THE IMPACT OF GUN LAWS ON POLICE DEATHS*

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ABSTRACT

This paper uses state-level data from 1984–96 to examine how right-to-carry laws and waiting periods affect the felonious deaths of police. Some people oppose concealed weapons carry laws because they believe these laws jeopardize law enforcement officials, who risk their lives to protect the citizenry. This paper strongly rejects this contention. States that allowed law-abiding citizens to carry concealed weapons had a slightly higher likelihood of having a felonious police death and slightly higher police death rates prior to the law. After enactment of the right-to-carry laws, states exhibit a reduced likelihood of having a felonious police death rate and slightly lower rates of police deaths. States that implement waiting periods have slightly lower felonious police death rates both before and after the law. Allowing law-abiding citizens to carry concealed weapons does not endanger the lives of officers and may help reduce their risk of being killed.

I. INTRODUCTION

RECENTLY, gun-related public policy issues have captured the country's attention. Tragic school shootings and the accidental deaths of children have made headline news. The felonious deaths of police officers, who regularly risk their lives[1] to enforce society's rules and protect its citizens, have a particularly profound impact on society. Since 1794, more than 15,000 law

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[1] David Lester, Civilians Who Kill Police Officers and Police Officers Who Kill Civilians: A Comparison of American Cities, 10 J. Police Sci. & Admin. 384 (1982). Lester argued that the lethal assault rate against police is higher than for any other profession. On the other hand, Lawrence Southwick claimed that when controlling for some basic characteristics like age and gender, the death rate of police has been declining for many years. By the 1990s, the rate for police was below the rate for manufacturing occupations and the general rate for the public. Lawrence Southwick, An Economic Analysis of Murder and Accident Risks for Police in the United States, 30 App. Econ. 593–95 (1998).

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enforcement officers have been killed.[2] Although many proposals have been set forth to reduce this violence, there is little quality evidence about how gun control laws affect the lives of police officers. This paper addresses this paucity of analysis by examining how two laws—waiting periods and right-to-carry laws—affect the numbers of felonious police deaths.

In examining this issue, we should first understand what police believe about the laws. The International Association of Chiefs of Police (IACP), the world's oldest and largest membership organization of police executives, has made official statements about concealed weapons laws and the Brady Act, a federally mandated background check. The IACP has over 16,000 members and represents law enforcement agencies of all sizes on the local, state, federal, and international levels. At its 103d annual conference in October 1996, the IACP passed two resolutions to oppose the federal preemption or liberalization of individual states' concealed carry laws.[3] The IACP argued the following to justify its position: "Whereas, there is an effort to liberalize states' CCW [concealed carry weapons] laws by enacting federal legislation, which would pre-empt current state CCW laws, with the argument that citizens wish to carry guns for self-protection, further arguing that the arming of private citizens will result in dramatically lowering the national crime rate by deterring criminals from victimizing these law-abiding citizens; and whereas, a majority of law enforcement professionals and an overwhelming majority of Americans do not support this theory."[4]

Although the IACP does not have a formal resolution on waiting periods, its Executive Committee has stated, "The IACP continues to strongly support the Brady Law."[5]

The Fraternal Order of Police, the world's largest organization of sworn law enforcement officers, with more than 280,000 members, strongly endorsed the Brady Bill but believes that the waiting period requirements should be eliminated as instant-check technology becomes reliable. However, because individual state lodges have widely differing views on right-to-carry or shall-issue laws, the Fraternal Order of Police takes no public stand on this issue.[6] Some police organizations in states that do not have concealed carry laws have criticized such laws. For example, the Illinois and Maryland state police have been very outspoken against concealed weapons laws and

[2] For information about slain officers, see the National Law Enforcement Officers Memorial Fund at http://www.nleomf.com/index1.html.

[3] Resolutions F008.a96 and F009.a96 were passed by the IACP in October 1996 at its 103d annual conference in Phoenix. See http://www.theiacp.org.

[4] IACP Resolution F008.a96, passed in October 1996.

[5] Minutes of the IACP Executive Committee Meeting on February 11, 1995, in Alexandria, Va.

[6] Private conversation in March 2000 with Jim Pasco, who works for the Fraternal Order of Police in its legislative office in Washington, D.C. Mr. Pasco stated that the organization has a stand on every citizen firearm proposal except concealed weapons laws.

regularly testify against concealed carry laws before state legislatures. One of the first and most frequently articulated criticisms of a study by John Lott and David Mustard[7] was that allowing people to carry concealed weapons would drastically alter the safety of the police. Such concern received a lot of attention when Elizabeth Dole, who at that time was a candidate for the Republican nomination for president of the United States, articulated it. In May 1999, Dole stated that she strongly opposed shall-issue laws because she believed they endangered officers' lives.

Although the above arguments are frequently made, the extent to which officers' lives are jeopardized by concealed carry laws is an empirical question. Debates in gun-related public policy are often driven by anecdotal evidence—the tragic tale of a young life lost to an irresponsible use of weapons or a heroic defensive use of a firearm to preserve life. However, the debate about law enforcement safety is unusual because those who believe right-to-carry laws threaten officers' lives do not even have anecdotal evidence. Although many states have had shall-issue laws for long periods, there are no known examples of a licensed permit holder using his weapon against an officer of the law. This lack of examples has led adherents to this belief to articulate indirect mechanisms about how officers' lives are threatened by concealed carry laws. For example, they argue that shall-issue laws lead criminals to arm themselves more heavily and that these criminals direct more violence toward police officers.

In contrast, there is at least one example of a permit holder using his weapon to assist a law enforcement officer.[8] On March 26, 1999, three drug dealers ambushed Officer Marc Atkinson of the Phoenix Police Department. Rory Vertigan, a concerned citizen with a concealed weapon permit, came to the officer's aid. Vertigan shot and wounded one suspect and tackled the driver of the getaway car and wrestled his gun away from him. Vertigan's valor was widely applauded in Phoenix police circles for allowing officers to quickly catch and arrest the men. Police Chief Harold Hurtt said that Vertigan "is one of the true heroes of our time. He realizes the officer is in trouble. Without regard for his own personal safety, he confronted these individuals, engaged in a gun battle. He put his life on the line for an officer."[9] In appreciation, the police union gave him a $500 reward and a certificate for a replacement gun.

Also, there is an inverse relationship between the rank of the officer and the degree to which law enforcement officials support rights of law-abiding citizens to carry concealed weapons for self-protection. The line officers,

[7] John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997).

[8] Private conversations with members of the Phoenix Police Department.

[9] Mark Shaffer, "True Hero" Helps Nab Trio: Security Guard Reacted to "What I Thought Was the Right Thing to Do," Ariz. Republic, March 28, 1999, at A1.

who spend the most time on the street and should be most threatened by the potential risk of additional permit holders, often express the greatest support for concealed carry laws.[10] In contrast, the highest-ranking, often politically appointed officers, whose lives are least threatened, are the most vocal opponents of the law. Survey results showed that 76 percent of street officers and 59 percent of managerial officers agreed that all trained, responsible adults should be able to obtain handgun carry permits.[11] Further evidence is provided by a 1976 poll by the Research Division of the Boston Police Department, which surveyed chiefs and high-ranking administrators throughout the nation. A slight majority of the respondents believed that law-abiding citizens should be able to carry firearms with them at all times for self-protection; the poll also found that lower-ranking officers are less supportive of gun control measures and more strongly support the right of citizens to bear arms than do their superiors.[12]

Law enforcement officers from states that switched to concealed carry laws provide additional evidence against the contention that such laws endanger their lives. Senior Corporal Glenn White, a patrol officer and president of the 2,350-member Dallas Police Association, lobbied against the law in 1993 and 1995 because he thought it would lead to wholesale armed conflict. However, that never happened. Said White, "All the horror stories I thought would come to pass didn't happen. No bogeyman. I think it's worked out well, and that says good things about the citizens who have permits. I'm a convert."[13] After the implementation of the Florida law, the president and the executive director of the Florida Chiefs of Police and the head of the Florida Sheriff's Association admitted that despite their best efforts to document problems arising from the law, they were unable to do so.[14] Consequently, they changed their views on the subject. Speaking on behalf of the Kentucky Chiefs of Police Association, Lieutenant Colonel Bill Dorsey, Covington assistant police chief, concluded after the law had been in effect for 9 months, "We haven't seen any cases where a [concealed carry] permit holder has committed an offense with a firearm."[15]

Many studies of lethal assaults against police have focused on a specific

[10] Private conversations police from various jurisdictions across the United States.

[11] John R. Lott, Jr., More Guns, Less Crime (1998), discussed the Gun-Control Survey, Law Enforcement Technology, July–August 1991, at 14–15.

[12] Don B. Kates, Jr., Handgun Prohibition and the Original Meaning of the Second Amendment, 82 Mich. L. Rev. 268, n.275 (1983).

[13] Scott Parks, Charges against Texans with Gun Permits Rise. Law's Supporters, Foes Disagree on Figures' Meaning, Dallas Morning News, December 23, 1997, at A1.

[14] Steve Patterson, Concealed-Weapons Law Opponents Still Searching for Ammunition, Florida Times-Union, May 9, 1988, at A1, A3.

[15] Kentucky State Police Trooper Jan Wuchner is also quoted as saying that he has "heard nothing around the state related to crime with a gun committed by permit holders. There has been nothing like that that I've been informed of." Terry Flynn, Gun-Toting Kentuckians Hold Their Fire, Cincinnati Enquirer, June 16, 1997, at A1.

time period,[16] geographic region,[17] or both.[18] If the chosen years or areas are not representative of the nation, their studies could suffer from sample selection bias.

Two recent studies exploited time-series data to examine police deaths. Robert Kaminski and Thomas Marvell, who studied police deaths during 1930–98, concluded that the two extreme peaks of fatal assaults were during Prohibition and the 1970s.[19] Lawrence Southwick, using a system of four equations to evaluate data from 1931–93, concluded that the likelihood of being murdered was positively related to the fraction of sworn officers that are male and negatively related to police wages.[20]

Only a few studies have examined the impact of changes in laws on police deaths, and none have examined gun laws. One study argued that the implementation of a three-strikes law increased lethal assaults against police by about 25 percent,[21] while others maintained that neither the provision nor the likelihood of capital punishment affects police killings.[22] Lott showed that the increased hiring of women police officers as a result of new hiring requirements significantly increased assaults on police officers.[23] The only

[16] A. P. Cardarelli, An Analysis of Police Killed by Criminal Action: 1961–1963, 59 J. Crim. L. Criminol. & Police Sci. 447 (1968), provided summary statistics for police deaths between 1961 and 1963. Kenneth C. Meyer *et al.*, Ambush-Related Assaults on Police: Violence at the Street Level (1986), examined all 35 ambush attacks against police from September 1972 to August 1973. Mitchell B. Chamlin, Conflict Theory and Police Killings, 10 Deviant Behavior 353 (1989), used state-level data for 1980–82 and concluded that the fractions of the population in the state that were poor, black, and Latino increased the rate of police officers killed.

[17] Mona Margarita, Killing the Police: Myths and Motives, 452 Annals Am. Acad. Pol. & Soc. Sci. 63 (1980), analyzed the criminal homicides of the New York City Police Department from 1844 to 1978. She concluded that contrary to popular opinion, police are rarely killed during domestic disturbances and are rarely senseless victims of madmen or lunatics. Instead, they are more likely to be killed by rational robbers. Samuel G. Chapman, Cops, Killers, and Staying Alive: The Murder of Police Officers in America (1986), provided summary statistics of the circumstances of 52 incidents in which 54 police officers from Oklahoma were murdered on duty.

[18] William A. Geller & Kevin J. Karales, Shootings of and by Chicago Police: Uncommon Crises. Part I: Shootings of Police. Shooting Correlates, 72 J. Crim. L. & Criminol. 1813 (1981). See also William A. Geller & Kevin J. Karales, Shootings of and by Chicago Police: Uncommon Crises. Part II: Shootings of Police. Shooting Correlates, 73 J. Crim. L. & Criminol. 331 (1982). This two-part series provided summary statistics of the circumstances surrounding all the Chicago police who were shot (including those not killed) between 1974 and 1978.

[19] Robert J. Kaminski & Thomas B. Marvell, An Analysis of Long-Term Trends in Killings of Police with a Comparison to General Homicides (Working paper, 2000).

[20] Southwick, *supra* note 1.

[21] Carlisle E. Moody, Thomas B. Marvell, & Robert J. Kaminski, Unintended Consequences: Three-Strikes Laws and the Killing of Police Officers (Working paper, 2000).

[22] William C. Bailey, Capital Punishment and Lethal Assaults against Police, 19 Criminology 608 (1982); William C. Bailey & Ruth D. Peterson, Murder, Capital Punishment, and Deterrence: A Review of the Evidence and an Examination of Police Killings, 50 J. Soc. Issues 53 (1994); William C. Bailey & Ruth D. Peterson, Police Killings and Capital Punishment: The Post-Furman Period, 25 Criminology 1 (1987).

[23] John R. Lott, Jr., Does a Helping Hand Put Others at Risk? Affirmative Action, Police Departments, and Crime, 38 Econ. Inquiry 239 (2000).

paper that examined the role of guns is by Southwick, who in a time-series analysis at the national level argued that the number of guns in civilian hands decreased the police fatality rate.[24]

This paper is the first to determine how changes in gun control laws affect felonious police deaths. Specifically, it examines the relationship between waiting periods and laws that allow law-abiding citizens to carry concealed weapons. I focus on these laws because they are two of the most frequently cited in the policy arena. The remaining portion of this paper is organized as follows. Section II outlines the data, describes the empirical model, and discusses the theoretical determinants of police deaths, focusing on the impact of concealed weapons laws and waiting periods. Section III presents the empirical results, and Section IV concludes the analysis.

II. Data and Empirical Model

The Federal Bureau of Investigation (FBI) provided the total number of officers feloniously killed and the number feloniously killed with handguns.[25] In its annual publication, the FBI summarizes each incident, provides detailed information about the victim and offender (if known), and explains the circumstances (date, time, location, type of weapons) and whether the incident has been cleared. The data are collected through the Uniform Crime Reporting program. Contributors submit preliminary data on any officer killed in the line of duty within their jurisdictions. When the national program receives notification of a line-of-duty death, it obtains additional details about the incident's circumstances from the victim officer's employing agency and gives the local agency information about the federal programs that provide benefits to survivors of nonfederal law enforcement officers killed in the line of duty.[26]

A. Trends in Felonious Police Deaths

Figure 1 shows the total number of police deaths and deaths from handgun use from the period 1977–96. Both numbers generally decrease from the late 1970s through the mid-1980s and remain relatively constant for the following years, when there was a large increase in the number of states with shall-issue laws.

Figure 2 displays the total felonious police death rates (per million people) for states with and without shall-issue laws, and Figure 3 shows the same

[24] Southwick, *supra* note 1, at 596–97.

[25] U.S. Department of Justice, Uniform Crime Reports: Law Enforcement Officers Killed and Assaulted (1977–97). The report also showed that the number of accidental deaths, which are excluded from felonious police deaths, ranged from a low of 47 (1996) to a high of 79 (1989) between 1988 and 1997. The general long-term trend in accidental police deaths is similar to the trend in accidental deaths for the entire nation, which has decreased steadily.

[26] For more information about the details and procedures of the program, see note 25 *supra*.

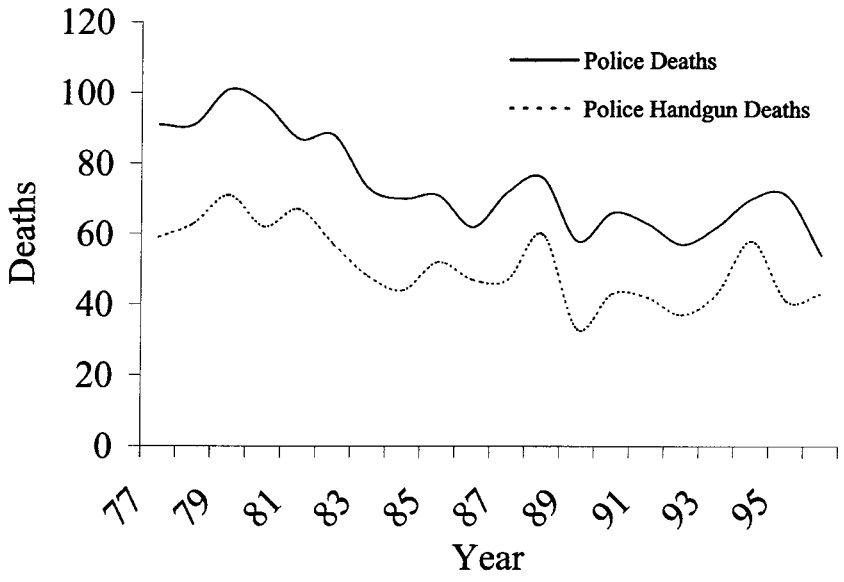

FIGURE 1.—Police deaths, 1977–96

rate for states with and without waiting periods.[27] States that allow people to carry concealed weapons have lower police death rates for 12 of the 20 years.[28] The rates in shall-issue states are generally lower early in the sample, are higher for a few years in the middle of the sample, and are similar in the 1990s. The average death rate is lower in shall-issue states (.28 compared with .31). Figure 3 shows that states with waiting periods have lower felonious police death rates than states without such laws in 14 of the 20 years and lower average rates (.26 vs. .34).

B. Empirical Model

To determine the impact of gun laws on felonious police deaths and to control for other variables that affect these deaths, I model annual police deaths at the state level as a determinant of many characteristics:

$$y_{it} = \alpha + \beta_1 \log(\mathrm{POP}_{it}) + \beta_2 \log(\mathrm{FTEP}_{it}) + \beta_3 \mathrm{LAWS}_{it} + \beta_4 \mathrm{POL}_{it} + \beta_5 \mathrm{CRIME}_{it} + \beta_6 X_{it} + T_t + S_i + \varepsilon_{it}. \quad (1)$$

[27] The rates in Figures 2 and 3 include the data for each state classified as having either a shall-issue or waiting period law in the given year. Therefore, the lines reflect changing compositions of states over time.

[28] Shall-issue states also have lower handgun death rates for 11 of the 20 years.

The dependent variable, y_{it}, is one of six variables that measure police deaths: whether a state has a police death (and death by handgun), total police deaths per 1,000,000 persons (and deaths from a handgun), and total police deaths per 100,000 police officers.

At the most basic level, some fraction (f) of state i's population at time t (POP_{it}) may kill a police officer ($FTEP_{it}$).[29] Therefore, the expected number of felonious police deaths is $f \times POP_{it} \times FTEP_{it}$, which is log linear in population and the number of full-time-equivalent police officers, as shown in equation (1).

The variable $LAWS_{it}$ indicates the status of state i's right-to-carry and waiting period legislation at time t. It includes dummy variables that indicate whether the state has a shall-issue[30] law or a waiting period.[31] However, most specifications will not use the simple dummy variable but instead will use trends that measure the before and after periods from when the shall-issue and waiting period laws went into effect. If allowing law-abiding citizens to carry concealed weapons raises the cost of attacking others, such laws could reduce the number of violent offenders and violent encounters, which might in turn reduce police deaths.[32] Furthermore, if criminals are less likely to use

[29] Police officers are measured in full-time-equivalency units.

[30] The shall-issue dates are taken from Clayton E. Cramer & David B. Kopel, "Shall Issue": The New Wave of Concealed Handgun Permit Laws, 62 Tenn. L. Rev. 679 (1995). Eight states (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington) have had shall-issue laws since 1977. Maine became shall issue in 1985, followed by Florida (1987), Virginia (1988), Georgia (1989), Pennsylvania (1989), West Virginia (1989), Idaho (1990), Mississippi (1991), Oregon (1990), Montana (1991), Alaska (1995), Arizona (1995), Tennessee (1995), Wyoming (1995), Arkansas (1996), Kentucky (1996), Louisiana (1996), Nevada (1995), North Carolina (1996), Oklahoma (1996), South Carolina (1996), Texas (1996), and Utah (1996). There is some discussion about the accuracy of the Maine and Virginia dates. Maine passed a series of laws relating to concealed carry. To test whether classifying Maine and Virginia in this manner affected the results, I ran additional regressions that used the other potential dates. Lott & Mustard, *supra* note 7, used the 1985 date for Maine because Cramer & Kopel, *supra,* indicated it was the best date to use when classifying the changes in laws as binary variables. Virginia presents some confusion because the counties near Washington, D.C., refused to grant permits even after the shall-issue law was passed. Consequently, the laws were strengthened over time, and citizens filed complaints against these counties to force them to obey the law. The results of the empirical work in this paper were robust to using the other possible enactment dates for shall-issue laws.

[31] The waiting period dates are taken from Lott, *supra* note 11. A state was defined as having a waiting period when either a state or federal waiting period applied. When the Brady Bill was passed, states that did not have their own waiting period and did not have instant-check capacity became subject to the federal period. Eleven jurisdictions (Alabama, California, Washington, D.C., Maryland, Massachusetts, Minnesota, New Jersey, New York, Pennsylvania, Rhode Island, and Washington) had waiting periods since 1977. The other states adopted waiting periods in the following years: Alaska (1994), Arkansas (1994), Connecticut (1994), Georgia (1994), Hawaii (1988), Indiana (1983), Iowa (1979), Kansas (1994), Kentucky (1994), Louisiana (1994), Maine (1994), Maryland (1979), Michigan (1994), Mississippi (1994), Montana (1994), Nebraska (1994), Nevada (1994), New Mexico (1994), North Dakota (1994), Ohio (1994), Oklahoma (1994), Oregon (1989), South Dakota (1994), Tennessee (1994), Texas (1994), Vermont (1994), West Virginia (1994), and Wyoming (1994).

[32] Lott & Mustard, *supra* note 7.

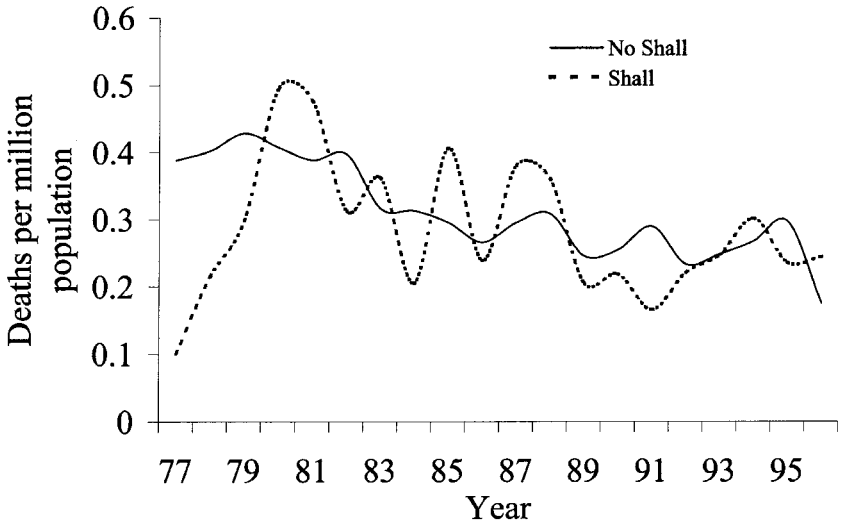

FIGURE 2.—Police death rates by shall-issue status

guns after the passage of right-to-carry laws, these laws could further reduce police deaths.[33] In contrast, if right-to-carry laws lead to greater uses of guns against officers, such laws could increase police deaths. If the existence and length of waiting periods reduce the number of guns obtained by those who should not have them, waiting periods should reduce the likelihood that people will use them against police. However, if waiting periods make it more costly for people to use the guns for self-defense, the underlying level of violence may increase, including violence toward police officers. Therefore, because both shall-issue laws and waiting periods could increase or decrease police deaths, the expected total effect is uncertain.

The variable POL_{it} includes direct public expenditures and police salaries per full-time-equivalent police officer.[34] Expenditures proxy for officer train-

[33] David E. Olson & Michael Maltz, Magic Bullets, Deterrence, and Gun Laws (Working paper, 2000), argued that after shall-issue laws were passed, the relative share of murders that result from guns fell, while the relative share of nongun murders rose.

[34] U.S. Department of Justice, Justice Expenditure and Employment Extracts (1984–95). Expenditures and payroll data are expressed in terms of real 1983 dollars. In private conversations with Sue Lindgren of the Bureau of Justice Statistics (BJS), she stated that the BJS has not made public the data for 1987, 1989, and 1996. To obtain data for 1987 and 1989, I averaged the preceding and following year figures. The 1996 data are unavailable because the BJS is currently transforming the way it obtains employment, expenditure, and payroll information. Historically, it gathered this information in October of a given year, but changed its collection date to April. The last year of October collection was 1995, and BJS waited until April 1997 to collect the data again, thus skipping 1996. For 1996 data, I extrapolate the growth rate between 1994 and 1995 and apply that to 1995–96.

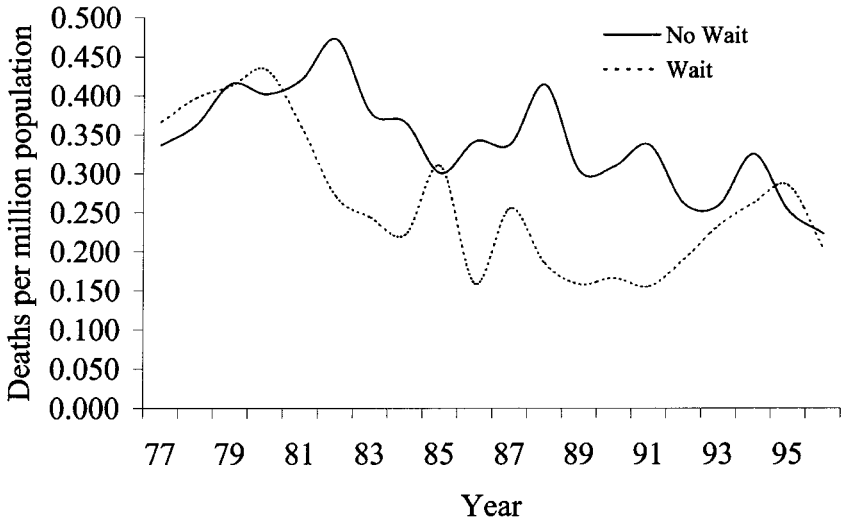

FIGURE 3.—Police death rates by waiting period status

ing and technological investments in the police force such as protective armor and the type of weapons they carry, which should decrease the likelihood of being killed. The anticipated coefficient on the payroll variable is less clear. Because higher-paid police typically spend less time in situations that could be life threatening, higher police pay may imply that there are fewer officers who potentially face dangerous situations. However, payroll information also proxies the age and experience of police officers, which may be related to their ability to diffuse potentially dangerous situations. Further complicating the effect is that if police who patrol in higher-risk areas must be paid compensating wages, the relationship between payrolls and deaths would be positive.[35] Consequently, the expected effect is ambiguous.

The term $CRIME_{it}$ contains four crime-related variables for state *i* at time *t*—violent and property crimes and violent and property arrests, all of which are per full-time-equivalent police officer.[36] The FBI defines these crime

[35] See Southwick, *supra* note 1.

[36] Police deaths may be a function of other types of arrests, rather than just arrests for the Index I crimes. When police deaths are discussed, arrests for four other crimes receive prominent attention: drugs, intoxication, family crimes, and weapons offenses. Because the FBI has only state-level data on these arrests for 6 years, I do not report the results. The unreported regressions using the 6-year sample with the additional arrest variables show that the number of family crime arrests positively affects the death rate, while the other arrests are not statistically different from zero.

categories.[37] Police should be more at risk when there are many crimes and arrests per police officer.

The term X_{it} is a vector of population and income control variables that includes the percent of the population that is male, black, and neither white nor black, the fraction of the population in various age cohorts, personal income, unemployment transfers, income maintenance, and retirement transfers. These variables control for differences across demographic and socioeconomic groups in the way these groups interact with and relate to police. The specifications reported in the paper express many of the variables in terms of full-time-equivalent police officers. To test the robustness of the results, I used a variety of other measures of these variables.[38] All variables measured in dollar figures are real per capita measures, denominated in 1983 dollars. The terms T_t and S_i are vectors of time and state fixed effects that control for differences over time and across states, respectively.

Table 1 shows the summary statistics.[39] The data are state level and include 51 observations (50 states and Washington, D.C.) per year for 13 years (1984–96), for a total of 663 observations.[40] I begin in 1984, because the changes in right-to-carry laws generally occur after 1984 and 1984 is the first year the FBI provides state-level arrest data.

III. Results

A. The Likelihood of a Having a Felonious Police Death

Because felonious police deaths occur infrequently,[41] it is initially important to determine what types of states are more likely to experience a police death. Table 2 shows the fixed-effects[42] logit regression results that indicate the likelihood that a state has a police death. In the first two columns, dummy variables for the state laws are used to estimate the average effect before

[37] Violent crime is the sum of murder, rape, robbery, and aggravated assault. Property crime is the sum of burglary, larceny, and auto theft.

[38] The general pattern of results was robust to the inclusion of alternative measures of some of the variables such as full-time-equivalent officers per population, full-time-equivalent officers per square mile, number of arrests and offenses, and price-adjusted level of police expenditures and payrolls.

[39] All dollar values are expressed in terms of real 1983 dollars.

[40] I use state-level data, because there are very few felonious police deaths and most state-level observations are zero. Therefore, nearly all the county-level observations would be zero and there would be almost no variation in the data.

[41] Table 1 shows that 52 percent of states have a police death and only 44 percent have a police death as a result of a handgun in a given year.

[42] The state population and the coefficient estimate on the state fixed effects have a correlation coefficient of between .3 and .4, depending on the specification. When the states with more than 10 million people are excluded, the correlation drops by half. The District of Columbia, Georgia, New York, and Texas typically have the largest positive coefficient estimates, while Delaware, Iowa, Minnesota, Oregon, Vermont, Washington, and Wisconsin generally have the most negative coefficient estimates.

TABLE 1

SUMMARY STATISTICS

Variable	Mean	Standard Deviation	Minimum	Maximum
Police death variables:				
Total deaths	1.28	1.83	0	12
Handgun deaths	.89	1.38	0	10
Deaths in state	.52	.50	0	1
Deaths by handgun in state	.44	.50	0	1
Deaths per million people	.03	.05	0	.51
Handgun deaths per million people	.02	.04	0	.51
Deaths per 100,000 FTE officers	10.69	16.82	0	132.63
Handgun deaths per 100,000 FTE officers	6.84	12.06	0	101.21
Police variables:				
FTE police employment	13,617	16,388	1,082	93,675
Real expenditure (thousands) per FTE officer	41.31	11.43	21.65	86.67
Real pay (thousands) per FTE officer	2.43	.64	1.32	4.93
Gun law variables:				
Presence of shall-issue law	.30	.46	0	1
Presence of waiting period	.39	.48	0	1
Crime and arrest variables (per FTE police officer):				
Violent crime	1.89	.85	.24	4.04
Property crime	17.34	5.11	7.53	97.06
Violent crime arrests	.63	.31	.05	2.22
Property crime arrests	2.66	1.05	.28	6.55
Population variables:				
Population (millions)	4.9	5.3	.454	31.8
Population per square mile	354.44	1,353.44	.96	10,372
Percent female	51.06	.95	47.17	53.68
Percent black	10.79	12.04	.26	68.34
Percent of population <9 years old	14.85	1.62	10.61	23.54
Percent of population 10–19 years old	14.58	1.41	9.48	20.14
Percent of population 20–29 years old	15.92	1.88	11.85	22.66
Percent of population 30–39 years old	16.49	1.29	13.39	22.35
Percent of population 40–49 years old	12.64	1.64	8.64	17.73
Percent of population 50–64 years old	13.22	1.02	9.36	16.13
Percent of population 65+ years old	12.33	2.13	2.99	18.54
Percent black	10.79	12.04	.26	68.34
Percent white	83.15	14.54	27.44	99.07
Percent neither white nor black	6.07	10.49	.44	67.47
Real per capita variables:				
Personal income (dollars)	13,647	2,350	8,704	21,808
Income maintenance	174.35	64.38	55.70	472.12
Unemployment compensation	63.40	37.65	10.73	282.35
Retirement compensation	160.50	55.66	107.25	547.99

NOTE.—The base year for all dollar denominated variables is 1983. FTE = full-time-equivalent. For violent crime and property crime arrests, $N = 638$. For all other arrests, $N = 663$.

TABLE 2

LOGIT REGRESSIONS

Variable	Felonious Police Death (1)	Felonious Police Handgun Death (2)	Felonious Police Death (3)	Felonious Police Handgun Death (4)
Shall-issue dummy	.525 (.533) [.015]	.488 (.539) [.019]		
Time trend for years before shall-issue law			.049 (.098) [.009]	.047 (.096) [−.006]
Time trend for years after shall-issue law			−.311* (.133) [−.010]	−.168 (.134) [−.001]
Waiting period dummy	.140 (.538) [.010]	−.254 (.571) [−.042]		
Time trend for years before waiting period			−.048 (.074) [−.013]	−.117 (.078) [−.013]
Time trend for years after waiting period			−.167 (.106) [−.021]	−.102 (.113) [−.010]
F-statistic for differences in shall-issue variables			5.37*	1.86
Probability > *F* for shall-issue before/after variables			.021	.173
Test for differences in waiting period variables			1.03	.01
Probability > *F* for waiting before/after variables			.310	.904
Time fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
χ^2	26.84	30.02	34.39	33.48

NOTE.—The first number in each row is the coefficient estimate from the logit regression. The second number (in parentheses) is the standard error. The third number (in square brackets) is the estimate of the marginal probability evaluated at the mean for each variable.

* Significant at the 5 percent level.

and after the law. The last two columns use before and after trends to more accurately estimate the impacts of the laws. Column 1 shows that the coefficient estimates on both the shall-issue and waiting period variables are positive and insignificant. Table 2 displays only the results for the gun laws, the primary variables of interest. (The results for the other variables are listed in the Appendix, in Table A1.)[43] The results for handgun deaths in column 2 show that the shall-issue dummy estimate is slightly smaller and the impact of the waiting period is negative. However, both of the coefficient estimates are smaller than their respective standard errors.

The first two columns are biased because they use only the dummy variables for the laws and ignore the trends before and after the law went into effect.[44] If the death rate increased significantly before the law and decreased slightly afterward, the average rate after the law could be higher than before, but the law would still have lowered deaths. The last two columns control for this problem by replicating the results from the first two regressions and replacing the dummy variables with before and after trends for the laws. Both the qualitative and quantitative results for the two latter columns are very similar to the results from the first two columns for the majority of variables, but results on the gun laws change dramatically. The before trends for the shall-issue variable are positive, while the corresponding after trends are negative. The after trend for the shall-issue law is statistically significant at the .05 level. Consequently, when I control for the bias generated by the dummy variables, right-to-carry laws reduce the likelihood that a state will have a felonious police death. The waiting period variables are negative both before and after the change, but neither is significant. Further evidence of the impact of gun laws is shown in testing whether the before and after trend variables differ from each other. These tests indicate that the increasing trend in felonious police deaths before the shall-issue laws are passed is significantly different from the decreasing trend after the law was passed. There is no such difference in the waiting period trends. The coefficients on the gun laws for police deaths by handgun (column 4) have the same sign but are no longer statistically significant.

The basic results of the gun laws were robust to many specification changes in the police variables[45] and the waiting period.[46] However, the effect of

[43] The estimates for the population and full-time-equivalent police officers are both positive, as expected, with the latter being statistically significant at the .10 level. States with more officers are significantly more likely to have an officer killed. The percent of the population that is aged 10–19, 30–39, 40–49, over 65, and black also are significant among the control variables.

[44] Lott, *supra* note 11.

[45] The results for the gun law variables in all the tables are robust to using alternative specifications of the arrest, crime, expenditure, and pay variables (measuring them in totals or totals per population instead of fractions per full-time-equivalent police).

[46] I reran columns 1 and 2, replacing the length of the waiting period in days and the length

concealed carry laws to reduce the likelihood that a state will have a police death is understated in Table 2, which uses unweighted regressions. Since Isaac Ehrlich,[47] who pioneered regression analysis of crime data, crime-rate regressions have typically weighted the results by population size, because unweighted estimates produce heteroskedasticity whereby the magnitude of the error terms is inversely correlated with the population size. Low-population states exhibit greater variance in police death rates because they have relatively low rates, and small changes in the number of deaths generate large percentage changes. Weighted regressions provide stronger support for the assertion that right-to-carry laws lower the likelihood that a state will have a felonious police death. For example, when the regression in column 3 of Table 2 is run using weights, the shall-issue before trend increases from .049 to .166 and the shall-issue after trend changes from −.311 to −.356. Consequently, the difference between the trends increases and the after effect is more negative. In contrast, the waiting period trends are relatively unaffected when weights are used.[48] Because this difference is so sharp for the concealed carry effect, the remaining results report both results.

B. Felonious Police Death Rates—Tobit Regressions

To analyze felonious police death rates, I use Tobit and Poisson regressions, because the dependent variable is censored at zero and conventional regression methods bias the results because they fail to account for the qualitative difference between the zero observations and continuous observations. The Tobit method has been used to address similar problems like whether right-to-carry laws reduce mass public shootings.[49] Table 3 presents the Tobit results, showing both the weighted and unweighted results for each specification.[50] Columns 1–4 measure death rates by felonious police deaths per million residents, and columns 5–8 measure death rates per the number of full-time-equivalent police officers. Columns 1 and 2 and 5 and 6 display results for total police deaths, and the other columns show the results for handgun death rates.

Column 1 shows that states that enact both concealed carry laws and waiting periods have statistically significantly lower total felonious police

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of the period squared, for the waiting period dummy variable. The coefficients for these variables were not statistically significant in either case.

[47] Isaac Ehrlich, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, 81 J. Pol. Econ. 545–46 (1973).

[48] The coefficient estimate for the waiting period before variable changed from −.048 to −.010, and the estimate on the after variable changed from −.167 to −.162.

[49] John R. Lott, Jr., & William M. Landes, Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws (Working Paper No. 73, John M. Olin Program L. & Econ., Univ. Chicago 1999).

[50] The regressions in Tables 3 and 4 use the same population and income control variables as the Table 2 regressions but do not report the results for these variables. The Tobit regressions in Table 3 are run with a lower limit for left censoring.

TABLE 3

TOBIT REGRESSIONS

Variables	Total Deaths per Million People (1)	Total Deaths per Million People (2)	Handgun Deaths per Million People (3)	Handgun Deaths per Million People (4)	Total Deaths per FTE Police (5)	Total Deaths per FTE Police (6)	Handgun Deaths per FTE Police (7)	Handgun Deaths per FTE Police (8)
Time trend for years before shall-issue law	−.001	.0003	−.002	$1.8\ e^{-5}$	.003	.336	−.202	.254
	(.003)	(.0015)	(.003)	(.001)	(1.027)	(.568)	(.842)	(.444)
Time trend for years after shall-issue law	−.006+	−.004*	−.003	−.002	−2.354+	−1.482*	−1.002	−.756
	(.004)	(.002)	(.003)	(.001)	(1.280)	(.638)	(1.041)	(.494)
Time trend for years before waiting period	−.003	−.001	−.003+	−.001	−1.140	−.532	−1.346+	−.511
	(.002)	(.001)	(.002)	(.001)	(.771)	(.416)	(.658)	(.324)
Time trend for years after waiting period	−.006+	−.003+	−.004	−.001	−2.123+	−1.019	−1.172	−.374
	(.003)	(.002)	(.003)	(.002)	(1.245)	(.070)	(1.036)	(.550)
Constant	.651	4.329	1.304	2.634	−494.897	1,047.581	−296.501	440.364
	(3.110)	(2.145)	(2.873)	(1.778)	(1,093.70)	(801.087)	(927.030)	(629.814)
F-statistic for differences in shall-issue variables	1.72	3.41	.07	.83	2.39	5.20	.41	2.86
Probability > *F* for shall-issue before/after variables	.190	.066+	.797	.361	.123	.023*	.521	.098+
Test for differences in waiting period variables	.92	.97	.01	.03	.51	.39	.02	.05
Probability > *F* for waiting before/after variables	.337	.326	.922	.856	.477	.530	.881	.822
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted by population	No	Yes	No	Yes	No	Yes	No	Yes

NOTE.—The first number in each row is the coefficient estimate from the regression. The second number (in parentheses) is the standard error. The same income and population control variables used in Table 2 were included in these regressions but are not reported. FTE = full-time-equivalent.

+ Significant at the 10 percent level.

* Significant at the 5 percent level.

death rates after the law was passed. The probabilities that there are differences in the shall-issue and waiting period before and after trends are .19 and .34, respectively. However, when the weighted regressions are used for this specification in column 2, the concealed carry results become stronger and the waiting period results are relatively unaffected. The shall-issue before estimate is large and positive, the shall-issue after estimate is negative and statistically significant at the .05 level, and the *F*-test for their difference is significant. In contrast, in the weighted regressions, the after trend for the waiting period is cut in half and the *F*-test for their difference remains insignificant.

Column 3, which examines felonious handgun deaths, shows the same signs of the gun law coefficients, but the results are slightly weaker. The only statistically significant result is that states with waiting periods have lower death rates prior to the enactment of the laws. Column 4, a weighted regression of column 3, shows no statistically significant results.

Column 5 uses the ratio of handgun deaths per police officer as a dependent variable. States with concealed weapons laws are more likely to have high felonious death rates before the law is implemented and low felonious death rates after the law is implemented, a result significant at .10. The waiting period trends are both negative, and the after effect is statistically significant at .10. The probability of rejecting the *F*-test for the differences in the shall-issue and waiting period variables is .12 and .48, respectively. In column 6, when the column 5 regressions are rerun with weights, the right-to-carry results are made stronger and the waiting period results are substantially mitigated. The shall-issue before trend is much more positive, the shall-issue after trend continues to be negative and significant, and the before and after differences are significant at .02. In contrast, the waiting period after trend is no longer significant.

Column 7 measures handgun deaths per police officer and uses unweighted regressions. The before and after trends for both variables are negative, but only the before waiting period trend is statistically significant. The weighted results in column 8 once again strengthen the results of the concealed carry laws as the shall-issue before trend is positive, the shall-issue after trend is negative, and the difference between the before and after trends is significant. Neither of the waiting period trends is significant, and they do not differ from each other.

All eight specifications in Table 3 strongly reject the contention that concealed carry laws increase the felonious death rate of police officers. Instead, states that implement shall-issue laws generally have slightly higher death rates before the laws are implemented and slightly lower rates after the laws are implemented, and the after trend is statistically significant in half the specifications. Furthermore, the before and after trends in the shall-issue variable are statistically different in three specifications. Concealed carry laws certainly do not jeopardize the lives of police officers, and there is

moderate evidence that passing such laws likely saves officers' lives. In contrast, the before and after trends for waiting periods are negative in every specification, with the after trend being significant in these specifications. However, the before and after trends for waiting periods never differ from each other.

C. Felonious Police Death Rates—Poisson Regressions

Florenz Plassmann and Nicolaus Tideman argued that a generalized Poisson process is even more appropriate for count data with a low number of instances per observation,[51] which is clearly the case in this situation. Table 4 examines the robustness of the Table 3 results by using the Poisson process to estimate all eight regressions. In the first four columns, which measure total and handgun deaths as a fraction of the states' populations, there are no statistically significant results. The coefficient estimates for both the before and after waiting period trends are always negative. The before trend for the shall-issue variables is positive for two specifications and negative for two. The after trend for the shall-issue variables is always negative.

Columns 5–8 measure the death rates per full-time-equivalent police officers. In every specification, the time trends before and after the waiting period and after the shall-issue law are negative and statistically significant. The before and after shall-issue trends are different in two of the specifications, and the before and after trends for the waiting period differ in one specification.

One concern about using the Poisson model is that it assumes that the mean of the dependent variable is equal to the variance, which is not true in this case (see Table 1). To determine whether the results are affected by this assumption, I used the negative binomial model, which relaxes this restriction. The qualitative results are robust to this alternative specification.[52]

IV. Conclusions

This is the first study to examine how felonious police deaths are affected by changes in waiting periods and laws that allow law-abiding citizens the right to carry concealed weapons for self-defense. Although some people oppose shall-issue laws because they believe the laws endanger the officers'

[51] Florenz Plassmann & T. Nicolaus Tideman, Geographical and Temporal Variations in the Effects of Right-to-Carry Laws on Crime (Working Paper No. 9910, State Univ. New York Binghamton, Dep't Econ. 1999).

[52] For regressions of the total death rate per population, the coefficient estimate on the shall-issue before trend is positive (.021) and is negative on the shall-issue after trend (−.105). Both the waiting period before (−.033) and after (−.042) trends are negative. For regressions of the rate of deaths due to handguns per population, the coefficient estimate on the shall-issue before trend is positive (.097) and is negative on the shall-issue after trend (−.105). Both the waiting period before (−.055) and after (−.057) trends are negative. None of the four coefficient estimates are significant at the .10 level.

TABLE 4

POISSON REGRESSIONS

Variables	Total Deaths per Million People (1)	Total Deaths per Million People (2)	Handgun Deaths per Million People (3)	Handgun Deaths per Million People (4)	Total Deaths per FTE Police (5)	Total Deaths per FTE Police (6)	Handgun Deaths per FTE Police (7)	Handgun Deaths per FTE Police (8)
Time trend for years before shall-issue law	.005	.003	−.015	−.007	.003	.004	−.012	−.006
	(.222)	(.220)	(.274)	(.270)	(.011)	(.011)	(.014)	(.014)
Time trend for years after shall-issue law	−.109	−.083	−.039	−.031	−.103**	−.086**	−.049*	−.040*
	(.315)	(.305)	(.379)	(.369)	(.016)	(.016)	(.020)	(.019)
Time trend for years before waiting period	−.046	−.047	−.075	−.058	−.052**	−.048**	−.083**	−.056**
	(.180)	(.176)	(.231)	(.224)	(.009)	(.009)	(.012)	(.012)
Time trend for years after waiting period	−.072	−.045	−.041	−.023	−.086**	−.054**	−.061**	−.036^{+}
	(.306)	(.305)	(.398)	(.396)	(.016)	(.016)	(.020)	(.020)
F-statistic for differences in shall-issue variables	.11	.06	.00	.00	34.79**	25.58**	2.60	2.37
Probability > *F* for shall-issue before/after variables	.743	.799	.955	.955	.000	.000	.107	.124
Test for differences in waiting period variables	.01	.00	.01	.01	3.95*	.13	.94	1.13
Probability > *F* for waiting before/after variables	.938	.995	.938	.937	.047	.722	.332	.287
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighting by population	No	Yes	No	Yes	No	Yes	No	Yes

NOTE.—The first number in each row is the coefficient estimate from the regression. The second number (in parentheses) is the standard error. The same income and population control variables used in Table 2 were included in these regressions but are not reported. FTE = full-time-equivalent.

$^{+}$ Significant at the 10 percent level.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

lives, there is no evidence for that belief. After controlling for an array of factors, including trends before and after the law went into effect, I show that states that enact concealed carry laws are less likely to have a felonious police death and more likely to have lower rates of felonious police deaths after the law is passed. This result is statistically significant in seven of the nine specifications, and the difference between the before and after trends is significant in over half the specifications. Although point estimates show that the same qualitative results are true for felonious police deaths due to handguns, the results are statistically significant in only about one-quarter of the specifications. Also, the before and after trends in the shall-issue variable are statistically different in about half of the specifications. Furthermore, those who believe allowing private citizens to carry concealed weapons will endanger the lives of law enforcement officials do not even have anecdotal evidence to support their position. To date, we have no examples of law-abiding citizens with concealed weapons permits assaulting police officers. In contrast, there is at least one example of such a citizen coming to the aid of an officer.

States that implement waiting periods typically have slightly lower probabilities of having a felonious police death and slightly lower death rates of law enforcement officials before the law is implemented. After the implementation of the law, the states continue to experience lower felonious death rates. However, the before and after trends for waiting periods are rarely different from each other.

These results are robust across different estimation procedures using logit, Tobit, Poisson, and negative binomial regressions. Finally, this paper confirms that using only a dummy variable to show the average before and after effects of laws can substantially bias the results and including time trends before and after the passage of the law can correct this bias.

APPENDIX

TABLE A1

Control Variables Not Reported in the Table 2 Logit Regression

Variables	Felonious Police Death (1)	Felonious Police Handgun Death (2)	Felonious Police Death (3)	Felonious Police Handgun Death (4)
log(population)	1.054 (4.704) [.026]	−.560 (4.858) [−.159]	1.361 (4.769) [.036]	−.289 (4.902) [−.142]
log(FTE police)	6.244^{+} (3.721) [.260]	7.135** (3.717) [.455]	6.046^{+} (3.467) [.268]	6.236^{+} (3.772) [.449]
Pay per FTE police	.026 (1.101) [−.166]	1.248 (1.167) [.165]	.080 (1.120) [.175]	1.139 (1.171) [.147]
Expenditures per FTE police	−.004 (.057) [−.006]	−.011 (.058) [.003]	−.011 (.059) [.008]	−.014 (.059) [.003]
Violent arrests per FTE police	1.180 (.989) [.101]	−.470 (1.010) [−.007]	.692 (.960) [.093]	−.804 (1.030) [−.015]
Property arrests per FTE police	−.017 (.316) [−.042]	.382 (.348) [.016]	.356 (.320) [.038]	.433 (.352) [.013]
Violent crimes per FTE police	−.282 (.661) [−.061]	−.195 (.640) [−.098]	−.521 (.661) [−.047]	−.345 (.641) [−.094]
Property crimes per FTE police	−.017 (.119) [−.001]	.038 (.100) [.005]	.007 (.121) [.001]	.044 (.106) [.005]
Real per capita personal income	.0000 (.0003)	−.0000 (.0004)	−.0001 (.0003)	−.0001 (.0004)
Real per capita income maintenance	−.0005 (.0089)	.001 (.008)	.001 (.009)	.003 (.008)
Real per capita unemployment compensation	.004 (.007)	.004 (.007)	.005 (.007)	.004 (.007)
Real per capita retirement compensation	.009 (.017)	.021 (.021)	.005 (.017)	.015 (.020)
Percent of population 10–19 years old	.554^{+} (.340)	.577 (.394)	.498 (.364)	.681 (.442)
Percent of population 20–29 years old	.381 (.425)	.357 (.486)	.688 (.473)	.627 (.535)
Percent of population 30–39 years old	1.632* (.766)	1.610* (.808)	1.728* (.770)	1.694* (.800)
Percent of population 40–49 years old	1.195^{+} (.726)	1.230^{+} (.743)	1.541* (.781)	1.319^{+} (.791)
Percent of population 50–64 years old	.818 (.757)	1.212 (.782)	.704 (.777)	1.163 (.805)
Percent of population 65+ years old	1.319^{+} (.721)	1.900* (.821)	1.248^{+} (.750)	1.955* (.860)
Percent of population that is female	−2.439 (1.852)	−3.078 (2.132)	−1.910 (1.841)	−2.697 (2.126)
Percent of population that is black	−.682^{+} (.409)	−.662 (.427)	−.727^{+} (.417)	−.628 (.429)
Time fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
χ^2	26.84	30.02	34.39	33.48

Note.—The first number in each row is the coefficient estimate from the logit regression. The second number (in parentheses) is the standard error. The third number (in brackets) is the marginal effect of the variable, calculated from incremental changes from the mean. FTE = full-time-equivalent.

$^{+}$ Significant at the 10 percent level.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

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