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WHY DOES RIGHT-TO-CARRY CAUSE VIOLENT CRIME TO INCREASE?

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ABSTRACT

While the recent state panel data literature has broadly concluded that “right-to-carry” (RTC) concealed handgun regimes increase violent crime, there is little empirical evidence on the precise mechanisms that drive this increase. Using data from 217 US cities, we find that the effect of RTC on violent crime is concentrated to large urban centers. In cities with an average population of over 250,000 between 1979 and 2019, we find that the introduction of RTC increases violent crime by 20 percent. We then present novel estimates that RTC increases gun theft by 50 percent and lowers violent crime clearance rates by 9 percent in these large cities. Leveraging city-level heterogeneity in RTC-induced violent crime effects, we demonstrate that these two mechanisms explain a substantial portion of the RTC-induced increase in violent crime.

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Why Does Right-to-Carry Cause Violent Crime to Increase?

By JOHN J. DONOHUE, SAMUEL V. CAI, MATTHEW V. BONDY, AND PHILIP J. COOK *

While the recent state panel data literature has broadly concluded that “right-to-carry” (RTC) concealed handgun regimes increase violent crime, there is little empirical evidence on the precise mechanisms that drive this increase. Using data from 217 US cities, we find that the effect of RTC on violent crime is concentrated to large urban centers. In cities with an average population of over 250,000 between 1979 and 2019, we find that the introduction of RTC increases violent crime by 20 percent. We then present novel estimates that RTC increases gun theft by 50 percent and lowers violent crime clearance rates by 9 percent in these large cities. Leveraging city-level heterogeneity in RTC-induced violent crime effects, we demonstrate that these two mechanisms explain a substantial portion of the RTC-induced increase in violent crime.

I. Introduction

The United States has undergone a major shift in gun policy over the last four decades. In 1979, most states either banned the concealed carry of firearms or had “may-issue” laws that required someone who wished to carry a concealed firearm to obtain a license, which would screen out those who had no particularized need or who failed to meet other qualifications. An extensive gun lobby campaign persuaded most states to adopt shall-issue laws, which mandate that authorities issue a concealed-carry permit to virtually any individual who applies and is not legally prohibited from possessing a gun. Half the states have taken another step to deregulation by adopting “constitutional carry” or permitless carry regimes, which eliminate the need for any permit or license to carry a concealed handgun. The vast majority of states that have adopted permitless carry have done so in the last few years, and in June 2022, the Supreme Court decided 6-3 in *New York State Rifle Association v. Bruen* to strike down New York’s century-old statute requiring a person to show “proper cause” to obtain a concealed carry license. The *Bruen* decision could lay the groundwork for right-to-carry (RTC) to become the law of the land if the Court goes further to strike down the remaining state laws that provide authorities with some discretion in screening applicants.

Many economists, beginning with Lott and Mustard (1997), have exploited the staggered adoption of RTC laws to examine whether concealed carrying affects violent crime. Lott and Mustard argued that these laws could actually reduce crime, while much of the early literature found no statistically significant effects (Black and Nagin, 1998; Dezhbakhsh and Rubin, 1998; Ludwig, 1998). These initial attempts to evaluate the impact of RTC laws were hampered by the small number of RTC adoptions and limited number of years the laws were in place, as well as the confounding effect of the crack-cocaine induced crime rise of the late 1980s to early 1990s. Substantially more RTC

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adoptions, many more years of data allowing for longer panels, and the development of new tools—such as synthetic controls (Donohue, Aneja and Weber, 2019), marginal structural models (Van Der Wal, 2022), and Bayesian methods (Schell et al., 2020)¹—have enabled a substantial number of researchers to provide a clearer picture of the impact of RTC laws in the last five years. The predominant conclusion of this recent literature is that RTC laws increase violent crime.² This development has enabled the RAND Corporation, which has conducted an extensive project to evaluate the research concerning various gun regulations, to update its earlier tepid endorsement of the harmful effects of RTC laws to a much clearer statement that RTC laws impose substantial crime burdens on adopting states. The report recently summarized its overall conclusion as, “Across all of the 18 policies that we examined, only three—child-access prevention laws, concealed carry laws, and stand-your-ground laws—had evidence that we classified as *supportive*, our highest evidence rating, for an effect on a particular outcome” (emphasis in the original) (RAND, 2023b).³ With the direction of the effect now becoming clear, a natural next step is to clarify *the mechanisms* through which RTC laws increase crime. Of course, gun carrying may thwart and deter some criminals.⁴ But if the current weight of the evidence on RTC laws is correct, any such crime-reducing benefits are small relative to the crime-enhancing impacts of increased gun carrying.⁵

To the extent the literature addresses mechanisms, it tends to focus on the somewhat narrow question of how a normally law-abiding person carrying a concealed weapon may commit a crime that they would not have committed but for a law that eases restrictions on carrying or using a firearm. Work by Cheng and Hoekstra (2013) and McClellan and Tekin (2017) has found that the expansion of Stand Your Ground laws, which make it harder to prosecute those who use deadly force in cases of real or apparent self-defense, results in more (legally unjustified) homicides. Other literature has studied the effect of concealed carrying on mechanisms that affect both law-abiding citizens and criminals. Colmer and Doleac (2022) find that lenient concealed carry laws strengthen the temperature-homicide relationship, suggesting that RTC may increase the likelihood of firearm confrontations to occur as stress increases. Some journalistic commentators and law-enforcement officials have suggested RTC laws are associated with more road rage incidents (Goodman, 2022), while Bushman et al. (2017) find the mere presence of a gun in the vehicle encourages aggressive driving. To our knowledge, McElroy and Wang (2017) is the only study to explicitly focus on career criminals rather than permitholders, modeling how RTC shifts entry and exit patterns for different cohorts. While this body of work is informative, a comprehensive explanation of the mechanisms

¹Schell et al. (2020) find that firearm homicides increased one year after implementation of RTC laws with probability of .99, but suggest that this effect weakens over time.

²See Table 1 of Donohue (2023) summarizing 14 recent papers.

³In earlier versions of the report, the RAND authors found that there was support for the finding that RTC laws increased violent crime, but they concluded that on their five-level criteria, this support only reached level 3, indicating “limited” evidence of this finding (and “no studies with equivalent or stronger methods provided contradictory evidence”). After the latest flurry of articles finding substantial crime increases flow from RTC adoption, RAND adjusted its evaluation of the literature, stating that the evidence in support of this harmful effect was now at their highest level 5, indicating it was “supportive” of the finding that RTC laws increase total and firearm homicides (RAND, 2023a).

⁴There is anecdotal evidence of some thwarting, injuring, and killing of criminals by permit holders as well as anecdotal evidence of some malign effects when permit holders are killed when they try to thwart a crime (Mettler, 2016), inadvertently kill non-criminals (Flores, 2022), or try to portray their own criminal assaults as self-defense gun use (Booker, 2019). Prior empirical research has linked observable victim precaution measures, such as private security (Meehan and Benson, 2017), security alarm systems (Zimmerman, 2014), and neighborhood public safety organizations (Cook and MacDonald, 2011), to lower rates of robbery. Ayres and Levitt (1998) analyze the staggered rollout of Lojack across US geographic markets and find that the introduction of Lojack decreased auto-theft rates, which suggests that the deterrence effect of unobservable victim protection can have positive externalities even for unprotected victims. It will take further work to establish if there is any similar deterrent effect from concealed carry, and also to ascertain whether and to what extent criminals respond with greater violence to all potential victims because they cannot observe those who are carrying weapons.

⁵This point is driven home most dramatically by the fact that when we disaggregate violent crime into its various subcategories, we find that RTC laws induced large *increases* in robberies (results available upon request), despite this being the crime most often committed outside the home and therefore the crime with the largest potential for RTC-induced deterrence (Cook, Moore and Braga, 2004).

driving the RTC-induced increase in violent crime has been elusive.

In this article, we explore two underappreciated mechanisms by which RTC laws might stimulate crime: a decline in criminals' perceived probability of detection and an increase in the net returns from firearm crime. Given the consensus in the literature that policing can reduce violent crime (Chalfin and McCrary, 2017; Klick and Tabarrok, 2005; Levitt, 2002; Weisburd, 2021), RTC laws may increase crime if they interfere with or degrade the quality or effectiveness of law enforcement. Police may have fewer resources to fight crime if they are encumbered by processing more complaints about road-rage incidents, gun thefts, accidental discharges, and injuries or deaths. Additionally, in the case of shall-issue regimes, police may spend more time processing concealed carry permit applications and taking time to check permit validity (Donohue, Aneja and Weber, 2019). Further, police officers may be both more likely to shy away from engaging in effective crime-fighting actions due to the increased risk posed by guns and quicker to use lethal force (Doucette et al., 2022). This use of force could strain police-community relations (Ang et al., 2021; Cook and MacDonald, 2010) in ways that ultimately impair the solving of crimes. While Donohue, Aneja and Weber (2019) has speculated on a possible decline in the quantity or quality of policing due to RTC, we provide the first empirical estimates of this effect and demonstrate how it may be linked to the overall criminogenic effect of RTC laws.

A second possible mechanism is that RTC laws increase both the supply and demand for guns used in crime. On the supply side, more widespread carrying in public facilitates gun theft, which makes guns more readily available on the underground market (Cook, 2018). There is direct evidence that increased legal carrying of firearms outside the home creates more opportunities for gun theft, particularly from vehicles (Elinson and McWhirter, 2022). A working paper by Billings (2023) uses data from Mecklenburg County, North Carolina (a shall-issue regime for Billings (2023)'s entire study period) to match concealed carry permit holders to non concealed-carry permit holders with similar demographic characteristics and finds that concealed carry permit holders are 268 percent more likely to have a gun stolen. Billings (2023) also finds that obtaining concealed carry permits also leads to statistically significant increases in violent crimes committed with a gun in the neighborhoods of permit holders. Although the study does not measure the impact of change in gun policy, Billings (2023) provides strong evidence that increasing the number of concealed carry permits may lead to increases in firearm theft and also bolsters confidence in our hypothesis that increases in gun thefts leads to increases in violent crimes.

Our study of RTC laws covers a broader set of mechanisms than Billings (2023)—highlighting how RTC may also affect the demand side factors that make stealing and using a gun more valuable to criminals. Since RTC laws increase the level of lawful gun carrying, it becomes easier and less costly for criminals to carry guns as police become less proactive about searching individuals for illegal firearms. RTC may also raise the prospect that criminals meet armed resistance while executing a crime, which increases the opportunity cost of *not* carrying a gun to the scene of the crime. Further, a general increase in criminality due to RTC, whether due to declines in police effectiveness or other mechanisms, may lead to an increase in the number of guns stolen as the number of thefts increases. These demand-side factors, coupled with the supply-side factors mentioned previously, will have the net effect of increasing firearm theft.

We exploit the differential timing in the adoption of RTC laws to demonstrate two important forces driving the criminogenic effect of RTC laws—a reduction in the quality of policing and an increase in gun theft—in America's largest cities, focusing on the period from 1979 to 2019. We view our treatment as plausibly exogenous for several reasons. RTC adoption came via different paths for different states (either court ordered or legislatively mandated), often only after repeated failed attempts, and was driven by a relatively small group of progun operatives and citizens (Patrick, 2010). Additionally, the RTC laws present in the cities in our sample were adopted at the state

level, suggesting that forces driving the passage of state RTC laws were at least one step removed from the forces driving city crime trends. In our results section, we produce event-study plots and perform tests of conditional parallel pre-trends to further justify our empirical strategy.

Using Uniform Crime Reporting (UCR) records, we perform a difference-in-differences (DiD) analysis to estimate the impact of RTC laws on violent crimes, the monetary value of stolen guns, and clearance rates (the proportion of violent crimes for which police are able to identify and arrest the perpetrators or resolve a case in which the perpetrator may have been killed).⁶ As expected, we confirm that RTC laws cause statistically significant increases in violent crime, and find that the effect is concentrated in large cities. We therefore focus our analysis of the underlying mechanisms on cities on the 65 cities with an average population throughout our time period of more than a quarter of a million residents, where violent crime rises by 20 percent in the wake of RTC adoption. In these cities, we find that RTC laws cause a roughly 10 percent decline in the rates that police clear violent crime, suggesting that RTC laws strike at the very heart of law enforcement’s abilities to address criminal conduct. We also find that the introduction of an RTC law elevates gun thefts by roughly 50 percent, introducing tens of thousands of guns into the hands of criminals or illegal gun markets each year. We address issues regarding the robustness of our findings to threats from heterogeneous treatment effects using the De Chaisemartin and d’Haultfoeuille (2020) estimator. Our main findings are robust to adjustments to the data we include, the covariates selected, and our level of clustering.

Next, we demonstrate that the cities with the largest declines in clearance rates and the greatest surges in gun theft due to RTC also were the ones that had the most profound increases in violent crime. While our analysis likely understates the relationship between violent crime and these mechanisms due to data constraints, we find that declines in clearance and increases in gun theft may explain at least 30 percent of the RTC-induced increase in violent crime.

The central contribution of our paper is to illustrate that RTC regimes have considerable implications for the dynamics of crime beyond the behavior of permit holders. Indeed, the criminogenic mechanisms associated with RTC laws we document are only inadvertently caused by the action of law-abiding concealed carry permit holders. Understanding these mechanisms is crucial at this moment, as policymakers in New York and the seven other states compelled by the *Bruen* decision to weaken at least some of their protections against the burdens of more permissive concealed carry seek to develop strategies and policies that can mitigate the expected harmful consequences.

The remainder of this paper is organized as follows. Section II describes the data used to complete our empirical analysis. Section III presents our methodology. Section IV contains and discusses our empirical estimates. Section V concludes.

II. Data

Our data come from U.S. cities with yearly population between 1979 and 2019 averaging above 100,000, although for reasons we describe below, we focus our analysis on the subset of 65 cities with an average population above 250,000.⁷ We choose to focus on cities for three main reasons: first, since the vast majority of studies analyzing RTC laws rely on state data, we advance the literature by increasing within-unit homogeneity by using large-city data. Second, in addition to

⁶We will frequently refer to our findings on “gun thefts”, which implicitly assumes that the passage of RTC laws is uncorrelated with the value of the average stolen gun. RTC adoption typically leads to the purchase of more concealable handguns, which on average tend to be cheaper than other firearms, such as rifles, shotguns, and handguns purchased for the home. This would mean our estimates on gun thefts based on dollar value understate the actual number of guns stolen. On the other hand, if the RTC law prompts the purchase of more new guns, that might tend to enhance the average dollar value of stolen weapons. The two effects cut in opposite ways, so the assumption that the dollar value of stolen guns is unchanged by RTC passage is reasonable, and, if anything, likely to be conservative (Buck, 2022).

⁷Although Charlotte, NC would qualify as a large city, we drop Charlotte due to a change in the area served by the police department in 1993.

being a preferable unit of observation, city data is also higher quality than state data because large, concentrated population centers suffer from smaller measurement error biases than other geographic areas in the US (Boylan, 2019). Finally, restricting the sample to more homogeneous units bolsters our confidence that our proposed counterfactuals are reasonable.

We take agency-level monthly crime, clearance, and stolen property data cleaned by Jacob Kaplan from the FBI Uniform Crime Reporting (UCR) Return A files (Kaplan, 2021*b,c*) and aggregate to the city-year level using an agency-city crosswalk published by the Bureau of Justice Statistics (2018). We use the RTC adoption dates from Donohue, Aneja and Weber (2019), updating the statutory history and newspaper archive research through to 2019. City-years in which concealed carrying is prohibited or the state grants concealed carry permits on a “may-issue” basis are regarded as non-RTC observations. Because we expect permitless carry to generally act in the same direction as shall-issue laws on our proposed criminogenic mechanisms,⁸ and because permitless carry regimes account for only 2 percent of the city-years in our data, we code both permitless carry and shall-issue identically as RTC regimes.

Our choice of control variables makes only one addition to the nine socioeconomic and demographic controls used in Kovandzic, Marvell and Vieraitis (2005), one of the few city-level studies of the effect of RTC laws. That study’s nine controls are the percentage of the population made up of female headed households, the percentage of people living alone, per capita income, percentage of people in poverty, and four demographic controls—all of which we obtain at the city level through Census and American Community Survey (ACS) data—as well as the one-year lagged prison incarceration rate, for which we construct a proxy using county- and state-level jail and prison data. We add one-year lagged sworn officers per capita at the city level obtained from UCR police employment data (Kaplan, 2021*a*), due to the well-established relationship between police force size and crime.

We apply a light cleaning procedure that removed crime observations that were sharp discontinuities from the preceding and following years, removing about 8.8 percent⁹ of the data across our main specifications in Tables 2 and 3. We also removed all observations for a particular city-crime if that city-crime was missing more than 15 observations out of the 41 total city-crime observations from 1979 to 2019. This deletion is justified by our sense that the data with high degrees of missingness would lead to more unreliable estimates for the years in which the particular crime data for that city does exist and also makes it impractical to apply our cleaning procedure.¹⁰ Our results are robust to specifications that do not remove any data¹¹ and a fully balanced panel¹² using a multiple imputation approach (Honaker, King and Blackwell, 2011).

Table 1 reports summary statistics from UCR violent crime, clearance, and stolen property data. Column 1 shows overall values for our entire sample, and columns (2)-(5) show these values for both 1979 and 2019 for never-adopters (the cities in our sample that were never covered by RTC laws), and for switchers (cities in our sample that adopted an RTC law between 1979 and 2019). A superficial comparison of the change in crime between 1979 and 2019 for the 16 never-adopting and 48 switching cities helps motivate our empirical estimates.¹³ Violent crime in the never-adopting

⁸All cities in our sample that eventually adopt permitless carry move from shall-issue to permitless carry. While the burden on police from processing concealed carry permits may decrease, permitless carry imposes other strains on police time and likely enhances police fear due to increased civilian concealed carry.

⁹This calculation does not include the regressions underlying Columns 1 and 2 of Table 2. In these regressions, our cleaning procedure removes 16.7 percent of the data, owing to the more erratic nature of crime data reported from smaller cities.

¹⁰Additionally, if a city is missing data for any component of calculating violent crime or violent crime clearance, we drop that city-year in our regressions.

¹¹We always drop crime-years for which a city reported zero cases of a certain type of crime, which would be incompatible with our logged models and are almost certainly due to a failure to report given how large the cities in our sample are.

¹²The fully balanced panel still excludes city-crimes that were missing for more than 15 out of the 41 years, since it would be impractical to attempt to impute a city-crime rate from data that sparse.

¹³One city in our sample, Seattle, is always treated.

cities is much higher in 1979 than in the cities adopting RTC (the switching cities), but by 2019 violent crime was over 30 percent higher in the RTC-adopting cities. Violent crime clearance rates rose substantially in the never-adopting cities but deteriorated in RTC-adopting cities. While the value of stolen guns per capita declined by a similar amount in both groups of cities, the percentage decline is substantially larger for never-adopting cities (76 percent) than for RTC cities (63 percent). These simple comparisons are consistent with our theory of the criminogenic effects of RTC laws, which is more fully explicated by the more complete statistical analysis below.

Table 1—: Summary Statistics

Variable	Overall	Never-Adopters 1979	Never-Adopters 2019	Switchers 1979	Switchers 2019
Violent Crime	11.43 (6.39)	15.38 (4.75)	5.95 (2.57)	9.66 (4.50)	8.06 (3.58)
Violent Crime Clearance	40.32 (11.44)	32.00 (9.07)	48.77 (14.45)	45.74 (10.27)	36.72 (10.47)
Stolen Gun Value	1166.93 (1042.94)	1910.74 (1145.83)	453.30 (361.96)	2042.34 (1280.66)	764.97 (453.04)

Note: All values are weighted by population. Crime variables are measured per 1,000 population, stolen guns are thousands of 2019 US dollars per 1,000 population, and clearance rates are percentages. Population-weighted standard deviations in parentheses. Owing to missing data on stolen guns for the large city of Los Angeles, the values reported in columns 2 and 4 for stolen guns are not 1979 values but 1986 values (the first year Los Angeles reported stolen guns data). Some cities are not included in this table because of missing data in the first or final year, because our data cleaning procedure has identified the city as missing too much data to be included in our regressions, or because the city had RTC in 1979 (and therefore is neither a switcher nor a never-adopter). The violent crime values in this table are derived from 59 cities, the violent crime clearance values are from 56 cities, and the gun theft values are from 44 cities.

III. Methods

Our two-way fixed effects model takes the following form:

$$(1) \quad Y_{it} = \beta RTC_{it} + \gamma' X_{it} + \alpha_t + \delta_i + \epsilon_{it}$$

where Y_{it} represents 100 times the log rate of a given outcome variable in unit i at time t and X represents a constant and our set of covariates. The coefficient β reflects the average estimated treatment effect of adopting an RTC law on crime (in comparison with a restrictive law), and α and δ represent time and city fixed effects, respectively. We code the first RTC year as a fractional value that reflects the proportion of the year that the law is in place. We define violent crime as the sum of robbery, homicide, and aggravated assault.¹⁴ Standard errors are clustered at the *state* level, the level at which “random assignment” occurs.¹⁵

We first estimate the average effect of RTC laws using a static model and then provide event-study analyses that illustrate the dynamic effect of RTC laws. Our event studies regress our outcome variables on a set of yearly dummies for each of the 10 years prior to RTC adoption to 10 years after RTC adoption and bin all more distant periods into 11-or-more years before (or after) RTC adoption dummies, omitting the dummy for one year prior to adoption. Equation 2 shows the model

¹⁴We exclude rape due to the change in definition of rape in UCR coding in 2013. The quality of the data on rape is poor relative to other violent crimes (Kruttschnitt et al., 2014). Moreover, legal and social changes on the reporting of sexual violence and how police handle this reporting (Clay-Warner and Burt, 2005; Miczek, Reiss and Roth, 1993, 408-416) suggest trends in rape could be changing temporally and geographically throughout our study window to a greater degree than for other types of crime in ways that are difficult to measure. The study of the effect of RTC laws on this category of violence merits further research using more suitable data.

¹⁵Because Philadelphia adopted RTC later than the rest of Pennsylvania, we treat it as a separate state for the purpose of our clustering of standard errors. Philadelphia’s RTC was mandated by state-level legislation.

used for our event-study analyses: let $AdoptYear$ be the rounded year of RTC implementation for the cities that came under a RTC regime during our 1979-2019 data period, and equal to zero for all other cities. That is, we compute the least squares fit for:

$$(2) \quad Y_{it} = \sum_{k \in \{-11^+, -10, \dots, 10, 11^+\} \setminus \{-1\}} \beta_k \mathbf{1}[t = AdoptYear_i + k] + \gamma' X_{it} + \alpha_t + \delta_i + \epsilon_{it}$$

IV. Results

A. Effects of RTC on Violent Crime, Gun Theft, and Clearance

We present our results in Table 2, which shows a positive effect of RTC on violent crime concentrated in major cities. This finding is consistent with Siegel et al. (2020), which finds that RTC causes larger increases in firearm homicides in large cities compared to suburban or rural areas. The first column of Table 2 adds an interaction term between the RTC indicator and a binary variable for whether a city has an average population less than 250,000 (“small cities”) across the years in our study. We select this cutoff given the common use by federal agencies as an indication of a large urban area (US Department of Agriculture, 2013; United States Government Publishing Office, 2021; National Center for Education Statistics, 2019). Column (1) reveals that RTC laws led to a statistically significant increase in violent crime of 0.20 log points in large cities, which corresponds to a 22 percent increase. This finding is consistent with the majority of the recent state-based empirical literature and theoretical predictions, which also concludes that RTC adoption elevates violent crime.¹⁶ We also see that the effect is on average roughly 0.09 log-points lower in small cities compared to large cities. This finding does not depend on the precise threshold for demarcating small and large cities. In Column (2), we break our cities up by average population quartiles rather than use a binary threshold, again using the most populous cities as the reference group. While Quartile 3 experiences an RTC-induced increase in violent crime of a very similar size as in Quartile 4, and the criminogenic effect in Quartile 2 is a non-significant 0.07 log-points less severe than Quartile 4. The difference in effect size between the smallest and largest quartiles is significant at the 5-percent level, with cities in Quartile 1 experiencing an average increase in violent crime due to RTC of only 2 percent, which is not significant. Finally, Column (3) restricts the sample to large cities only. Because the effect seems to be concentrated in major urban areas, we focus on the large-city subsample for the remainder of this paper.

Table 3 presents novel evidence suggesting that RTC adoption causes a statistically significant 0.40 log-point, or 50 percent, increase in the value of stolen guns, in Column (1), and a statistically significant 9 percent decline in the clearance rate of violent crimes, in Column (3). The strikingly high magnitudes of these results suggest that the aggregation of these two mechanisms alone may explain a significant proportion of the overall criminogenic effect of RTC laws.

We note that one reason that gun thefts may increase in the wake of RTC adoption is that the robbery, larceny, and burglary increases resulting from the new regime mechanically leads to more gun thefts. To determine if this factor alone explains some or all of the RTC-induced increase in gun thefts, we re-estimate our stolen guns regression in Column (2), controlling for the one-year lagged log rate of the total value of stolen property (excluding firearms) per capita. We lag this

¹⁶While the focus of our article is on the mechanisms driving the RTC-induced increase in violent crime, agency level UCR and Supplemental Homicide Report (SHR) provide rich data decomposing violent crime by sub-categories of crime—robbery, homicide, and aggravated assault—and by weapon type. In the appendix, we show the effect of RTC on firearm and nonfirearm violent crime. As would be theoretically predicted, both nonfirearm and firearm violent crime are estimated to increase following RTC adoption, and the increase in firearm violent crime is larger in magnitude than nonfirearm violent crime (analyses available by request).

Table 2—: Effect of Right to Carry on Crime by City Population, OLS Estimates (1979-2019)

	100 x log violent crime rate		
	Full Sample	Full Sample	Large Cities Only
	(1)	(2)	(3)
RTC Law	20.00*** (4.91) p = 0.00	19.78*** (5.08) p = 0.00	18.52** (6.14) p = 0.01
RTC x Small (Less than 250k)	-9.01* (3.84) p = 0.02		
RTC x Quartile 1 (100k-126k)		-17.92** (5.88) p = 0.01	
RTC x Quartile 2 (127k-176k)		-7.03 (7.34) p = 0.34	
RTC x Quartile 3 (177k-303k)		-3.15 (5.16) p = 0.55	
Observations	7,094	7,094	2,468
R ²	0.88	0.88	0.86
Adjusted R ²	0.87	0.87	0.85

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: City-level panel data estimates with city and year fixed effects, 1979-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II, with coefficients reported in our Online Appendix but not shown here to conserve space. All regressions are weighted by time-varying city populations.

control variable to mitigate endogeneity concerns, but recognize that even with this correction the model is less-than-ideal because of the risk that it includes a covariate that is affected by our treatment variable. For this reason we prefer the specification in Column (1). The high magnitude of the estimated effect even when controlling for the total value of stolen property indicates that the RTC-triggered increase in stolen guns is caused primarily by factors other than the overall increase in stolen property that RTC laws may stimulate. This result suggests that the greater vulnerability to theft afforded by increased firearm carrying and other factors described in the introduction are the leading factors driving the increase in gun thefts following the introduction of RTC laws.

Similarly, one factor that might contribute to the 9 percent decline in clearance rates shown in Column (3) is that the increased crime resulting from RTC adoption may impair the ability of the police to clear crimes at the same rate. To determine if this factor alone explains some or all of the RTC-induced drop in clearance rates, we re-estimate our preferred clearance rate regression, Column (3), controlling for the lagged log violent crime rate in Column (4). Adding this control reduces the size of the clearance drop from about 9 percent to 8 percent ($p = 0.06$).¹⁷ This suggests that the RTC-triggered decline in police effectiveness is largely driven by factors other than the overall increase in violent crime that RTC laws stimulate. While an in-depth examination of the

¹⁷Note that the regression estimate controlling for violent crime is likely biased downward by the presence of ratio bias. This occurs because overall violent crime is in the denominator of the clearance rate dependent variable and in the numerator of the independent variable, which will bias the estimate downward because violent crime is measured with error.

Table 3—: Effect of Right to Carry on Gun Theft and Policing, OLS Estimates (1979-2019)

	100 x log rate of . . .			
	Gun Theft	Gun Theft	Clearance	Clearance
	(1)	(2)	(3)	(4)
RTC Law	40.27*** (12.19) p = 0.00	30.96** (10.15) p = 0.01	-9.22* (4.47) p = 0.04	-8.09† (4.27) p = 0.06
Lagged 100 x Log Value of Nongun Property Theft		0.40*** (0.08) p = 0.00		
Lagged 100 x Log Violent Crime Rate				-0.07 (0.07) p = 0.35
Observations	2,157	2,127	2,286	2,283
R ²	0.80	0.81	0.54	0.54
Adjusted R ²	0.79	0.80	0.52	0.52

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: City-level panel data estimates with city and year fixed effects, 1979-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II, with coefficients reported in our Online Appendix but not shown here to conserve space. Models (1) and (2) drop the cities of Chicago, Minneapolis, New York, Newark, Omaha, San Jose, and Washington, because our data cleaning procedure described in Section II identified these cities as missing more than 15 years of gun theft data. All regressions are weighted by time-varying city populations.

“mechanisms driving the mechanisms” of RTC-induced declines in violent crime clearance and increases in gun theft is outside the scope of this paper, we do find evidence that is consistent with the literature suggesting that officers pull back from more active policing when their feeling of danger on the job is elevated.¹⁸ Since the greatest danger to police from criminal activity is posed by those with firearms, it would not be unreasonable to assume that police would feel greater apprehension as the number of their contacts with armed individuals—often in highly contentious interactions—increases. Following Cho, Gonçalves and Weisburst (2021), we use rates of “quality-of-life” (low-level) arrests as a measure of risk-avoidance behavior by police. We find suggestive evidence of a sizable 17 percent decline in quality-of-life arrests following the passage of RTC laws ($p = 0.08$), and we also demonstrate that the magnitude of the decline in quality-of-life arrests following RTC is associated with the magnitude of the decline in violent crime clearance following RTC.¹⁹

To address concerns that staggered DiD estimates such as those of Tables 2 and 3 can be contaminated by “unclean” comparisons (Roth et al., 2022), we re-estimate our results using the method from De Chaisemartin and d’Haultfoeuille (2020).²⁰ We use the Honaker, King and Blackwell (2011) multiple imputation approach to achieve balanced panel data, improving the efficiency of the De Chaisemartin and d’Haultfoeuille (2020) estimator. While the Table 4 point estimates on violent crime and gun theft are not meaningfully changed relative to Tables 2 and 3, the column (4) point estimate on the decline in violent crime clearance rates resulting from RTC adoption doubles

¹⁸We do not examine the number of law enforcement officers killed in action as an outcome variable because the effect of RTC is theoretically ambiguous: a proliferation of illegal firearms and elevated crime might lead to more officers being killed, but on the other hand, if police become less engaged (as our regressions suggest), they may be exposed to fewer situations that could result in death.

¹⁹See appendix for details.

²⁰Among the proposed estimators to address bias from heterogeneous treatment effects in staggered treatment settings, the procedure from De Chaisemartin and d’Haultfoeuille (2020) is most similar to Callaway and Sant’Anna (2021) and Sun and Abraham (2021), particularly when treatment is monotonic. For our modeling, we prefer De Chaisemartin and d’Haultfoeuille (2020) because it allows us to condition on time-varying covariates and use population weights in Stata.

to -0.20 log points, or 18 percent ($p = 0.077$). Although the gun theft and clearance estimates in Table 4 are less precise than the OLS estimates, pre-passage trends appear flat in the corresponding event studies.

Table 4—: Effect of Right to Carry on Crime, de Chaisemartin and D’Haultfoeuille Estimates

	100 x log rate of ...				
	Violent Crime	Gun Theft	Gun Theft	Clearance	Clearance
	(1)	(2)	(3)	(4)	(5)
RTC Law	17.35** (6.24) $p = 0.005$	36.98* (18.42) $p = 0.045$	40.98* (20.25) $p = 0.043$	-20.26† (11.44) $p = 0.077$	-23.22† (12.44) $p = 0.062$
Stolen Property Control	No	No	Yes	No	No
Violent Crime Control	No	No	No	No	Yes

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: Average treatment effects using De Chaisemartin and d’Haultfoeuille (2020) difference-in-differences estimator on 1979-2019 panel data, using same controls as in Tables 2 and 3. As in Table 3, Models (2) and (3) drop the cities of Chicago, Minneapolis, New York, Newark, Omaha, San Jose, and Washington. All regressions are weighted by time-varying city populations. The OLS analogue to the violent crime estimate in Column 1 of this table is Column (3) of Table 2, and Columns (2) through (5) above correspond to Columns (1) through (4) of Table 3.

Additionally, we provide a battery of tests in our Online Appendix showing that our findings from Tables 2 and 3 are robust to the inclusion or exclusion of certain controls,²¹ the precise population threshold for our analysis, whether we impute missing data or use a complete-case analysis, and the exclusion of particular cities.

B. Event Study Analysis

We supplement our static regression models with an event-study analysis. The purpose of this analysis is two-fold: first, we comment on the validity of the conditional parallel trends assumption, and second, we can observe how the impact of the RTC on the outcome variables changes over time.

A standard test for assessing the parallel trends assumption is to establish that one cannot reject the assumption that the yearly dummies from two up to five years prior to adoption are jointly different from zero. We find non-significance at the 0.1-level for all F-tests on all our Table 2 outcome variable regressions.²² Beyond our F-tests and visual inspections of the event-study analyses, we also note that for all our statistically significant outcome variables, any deviations from parallel pretrends are small relative to the treatment effect. Manski and Pepper (2018) used partial identification analysis to study the effect of RTC laws on crime in three states.²³ As a quantitative analysis in the spirit of Manski and Pepper (2018), we consider the magnitude of our estimated treatment effects relative to any prepassage deviation from parallel pretrends. For example, consider how our point estimate that RTC laws increase violent crime by 0.1852 log points compares to the largest deviation from zero in any of the five placebo years prior to RTC passage, $\hat{\theta} = \max\{|\hat{\beta}_{-5}|, \dots, |\hat{\beta}_{-2}|\} = 0.0252$. The estimated treatment effect is $0.1852/0.0252 = 7.36$ times as large as this $\hat{\theta}$, meaning that the magnitude of the deviation from parallel trends in the *post*-treatment period that would be required to reverse the sign of our average treatment effect is

²¹Due to possible concerns about the quality of the incarceration data, we run a version of our model that excludes this control. We also try a specification that adds a control for the unemployment rate.

²²See Appendix D for event-study plots and F-tests of all variables.

²³We note Manski and Pepper (2018) base their primary analysis on a simple comparison of RTC-adopting Virginia versus non-adopting Maryland, but use 1989 as the RTC passage date for Virginia when the correct date is 1995. As a result, the RAND report concluded that Manski and Pepper (2018) did not provide useful estimates of the impact of RTC laws, but their analytical structure is valuable for our purposes.

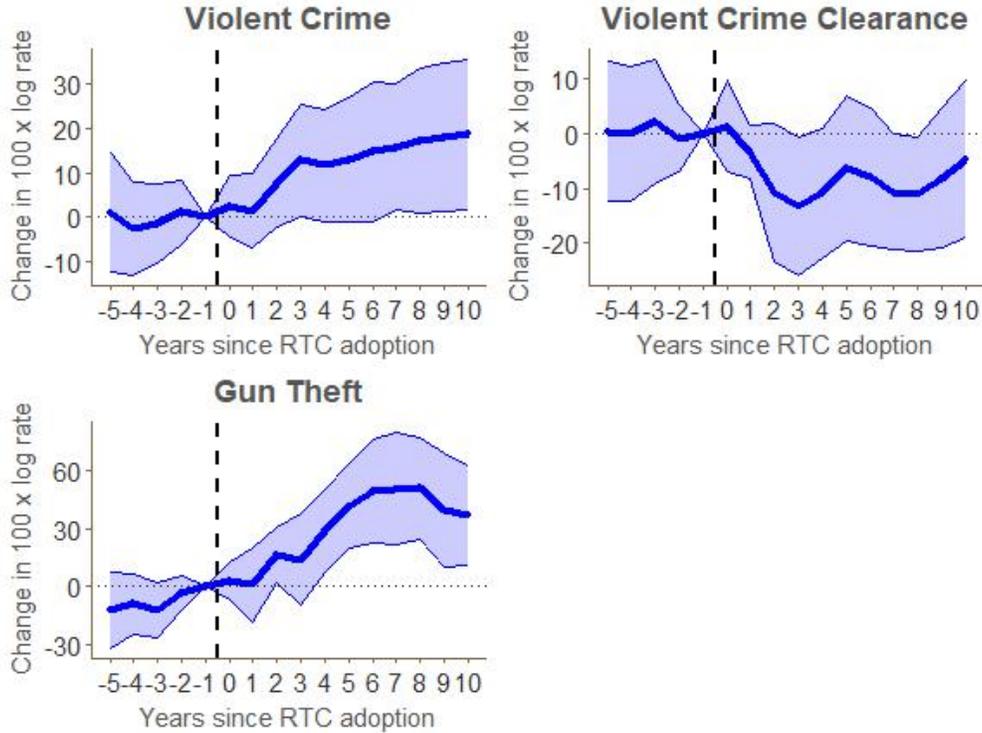


Figure 1. : Event Plots for Key Outcomes

Note: 95 percent confidence intervals with cluster-robust standard errors displayed. Clearance model displayed here does not include control for violent crime rate and gun theft model does not control for property crime.

more than seven times as large as any deviation observed in the five years prior to passage.²⁴ De Chaisemartin and d’Haultfoeuille (2020) style event plots can be found in the Appendix and are qualitatively similar to the OLS event plots.

C. Relationship Between Mechanism and Outcome Effect Magnitudes

We now turn our attention to the question of whether there is indeed a connection between the violent-crime effects of RTC and our findings regarding our proposed mechanisms—gun theft and clearance. We present a collage of evidence that suggests causal relationships in both cases. Our approach draws on a treatment-intensity analysis similar to Khanna (2020) and Chalfin et al. (2022). It should be noted upfront that we view this task as inherently less precise than our main findings presented above because it exploits heterogeneity in the effects of RTC adoption across the cities in our sample rather than a strictly exogenous source of variation. Obviously, using an estimated treatment effect for an individual city as a dependent variable will necessarily introduce greater imprecision than trying to get an average treatment effect across 39 cities, but our analysis in this section corroborates the hypothesis that RTC laws increase crime by increasing gun thefts and damaging police effectiveness in clearing crimes.

If our proposed mechanisms are correct, then one testable implication should be that the cities that experienced especially severe declines in clearance rates due to RTC should have suffered relatively large increases in violent crime, and similarly, the cities with the largest increases in gun theft due to RTC should have on average experienced greater increases in violent crime relative

²⁴A similar analysis for gun theft and violent crime clearance yields multipliers of 3.35 and 4.35, respectively.

to other cities. As a first step, we calculate uncontaminated city-level effects of RTC on violent crime, gun theft, and clearance in the following manner. For each RTC-adopting city, we run the same model used in our main results presented in Equation 1 using only that single RTC-adopting city and all never-treated cities, constraining the data to be from 10 years before the RTC date through 10 years post RTC to maximize comparability across cities.²⁵ Figure 2 shows the pattern of city-level estimated effects of RTC on violent crime conditional on having above- or below-median changes in clearance and gun theft conform to our theoretical expectations.

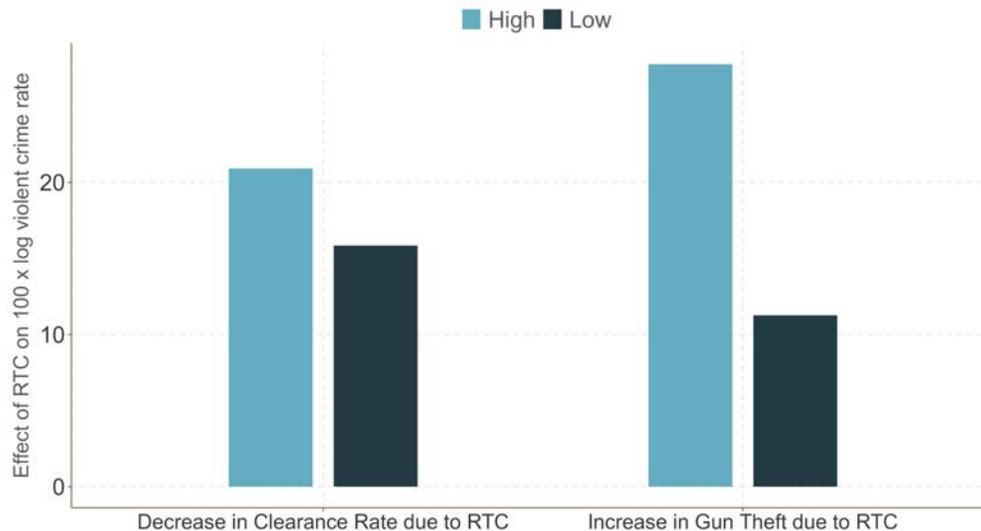


Figure 2. : Violent Crime Effect Sizes Conditional on Mechanism Magnitudes

Note: Estimates derived from 39 treated cities for which estimates of the effects of RTC on all three outcomes—clearance, gun theft, and violent crime—were obtainable. Cities with above-median effects are labeled "high" and all others are "low."

We can go one step further and calculate the implied elasticities of violent crime with respect to clearance and gun theft using the following regression:

$$(3) \quad \Delta_{RTC}(\ln Crime)_i = \alpha + \beta(\Delta_{RTC}(\ln Clearance)_i) + \gamma(\Delta_{RTC}(\ln Gun Theft)_i) + \epsilon_i$$

where Δ_{RTC} represents the effect of RTC on an outcome relative to the counterfactual for individual city i . Column (1) of Table 5 shows the results of this regression for violent crime, while Columns (2) and (3) show the regression results when only the clearance or the gun theft effect variable is included respectively.

For our regression results to take on a causal interpretation, one must assume that the magnitudes of the *RTC-induced* clearance and gun theft effects for each city i are uncorrelated with the error term ϵ_i . The most prominent mechanisms driving violent crime increases discussed in the literature are related to impulsive human behavior (Colmer and Doleac, 2022), which is plausibly stable over time and place and at most weakly correlated with our observed mechanisms, which relate to local criminal conditions and institutions. Thus, we reason that omitted variables exert a relatively small influence on our estimates relative to the magnitude of the coefficients observed.

²⁵Due to endogeneity concerns we use the specification from Columns (1) and (3) of Table 3, which do not control for the level of violent crime in our clearance estimates and do not control for stolen property in our gun-theft estimates. However, we present an alternative specification of Table 5 in the Online Appendix that uses the specifications from Columns (2) and (4) of Table 3, which do include such controls.

Table 5—: Implied Elasticities of Crime With Respect to Clearance and Gun Theft

	Change in 100 times log rate of ...		
	Violent Crime	Violent Crime	Violent Crime
	(1)	(2)	(3)
Change in 100 times log clearance rate	-0.12 (0.10) p = 0.24	-0.14 (0.11) p = 0.21	
Change in 100 times log value stolen guns rate	0.09 (0.07) p = 0.19		0.10 (0.07) p = 0.17
Constant	13.58** (4.38) p = 0.01	17.09*** (3.85) p = 0.00	14.97*** (4.21) p = 0.00
Observations	39	39	39
R ²	0.09	0.04	0.06
Adjusted R ²	0.04	0.01	0.04

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: Some treated cities are not included in these regressions because they do not have ten years of observations both before and after RTC adoption, or because of excessive missing data or other anomalies in at least one of the following variables: violent crime, gun theft, or clearance. Those cities are: Chicago, Indianapolis, Jacksonville, Miami, Milwaukee, Minneapolis, Omaha, Tampa, and Washington. All regressions are weighted by time-varying city populations.

Another possible concern in interpreting Table 5 is that causality runs in the opposite direction of what we propose. For instance, a positive correlation between the size of the increase in gun theft and the size of the increase in violent crime may simply reflect the fact that RTC regimes increase robberies; some robbery victims will inevitably be carrying concealed weapons and hence gun thefts will also rise. This concern is mitigated by the fact that the vast majority of guns are stolen through (nonviolent) property crimes (Langton, 2012). We find it more plausible that causality runs in the direction we propose because existing empirical evidence indicates firearm availability is an important margin influencing criminal behavior. Only 40 percent of robberies are committed with a firearm in our sample, despite the fact that firearms greatly increase payoff to robbery by increasing the probability of “success” and allowing criminals to pursue more robust targets like commercial establishments (Cook, 1987, 1991). Likewise, we believe that on balance the data casts doubt on the notion that causality flows purely from rising crime to a mechanical decrease in clearance. Our results in Column (4) of Table 3 and the observed decline in quality-of-life arrests suggest that a decline in the criminal’s probability of detection plays some role in the RTC effect on clearance.

Despite these caveats, the directions of the coefficients in Table 5 bolster our confidence that declines in violent crime clearance and increases in gun theft are among the mechanisms driving increase in violent crime. The left-hand-side and right-hand-side variables are both measured in logarithmic terms, so we can roughly interpret the coefficients (between -1 and 1) roughly as elasticities. As noted above, the effects of RTC in each individual city are estimated with substantial measurement error, these elasticities are likely attenuated toward zero. Attenuation bias is probably one reason why our estimated elasticity of violent crime with respect to policing, -0.12, is noticeably smaller in magnitude than estimates in the literature on police force size and visibility (Bun et al.,

2020, p.2308-2310), which has found elasticities ranging from -0.225 to -0.9. (It is also worth noting, however, that ours is a slightly different elasticity, since we measure the elasticity of the *RTC effect on crime* with respect to the *RTC effect on clearance*.) The literature on the elasticity of gun theft with respect to violent crime is underdeveloped, but Khalil (2017)²⁶ estimates elasticities of gun theft with respect to various subcomponents of violent crime that would imply an elasticity of gun theft with respect to overall violent crime of about 0.04, which is lower than our estimated elasticities of 0.09 and 0.11. We note that because of data constraints, both our current analysis and Khalil (2017) attempt to estimate the elasticity of stolen firearms with respect to violent crime *in the jurisdiction in which the firearm was reported stolen*. Thus, our calculated elasticity is an underestimate of stolen firearms with respect to violent crime in all jurisdictions since it does not consider how illegal gun flows may cross borders, creating negative spillover effects. Studies of firearms recovered in Chicago between 2009 and 2013 (Cook et al., 2014) and Boston between 1991 and 1995 (Braga, 2017) suggest that around 60 to 70 percent of guns recovered come from out of state, and Knight (2013) suggests that guns may flow from states with weaker gun laws to those with stronger gun laws. The large externalities associated with increased gun theft in a particular jurisdiction suggest that our implied elasticity understates the harmful impact of RTC laws on crime caused by increased firearm theft.

If we consider the regression constant in Table 5 to be the RTC-induced rise in violent crime that is not due to either of our two proposed mechanisms, then given that the cities contributing to these regressions have a weighted average of an RTC-induced rise in violent crime of .1896 log points, the regression from Column (1) suggests that our two mechanisms explain 28 percent of the increase in violent crime due to RTC laws. An alternative way of calculating the salience of our two mechanisms is to multiply the RTC effects on clearance and gun theft by the elasticities in Table 5 and compare the sum to the estimated effect of RTC on violent crime. Using OLS estimates (from Tables 2 and 3), we find our mechanisms explain 26 percent, and using the De Chaisemartin and d’Haultfoeuille (2020) estimates (from Table 4), 31 percent, of the increase in violent crime due to RTC. However, due to the measurement error in our estimating procedure and the negative externalities of firearm theft due to gun flows across borders, we believe that our elasticities likely understate the true relationship between our mechanisms and violent crime. If we hold our elasticity for gun theft fixed at a conservative value of 0.09 and vary the clearance elasticity to be within the bounds of what is accepted in the policing literature, our two mechanisms explain a more reasonable 30 to 64 percent of the criminogenic effect of RTC.

V. Conclusion

Our study solidifies the prior empirical support for the finding that RTC laws substantially increase violent crime based on state panel data analyses and importantly demonstrates that this effect is found on a large-city panel data set. We begin our analysis by demonstrating that criminogenic effect of RTC is primarily focused in large urban centers. The central and most novel contribution of our article, however, is to advance the understanding of two mechanisms governing criminal and police responses to gun laws in America’s largest cities: RTC laws inhibit the ability of police to solve crimes and lead to a surge in gun theft. In doing so, we demonstrate that the empirical landscape is more complicated than a simple matter of whether concealed carry can deter or thwart crime or whether it allows for people to more easily reach for a gun in a moment of anger. Our work illuminates two significant and largely unexpected harmful consequences that flow from the adoption of RTC laws.

²⁶Although Khalil (2017) does not claim a source of exogenous variation in gun thefts, we discuss our finding in reference to this value due to the current gap in the literature.

More positively, our findings suggest some possible initiatives that major urban areas might adopt to try to mitigate some of the harmful consequences of permissive rules governing concealed carry. Efforts to discourage concealed carry permit holders from engaging in careless behavior that allows guns to fall into the hands of criminals may have large benefits. Efforts at alerting gun owners to the substantial problem of gun theft may also prove beneficial. Steps can also be taken to reduce officer fear or to directly offset the RTC-induced decline in police effectiveness through various means, including improving police monitoring and incentives in ways that maximize the social benefits and minimize social costs of policing (Kapustin, Neumann and Ludwig, 2022; Cheng and Long, 2018; Owens and Ba, 2021).

At least two avenues for future research are apparent. First, more granular data on the illicit market for stolen guns and the flow of illegal firearms could clarify the extent to which firearms stolen under an RTC regime increase crime by converting thieves into violent criminals or simply increasing the weaponry available to violent criminals, who thereby become more dangerous and criminally productive. Second, given the right data, it may be possible to clarify the reasons for RTC-induced declines in police effectiveness by measuring whether RTC laws reduce the quantity or quality of police-civilian interactions, effectively pose a tax on police time, degrade police-community relations, or diminish risky but effective crime-suppressing police interventions or operations.

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