimates over the 2000 to 2014 period, while weakening the finding on overall homicides ("Modified model, 2000–2014" row).

For completeness, I also repeated the approach of the modified model rows (now shown in the DAW model rows), while using the basic model that I used in Donohue et al.[2] (as opposed to the original model rows or my modified version of the modified model rows). The DAW model rows are quite similar to the results of the modified model rows.

OVERWHELMING SUPPORT

The evidence in Table 1 overwhelmingly supports the view that RTC laws increase firearm homicides by at least 8.5% and handgun homicides by perhaps as much as 16% while having no statistically significant impact on nonfirearm homicides. In all of the rows in the table, RTC laws are consistently associated with increases in total homicides of from 5% to 9.5% (but these estimates lost statistical significance for the shortened 2000–2014 period in the modified and DAW models). This will be important information for judges and policymakers to consider in evaluating RTC laws.

The impact of RTC laws on long-gun homicides is less certain because the estimates are not statistically significant in five of the seven rows and vary more widely than do those in the other four columns from a slight drop to a 20% increase (presumably because of the lower number of long-gun homicides and the weaknesses of the Federal Bureau of Investigation's Uniform Crime Reports, Supplemental Homicide Reports data). The somewhat anomalous statistically significant estimates of RTC laws on long-gun homicides (for the years 1991–2014 in the "Modified model, 1991–2014" and "DAW model, 1991–2014" rows) may provide evidence that the states with large crack problems (who often resisted passing RTC laws) saw larger drops in long-gun homicides in the postcrack era. AJPH

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