The Decision to Carry: The Effect of Crime on Concealed-Carry Applications

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Abstract

Despite persistent debate on the role of concealed-carry legislation, decisions to legally carry concealed handguns are not well understood. Using detailed data on concealed-carry permit applications, we explore whether individuals apply for concealed-carry permits in response to crime. We find that recent homicides increase applications in areas relatively near to the incident. The effects are driven by gun-related homicides, and are more pronounced for white, male, and Republican applicants. We also find suggestive evidence that applicants are more responsive when they share a demographic characteristic with the homicide victim. The results further indicate that applications after recent homicides are more likely to be renewed, consistent with persistent precautionary behaviors. Our findings provide causal evidence that crime risk influences individual decisions regarding legal gun use.

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I. Introduction

The presence of concealed handguns in public spaces is a divisive issue central to ongoing gun-control debates. Every state in the U.S. has legislated a permit application process whereby citizens can legally carry a concealed firearm in public and estimates indicate that the number of concealed-carry permit holders has increased from 2.7 million in 1999 to 12.8 million in 2015 (Lott, Whitley and Riley, 2015). More recently, states have expanded concealed-carry policies by relaxing restrictions on permit holders or removing restrictions on "gun free" zones. For instance, since 2013 at least 36 states have introduced highly contested legislation to allow some form of concealed carrying on college campuses.[1]

The prevalence of concealed-carry legislation and limited data on gun ownership have resulted in an intense scrutiny of concealed-carry laws and a large body of research showing mixed results of the reduced-form effect of these laws on crime.[2] While the implications of legal concealed carrying have generated considerable interest from researchers and policy-makers alike, it is surprising that the determinants of the decision to legally carry a concealed firearm largely remain in the periphery of rigorous quantitative analysis. In this paper, we deviate from the large literature analyzing the reduced-form effect of concealed-carry laws on crime by instead considering whether individuals respond to crime by applying for permits to legally carry a concealed firearm.

To do so, we use unique concealed-carry application data from North Carolina spanning 1998 to 2012 to analyze the effect of crime on the number of applications for concealed-carry permits. We initially focus on homicides using North Carolina vitality data, but also analyze crime more generally using the FBI's Uniform Crime Reports. Our empirical strategy exploits monthly variation in the timing of recent crime incidents, most notably homicides. Intuitively, our approach compares the number of applications in months with recent homicide incidents to months without recent homicide incidents within the same year for a given city after controlling for differences that are expected across different months of the year.

We find that recent homicides increase concealed-carry applications for residents near the homicide incident. Specifically, our estimates suggest that a homicide incident increases the number of citywide applications by approximately 13 percent over the following two months in relatively small cities and by 8 percent over the following two months in larger cities when using disaggregate data that measures recent homicides and applications at the census tract level. For comparison, Depetris-Chauvin (2015) finds that Barrack Obama's 2008 election victory led to a 38 percent increase in firearm background checks, which proxy for the demand for guns. We note, however, that homicides are infrequent and that our estimates indicate an effect only in areas close to recent homicides, which together suggest that responses to crime do not explain recent dramatic increases in concealed-carry permits in the U.S.

We further show that our results are robust to various model specifications and find similar results using alternative data sources to measure crime. Crucial to the validity of our research design, we demonstrate that the effects are present following and not prior to homicide incidents, thus reinforcing a causal interpretation of the estimates. Our estimated effects are driven by gun-related homicides and the effect is not apparent for less-serious crimes, suggesting that individual application decisions are more responsive to crimes that likely represent a more serious perceived threat.

The detail of our data also allow us to explore heterogeneous effects by applicant characteristics and identify specific circumstances that lead to precautionary gun-related behaviors. Our finding that the severity of the crime incident and the proximity to the incident are systematically salient to applicant behaviors is consistent with recent research suggesting that individual perceptions of crime risk depend on extreme experiences with crime in the local neighborhood rather than reported aggregate crime rates (Salm and Vollaard, 2016). We also find evidence that males, whites, and Republican applicants are more responsive to recent homicides. Furthermore, we find suggestive evidence that the demographic salience of the homicide victim affects the responsiveness of certain applicants. For instance, we see a pronounced effect of female applicants responding to female-victim homicides. Finally, we analyze permit renewals and find that concealed-carry permits issued after recent homicide incidents are more likely to be renewed, suggesting that homicide incidents lead to persistent updated beliefs.

Our study provides the first causal evidence linking homicide incidents—plausibly related to perceptions of crime risk—to legal gun carrying. As such, our findings contribute to a better understanding of when and why individuals choose to legally carry guns in public. As gun carrying has important public safety implications, our results are relevant for current and future research seeking a more comprehensive understanding of the effect of guns in society. Our analysis also adds to the literature seeking to understand the demand for guns as concealed-carry permit applications act as a proxy for legal handgun ownership. Given the difficulty of measuring gun ownership and the lack of exogenous variation, past research has primarily relied on the General Social Survey to document important correlates of gun ownership (Glaeser and Glendon, 1998; Kleck and Kovandzic, 2009). Though concealed carry permit applications are an imprecise proxy for gun ownership, our paper is the first to directly consider the causal effect of recent crime on gun-related behaviors.

While we have thus far emphasized how our study provides insight into gun-related behaviors in a highly relevant policy setting, our study also contributes to a large literature analyzing the evolution of beliefs in response to uncertainty or a change in environment. Studies analyzing experience-based learning models have provided consistent evidence that changes in environment can shape decisions associated with risk and that these decisions often have important implications. For instance, recent studies have focused on insurance take-up following natural disasters (e.g. Browne and Hoyt, 2000; Gallagher, 2014). Others have considered the willingness to bear financial risk based on individual experiences with macroeconomic outcomes (Malmendier and Nagel, 2011), housing decisions for those in cancer clusters (Davis, 2004), and—particularly relevant for our context—changes in precautionary behaviors following perceived changes in crime risk (Salm and Vollaard, 2016). Our analysis contributes to this literature by analyzing decisions to apply for concealed-carry permits in a fully natural setting with significant uncertainty regarding the actual crime risk as well as the effectiveness of guns as precautionary devices.[3]

Related settings where economists have identified precautionary responses to perceived changes in crime risk include homeowner purchases of bars on windows, locks, and alarms following increases in burglaries and robberies, (Clotfelter, 1978; Philipson and Posner, 1996) and families moving out of neighborhoods where crime is increasing or sex offenders are identified (Cullen and Levitt, 1999; Pope, 2008).[4] Relative to bars on windows, locks, alarms and out-migration, precautionary responses that lead to increases in gun carrying have serious potential externalities. Moreover, it is unclear how legal gun carrying interacts with public policing efforts intending to reduce crime.[5] Notably, survey evidence does support the notion that gun owners respond to the fear of crime, however the lack of causal estimates stresses the need to understand the link between crime and the updating of beliefs leading to gun-related precautionary behaviors.[6]

II. Background

Modern concealed-carry laws—establishing a permit application process—were largely implemented in the early 1990s. For instance, only ten states had concealed-carry laws in 1988, but by 1996 this number had increased to 30. To date, all 50 states have a concealed-carry application process, though eligibility requirements differ significantly across states.[7] These laws can be broadly categorized as shall-issue, may-issue, or unrestricted carry. The majority of laws are shall-issue laws that issue concealed-carry permits to qualified applicants without stated justification for a permit. That is, as long as an individual has met the age, training, and background requirements the state shall issue a permit. In addition to considering whether the applicant meets the eligibility requirements, may-issue laws require a determination of whether justification is warranted based on the stated reasons for the permit.[8] More recently, several states have enacted unrestricted-carry laws that do not require a license or permit to carry a concealed weapon. As of 2015, 35 states have shall-issue laws, 9 have may-issue laws, and 6 have unrestricted-carry laws.[9]

A large literature explores the reduced-form effects of concealed-carry laws on crime. Lott and Mustard (1997) were the first to show a deterrent effect of concealed-carry laws on crime, which initiated a flood of research and contentious debate on the effects of concealed-carry laws. Among those critical of Lott and Mustard (1997) include Black and Nagin (1998), Ludwig (1998), Dezhbakhsh and Rubin (1998), Duggan (2001), Ayres and Donohue (2003), Rubin and Dezhbakhsh (2003), and Donohue, Aneja and Weber (2017) who find that shall-issue laws have either no significant effect on crime or slight increases in certain types of crime.[10] Others have found supporting evidence for a deterrent effect of concealed carrying on crime including Lott (1998), Bronars and Lott (1998), Moody (2001), Plassmann and Tideman (2001), Olson and Maltz (2001), and Mustard (2001). We do not take a position on the consequences of these laws; rather, our focus on the determinants of concealed carrying is motivated by the many potential positive and negative externalities associated with the decision to legally carry a gun in public. Moreover, the mixed findings on this topic stress the importance of understanding behavioral mechanisms contributing to reduced-form estimates of concealed-carry laws on crime and, more generally, any estimates of the effects of gun-related policies on societal outcomes. Though the underlying reasons for concealed carrying are typically overlooked, several studies have documented correlates of concealed-carry permits. Due to the poor quality and availability of concealed-carry data, these studies typically rely on cross-sectional comparisons of aggregate data.[11] In such cases, the estimates cannot be interpreted as causal and inference regarding individual behaviors related to gun activity is severely limited. To our knowledge, this paper provides the first analysis exploring the causal effect of a potential determinant of gun carrying—recent crime incidents—on concealed-carry applications.

A. North Carolina Shall-Issue Law

North Carolina implemented a shall-issue law in July of 1995, joining the nationwide movement allowing qualified individuals to carry a concealed handgun in public. Prior to the law change, North Carolina statutes prohibited concealed carrying of deadly weapons outside of one's own premises. The 1995 law mandates a permit obtained through a statewide application program for any individual carrying a concealed handgun. Each applicant must be a U.S. citizen, a resident of the state for 30 days or longer, at least 21 years of age, must not suffer from a "physical or mental infirmity that prevents the safe handling of a handgun," and complete an approved course in firearm safety and training. Individuals seeking a permit must apply to the county sheriff's office and pay a non-refundable permit fee.[12] A permit can be denied if the individual is under indictment, has a felony record, is a fugitive from justice or is ineligible to own, possess, or receive a firearm under state or federal law. The permit is valid for five years and, unless revoked, can be renewed for consecutive five-year periods.

As highlighted by Thompson and Stidham (2010), North Carolina offers a unique setting to study behaviors leading to concealed-carry permit applications. In particular, North Carolina offers substantial variation in demographic characteristics, degrees of urbanization, income levels, educational attainment, and political ideology. The state ranks 9th in population with nearly 10 million residents and is racially diverse, with 35 percent of the population consisting of minorities and 22 percent black.[13] Historically, the state has been politically balanced and is typically labeled a swing state in presidential elections.[14] Furthermore, North Carolina's 1995 adoption of its shall-issue law provides substantial variation over time to study concealed-carry take-up.

III. Data

We use individual concealed-carry application information from a statewide database managed by the North Carolina State Bureau of Investigations.[15] The database is updated as sheriffs receive and record individual applications. Our data span 1996 to 2012, throughout which we observe over 378,000 new concealed-carry applications. The data identify each applicant's city of residence, gender, age, race, date of application and date the permit is issued.[16] The data also include information on permit expirations, renewals, and whether the permit application is approved or denied.

We restrict our sample to first-time permit applicants in order to exclude individuals who renew a prior permit or submit a new application because of an expired permit. To avoid potential confounding effects due to the initial passage of the law, we also restrict the data to applications submitted after 1997.[17] Figure 1 shows the number of new monthly permit applications in North Carolina from January 1998 through December 2012. The number of monthly applications remained relatively flat through the early 2000s prior to rapidly increasing in the second half of the decade. The dramatic increase in permit applications, as seen in Figure 1, is consistent with national permit trends documented by Lott, Whitley and Riley (2015).

We initially focus on changes in concealed-carry applications following homicide incidents, though we also consider less serious crimes and alternative external causes of death. We measure homicides using multiple independent data sources. Our primary source is the North Carolina State Center for Health Statistics (NCSCHS) Vital Records that include all recorded deaths in North Carolina.[18] In these data we observe the cause of death, the city of occurrence, the date of occurrence, and the deceased individual's gender, age, race and marital status.[19] We use census-incorporated place identifiers in the NCSCHS to merge cities with those identified in our concealed-carry sample. As such, our analysis includes incorporated areas in North Carolina from January 1998 through December 2012.[20]

Our secondary source of data is the Uniform Crime Reports (UCR) collected by the Federal Bureau of Investigation (FBI). UCR data include monthly crime statistics reported by local law-enforcement agencies to the FBI. The details available in the UCR data also allow us to consider the effects of crimes, other than homicides, on concealed-carry applications. The analysis using

UCR data focuses on municipal law enforcement agencies across North Carolina that are actively reporting crime data over our sample time frame.[21]

Although we use both the NCSCHS and UCR data in our city-level analysis, we focus primarily on the results obtained using the NCSCHS data due to several shortcomings of the UCR data. For instance, while the NCSCHS data are administrative records that include all deaths in North Carolina, the UCR is a voluntary program known to suffer from misreporting and inconsistent reporting.[22] Furthermore, the NCSCHS data include actual homicides rather than just homicide arrests, as observed in the UCR.[23] Finally, the UCR data is more difficult to match to our city-level application data as it is measured at the law enforcement agency level and municipal agency jurisdictions are not necessarily defined by city boundaries. The NCSCHS data, on the other hand, allow for a direct city-level match with our application data.

As the NCSCHS data are at the city-by-month level, we aggregate our application data similarly to obtain a city-by-month panel of concealed-carry permits and mortality outcomes. Our sample is a balanced panel of 30,180 city-by-month observations from 171 cities.[24] The first column in Panel A in Table 1 shows the average number of concealed-carry applications in our sample of cities for each demographic group explored in the analysis. In Columns 2 and 3, we show means by cities above and below the median population as we anticipate differential responses to crime across small and large cities. In particular, homicides in relatively small cities are more likely to affect average perceptions regarding crime risk. Indeed, because homicides are far less frequent and more "local" in terms of proximity, small cities provide a more natural setting to test for behavioral responses to crime that lead to concealed carrying.[25]

Based on the 2010 population of each city, there are 86 cities at or below the median population of approximately 8,500. Although there are roughly 10 times as many people in relatively large cities, the mean number of applications is only four times larger, which is illustrated by an application rate nearly twice as large in relatively small cities. Across both small and large cities, Table 1 reveals consistently higher average applications for males and whites.

In Panel B of Table 1, we show summary statistics for the NCSCHS homicide measures used in our analysis. Though we primarily focus on indicators for whether there was a homicide in a prior month, we also show results using each homicide measure shown in Panel B. Column 1 indicates that 11 percent of cities experience a homicide incident in the average month and that there are 0.181 homicides per city-month. While homicide incidents occur more frequently in relatively large cities, homicide rates are similar across cities above and below the median population. In small cities, 97 percent of monthly homicides are single homicide incidents, while the same is true for 65 percent of monthly homicides in relatively large cities. Notably, in both small and large cities approximately two-thirds of homicides are committed with a gun.

IV. Empirical Strategy

As discussed previously, we initially focus on the response of new concealed-carry applications to homicide incidents and later extend the analysis to other crimes. Given our focus on the number of applications and because we often have cells with zero applications, our estimates are based on Poisson models, which have several advantages over alternative count models such as a negative binomial. For instance, Poisson models avoid incidental parameters problems when including fixed effects and do not require the arrival process for the number of applications to follow a Poisson distribution. Rather, the consistency of the time-varying covariates simply depends on correct specification of the conditional mean of the outcome (Cameron and Trivedi, 1986). Furthermore, we relax the assumption of equality between the conditional mean and variance by calculating robust standard errors (Wooldridge, 1997; Cameron and Trivedi, 2013).[26]

Our empirical approach exploits variation in homicide incidents within cities over time to identify the effect of crime on new concealed-carry permit applications. In our baseline model we assume that the number of applications, App, in city i at time t, where t is a given year y and month m ($t \in \{y \times m\}$), is characterized by

$$App_{i,t} = \exp\left(\sum_{j=1}^{n} \beta_j homicide_{i,t-j} + \gamma_{i,y} + \theta_m\right), \qquad (1)$$

where $homicide_{i,t-j}$ is a measure of lagged homicides, $\gamma_{i,y}$ are city-by-year fixed effects, and θ_m are month fixed effects. We measure recent homicides using homicide rates, levels or indicator variables. We calculate standard errors corrected for potential clustering at the city level to address the possibility that monthly observations within cities are correlated.

The inclusion of city-by-year fixed effects ensures that the estimation controls for city-year specific shocks affecting concealed-carry permit applications such as annual changes in crime levels, population, demographic composition, policing, and other relevant city, county, or state shocks and policy changes. This is important as time-invariant city characteristics are likely related to crime rates and the number of concealed-carry permits. Our baseline model also controls for month fixed effects, which account for aggregate annual shocks and seasonality in the demand for concealed-carry permits. This also is important as Figure 1 shows spikes each year during the months of January through March. Finally, in our sensitivity analysis we show that the estimates are robust to models that also include county-specific linear trends and year-by-month fixed effects.

Our use of lagged homicides in Equation 1 implicitly assumes that recent homicides affect current application decisions and allows us to test the persistence of the effect. We also explore models including leads to address concerns regarding reverse causality. The results of this analysis, discussed in more detail below, reveal that monthly changes in homicides are not driven by recent changes in concealed-carry applications.

Intuitively, our preferred specification compares the number of applications within city-years following homicide incidents in previous months, while controlling for the differences that are expected across months of the year. Under the assumption that other determinants of concealed-carry permits are unrelated to the timing of local homicide incidents across months within city-years and after adjusting for seasonality, the estimate of β identifies the causal effect of a recent homicide incident on the number of new concealed-carry applications. Though we start by showing estimates for all cities in our sample, our estimates by city size lead us to focus exclusively on concealed-carry applications within relatively small geographic areas over time. In a subsequent section, we further explore the influence of geographical proximity to crime on concealed-carry applications using alternative disaggregated crime data in relatively large cities in North Carolina.

V. Results

A. Main Results

Panel A of Table 2 shows the estimated effects of lagged homicide measures on concealed-carry applications for all 171 cities in our sample. Panels B and C show the results separately for cities below and above the median population. Each specification includes month fixed effects and city-by-year fixed effects. Column 1 reports the effect using homicide rates (monthly homicides per

10,000 individuals), Column 2 reports the results using homicide levels, and Columns 3 and 4 use indicator variables for homicide incidents in prior months.

The results using the full sample of cities (Panel A) suggest that homicides have no significant effect on concealed-carry permit applications. This is not surprising given that many of these cities are large urban areas where homicides are relatively frequent and are less “local” in the sense that neighborhoods directly affected by the incident are likely only a small fraction of the city-wide population. Indeed, stratifying the estimates by median population reveals that the Panel A estimates mask important differences across city size. In particular, the results in Panel B suggest that a recent homicide incident has a significant effect on concealed-carry permit applications in cities below the median population. This is true whether we use homicide rates (Column 1), levels (Column 2) or indicator variables (Columns 3 and 4). Though the point estimates are noticeably smaller when using rates (Column 1), the actual effect sizes are only slightly smaller as an additional homicide in levels (i.e. in cities with an average population of 4,570) represents approximately 2.2 additional homicides per 10,000 residents. Focusing on Column 4, the point estimate suggests that a homicide incident increases applications by approximately 13 percent over the next two months ($(e^{0.124}-1)\times 100\%$).[27] On the other hand, the estimates in Panel C indicate no clear effects of homicides on permit applications in larger cities.[28] While the results in Table 2 provide evidence that applications respond in areas relatively near the homicide incident, we note that there may also be other differences between large and small cities with regards to concealed carrying. To provide some context for these estimates, the average city with below median population receives three applications per month; a homicide in these cities will increase applications by 13 percent over the next two months, or by roughly two-thirds of an application.[29] Though the estimates demonstrate a large response in percentage terms, it is worth noting that homicides are extremely rare events that explain only a small portion of the variation in the number of applications.[30] As reported in Table 1, the probability of a homicide in any given month in a city below the median is 0.03.

B. Sensitivity Checks

Focusing on the sample of cities below the median population, we next consider whether the estimates in Table 2 are sensitive to alternative specifications. Table 3 shows results that explore the sensitivity of our estimates to various specifications including models that alternatively control for year-month-specific shocks and county-specific linear time trends. For comparison, Column 1 first reports the estimates from the specification used in Column 4 of Table 2, Panel B, which includes month and city-by-year fixed effects. Column 2 additionally includes a county-specific linear time trend. In Column 3 we include year-by-month fixed effects to the model, which will account for state-wide shocks in any calendar month. Finally, in Column 4 we include both year-by-month fixed effects and a county-specific linear time trend. Notably, the estimates across the specifications in Table 3 are largely similar in magnitude and precision, which supports the validity of our estimates presented in Column 1. As such, our subsequent analyses continue to focus on the specification reported in Column 1, which includes month fixed effects and city-by-year fixed effects.[31]

C. Additional Estimates by City Size

To further investigate the role of city size, we explore how the estimates change when we focus on alternative stratifications of smaller and larger populated cities. Specifically, we use a moving sample size of 40 cities, starting with the 40 least populated cities and incrementally move to a sample of the 40 most populated cities, plotting each coefficient estimate. We continue to employ a similar specification as in Column 4 of Table 2. This process results in 132 estimates, which we plot in Figure 2. The point estimate for the 40 smallest cities is shown on the furthest left point of the graph (approximately 0.11). As seen in the figure, estimates in cities below the median are consistently positive, but incorporating variation from larger cities leads to point estimates close to zero and not statistically different from zero, reinforcing the finding that the effect is more salient in smaller, more localized settings. Given these results, our next set of tables focuses on cities below the median population, though in subsequent analysis we also consider the effects in several large cities in North Carolina using alternative disaggregated crime data.[32]

D. Treatment-Effect Dynamics and Event Study Analysis

In this section, we explore estimates from an event study model to consider the dynamic effects of homicides on concealed-carry applications. In addition to providing insight into the persistence of the effect, this analysis serves to address concerns that changes in the number of homicides may be driven by recent changes in concealed carrying and/or related activities. That is, this approach allows us to address potential concerns over the causal direction of the estimates and to capture the temporal relationship between permit applications and homicides.

Our approach closely follows Gallagher (2014). In particular, we include indicator variables for all periods leading up to and following homicide incidents using the following estimating equation,

$$App_{i,t} = \exp\left(\sum_{k=1}^{6} \beta_k H_{i,k} + \gamma_{i,y} + \theta_m\right). \qquad (2)$$

The indicator variables, $H_{i,k}$, take a value of one if city i had a homicide incident in the two-month time period k, and are zero otherwise.[33] In the analysis, we consider applications one year before and after the month of the homicide incident. Since many cities observe multiple homicide incidents in the data, each homicide is coded with its own set of indicator variables. Following Gallagher (2014), we bin $H_{i,k}$ by creating a single indicator variable for the end periods.[34] These end period bins pool the effect over multiple time periods, but are of little interest as we are concerned with applications in months surrounding the homicide incident. Finally, we omit the month of the homicide incident. The estimated coefficients can be interpreted as the approximate percent change in applications relative to the month of the homicide incident. Similar to our main specification, the model also includes month fixed-effects and city-by-year fixed effects.[35]

Figure 3 plots the two-month coefficient estimates and the 95 percent confidence interval of the event time indicators from Equation 2.[36] Month zero represents the month of the homicide incident. The estimate at month two corresponds to the change in applications in the first and second month after the homicide incident; the estimate at month four corresponds to the change in applications in the third and fourth months after the homicide incident; and so on. The results in Figure 3 show no significant effects on applications in the year leading up to a homicide incident, which reinforces the causal direction of the estimation. In the two months following a homicide incident, we observe a coefficient estimate of approximately 0.12. This effect does not persist in the following months, as the estimates in months 3 through 12 are close to zero and not significant. We find similar results in Appendix Table A2 where we systematically add leads and lags to our main specification outlined in Equation 1.

These results confirm that a homicide in the past two months significantly increases concealed-carry applications, but provide no evidence that current permit applications are related to homicides in future months. We view this as strong evidence supporting our identification strategy and reinforcing a causal interpretation of the results.[37]

E. Gun-Related Homicides and Other Causes of Death

Thus far, we have shown evidence that the number of concealed-carry applications in relatively small cities respond to homicide incidents, consistent with the notion that more proximal perceived threats affect individual gun-related decisions. If individuals are also sensitive to the severity of perceived threats, we may expect a more pronounced response following homicides committed with a gun and we would not expect a response to alternative external causes of death where the perceived threat is likely minimal or nonexistent. In Table 4 we show estimated effects separately for gun related homicides and consider other external causes of death available in the NCSCHS data. We are particularly interested in the degree to which homicides with a gun differentially affect the decision to apply for a concealed handgun permit, as shown in Column 2. The estimate reveals that the effect on all homicides is largely driven by gun-related homicides. In columns 4 through 6 of Table 4 we assess whether other external causes of death that are less likely to influence perceived security affect permit applications. We focus on the three most commonly observed external causes of death: motor vehicle accidents, suicides, and drug overdoses.[38] The bottom row in the table shows the mean monthly mortality rate for the cities below the median population in our data. The estimated effects of external causes are small relative to the estimated effect of a recent homicide and are not statistically significant, suggesting that these other common causes of death do not increase permit applications.

F. Heterogeneity by Applicant Characteristics

We next turn to identifying the types of applicants that are responsive to recent homicide incidents. We first explore heterogeneity using voter history data, and then consider differences across demographic groups including age, gender, and race. We also consider whether applicants are more responsive when they share a similar characteristic with the victim of the homicide.

1. Voter History

One reason to consider effects by voting patterns is because political affiliation serves as a proxy for a prevailing gun culture, with Republicans having higher gun ownership rates and less support for gun control measures (Costanza, Kilburn and Miles, 2013; Hepburn et al., 2007). In a recent study, Depetris-Chauvin (2015) finds that the fear of additional gun regulations surrounding the 2008 presidential election led to a dramatic increase in the demand for guns that was more pronounced in states with a higher Republican presence. Our analysis shifts the focus from the fear of potential gun regulations to the fear of crime victimization by testing whether homicide incidents differentially affect precautionary gun behaviors among individuals likely to be more supportive of gun use. This exercise is also interesting as the estimates potentially shed light on an underlying behavioral factor that, at least to some degree, contributes to the stark political disparity on opinions regarding gun control in the United States.

For this analysis, we obtained records from the North Carolina State Board of Elections that include voter registration and participation in party primaries for all voters participating in municipal, state, and national elections spanning 2004-2014.[39] For our proxy of voter affiliation, we label an individual a Republican if they have registered as Republican or voted in the Republican primary in at least 70 percent of all elections in which they have participated. We calculate a similar measure for Democrats and label the remaining individuals as otherwise affiliated. We were able to uniquely match voter history records to 34,958 of the 44,588 individuals that submitted concealed-carry applications in cities below the median population. Of the linked individuals, 16,086 are labeled as Republican, 9,749 as Democrat, and 9,120 as other. We also classify the individuals in the sample as either voters or non-voters.[40]

The results of this analysis are reported in Table 5 Panel A. Columns 1 through 3 show the estimates for our main sample, individuals without a voter history, and voters. While the response is more precisely measured among voters, the differences between voters and nonvoters is not significant. Columns 4 through 6 show the estimates by Republican, Democrat, and other. These estimates suggest that Republicans are more likely to apply for concealed-carry permits following a homicide incident, though the estimated effects for Republican and Democrat applicants are not significantly different.

In light of Depetris-Chauvin (2015), who documents dramatic changes in firearm background checks surrounding the 2008 presidential election, we also show estimates that consider if there are differential effects to a recent homicide incident before and after 2008. Panel B of Table 5 shows these results by including the interaction of a homicide in the previous two months with an indicator for post-election in November 2008. That the interaction is not significant suggests that the behavioral response is similar for the timespan before and after the election.

2. Demographics

We next estimate models including demographic-specific application counts in order to explore whether applications from particular individuals are more responsive to recent crime. As reported in Table 1, whites and males have much higher baseline application rates. Table 6 presents the estimated effects of a homicide in the previous two months on the number of applications across demographic groups. For comparison, Column 1 presents the estimated effect for all individuals, and estimates by race, gender and age categories are shown in columns 2 through 8. The results suggest that whites, males, and individuals ages 40-59 are most responsive, with the point estimates suggesting that a homicide incident in the previous two months increases applications by roughly 15 percent. The estimates for ages 21-39 and ages 60+ are positive but less precise, while the estimated effects on female and black applications are not significantly different than zero.

Our previous results demonstrate that gun-related homicides and homicides in relatively small cities are more salient to permit application decisions. However, spatial distance is just one dimension that may influence an individual's decision to apply for a permit. It may also be the case that sharing common characteristics with the homicide victim influences the perceived likelihood of victimization and subsequent decisions toward self-protection. Existing research provides such evidence suggesting that individual behaviors change as beliefs are updated based on experiences of those in comparable situations. For example, Lochner (2007) finds that perceived probabilities of arrest are related to a sibling's criminal history and avoidance of arrest. In the context of health behaviors, Lin and Sloan (2015) find that smokers are more likely to quit smoking when a nearby resident is diagnosed with lung cancer. Along these lines, we next test whether the salience of the victim influences potential applicants. In other words, are applications more responsive when the applicant shares a common characteristic with the victim?

To analyze the extent to which victim salience contributes to changes in applications we estimate a model similar to Equation 1 that focuses on incidents where applicants and homicide victims within the same city share a demographic characteristic. For instance, for females we estimate the following Poisson regression model,

$$FemApp_{i,t} = \exp\left(\beta_1 FemVic_{i,t} + \beta_2 MaleVic_{i,t} + \gamma_{i,y} + \theta_m\right), \qquad (3)$$

where $FemApp_{i,t}$ is the number of female permit applications in city i at time t, where t is a given year y and month m. $FemVic_{i,t}$ is an indicator that takes the value of one if there was a homicide in the previous two months and the victim was a female. Similarly, $MaleVic_{i,t}$ is an indicator that takes the value of one if there was homicide in the previous two months and the victim was male. θ_m and $\gamma_{i,y}$ represent month fixed effects and city-by-year fixed effects.

The results of this analysis are reported in Table 7. Column headers indicate the demographic-specific measure of concealed-carry applications and row titles correspond to the demographic-specific homicide indicator variables included in the model. Prior to inspection of the results, it is worth noting that the estimates presented in columns 1 through 4 of Table 7 are simply a weighted average of the point estimates presented in columns 2 through 5 of Table 6 where the weighting is determined by the share of victim homicides within each demographic group. We also note that the fluctuation in our sample size across columns 1 through 4 is a result of using a fixed-effects maximum likelihood approach that drops observations lacking variation in demographic-specific applications within city-years.

Column 1 of Table 7 shows the estimated response of black applications separately for incidents involving a white victim and incidents involving a black victim. Estimates from similar models focusing on white applications are reported in Column 2. These estimates reveal no significant differential effects across race. The estimates in columns 3 and 4 focus on gender-specific applications responding to homicide incidents separately by the gender of the victim. These estimates show large and significant effects on applications in cases where the applicant and the victim are the same gender. However, the estimates on female victim homicides are not significantly different from the estimates on male victim homicides in either Column 3 or Column 4.[41]

G. Estimated Effects on Permit Renewals

In this section, we explore whether permits obtained after recent homicide incidents are more or less likely to be renewed. Doing so allows us to speak to longer run decisions to extend the option to legally carry a concealed firearm. It is not clear a priori how recent homicide incidents lead to updated beliefs that might affect the likelihood of future renewal. On the one hand, responses to recent homicides may be due to a temporary emotional response to a perceived threat or an overreaction. In such cases, we would expect applications to have a lower likelihood of renewal. This would be consistent with the tendency of individuals to purchase precautionary devices, and then not use them after a period of time (Yechiam, Erev and Barron, 2006). On the other hand, homicide incidents may have longer-term effects on individuals and increase the likelihood of renewal relative to alternative motivations for permit applications.

Our analysis on permit renewals is also related to recent research testing for systematic projection bias in individual-decision making. In contrast to standard assumptions that individual forecasts of future utility are, on average, equal to realized utility, recent research has shown that individuals may be systematically biased when predicting future utility. To illustrate evidence of projection bias, Busse et al. (2015) show that contemporaneous weather influences new four-wheel drive and convertible purchases, but that weather-influenced vehicle purchases are more likely to be returned. Similarly, Conlin, O'Donoghue and Vogelsang (2007) find that decisions to purchase items for cold weather are influenced by the current weather, and that such items are more likely to be returned. In a similar spirit, we compare renewal rates of permits obtained following recent homicides to renewal rates of all other permits.[42] Note that this does not represent a formal test for projection bias since it may be the case that permit holders realize utility throughout the time period of their permit. Nonetheless, we can rule out projection bias in this context if permits obtained after a recent homicide have a greater or equal likelihood of renewal relative to other permits.

For this analysis we restrict our attention to the first renewal opportunity of applicants in cities below the median population that (i) are first-time permits holders and (ii) have permit expiration dates at least a year before the end of our data.[43] This effectively limits our sample to applications submitted before 2007 and reduces the number of applications in cities below the median population from 44,588, to 10,787. Of the permits in the resulting sample, 69 percent are renewed within six months after their expiration date. We test for differences in renewal rates using a linear probability model with the outcome being an indicator that takes the value of one if the permit was renewed within six months after the expiration date, and zero otherwise.[44]

Table 8 reports the estimated effects of a homicide within the previous two months of the application date on the probability of a permit renewal within six months after the expiration date. In columns 1 and 2 we show results using all homicides and gun-related homicides. While there is no apparent response to all homicides in Column 1, Column 2 suggests that a recent gun-related homicide increases the probability of renewal by five percentage points, representing a seven percent increase over the baseline renewal rate (69 percent). In columns 3 through 9 we show the effects of gun-related homicides by applicant demographic characteristics. Similar to Table 6, we find that the effects are driven by whites and males. Columns 7 through 9 indicate that the effect on renewals is more pronounced among individuals over the age of 40. Taken together, these results indicate that permits obtained shortly after a homicide incident are more likely to be renewed, suggesting that homicide incidents lead to persistent precautionary behaviors in the form of concealed-carry permits. Furthermore, the results yield no evidence for projection bias as an explanation for the immediate increase in permits after a homicide incident.

H. Estimated Response to Other Crimes

Consistent with evidence for precautionary behaviors following higher crime rates in other settings, our results reveal an increase in concealed-carry permit applications following homicide incidents. We next explore whether this is also true for less serious types of crime that may be less likely to influence perceptions of personal security. As discussed in the data section, we use municipal law-enforcement agency crime reports available in the UCR data.[45] Of the 171 cities in our NCSHS data, 132 cities are represented by a municipal law enforcement agency in the UCR data. This includes 69 of the 132 cities with populations below the median, for which we present estimates in Table 9.[46]

Columns 1 through 5 of Table 9 show estimates for violent crimes including homicide, rape, aggravated assault, and robbery, while columns 6 through 9 focus on property crimes including burglary, larceny, and motor vehicle theft. The estimates shown in Table 9 panels A and B suggest that individual permit applications are not responsive to violent or property crimes, with the exception of homicides. The estimated effect on homicides, shown in Column 2 suggests that a homicide in the previous two months increases concealed-carry applications by approximately 19 percent, slightly larger than the estimate obtained using NCSCHS data.[47] However, none of the non-homicide crime estimates are significant and all are small in magnitude relative to the effect following a homicide incident. That applications appear to respond to recent homicides, but not other crimes, reinforces the notion that application decisions are only sensitive to serious perceived threats. It may also be the case that the public is more aware of homicide incidents as they may be publicized to a greater degree than other crime incidents, especially in small communities.

VI. Ancillary Analysis Using Disaggregated Data

Our main estimates provide strong evidence that homicide incidents increase concealed-carry applications, but only in relatively small cities. Effects in small cities, in addition to the patterns highlighted in figures 2 and 3, and Table A4 in the Online Appendix, suggest that the nearness of the homicide incident likely influences decisions to apply for concealed-carry permits.[48] That is to say, homicide incidents in smaller cities likely provide a more natural setting to test for this behavioral response. The question remains whether such responses are present in relatively large cities when analyzed at more local geographic classifications. We explore this further using address-level crime data in a subsample of cities in North Carolina. Our data for this exercise were obtained directly from nine municipal law enforcement agencies including Chapel Hill, Fayetteville, Greenville, High Point, Raleigh, Rocky Mount, Sanford, Wake Forest, and Wilson.[49] These data contain detailed information on crime incidents including address, offense type, and date; and span 2002-2012, though the number of reported years varies by agency.[50] The populations in these nine cities range from 28 thousand to over 400 thousand; all are within the top 30 largest cities in North Carolina and all are large relative to the sample of cities used in our main analysis.

We aggregate the address-level crime data in several ways to explore the relationship between geographic classification and applicant responses to homicide incidents. It is important to keep in mind that analyzing data at various levels of aggregation has several tradeoffs. Consistent with our motivation to focus on local areas, using disaggregated data can potentially improve the precision of the estimates by focusing on salient treated groups that would otherwise be masked at higher levels of aggregation. On the other hand, estimates based on variation within local areas will not include spillover effects of homicide incidents also affecting neighboring areas. It is also the case that more disaggregate data can potentially increase measurement error.[51]

With these caveats in mind, we use census sub-city geographic classifications for this exercise, which provide a natural breakdown defined by criteria including a "homogeneity principle" that defines areas based on "a nucleus with its surrounding zone of influence" (US Census Bureau, 2012). These classifications allow us to aggregate to the municipality, census tract, block group, and block level. Census tracts, block groups, and blocks are geographic areas defined by visible features (e.g. streets roads, highways, rivers, railroads etc.) and specified population and geographical criteria (US Census Bureau, 2012).

We merge our crime data with concealed-carry application data and census demographic characteristics. The average population in municipalities, census tracts, block groups, and blocks is 112,830, 4,792, 1,796, and 148 respectively. In these cities, we observe an application rate of approximately 3.9 per 10,000 individuals, similar to the 3.75 applications per 10,000 that we observe in cities above the median population in the NCSCHS sample. Similar to our main analysis, we initially focus on homicide incidents.[52]

Following the identification strategy outlined in Section IV, we estimate the effect of a crime incident in the previous two months on applications for concealed-carry permits within municipalities and census areas. Similar to our main specification, our analysis controls for month fixed effects and area-by-year fixed effects. We calculate standard errors corrected for potential clustering at the geographic level of aggregation.

A. Ancillary Analysis Results

Table 10 shows the estimated effects of homicide incidents on concealed-carry applications analyzed at the municipality, census tract, block group, and block level. In Column 1 the estimate provides no evidence that homicide incidents in the previous two months affect concealed-carry applications in municipalities, which is consistent with our estimates in relatively large cities in Table 2. In Column 2 however, the point estimate indicates that a homicide in the previous two months increases concealed-carry applications in census tracts by approximately 8 percent. This estimate is smaller than the 13 percent estimate in our main analysis of below median population cities, which have roughly the same average population as census tracts.[53] In columns 3 and 4, the estimates remain positive but are no longer significant, consistent with the analysis being too disaggregated to yield information.[54]

We next expand our analysis in Table 11 to consider the effect of other violent and property crimes measured at the census tract level. Similar to our findings in Table 9, there is no evidence of an effect following violent, property, assault, robbery, larceny, or motor vehicle theft crimes. Notably different from Table 9 estimates, Column 5 of Table 10 suggests that a recent burglary increases the number of applications by approximately 8 percent within census tracts. Taken together, the estimates using disaggregated data support our main findings that applications are responsive to more serious perceived threats as measured by recent local homicide incidents. In the Online Appendix in Table A5 we show estimates from models that include additional lags and leads. Similar to Table A2, these results show no relationship between current applications and subsequent homicides, reassuring that endogeneity concerns are unlikely to be driving the results.[55]

VII. Conclusion

Using data on concealed-carry permit applications in North Carolina from 1998 to 2012, we exploit variation in recent homicide incidents to estimate the effect of crime on concealed-carry applications. We find that recent homicide incidents increase individual applications in relatively small cities and in larger cities within census tracts. Our city-level estimates indicate that a homicide incident increases applications by 13 percent for the following two months in relatively small communities (cities with below-median population). The magnitude of the city-level estimates diminish when analyzing effects in increasingly larger cites, though our analysis using disaggregated data reveals persistent effects in relatively large cities when using tract level data. Taken together, we view this as evidence that proximity to recent serious crime plays an important role as individuals make decisions regarding legal gun carrying.

The detail available in our data also allow us to provide insight into specific circumstances surrounding homicide incidents and characteristics of responsive applicants. We find that gun-related homicides drive our main estimates and that white, male, and Republican applicants are most responsive to homicide incidents. Our results also suggest that applicants respond more when the homicide victim is the same gender. Finally, our results suggest that these precautionary behaviors are persistent as individuals responding to recent gun homicide incidents are more likely to renew their permits upon expiration relative to other permit holders.

Our results provide the first causal evidence that crime risk shapes individual decisions regarding legal gun use. We view our research as taking an initial step toward a better understanding of the determinants of concealed carrying and contributing to a more informed debate regarding the interaction between legal gun ownership, public safety and trade-offs associated with public and private security efforts. Our analysis of the types of individuals and specific circumstances that drive our estimates can inform society and policy makers of specific scenarios that lead to more guns. Of course, whether these are desirable behavioral responses from a societal perspective depends on their effect on public safety, which remains a source of ongoing controversy. Given these ongoing debates and the recent dramatic increase in the number of concealed-carry permits, understanding the determinants of concealed-carrying and the demand for guns remains an important area for future research.

VIII. References

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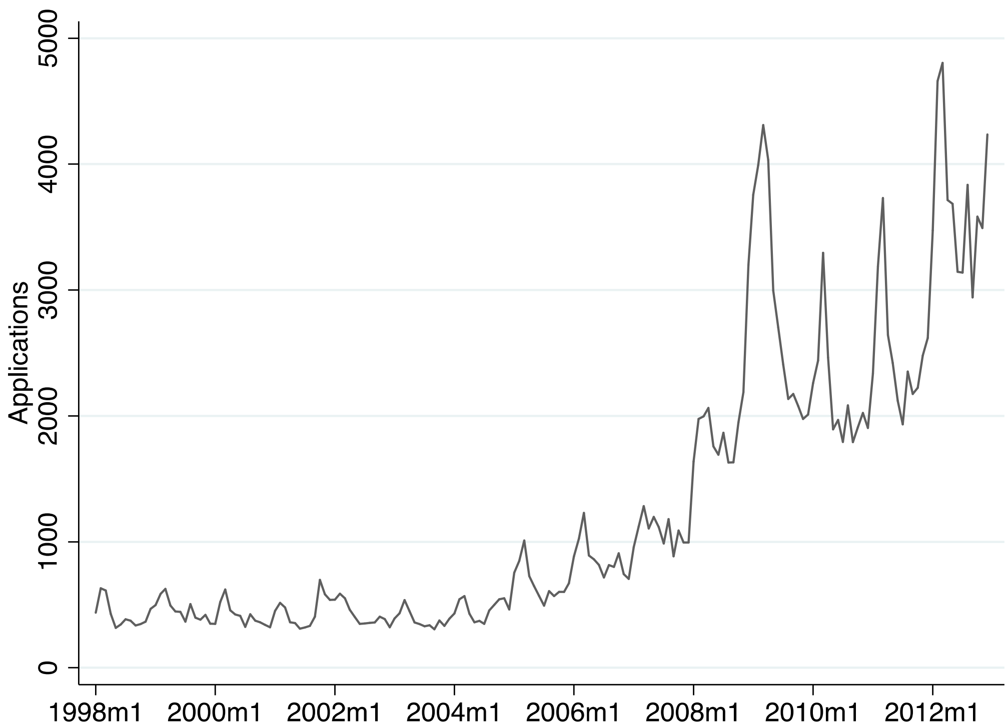
Figures and Tables

Figure 1 Monthly Concealed-Carry Applications in North Carolina

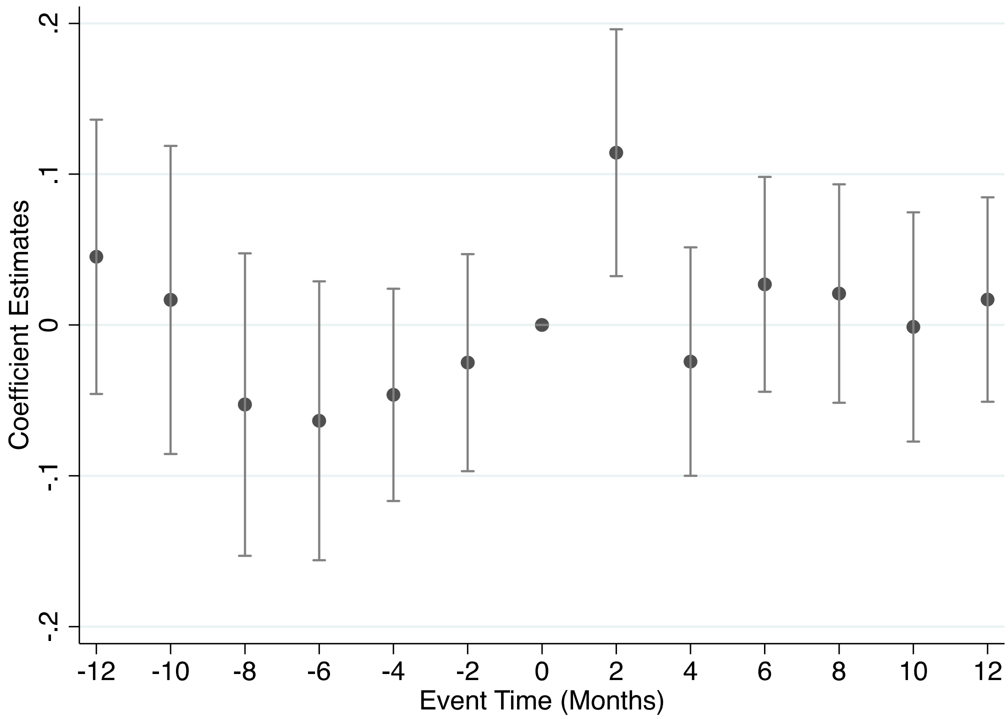


Notes: The figure shows the number of new concealed-carry permit applications for each month from January 1998 through December 2012 in the state of North Carolina. Application data is from the North Carolina State Bureau of Investigations.

Figure 2 Estimated Effect of Homicides on Concealed Carry Applications by City Size

Notes: The figure plots estimated effects of a homicide in the previous two months on concealed-carry applications. Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. Standard errors (in parentheses) are corrected for possible clustering at the city level. For each point estimate, the sample is limited to 40 cities. The far-left point is the estimate from the 40 least populated cities. We incrementally move to a sample of the 40 most populated cities (far right estimate). This process results in 132 estimates.

Figure 3 Concealed-Carry Applications After a Homicide Incident



Notes: This figure plots event study coefficient estimates from the model outlined by Equation 2. Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given city in a given month. Standard errors (in parentheses) are corrected for possible clustering at the city level. The sample is limited to cities below the median population. Coefficient estimates are pooled in two-month bins. The bars show the 90% confidence interval. The coefficient for month of the homicide is omitted from the regression.

Table 1
Summary Statistics

	(1) All Cities	(2) Below Median	(3) Above Median
Panel A: Permit Applications			
All	7.500	2.997	11.881
	(16.146)	(4.479)	(21.349)
Rate	5.288	6.868	3.753
	(8.212)	(10.289)	(5.028)
Black	0.659	0.165	1.140
	(2.913)	(0.579)	(3.993)
White	6.684	2.762	10.499
	(13.427)	(4.205)	(17.576)
Male	5.911	2.337	9.387
	(12.445)	(3.420)	(16.420)
Female	1.589	0.660	2.493
	(4.017)	(1.358)	(5.327)
Ages 21-39	2.776	1.019	4.484
	(7.054)	(1.983)	(9.402)
Ages 40-59	3.267	1.316	5.164
	(6.819)	(2.099)	(8.951)
Ages 60 plus	1.458	0.661	2.232
	(3.097)	(1.323)	(4.001)
Panel B: Homicides			
Homicide Indicator	0.110	0.029	0.188
	(0.313)	(0.169)	(0.391)
Homicides	0.181	0.030	0.327
	(0.698)	(0.177)	(0.941)
Homicide Rate (per 10,000)	0.066	0.069	0.063
	(0.329)	(0.426)	(0.192)
Gun Homicide	0.128	0.020	0.233
	(0.559)	(0.147)	(0.758)
Other Homicide	0.053	0.010	0.095
	(0.272)	(0.100)	(0.365)
Population (in 10,000s)	2.781	0.457	5.042
	(7.329)	(0.168)	(9.776)

Notes: Standard deviations are displayed in parentheses. Summary statistics are calculated from city-by-month level data. There are 30,180 city-by-month observations. 14,880 are at or below the median population and 15,300 observations are above the median population.

Table 2
Estimated Effects of Homicides on Concealed-Carry Applications

	(1)	(2)	(3)	(4)
	Rates	Levels	Indicators	
Panel A: All Cities				
Homicides last month	0.006	0.008	-0.012	
	(0.017)	(0.007)	(0.015)	
Homicides two months prior	0.024	0.000	0.009	
	(0.016)	(0.004)	(0.011)	
Homicide previous two months				-0.004
				(0.014)
Number of observations	30,180	30,180	30,180	30,180
Panel B: Below Median Population				
Homicides last month	0.039**	0.108***	0.107***	
	(0.016)	(0.036)	(0.037)	
Homicides two months prior	0.049***	0.115***	0.125**	
	(0.017)	(0.045)	(0.050)	
Homicide previous two months				0.124***
				(0.036)
Number of observations	14,880	14,880	14,880	14,880
Panel C: Above Median Population				
Homicides last month	-0.045	0.006	-0.021	
	(0.031)	(0.007)	(0.016)	
Homicides two months prior	-0.011	-0.001	0.003	
	(0.030)	(0.004)	(0.011)	
Homicide previous two months				-0.017
				(0.015)
Number of observations	15,300	15,300	15,300	15,300
Month FE	Yes	Yes	Yes	Yes
City-by-Year FE	Yes	Yes	Yes	Yes
County Linear Trend	Yes	Yes	Yes	Yes

Notes: Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given city in a given month. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 3
Robustness Checks: Estimated Effect of Homicide on Concealed Carry Applications

	(1)	(2)	(3)	(4)
Homicide previous two months	0.124***	0.115***	0.102***	0.092***
	(0.036)	(0.037)	(0.032)	(0.033)
Month Fixed Effects	Yes	Yes	-	-
City by Year Fixed Effects	Yes	Yes	Yes	Yes
Year by Month Fixed Effects	No	No	Yes	Yes
County Linear Time Trend	No	Yes	No	Yes
Number of observations	14,880	14,880	14,880	14,880

Notes: Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given city in a given month. Standard errors (in parentheses) are corrected for possible clustering at the city level.

Table 4
Estimated Effects by Various Causes of Death

	(1) Any Homicide	(2) Gun Homicide	(3) Other Homicide	(4) Motor Vehicle	(5) Suicide	(6) Drug Overdose
Mortality incident previous two months	0.124***	0.153***	0.032	-0.030	-0.021	0.022
	(0.036)	(0.044)	(0.055)	(0.021)	(0.024)	(0.023)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	14,880	14,880	14,880	14,880	14,880	14,880
Two month incident probability	0.030	0.020	0.010	0.048	0.049	0.056

Notes: Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given city in a given month. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 5
Estimated Effects by Applicant Voter History

	(1) All	(2) Non-Voters	(3) Voters	(4) Rep	(5) Dem	(6) Other
Panel A: Baseline Model						
Homicide previous two months	0.124***	0.113*	0.127***	0.153***	0.089	0.137*
	(0.036)	(0.062)	(0.035)	(0.045)	(0.055)	(0.071)
Number of observations	14,880	13,320	14,808	13,500	13,404	12,900
Panel B: Interacted Model						
Homicide previous two months	0.111**	0.049	0.126***	0.137**	0.118*	0.122
	(0.045)	(0.076)	(0.045)	(0.065)	(0.071)	(0.088)
Homicide previous two months × Postelection	0.027	0.127	0.001	0.034	-0.063	0.029
	(0.066)	(0.100)	(0.070)	(0.122)	(0.095)	(0.112)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	14,880	13,320	14,808	13,500	13,404	12,900

Notes: Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the demographic-specific number of new concealed-carry permit applications for a given city in a given month corresponding to the column titles. Standard errors (in parentheses) are corrected for possible clustering at the city level. * 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 6
Estimated Effects by Applicant Characteristics

	(1) All	(2) Black	(3) White	(4) Male	(5) Female	(6) Ages 21-39	(7) Ages 40-59	(8) Ages 60+
Homicide previous two months	0.124***	0.049	0.142***	0.137***	0.079	0.107*	0.150***	0.099*
	(0.036)	(0.089)	(0.037)	(0.032)	(0.073)	(0.060)	(0.041)	(0.058)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	14,880	8,280	14,736	14,844	12,732	13,776	14,472	13,032

Notes: Estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the demographic- specific number of new concealed-carry permit applications for a given city in a given month corresponding to the column titles. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 7
Estimated Effects by Victim Salience

	(1) Black Applicants	(2) White Applicants	(3) Male Applicants	(4) Female Applicants
Black Victim	0.051	0.180***		
	(0.109)	(0.051)		
White Victim	-0.013	0.078*		
	(0.163)	(0.046)		
Male Victim			0.142***	0.040
			(0.036)	(0.078)
Female Victim			0.054	0.221*
			(0.052)	(0.114)
Month FE	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes
Number of observations	8,280	14,736	14,844	12,732

Notes: These estimates are based on Poisson models using monthly data on homicides from the North Carolina State Center for Health Statistics and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the demographic-specific number of new concealed-carry permit applications for a given city in a given month corresponding to the column titles. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 8
Estimated Effects of Homicides Incidents on Permit Renewals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Type of Homicide		Gun Homicides						
	All	Gun	Black	White	Male	Female	Ages 21-39	Ages 40-59	Ages 60+
Homicide previous two months	0.018	0.051***	0.028	0.044**	0.055**	0.024	0.010	0.058**	0.078*
	(0.018)	(0.017)	(0.100)	(0.019)	(0.022)	(0.066)	(0.046)	(0.022)	(0.042)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10,787	10,787	600	10,014	9,038	1,749	3,578	5,118	2,091

Notes: Estimates are based on OLS regression models using individual level concealed-carry permit application data from the North Carolina State Bureau of Investigations and monthly homicides from the North Carolina State Center for Health Statistics. The outcome variable is an indicator that takes the value of one if the individual renewed their permit within six months of the expiration date. The estimates represent the marginal effect of the likelihood of renewal if the permit holder originally applied the permit in the two-month following a homicide incident. Column 1 considers all homicides. Column 2 considers only gun homicides. Columns 3-9 report the effects by demographic groups for gun homicides. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 9
Estimated Effects of Other Crime on Concealed-Carry Applications Using UCR Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Violent Crimes					Property Crimes			
	All	Homicide	Rape	Agg. Assault	Rob.	All	Burglary	Larceny	Motor Veh. Theft
Crime incident previous two months	0.026	0.172***	-0.044	-0.001	0.024	0.038	-0.005	0.013	0.011
	(0.024)	(0.042)	(0.037)	(0.022)	(0.024)	(0.061)	(0.018)	(0.031)	(0.029)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Two-month incident probability	.7900061	.0521496	.0743874	.7148339	.283858	.9688672	.6868405	.9316299	.211302

Notes: Estimates are based on Poisson models using monthly data on crimes from the FBI's Uniform Crime Reports and concealed-carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given city in a given month. The column titles correspond with the type of crime associated with the estimated coefficient. Standard errors (in parentheses) are corrected for possible clustering at the city level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 10
Estimated Effects of a Homicide Incident using Disaggregated Data

	(1) Municipal Agency	(2) Tract	(3) Block Group	(4) Block
Homicide previous two months	0.046	0.075**	0.009	0.243
	(0.046)	(0.038)	(0.070)	(0.387)
Month FE	Yes	Yes	Yes	Yes
City-by-Year FE	Yes	Yes	Yes	Yes
Number of observations	572	27,884	57,310	119,726
Number of Units	9.000	356.000	824.000	4401.000
Average Population	112829.9	4791.683	1796.408	148.126

Notes: Estimates are based on Poisson models using crime data from North Carolina law enforcement agencies and concealed- carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications for a given geographical area in a given city in a given month corresponding to the column titles. Standard errors (in parentheses) are corrected for possible clustering at the data aggregation level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

Table 11
Estimated Effects of Other Crime Incidents at the Census Tract Level

	(1) Violent	(2) Property	(3) Homicide	(4) Assault	(5) Burglary	(6) Robbery	(7) Larceny	(8) MV Theft
Previous two months	0.003	-0.031	0.075**	0.013	0.071**	-0.029	-0.033	-0.033
	(0.024)	(0.042)	(0.038)	(0.022)	(0.036)	(0.019)	(0.035)	(0.024)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	27,884	27,884	27,884	27,884	27,884	27,884	27,884	27,884
Two-month incident probability	0.479	0.629	0.030	0.421	0.562	0.342	0.610	0.413

Notes: Estimates are based on Poisson models using crime data from the North Carolina law enforcement agencies and concealed- carry permit applications from the North Carolina State Bureau of Investigations. The outcome variable is the number of new concealed-carry permit applications in a given census tract for a given city in a given month. The column titles correspond with the type of crime associated with the estimated coefficient. Standard errors (in parentheses) are corrected for possible clustering at the data aggregation level.
* 0.10, ** 0.05 and ***0.01 denote significance levels.

[1] According to the National Conference of State Legislatures, 18 states ban concealed carrying on college campuses, 22 states leave the decision to each college or university, eight states specifically allow concealed carrying on college campuses, and the remaining two states have mixed laws.

[2] See for instance Lott and Mustard (1997); Lott (1998); Bronars and Lott (1998); Dezhbakhsh and Rubin (1998); Black and Nagin (1998); Ludwig (2000); Olson and Maltz (2001); Moody (2001); Mustard (2001); Plassmann and Tideman (2001); Ayres and Donohue (2003); Durlauf, Navarro and Rivers (2016) and Donohue, Aneja and Weber (2017).

[3] Our thanks to an anonymous referee for insightful comments on this issue.

[4] Our research also relates to a larger literature analyzing the effect of precautionary behaviors on crime. For instance, Ours and Vollaard (2015) and Ayres and Levitt (1998) find declines in auto theft as anti-theft devices become available; Vollaard and Van Ours (2011) find declines in burglary following the installation of burglary-proof windows in newly built homes; Cook and MacDonald (2011) show that private investments in business improvement districts (BID), which include expenditures on security, significantly reduce crime in BID areas.

[5] Philipson and Posner (1996) emphasize the importance of accounting for such self-protective responses to crime as they may contribute to subsequent increases in public safety typically attributed to a public law-enforcement response to crime.

[6] A 2013 Pew survey found that 48 percent of gun owners cited protection as the main reason for gun ownership and 79 percent responded that owning/having a gun in the household makes them feel safer (Pew Research Center, 2013).

[7] Illinois was the last state to legalize concealed carry in 2013.

[8] There is significant variation in the circumstances necessary to justify a permit across may-issue states.

[9] Grossman and Lee (2008) find that three factors increase the likelihood of adopting a shall-issue rather than a may-issue law: rural status, decisions of neighboring states, and increases in crime.

[10] See also Durlauf, Navarro and Rivers (2016), which discusses the role of model uncertainty in estimating the effects of concealed carry laws on crime.

[11] For instance, Costanza, Kilburn and Miles (2013) find that income, political ideology, and crime are significantly correlated with permit rates using one year of concealed-carry data in

Connecticut townships. Bankston and Thompson (1989) and Costanza and Kilburn (2004) find that demographic measures and gun beliefs are correlated with concealed carrying, but show mixed results on income and crime using cross-sectional Louisiana data at the parish level. Thompson and Stidham (2010) use county-level North Carolina data aggregated to a 10-year period to estimate the correlates of concealed-carry permits and conclude that, “the important factors in explaining concealed-carry rates in North Carolina are Republicanism, annual hunting permits, and [geographic] shifts in Black population.”

[12] The fee is $80.00 as of 2015.

[13] Based on the 2010 Population Census.

[14] See Thompson and Stidham (2010) for addition discussion.

[15] Our data was obtained through a 2013 freedom of information request pursuant to North Carolina Public Records Law (G.S. 132-1 through 132-10). Please contact the authors for additional documentation.

[16] The median time between the application date and the issue date is 35 days.

[17] In results available upon request, we find results similar to our main estimates when including these earlier years.

[18] These data were obtained from the Odom Institute (2015).

[19] We use the following ICD-10 codes to identify homicides: X85-X99, Y01-Y09, Y87.1. In cases where an individual died in the hospital, the city of residence is used rather than city of occurrence.

[20] The Census designates incorporated areas if the population exceeds 2,500.

[21] To avoid problems with inconsistent or incomplete reporting in the UCR, we (i) visually inspect the data for lumpy reporting (e.g. quarterly/yearly reporting instead of monthly reporting

or disproportionate reporting at the end of the year) and (ii) keep agencies that report in 95 percent of months since being first observed in our sample.

[22] See Maltz (2010).

[23] In addition to unjustified criminal homicides, the NCSCHS includes justified homicides, which potentially affect decisions to apply for concealed-carry permits. According to 2013 UCR, 94 percent of homicides are unjustified criminal homicides.

[24] Our main analysis uses a non-linear maximum likelihood estimator that includes city-by-year fixed effects. As such, 600 of the 30,780 matched observations that are used in the analysis are dropped as some of the city-by-month observations have no variation in applications with a given year.

[25] Alternatively, homicides in relatively large cities occur more frequently and a relatively small fraction of a city's population is likely to perceive a change in victimization risk.

[26] We report results similar to our main Poisson estimates using negative binomial and OLS models in the Supplementary Online Appendix in Table A1.

[27] Throughout the remainder of the paper we calculate percentage effects as $(e^{\beta} - 1) \times 100\%$.

[28] Limiting the data to cities that have a population in the bottom tercile results in point estimates that are slightly larger than the estimates reported in Column B.

[29] The size of the effect is largely due to low concealed-carry permit rates among the general population. Over the 15 years in our sample, only 4.8 percent of the population applied for a concealed-carry permit.

[30] Notably, these estimates do not include potential changes in applications within neighborhoods in larger cities that are more proximal to homicide incidents. This is explored to some extent using alternative data in Section VI.

[31] Our primary model does not include year-by-month fixed effects or county-specific linear time trends in order to help facilitate convergence of the estimates in subsequent heterogeneity analyses that restrict the sample size.

[32] In the Online Appendix in Table A4 we extend the analysis to all cities and consider estimates at the zip code level. While the effect primarily shows up in the month after the homicide, the estimates are consistent with our main results in Table 2 in that they suggest effects in more local areas. Moreover, the estimates also line up nicely with our subsequent analysis across applicant demographic characteristics in Table 6.

[33] Similar to our main analysis, we use a Poisson model and calculate standard errors corrected for potential clustering at the city level.

[34] For example, Zebulon, NC, had homicide incidents in March, 2001, May, 2007, July, 2008, and February, 2011. Therefore, in January of 2008, $H_{i,4} = 1$, since it had been eight months since the May 2007 homicide. Similarly, $H_{i,-3} = 1$, since it is six months before the July 2008 homicide. Finally, each end period bin is coded as one, since there were homicide incidents more than a year before and more than a year after January 2008.

[35] See Gallagher (2014) for additional discussion of this event study framework and the practical purposes for including bins of at each end periods.

[36] Analysis using longer time horizons displayed similar results. Furthermore, we excluded the estimates on the end period bins for convenience because of the large standard errors.

[37] Though our analysis reveals no evidence that applications are related to future homicides, we acknowledge that consistent increases in applications could affect crime levels over a longer time horizon.

[38] Drug-overdoses consist of deaths from accidental poisoning and exposure to noxious

substances.

[39] Note that North Carolina has a semi-open primary that allows unaffiliated voters to vote in any one party's primary. Registered voters can only vote in their party's ballot.

[40] Unmatched records may be due to no voting history or our inability to identify unique matches across the samples.

[41] In Appendix Table A3, we show the complete set of gender and race combinations, which yield a largely similar pattern with regards to gender but typically lack precise estimation.

[42] Projection bias leading to precautionary gun behaviors gives rise to the possibility that wait times for gun purchases could reduce purchases influenced by projection bias and mitigate potential externalities.

[43] Similar to our main analysis, we find that homicides do not affect permit renewals in larger cities.

[44] In results available on request, we find similar estimates when using indicators for a renewal within three or 12 months of the expiration date.

[45] We also considered using the UCR Homicide Supplement, which contains additional detail on each homicide incident including relationships between the victim and offender. Unfortunately, the majority of agencies in below- median population cities do not report consistently to the homicide supplement, resulting in a limited sample size, far fewer homicides, and imprecise estimates.

[46] The difference in the number of cities between the two samples is related to the limited number of agencies in the UCR.

[47] In results not shown, we continue to find that homicide incidents increase applications only in cities with below median population. To further explore the differences between the estimates

using these two data sets, we restricted the NCSCHS data to the same city observations as the UCR data and found very similar estimates (see Online Appendix Table A6).

[48] The Online Appendix can be found at http://jhr.uwpress.org/.

[49] The data were obtained through individual requests to municipal law enforcement agencies (Fayetteville, Greenville, and Raleigh) and from publicly available crime reports on agency websites (Chapel Hill, High Point, Rocky Mount, Sanford, Wake Forest, and Wilson).

[50] Our sample includes the following years for each agency: Chapel Hill (2012), Fayetteville (2002-2012) Greenville (2010-2012), High Point (2006-2012), Raleigh (2005-2012), Rocky Mount (2006-2012), Sanford (2006-2012), Wake Forest (2010-2012), Wilson (2006-2012).

[51] See Lindo (2015) for a demonstration of these tradeoffs when analyzing the effects of economic conditions on health at various levels of geographic aggregation.

[52] We observe 585 homicides and 38,036 concealed-carry applications in these cities over the sample time-span.

[53] The average population in census tracts and below median population cities is 4,792 and 4,570, respectively. Note that average monthly applications are lower in census tracts (1.364) than below median population cities (2.997), leading to an even smaller effect size in terms of application counts.

[54] This may be due to the limited variation in homicides and concealed-carry applications within relatively small areas or related to the tradeoffs associated with analyzing disaggregated data discussed previously.

[55] In results not reported we also find that these effects are driven by white and male applications, similar to Table 6 and Online Appendix Table A4.