Can Easing Concealed Carry Deter Crime?*

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Objective. Laws reducing hurdles to legally carrying concealed firearms are argued to have a deterrent effect on crime by increasing its perceived costs. This argument rests on the assumption that these policies will either directly or indirectly increase the perceived distribution of firearm carriers, an assumption that is as yet untested. This article tests this assumption and, in so doing, suggests testing the necessary conditions of policy can be useful when assessing outcomes is difficult. *Methods.* I collect survey data on the perceived number of firearm carriers across the United States and then use a hierarchical regression model to assess the impact of concealed carry policies on these perceptions, controlling for several contextual and individual-level factors. *Results.* The data suggest that there is no statistically discernible relationship between concealed carry policies and the public's perceptions of the number of firearm carriers. Indeed, the data suggest that the perceived density of firearm carriers is similarly uncorrelated to the number of active concealed carriers. *Conclusion.* The link between concealed carry policy and people's beliefs about the number of firearm carriers in their community is unidentifiable in the data. The rationale for concealed carry deterrence, however, depends on such a link existing: it assumes that potential assailants are aware of the distribution of firearm carriers in the potential victim population, but the empirical evidence presented here suggests that that assumption simply does not hold. Because beliefs over the distribution of firearm carriers are impervious to permitting policies and do not respond positively to the true distribution of carriers, the data suggest easing concealed carry cannot deter crime.

Motivation

One of the most salient public health issues in the United States is its startlingly high rate of gun violence. Indeed, recent comparative research shows that the United States has the highest rate of firearm death of any country remotely comparable in wealth and quality of life (Bangalore and Messerli, 2013). Further, even as general crime rates have fallen precipitously in the United States from their high points in the 1980s and 1990s, the relative rates of crime—especially violent crime, robbery, and aggravated assault—involving firearms has remained stable or even *increased* over this period. More disturbingly, the percentage of firearm-related crimes resulting in fatality has nearly doubled over this period (Planty and Truman, 2013), implying that while nationwide trends in overall crime are encouraging, the crime that we do experience is just as likely to involve a gun, and even more likely to end in death. This trend is much more costly than typically acknowledged. It has been estimated that firearm violence shortens the average lifespan by over 100 days and exacts a public health toll on American males in excess of colon and prostate cancer combined (Lemaire, 2005).

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SOCIAL SCIENCE QUARTERLY

DOI: 10.1111/ssqu.12166

Many states have responded to these trends by easing the process of obtaining a concealed carry permit (CCP)—a permit that allows one to legally carry a concealed firearm.[1] Proponents of these policies argue that concealed carry deters crime by increasing the number of armed law-abiding citizens, therefore increasing the probability that a potential victim may be carrying a firearm or by increasing the number of people that could potentially halt a crime in progress. Both avenues increase the expected costs to a perpetrator of a crime, thereby giving pause to potential assailants. In short, the laws increase the price of crime.

In order to deter crime, however, laws that make it easier for citizens to carry concealed firearms must also increase people's *perceptions* of the number of concealed carriers in a community. Without increasing the number of perceived carriers, easing concealed carry may only lead to other, perhaps unforeseen, externalities if the number of actual concealed carriers increases. Unfortunately, whether or not these laws actually influence public perceptions of the number of firearm carriers has never been tested. This article presents new data that allow me to test whether or not laws easing concealed carry increase public perceptions of the distribution of firearm carriers and, in so doing, test the microfoundations of the argument that easing concealed carry can deter crime; an approach I argue can be helpful in assessing the potential effectiveness of policy more generally. To preview, I find no evidence that easing concealed carry increases the perceived number of firearm carriers; thus, no evidence that easing concealed carry can deter crime.

Concealed Carry and Crime

Following the national increase in concern over crime in the United States in 1970s, 1980s, and 1990s, several states attempted to quell perceived upward trends in crime by reducing the institutional hurdles to carrying concealed firearms.[2] This easing generally took the shape of a move from "may-issue" permitting policies—policies that require the permit requestor to provide a valid reason for carrying a concealed weapon and grant the issuing authority substantial discretion—to "shall-issue" permitting—which removes discretion from the granting authority and places upon it a burden of denial. That is, the grantor must demonstrate that the requestor is unfit for some clear, empirically verifiable reason, such as being a convicted felon (Grossman and Lee, 2008). Table 1 shows the distribution of policies across all 50 states.

As noted above, such policies are promoted as having a deterrent effect on crime. The intuition being that increasing the number of law-abiding citizens who carry firearms should (a) give pause to potential criminals by increasing the probability that a victim will be able to defend himself or herself with a firearm, or (b) increasing the number of people who could potentially halt a crime being committed against another. Fundamental to these mechanisms is the increase in perceived risk on behalf of potential aggressors. If, and only if, potential criminals perceive an increased risk of failure or harm in perpetrating crime will we observe a decrease in crime as a function of easing concealed carry.[3] Without this increase in perceived risk, easing concealed carry, if successful in increasing the number

[1] An anonymous reviewer notes that these policies may be more a function of lobbying by pro-firearm organizations than a response to crime. Nonetheless, legislators often promote these policies as a response to violent crime or as being in the interest of public safety because of their potential deterrence effects.

[2] I write "perceived" because crime began to fall in earnest in early 1990s, but public concern remained high throughout the majority of the decade (Polsby, 1995).

[3] A previous reader of this article has noted the expanding concealed carry can deter crime by increasing the number of attempted crimes halted in progress by firearm carriers. This, however, is not *deterrence*, but interruption, and interruptions are also quite likely to end in injury.

TABLE 1

Concealed Carry Permitting Procedure

Shall-Issue		May-Issue	No Permit Required
Alabama	Nevada	California	Alaska
Arkansas	New Hampshire	Connecticut	Arizona
Colorado	New Mexico	Delaware	Vermont
Florida	North Carolina	Hawaii	Wyoming
Georgia	North Dakota	Maryland	
Idaho	Ohio	Massachusetts	
Illinois	Oklahoma	New Jersey	
Indiana	Oregon	New York	
Iowa	Pennsylvania	Rhode Island	
Kansas	South Carolina		
Kentucky	South Dakota		
Louisiana	Tennessee		
Maine	Texas		
Michigan	Utah		
Minnesota	Virginia		
Mississippi	Washington		
Missouri	West Virginia		
Montana	Wisconsin		
Nebraska			

firearm carriers, will only increase the probability that an altercation ends in firearm violence by increasing the number of firearms in play.[4]

Many scholars have sought to test whether or not such laws are effective deterrents by studying their outcomes. One of the earliest, and perhaps most notable, purports to find evidence that easing concealed carry does deter crime (Lott and Mustard, 1997), and subsequent studies have come to similar conclusions (Bartley and Cohen, 1998; Bronars and Lott Jr., 1998; Mustard, 2001; Olson and Maltz, 2001; Plassmann and Tideman, 2001). Yet, reexaminations of the Lott Jr. and Mustard data (Donohue and Ayres, 2003; Duggan, 2001), as well as alternative research designs (Duwe, Kovandzic, and Moody, 2002; Grambsch, 2008; Kovandzic and Marvell, 2003; Kovandzic, Marvell, and Vieraitis, 2005; Ludwig, 1998), find no empirical evidence that easing concealed carry deters crime and, indeed, this is the modal finding in the extant research. However, the research is still far from consensus. What explains this variation in empirical findings?

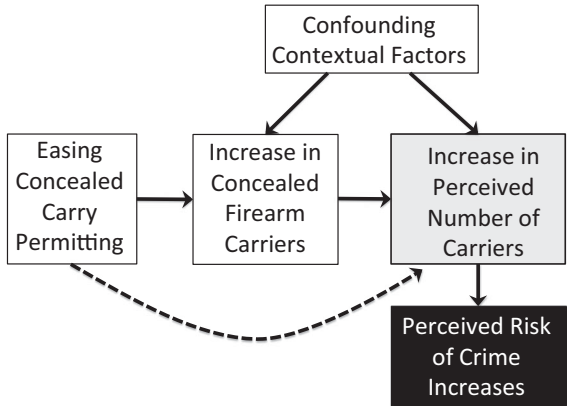
As many have noted, identifying the effects of a policy by studying outcomes is challenging for many reasons. For example, it may be difficult to come to consensus on what to measure and how,[5] and, even then, measurement may be troublesome, outcomes may covary with other factors such as time, or other policies with the same aim and treatment assignment (which may be nonrandom) may also have covariants, which confounds causal identification. These issues perturb several policy analyses over which we observe vigorous debate. For instance, does increasing the minimum wage increase unemployment (Card and Krueger, 2000; Neumark and Wascher, 2000); does increasing school choice improve learning and socioeconomic outcomes (Cullen, Jacob, and Levitt, 2005; Rouse, 1998)?

For many issues, particularly issues in which behavioral assumptions are foundational and information may be asymmetrically distributed or scarce, we may sidestep some of

[4]See Mialon and Wiseman (2005) for an interesting formalization.
[5]For example, which crimes should be affected, in which areas, over which time period?

FIGURE 1

Microfoundations of Deterrent Effects



these hurdles by considering the necessary conditions for policy success, or the policy's "microfoundations." Thus, instead of analyzing *outcomes*, this study considers the deterrence argument, identifies the necessary condition for the process, and evaluates this condition to assess whether easing concealed carry can *potentially* deter crime. If the answer is "yes," then the debate above should continue until resolved; if, however, the answer is "no," then we can infer that any detectable decrease in crime cannot be attributed to easing concealed carry and debate should transition to evaluating the costs and benefits of the policy's externalities such that an informed decision can be made on whether to keep it.

Though less common than studying outcomes, this approach to policy analysis has a fairly long history in the criminal law literature (see Paternoster 1987 for a review of early work, or Loughran et al. 2011 for a more recent example), and has been applied to studies, like this one, targeting particular policies such as drinking and driving laws (Bertelli and Richardson, 2008). Further, there is robust tradition of deterrence studies focusing on the perception of risk (of both injury and legal penalty) of drinking and driving (Gibbs, 1975; Homel, 1988; Vingilis, 1990). My results echo some previous findings, which inform this analysis, demonstrating a disconnect between perceptions of risk and the true distribution of risk (e.g., Houston and Richardson, 2004; Hjalmarsson, 2009). More to the point, I find no evidence that people's perceptions of the number of concealed carriers correlates to concealed carry policy, or the true number of concealed carriers.

The Deterrence Argument and the Perceived Distribution of Firearm Carriers

Consider the microfoundations of the deterrence argument with the help of Figure 1. The underlying logic of the deterrence argument is that easing CCP procedures increases the number of law-abiding citizens carrying firearms who may fight back if attacked or halt a crime being committed against another; this, in turn, increases the costs potential aggressors risk when choosing to commit a crime.

This process relies on the assumption that the increase in the number of firearm carriers is either *observed*—the horizontal, solid-arrowed process in Figure 1—or *inferred*—the curved, dashed-arrowed process. That is, it is possible that easing concealed carry actually

increases the number of firearm carriers in a community. Knowledge of the increase of firearm carriers could then filter into the mass public and increase the number of perceived firearm carriers in kind. Alternatively, a population may simply be aware of the policy itself and update its perceptions accordingly. That is, one may observe that one lives in a "shall-issue" state, note that it is easier to become a firearm carrier, and infer that there are more firearm carriers in one's community, whether or not there actually are.

In a sense, this is the ideal outcome. In this scenario, an increase in the perceived number of firearm carriers, by virtue of the policy alone, may have a deterrent effect on crime, without actually increasing the number of firearm carriers and the risk of injury, accidental or otherwise, that may accompany that increase. Either way, the increase in perceived firearm carriers, the *gray box*, is absolutely necessary to achieve the desired increase in the perceived risk of crime, the *black box*, which triggers deterrence. If easing concealed carry does not increase the number of perceived carriers, either directly, through inference, or indirectly, through observation, there can be no deterrence attributable to easing concealed carry. The task of this article, therefore, is to test the following hypothesis:

- Central hypothesis: *Easing concealed carry increases the distribution of perceived firearm carriers.*

Given this structure, there are two potential empirical relationships between the perceived number of firearm carriers and concealed carry policies that we may point to as evidence supporting the central hypothesis. There can be a direct effect; the number of perceived carriers will be significantly higher in "shall-issue" states and states where permits are not required than in "may-issue" states. Or there can be a mediated effect, where the institutional context influences the number of actual concealed carriers, which, in turn, influences the number of perceived carriers. Identifying the existence, or lack thereof, of these effects is the empirical challenge.

In addition to illustrating direct and mediated effects of concealed carry policies, Figure 1 also illustrates how other factors (in this case contextual, but they may also be individual) can influence population beliefs either directly or indirectly. For example, a possible confounding factor may be the depth of gun markets, or the level of firearm violence in a particular context. These factors may influence population beliefs either by increasing the number of firearm carriers, which, in turn, increase perceptions, or simply increasing perceptions directly. Such potential confounders present a challenge to the empirical analysis, as any factor omitted from the analysis may bias our estimates of the policy's effects. Therefore, the primary theoretical challenge is in identifying what these factors may be and the empirical challenge is to measure or proxy for them as best as possible.

Potential Confounders

To identify the salient potentially confounding factors, we need to consider what may influence people's beliefs over how easy it is to come by a firearm (or a permit) or how likely the typical person in their community is to want to carry a firearm, and therefore, by extension, how many firearm carriers there may be. In other words, we must account for supply and demand. Let us begin with factors determining how easy it is to come to own a firearm.

Supply

The only factor we must consider in determining how easy it is to come to possess a firearm is market depth. But gun markets take different shapes (Cook, Molliconi, and Cole, 1995; Cook et al., 2007). First, there are legal, or "white," markets, where sales are between a retailer and a consumer and the terms are guided by federal and state regulation. As such, we must only measure the hurdles to these sales, or, the degree to which such sales are regulated in a given state in order to account for the depth of white markets. The second firearm market is the illegal, or "black," market. Although such markets do tend to be correlated with white markets (Braga et al., 2002), they are not *strictly defined* by the white market; that is, the black market is not perfectly positively correlated with the white market, nor is it its probabilistic complement. As such, a measurement of the black market, beyond our white market measurement (which would certainly capture a portion of the black market), will be needed. Finally, there are "gray" markets—markets that are not necessarily illegal, but are nonetheless unregulated. These markets may exist by virtue of regulatory loophole—the gun show exemption is one example of this—or may simply exist outside of regulatory reach, such as the transfer of family heirlooms.

Demand

Demand for firearms may have several sources. There are cultural factors as well as ideological factors and safety concerns. For example, in a context where there is a tradition of firearm ownership and operation stemming from a hunting culture, it is likely that people would be inclined to believe there are more carriers because they are more likely to interact with firearm owner/operators, observe operation directly, or because they themselves are owner/operators.

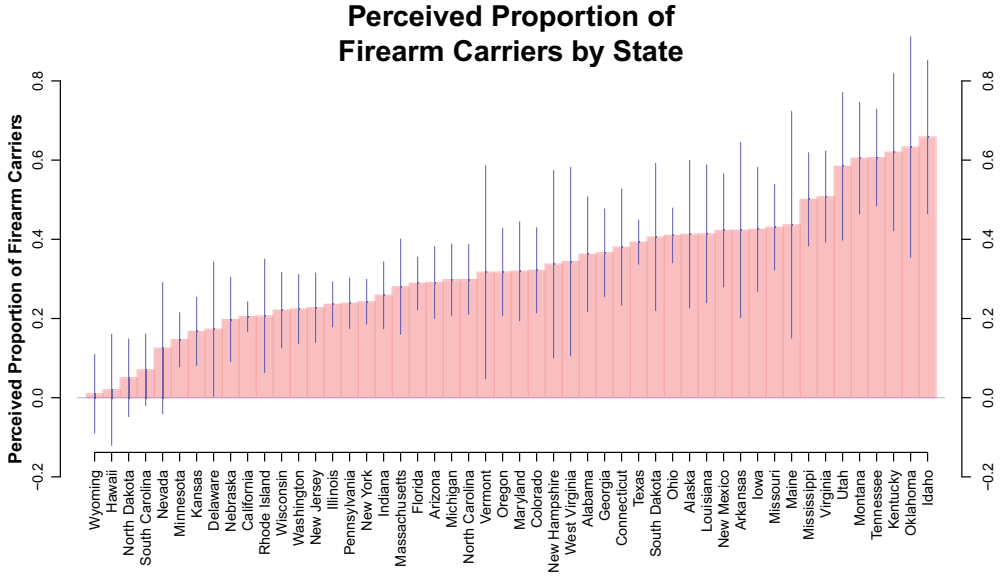
Alternatively, the ideological context may play a role. For instance, there is a well-established connection between a general conservative ideology and support for an absolutist stance on the Second Amendment (e.g., Lax and Phillips, 2012; Levendusky, 2009; Oxley et al., 2008).[6] These preferences may create an environment where people are more apt to believe that there are many firearm carriers, as they are, on average, more likely to come into contact with people who prefer lax firearm regulation or who wish to carry firearms themselves. This context is especially important given the desired relationship between public opinion and laws—that is, if our democratic institutions are functioning properly, policy should, on average, reflect the preferences of the relevant constituency; thus we would expect that public opinion or ideology would covary with the shape of firearm policies. In other words, cultural or ideological differences across states should be reliable predictors of treatment (a "shall-issue" or nonpermitting policy).

Finally, the perceived need for protection may play a role in creating demand for firearms or carrying firearms. If relaxing concealed carry is primarily intended as a self-defense measure, then we may expect the level of gun violence in a particular context to influence people's perceptions over how many firearm carriers there are in their community.

[6]But see Tyler and Lavrakas (1983) for an exception.

FIGURE 2

Descriptive Plot of the Dependent Variable Summarized by State



Data and Measurement

To reiterate, the question this article is attempting to answer is: "Does easing concealed carry change people's perceptions of the number of firearm carriers in their community?" Thus, the dependent variable in the analysis is simply the proportion of firearm carriers an individual believes there are in her community—because concealed carry policies are determined at the state level, we should evaluate people's perceptions at the state level. To measure these perceptions, I administered a survey to a probability sample of 1,000 Americans asking them: "In your opinion, how many people out of 1,000 carry a gun in your state?"[7] Respondents were allowed to give any answer between 0 and 1,000. The mean response was approximately 300 (or 30 percent) and the median response was 150 (15 percent). These responses are summarized by state in Figure 2, where the bars indicate the mean proportion of perceived carriers in a state and the lines indicate the positive–negative *SD* for the state mean.[8]

These responses will be regressed on the type of concealed carry policy each individual's state has—"may-issue," "shall-issue," or no permit required—and the number of active permits in their state,[9] while controlling for measures of supply and demand for firearms.

[7]The survey was administered using Google Consumer Surveys and the sample was an excellent match to the U.S. Census Current Population Survey with an RMSE deviation of only 2.4 percent. More substantively, this means that the average deviation of my sample from the true American population, in terms of age, gender, and location according to the demographics yielded by the CPS, is quite small. For more on Google Consumer Surveys, see McDonald, Mohebbi, and Slatkin (2012) and Santoso, Stein, and Stevenson (2015).

[8]A proper accounting of the data is provided in the online appendix.

[9]These data were collected by the U.S. Government Accountability Office (2012) and reflect the number of active permits as of December 31, 2011. There are five states for which these data are unavailable and thus were imputed using the method suggested by King et al. (2001). Omitting these observations, rather than imputing, does not alter the substantive results.

There are three measurements used to capture firearm *supply*, one for each market type. To measure the depth of the black market, I use the number of criminally exported guns per 1,000 people in the state. This metric is maintained by the FBI and ATF and is logged by tracing a firearm used in a crime to its state of origin. For example, if a firearm crime is committed in Chicago and the weapon is traced back to Indiana, it is said to have been "exported" from Indiana to Illinois. This measure tells us how difficult it is to come by a firearm illegally from state to state. This can be thought of simply as the movement of surplus goods. In states where black markets are deeper, black market guns are less profitable and more likely to be exported to states where black markets are thinner (e.g., from Indiana to Illinois) and therefore black market gun sales are more profitable. The most recent measure of criminal export was 2009.[10]

As every firearm can potentially enter the gray market by being transferred from one person to another, I use an exogenous measure of how many guns are owned in a state taken from the CDC's 2012 Behavioral Risk Factor Surveillance System survey—a survey of over 300,000 American homes across all states and three territories. This measurement is the estimated number of firearms owned per 1,000 residents in a given state.

Finally, to proxy for the depth of the white market, I create a measurement of regulation environment for firearm sales and ownership for each state. I create this measure by converting over 45 firearm regulations into a matrix of binary data, similar to a roll-call vote matrix, indicating whether or not each regulation is present in a given state.[11] This matrix is then scaled with a Bayesian parametric item response model that yields an estimate and confidence interval for each state (Clinton, Jackman, and Rivers, 2004). These estimates are simply measures of how strictly firearm sales, ownership, and operation are regulated in each state, with more positive values indicating strict regulation and more negative values indicating lax regulation. The variable is plotted in Figure 3. As the figure shows, the results generally conform to our naïve expectations of how strictly firearms are regulated from state to state, with California and Connecticut among the most strictly regulated and Alaska and Mississippi among the most lax. Note also that this measurement can be utilized as a "pretreatment" condition—a coarse estimate of how likely we are to observe the treatment (a "shall-issue" policy) in a given state.

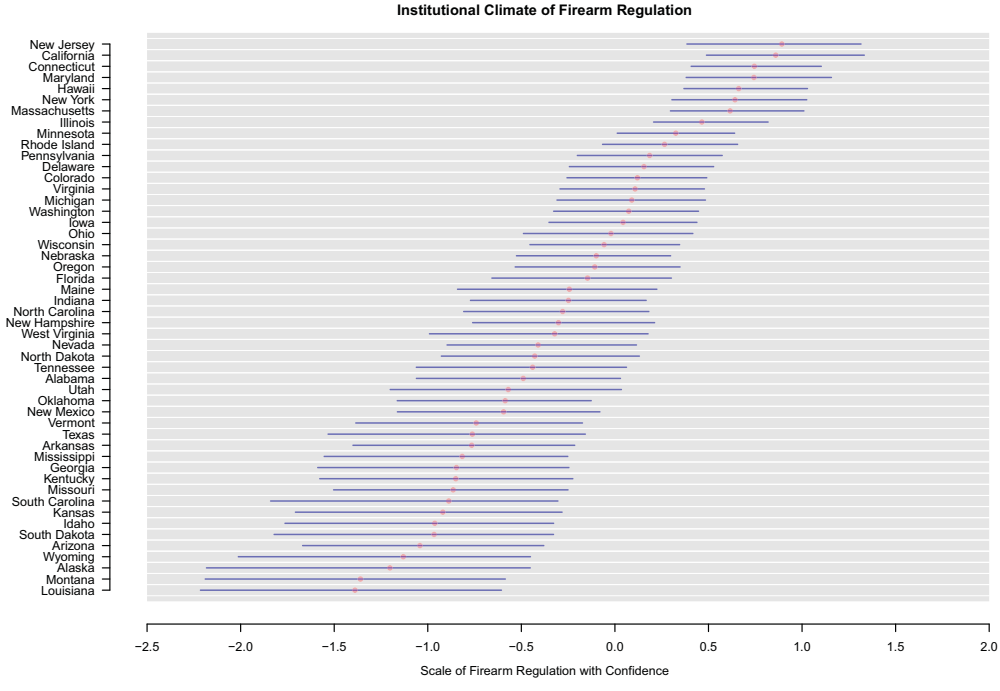
To measure the demand for firearm carrying, I include measurements of the state ideology and firearm violence. The ideology measure is simply the share of the two-party vote the state awarded to Mitt Romney in the 2012 presidential election—this rough measure of conservative ideology should help model out any influence on perceptions of concealed carriers that may be exerted by the state's political or ideological context. The measure of firearm violence is the number of firearm deaths per 1,000 people, as measured by the CDC. The most recent publication of this statistic was 2010. It is also important to note that the level of firearm ownership is a salient control here as well. As discussed above, a higher percentage of gun owners in a state may influence people's perceptions of the number of gun carriers in several different ways. Related to this variable, I also include an indicator for whether or not the state allows for legal "open carrying" of firearms. That is, are people allowed to carry a firearm where it is visible to others, similar to the manner in which a police officer carriers a firearm? It is perfectly reasonable to suspect that beliefs over the distribution of firearm carriers could be influenced dramatically by the observation of people carrying firearms in the open.

[10]Note that reversing this measure and using, instead, criminal *import* would give us an idea of black market demand. Our goal, however, is to measure *supply*.

[11]A listing of these regulations is given in the online appendix.

FIGURE 3

Ordering of State Firearm Regulation



Potential for Mediated Effects

Before evaluating the effect of CCP procedures on the perceived number of carriers, let us first assess whether or not "shall-issue" states actually issue more CCPs. This analysis will inform us as to whether or not we must consider the possibility of mediated, or indirect, effects (the horizontal, solid-arrowed process in Figure 1). The dependent variable in this analysis is the (logged) number of CCPs per 1,000 residents issued in each state and the covariate of interest is an indicator for whether or not a state has a "shall-issue" policy (nonpermitting states are omitted for the time being).[12] As desire for a CCP is likely driven by the same factors that influence perceptions of how many carriers there are in a state (the supply and demand factors just discussed), I include all of the same contextual factors that are included in the perceptions model below: open carry allowance, ownership, criminal export, firearm death rates, institutional climate, and political ideology. The results of the model, a linear regression estimated via OLS, are given in Table 2.[13]

The results in Table 2 show that permitting process has a robust effect on the number of permits issued; thus, if the number of active permits exerts a positive influence on the number of perceived carriers, additional analysis will be required to estimate the mediated effects of the policy on carrier perceptions. To preview, this is not the case.

[12] Because the natural log of 0 is undefined, and there are several 0 permit states, 1 is added to each observation before logging. This transformation does not change the substantive results, but increases estimation efficiency.

[13] An anonymous reviewer pointed out that states vary in their propensity to issue permits to nonresidents and this may potentially perturb the results shown here. Unfortunately, a detailed accounting of the mix between resident and nonresident permits for all states is not presently available.

TABLE 2

Normal Linear Model of Active CCPs per 1,000 Residents (Log Transformed)

Parameter	Estimate
Shall-issue	1.650
	(0.647)
Open carry	0.367
	(0.337)
ln(ownership)	−0.208
	(0.678)
ln(criminal export)	0.551
	(0.340)
ln(gun death)	−0.812
	(0.708)
Institutional climate	0.442
	(0.428)
Republican vote	5.281
	(2.604)
Intercept	−0.751
	(2.083)
R^2	0.519
N	46

Perceptions Model Specification and Estimation

As our dependent variable is a proportion, or a number of "successes" in a series of trials, how many people out of every 1,000 are perceived to carry a firearm by an individual survey respondent, the proper distribution to model the data-generating process is the binomial. To model concealed carry policy, indicator variables for "shall-issue" policies and nonpermitting policies are included, where "may-issue" serves as the baseline category. I also include the number of active CCPs—these are our independent variables of interest.

In addition to accounting for the potential contextual cofounders discussed above and included in the permit model, I also include the available individual-level characteristics: gender, age, income, and population density, which is classified as urban, suburban, or rural.[14] Finally, we must consider the structure of the data: because respondents are nested within states, which are groupings that may influence perceptions through some unmeasured variables, the potential for stochastic dependencies between rows of the data within states is certainly present and these dependencies may bias the parameter estimates. Thus, in order to get clean estimates of our independent variables of interest, it is necessary to model out noise from these unmeasured state-level factors. Therefore, I present a hierarchical, or "error-components," binomial logistic regression where random intercepts are allowed at the state level. This more appropriate model is presented next to a model that ignores the potential for state-level effects not captured by our specified variables to

[14]Note that the model's logistic systematic component functionally interacts all variables. That is, the marginal effect of any variable is a function of every other variable in the model. For instance, the derivative of $\frac{e^{\beta_1 x_1+\beta_2 x_2}}{1+e^{\beta_1 x_1+\beta_2 x_2}}$ with respect to $\beta_1 x_1$ is $\frac{\partial \frac{e^{\beta_1 x_1+\beta_2 x_2}}{1+e^{\beta_1 x_1+\beta_2 x_2}}}{\partial \beta_1 x_1} = \frac{e^{\beta_1 x_1+\beta_2 x_2}\times x_1}{1+e^{\beta_1 x_1+\beta_2 x_2}} - \frac{e^{2\times\beta_1 x_1+2\times\beta_2 x_2}\times x_1}{(1+e^{\beta_1 x_1+\beta_2 x_2})^2}$. Thus, any interactions between the contextual or individual variables that may influence our results should be captured without the explicit modeling of a product term that would degrade estimation efficiency. See Berry, DeMeritt, and Esarey (2010).

TABLE 3

Model Estimates

	Standard	Hierarchical
Permitting: Baseline category is *May Issue*		
Shall-issue	−0.284	0.219
	(0.017)	(0.400)
No permit required	−0.382	−0.987
	(0.010)	(0.618)
ln(active permits)[a]	0.135	−0.108
	(0.003)	(0.007)
Individual: Baseline urban density is *Rural*		
Male	−0.208	−0.208
	(0.004)	(0.004)
Age	0.031	0.039
	(0.001)	(0.002)
Income	−0.087	−0.128
	(0.003)	(0.003)
Suburban	−0.122	−0.162
	(0.006)	(0.006)
Urban	−0.143	−0.227
	(0.007)	(0.007)
Contextual		
Open carry	−0.075	0.106
	(0.005)	(0.255)
ln(ownership)[a]	0.011	0.212
	(0.002)	(0.100)
ln(criminal export)[a]	−0.056	0.183
	(0.006)	(0.267)
ln(gun death)[a]	0.272	0.036
	(0.014)	(0.597)
Institutional climate	−0.234	−0.230
	(0.008)	(0.318)
Republican vote	0.621	−1.153
	(0.054)	(1.649)
Intercept	−1.862	−2.294
	(0.033)	(0.936)
Random effects		
var(state random intercepts)		0.445
		(0.667)
Model fit		
ln(likelihood)	−137,079	−126,886
Parameters	15	16
AIC	274,188	253,804

[a]Measured as number per 1,000 residents.

(a) highlight the differences in the estimates and (b) assure the reader that the results are not model dependent. The results of the models are given in Table 3. Recall that our hypothesis expects that all independent variables of interest will exert positive effects on the dependent variable.

As the table shows, the effects of the concealed carry policies are not consistent with the hypothesis: "shall-issue" and nonpermitting contexts exert no positive effect on perceptions when the data are modeled correctly (hierarchical model) and *negative* effect when the data are mishandled. Further, the number of active permits is *negatively* correlated with the

perceived number of carriers in the proper specification—meaning that any mediated effects of the treatment would be contrary to expectations. Nonetheless, to be certain, I perform a mediation analysis of the total effects of permitting procedure: direct and indirect, via change in the number of permits (Imai, Keele, and Tingley, 2010; Imai et al., 2010). As in the permit model, this analysis excludes nonpermitting states. The average direct effect is effectively 0, quite insignificant statistically, and the average mediated effect is negative. Thus, the total effect of "shall-issue" permitting procedures on people's perceptions of the number of concealed carriers is weakly negative and statistically insignificant.

These results suggest that there is no statistically detectable relationship between a state's concealed carry permitting policies and people's perceptions of how many firearm carriers there are in their state. Thus, there is no evidence for the microfoundations of the concealed carry deterrence argument and the data suggest, therefore, that any change in crime rates cannot be attributed to a state's concealed carry policy.

Discussion

The link between the structure of concealed carry policy and people's beliefs over the number of firearm carriers in their community is unidentifiable in the data. The rationale for concealed carry deterrence, however, depends on such a link existing: it assumes that potential assailants are aware of the distribution of firearm carriers in the potential victim population, but the empirical evidence presented here suggests that that assumption simply does not hold. Because beliefs over the distribution of firearm carriers are impervious to permitting policies and do not respond positively to the true distribution of carriers, the data suggest that *easing concealed carry cannot deter crime*.

That is not, however, the end of the story. Above, I mentioned there are possible ramifications of easing concealed carry without influencing perceptions of the number of firearm carriers. In short, if we increase the number of people carrying firearms, but fail to increase people's beliefs over the distribution of firearm carriers, as the data suggest is the case, we get no criminal deterrence, but still bear the additional risk of increased firearm carrying. At best, we increase the probability of accidental discharge.[15] At worst, these policies open the door for more violent, potentially deadly, escalations of altercations—altercations that may have ended peacefully if not for the presence of a firearm.

It has been estimated that the social cost of gun ownership may be as high as $1,800 per year, *per household* (Cook and Ludwig, 2006). Allowing for the free travel of firearms, in a world where deterrence by concealed carry is infeasible, can only exacerbate these costs. Thus, it is perhaps time to pivot scholarly debate away from the ability of easing concealed carry to deter toward assessing the potential cost of externalities imposed by the policies and determining whether or not it is in the best interest of the state to maintain them. Research by Ludwig (1998) suggests that concealed carry may actually exacerbate murder rates—a negative externality that is mirrored by other firearm policies, such as the liberalization of sales policies (Knight, 2013) and the lifting of assault weapons bans (Dube, Dube, and García-Ponce, 2013).

Finally, this analysis reinforces the idea that, rather than focusing purely on outcomes, the potential efficacy of laws may be evaluated by examining their necessary conditions. By considering the plausibility of the behavioral assumptions that laws are built upon, or

[15] Such accidents are unfortunately more common than we may expect and have even happened in places as unlikely as the Kentucky House of Representatives (Loftus, 2014).

examining the microfoundations of policy, we can assess whether or not it is even possible for a policy, in its current form, to succeed. At the least, such analyses may alert us to informational deficiencies or asymmetries that may inhibit the efficiency of policy.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Table S1: Regulations per State
Table S2: Law Key
Table S3: Number of Respondents per State
Table S4: Descriptive Statistics for Included Covariates
Table S5: Correlation Matrix for Included Variables
Table S6: Model Estimates