More Guns, More Crime

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This paper examines the relationship between gun ownership and crime. Previous research has suffered from a lack of reliable data on gun ownership. I exploit a unique data set to reliably estimate annual rates of gun ownership at both the state and the county levels during the past two decades. My findings demonstrate that changes in gun ownership are significantly positively related to changes in the homicide rate, with this relationship driven almost entirely by an impact of gun ownership on murders in which a gun is used. The effect of gun ownership on all other crime categories is much less marked. Recent reductions in the fraction of households owning a gun can explain one-third of the differential decline in gun homicides relative to nongun homicides since 1993.

I. Introduction

Do changes in gun ownership influence the crime rate? Although guns are involved in nearly 70 percent of all homicides and a substantial share of other violent crimes, the direction of this relationship is theoretically ambiguous. For example, if guns increase the likelihood that any particular dispute will result in an individual's death, then increases

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in gun ownership will serve to increase the number of homicides. Alternatively, if criminals are deterred from committing crimes when potential victims are more likely to possess a firearm, then more gun ownership may lead to a reduction in criminal activity.

Until recently, empirical work that attempted to answer this question typically took one of two approaches. In the first, researchers estimated the effect that changes in the total stock of guns in the United States had on the nation's crime rate (Kleck 1984; Magaddino and Medoff 1984). A more developed branch of studies estimated the level of gun ownership in a region, state, or city and then explored whether crime and gun ownership were significantly related (Cook 1982; Kleck and Patterson 1993). The results of these studies were mixed, with some implying that guns increased the amount of criminal activity and others finding the opposite.

Both types of studies had important limitations. The time-series analyses that used annual, national-level data were limited because of the small number of observations that could be used for estimating the relationship between gun ownership and crime. Furthermore, the level of aggregation prevented researchers from examining whether the relationship held within smaller geographic areas, or whether instead gun ownership was changing in one region of the country while criminal activity was changing in another. The cross-sectional studies had two weaknesses. First, because reliable data on gun ownership were available at only the national level, researchers constructed proxies, such as the fraction of crimes committed with a gun, to estimate the level of gun ownership in an area. It is not clear, however, if these proxies accurately captured differences in gun ownership across areas. More important, any significant statistical relationship between guns and crime could have been driven by reverse causation or omitted variables.

The main impediment to applied work in this area was the absence of a reliable measure of gun ownership that could be measured across geographic areas over time. In this paper I propose a new way to measure gun ownership at both the state and county levels on an annual basis. Specifically, I argue that state- and county-level sales data for one of the nation's largest gun magazines, *Guns & Ammo,* provide a much more accurate way to measure both the level and the change in gun ownership within an area.

I use several methods to test the validity of this new proxy variable. First, I show that sales rates of gun magazines are significantly higher in counties with average individual-level characteristics similar to those of the average gun owner. Second, I use death data from the National Center for Health Statistics (NCHS) to show that there is approximately a one-for-one relationship between sales rates and the death rate from gun accidents. Third, using gun show data from publications of the

National Rifle Association (NRA), I show that the number of gun shows per capita is significantly positively related to the sales rate of this magazine. Fourth, using annual state-level data on NRA membership, I demonstrate that sales of *Guns & Ammo* are significantly positively related to the level of and changes in NRA membership rates. And finally, I use data from the General Social Survey (GSS) to show that state-level estimates of gun ownership are significantly positively related to sales rates of gun magazines and that this proxy also captures variation within a state over time in rates of gun ownership. While none of these tests individually proves that this magazine is a sufficiently accurate proxy variable, taken together they suggest that this panel data set represents the richest one ever assembled for measuring gun ownership.

Having demonstrated the validity of this proxy variable, I next use these data to examine the dynamic relationship between gun ownership and crime. My findings reveal that changes in homicide and gun ownership are significantly positively related. This relationship is almost entirely driven by the relationship between lagged changes in gun ownership and current changes in homicide, suggesting that the relationship is not driven simply by individuals' purchase of guns in response to increases in criminal activity.

One possible explanation for this finding, however, is that individuals purchase guns in response to expected future increases in crime. My finding that lagged changes in gun ownership are strongly positively related to changes in gun homicide rates, but bear no corresponding relationship with nongun homicide rates, does not support this hypothesis. Instead, it suggests that an increase in the number of guns leads to a substantial increase in the number of homicides. The relationship with all other crime categories is much less marked, implying that firearms increase criminal activity primarily through their impact on homicides.

These findings contradict the results from recent work suggesting that legislation allowing individuals to carry concealed weapons (CCW) caused a significant decline in violent crime (Lott and Mustard 1997). I therefore use the magazine sales data to revisit in greater detail the impact of state CCW laws on crime rates. Theoretically, CCW legislation could have reduced the crime rate by increasing the likelihood that potential victims would be carrying a firearm. This could change if (1) the fraction of individuals owning a gun increased or (2) the frequency with which existing owners carried their guns increased.

Using the magazine sales data, I first examine whether the passage of CCW laws led to increases in the rate of gun ownership and find no evidence of such a pattern. I then investigate whether criminals were deterred from committing crimes because of a perception that the existing set of gun owners would carry their guns with them more frequently. I find no evidence that counties with above-average rates of gun ownership within CCW states experienced larger declines in crime than low-ownership counties did, suggesting either that gun owners did not increase the frequency with which they carried their guns or that criminals were not deterred by the greater likelihood that their victims would be armed. These findings weaken the claim that CCW legislation could plausibly have reduced violent crime rates. Consistent with this, robustness tests of the Lott-Mustard results demonstrate that their central results are inaccurate.

From 1993 to 1998, the number of gun homicides declined by 36 percent, whereas the number of nongun homicides declined by only 18 percent. During that same time period, national survey estimates suggest that the share of households with at least one gun fell by more than 17 percent. My point estimates imply that this decline in gun ownership can explain approximately one-third of the differential decline in gun homicides during this time period, with the largest declines occurring in areas with the largest reductions in ownership of firearms. Whether this decline in gun ownership also partially explains the substantial decline in the number of gun suicides during this same time period is an important topic for future research.[1]

II. Do Sales of *Guns & Ammo* Accurately Estimate Gun Ownership?

Guns & Ammo is the nation's fourth largest firearms magazine. Approximately 600,000 copies were sold in 1998, with almost 90 percent of these sales resulting from subscriptions and the remainder sold as single copies. In contrast to the three gun magazines with greater circulation (*American Rifleman, American Hunter,* and *North American Hunter*), sales data for this magazine are available annually at both the state and the county levels. More important, *Guns & Ammo* is focused relatively more on handguns than these other three magazines. Because handguns are the weapon of choice in the vast majority of firearms-related crimes and are more likely to be purchased for self-defense purposes than rifles or shotguns, this magazine is a more appropriate one for analyzing the dynamic relationship between crime and gun ownership.

In this section, I first examine whether sales rates of gun magazines are significantly higher in counties with average individual-level characteristics similar to those of the typical gun owner. Recent work by Glaeser and Glendon (1998) uses data from the annual GSS to deter-

[1] After reaching a peak of 18,964 in 1993, the number of gun suicides fell in each of the next five years and stood at 17,424 in 1998. During that same time period, the number of nongun suicides actually increased (although by a smaller amount) from 12,200 to 13,151.

TABLE 1
Determinants of Gun Magazine Sales at the County Level

Variable	Log(*Guns & Ammo* Sales in 1990 per 1,000 Residents) ($N=492$) (1)	(2)	(3)
% college grads$_{90}$	−1.39***	−1.66***	−2.06***
	(.27)	(.24)	(.32)
% no high school diploma$_{90}$	−1.95***	−1.88***	−1.39***
	(.25)	(.23)	(.27)
Log(income per capita$_{90}$)	.205**	.313***	.356***
	(.080)	(.079)	(.116)
% rural$_{90}$	.249***	.160***	.216***
	(.059)	(.056)	(.055)
% white$_{90}$	.396***	.364***	.008
	(.101)	(.101)	(.123)
% males$_{81}$	5.18***	5.07***	3.20***
	(1.08)	(1.05)	(1.17)
% aged 0–17$_{90}$			−2.45***
			(.55)
% aged 65+$_{90}$			−1.67***
			(.50)
% poor$_{90}$			−.446
			(.427)
Population density$_{90}$			−.0198***
			(.0028)
Midwest	.049*		
	(.028)		
South	.162***		
	(.028)		
West	.267***		
	(.034)		
State fixed effects?	no	yes	yes
R^2	.548	.747	.780

Note.—The dependent variable is the log of the sales rate of *Guns & Ammo* per 1,000 residents. The data set consists of all counties in the United States in 1990 with a population of 100,000 or more. All other counties are collapsed into "rest of state" observations. County-level demographic and economic indicators are obtained from the 1990 Census. Standard errors are included in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

mine which types of individuals are most likely to own guns. Their findings reveal that people living in western and southern states are significantly more likely to own one or more guns than midwesterners, with people from eastern states being the least likely. The authors also find that high school dropouts and college graduates are relatively unlikely to own a firearm and that white males living in rural areas are the most likely to own a gun. Finally, when they control for an individual's educational attainment, the authors demonstrate that the probability of gun ownership is increasing with the person's income.

The county-level regression results presented in table 1 suggest that the readers of this magazine are quite similar to typical gun owners.

These regressions explain the log of the sales rate for *Guns & Ammo* at the county level in 1990.[2] Consistent with the findings of Glaeser and Glendon, column 1 reveals that counties with more high school dropouts or college graduates have significantly lower sales rates than other areas. The coefficient estimates on the region dummies demonstrate that counties in the West or South have much higher sales rates than counties in the East or Midwest. Rural counties with relatively many white males also have more per capita sales of gun magazines than other areas, and average income is significantly positively related to sales of this magazine.

In column 2, I include state fixed effects in the regression and find that the estimates from the first specification are largely unchanged. This suggests that the proxy variable is not simply picking up variation across states in gun ownership but that there is substantial within-state variation as well. In column 3, I include additional explanatory variables in the regression and find that densely populated counties and those with relatively many children or elderly individuals have lower rates of gun ownership. This first set of regressions suggests that the observable characteristics of those individuals who purchase this magazine are quite consistent with those of gun owners, implying that sales rates of *Guns & Ammo* accurately proxy for the rate of gun ownership in an area.[3]

The next set of regressions summarized in table 2 provides further support for the accuracy of this proxy. In places with higher rates of gun ownership, one would expect to find more sales of firearms. While state-level data on gun sales are unavailable, the first specification utilizes data on the location of gun shows[4] in the United States to examine whether states with high sales rates of *Guns & Ammo* have relatively more gun shows (and presumably more gun sales) per person. The significant coefficient estimate of .995 implies that a 10 percent increase in the magazine's sales rate is associated with a 10 percent increase in the number of gun shows per capita.

[2] The 444 counties with a population of 100,000 or more in 1990 are included in the regression, with all other counties combined into a "rest of state" category. While these 444 counties account for only 14 percent of the 3,142 counties, approximately 75 percent of the U.S. population resides in one of these counties.

[3] Cook (1987) uses the fraction of robberies committed with a gun to proxy for the rate of gun ownership in 44 U.S. cities. While this may be an accurate proxy at the city level, the sales rate of *Guns & Ammo* appears to be a significantly better measure overall. The correlation between this magazine's state-level sales rate and the estimated ownership from the GSS is significantly positive at .61, whereas the correlation is only .09 (and statistically insignificant) between the fraction of robberies committed with a gun and the GSS estimates.

[4] These data are obtained from the NRA's publication *American Rifleman*. Eight states had no gun shows reported in 1996. Including these states in the regression by replacing the dependent variable with log (0.1/pop) leads to a slightly greater and more significant coefficient estimate.

TABLE 2
Relationship of Gun Magazine Sales with Gun Shows, Gun Accidents, and Gun Suicides

	Log(Gun Shows) (1)	Log(Gun Accidents) (2)	Log(Gun Suicides) (3)	Log(Nongun Suicides) (4)
Log(*Guns & Ammo*)	.995***	1.183***	1.437***	.044
	(.363)	(.370)	(.169)	(.128)
R^2	.155	.179	.595	.002
Observations	43	50	51	51

Note.—The dependent variable in the first specification is the log of the number of gun shows per state resident in 1996. The next three dependent variables are the log of the 1996 state-level death rate from gun accidents, gun suicides, and nongun suicides, respectively. Regressions are weighted by state population. Standard errors are included in parentheses.
*** Significant at the 1 percent level.

Columns 2–4 use state-level data from the NCHS on the underlying cause of all deaths in the United States in 1996. The second specification shows that there are significantly more deaths (per capita) from gun accidents in those states with higher sales rates of gun magazines and that there is again an elasticity of approximately one. The third and fourth specifications show that gun suicide rates are significantly greater in states with relatively high sales rates of gun magazines; there is no corresponding relationship between estimated rates of gun ownership and nongun suicides. Previous work has used the fraction of suicides that are committed with a gun as a proxy for gun ownership (Cook and Moore 1995), and this pair of regressions demonstrates that this fraction is significantly higher in places with more gun ownership.[5]

The results of the first two specifications displayed in table 3 use state-level data for 1982–98[6] on membership in the NRA to investigate its relationship with *Guns & Ammo* sales. The significant coefficient estimate of .807 in the first specification shows that states with more NRA members per capita have significantly higher sales rates for this magazine. The second specification uses annual state-level data and includes both year and state fixed effects. The significant estimate of .389 implies that, within a state, rates of NRA membership are significantly positively

[5] Codes from the international classification of diseases (ICD9) for gun suicides, suicides, and gun accidents begin with 9550–54, 95, and 922, respectively. The findings of Sloan et al. (1990) and Kellermann et al. (1992) suggest that gun ownership leads to more suicides because of the relatively high success rate for this method. Whether their findings are due to a true causal effect or are instead driven by unobserved differences in the propensity to commit suicide by gun and nongun owners is unclear.

[6] This paper focuses on the 1980–98 time period, but NRA data are unavailable before 1982. To calculate membership, I use data from the Audit Bureau of Circulations and calculate the sum of magazine subscriptions for *American Rifleman* and *American Hunter* (and *American Guardian* beginning in 1998). Each member of the NRA receives a subscription to one of these magazines, and the magazine is not sold on the newsstand. Only NRA members can subscribe.

TABLE 3
Relationship of Gun Magazine Sales with NRA Membership and GSS Gun Ownership

	Log(NRA Membership)		Log(GSS Ownership)	
	(1)	(2)	(3)	(4)
Log(*Guns & Ammo*)	.807***	.389***	.975***	.354***
	(.132)	(.022)	(.188)	(.114)
R^2	.431	.967	.384	.712
Observations	51	867	45	488
Year fixed effects?		yes		yes
State fixed effects?		yes		yes

Note.—The dependent variable in the first specification is the log of the average NRA membership rate (per 1,000 state residents) from 1982 to 1998. The dependent variable in col. 2 is the log of the NRA membership rate, with annual state-level observations from 1982 to 1998. The dependent variable in the third specification is the log of the fraction of 1980–98 GSS respondents within a state who claim to own at least one gun. Only 45 states are included because GSS data are not available for six of the smaller states. The dependent variable in the fourth specification contains annual state-level observations for the same variable. There were 18,755 respondents to the question during the 1980–98 surveys. Regressions 1 and 2 are weighted by the state population and regressions 3 and 4 are weighted by the number of GSS survey respondents within each state. Standard errors are included in parentheses.
*** Significant at the 1 percent level.

related with magazine sales rates, providing additional evidence that this proxy is a good measure both of the level and of the change in gun ownership within an area.

In the last two specifications I use data from the GSS to provide a final test of the validity of this proxy. In many of the GSS surveys, respondents are asked whether they own a gun. I use data from the 1980–98 surveys to examine whether states with more reported gun ownership have higher sales rates for this gun magazine during the same time period.[7] In column 3 of table 3, I use all the GSS survey data and find that states with higher average rates of gun ownership during this time period have significantly more magazines sold per state resident. The significant coefficient estimate suggests an approximate one-for-one relationship between the reported rate of gun ownership and the sales rate for this magazine. The specification summarized in column 4 uses annual state-level data and includes both state and year fixed effects. The significantly positive estimate of .354 suggests that this magazine's sales are a valid measure both of the level and of the change in gun ownership within an area. It is worth noting that the *Guns & Ammo* sales rate does not explain all of the variation in specification 4, presumably because the sample size within each state-year cell of the GSS

[7] It is worth noting that the GSS was designed to be nationally representative, and therefore the samples from any individual state will not necessarily be a representative sample of state residents. Furthermore, the number of respondents for the average state in a typical year is only 38. Small sample sizes, coupled with the potential nonrepresentativeness of respondents, have prevented researchers from using GSS data to reliably estimate the dynamic relationship between gun ownership and crime. In the absence of a better source of survey data, I nevertheless use the GSS to provide one final test of the validity of my proxy.

is small and because the magazine sales rate is an imperfect proxy for the rate of gun ownership.

Taken together, the results in this section strongly suggest that this panel data set of *Guns & Ammo* sales rates provides a much richer set of information about gun ownership than any that has previously been assembled. One potential concern, however, is that very few readers of this magazine may be criminals. Even if only law-abiding citizens read this magazine, nearly 500,000 guns are stolen annually, suggesting that increases in gun ownership among law-abiding citizens will increase the availability of firearms for criminals. Additionally, many criminals purchase their firearms on the secondhand market, which is much less regulated than the market for new guns from federally licensed dealers. Evidence from the Bureau of Alcohol, Tobacco, and Firearms reveals that a substantial share of the transactions at gun shows involve at least one previously convicted criminal (Handgun Control Inc. 1999). Thus the ease with which criminals can acquire their guns is bound to be much greater in those places with the most gun ownership among law-abiding citizens.

III. The Relationship between Crime and Gun Ownership

Because firearms are used in a significant fraction of all violent crimes, but are also frequently used for self-defense purposes, changes in the number of guns within an area could have an important impact on the level and average seriousness of criminal activity. Kleck (1991) and Kleck and Patterson (1993) argue that, because guns are used frequently in self-defense, they act as an effective deterrent to criminal activity. Thus increases in gun ownership reduce the crime rate,[8] suggesting that there may be positive externalities associated with gun ownership. As the fraction of individuals owning a firearm increases, the expected punishment from committing a crime may also increase.[9]

Findings to the contrary claim that a greater availability of firearms will lead to more crime, either by increasing the likelihood that any crime will result in a victim's death (Zimring 1972; Cook 1979) or by increasing the probability that a domestic dispute will result in the death

[8] Estimates regarding the frequency with which gun owners successfully defend themselves from criminals vary widely. Data from the National Crime Victimization Survey suggest approximately 75,000 cases of self-defense annually, amounting to approximately 2 percent of all violent crimes (McDowall, Loftin, and Wiersema 1992). Using the same data, Cook (1991) finds that only 3 percent of victims successfully used a gun against a criminal who intruded while they were home. Kleck's (1991) and Kleck and Patterson's (1993) estimates, at more than one million, are greater by an order of magnitude.

[9] Becker (1968) develops a model in which criminal behavior is substantially affected by the expected costs of committing a crime. Positive externalities have been found for other types of victim protection (Ayres and Levitt 1998).

of one or more individuals (Kellermann et al. 1992). Donohue and Levitt (1998) develop a model in which firearms may reduce the predictability of the outcomes of fights and consequently increase the number of violent confrontations that occur.

Much of the previous empirical work that examined this issue used cross-sectional estimates of gun ownership. These studies were unable to control for unobserved differences across areas that could plausibly affect both gun ownership and crime, and the estimates were typically quite sensitive to precisely which control variables were included in the regressions. Furthermore, any significant relationship could have been driven by a causal effect of guns on crime or the reverse.[10] In this section I build on previous work by exploiting nearly 20 years of both state- and county-level sales data of gun magazines to explore the dynamic relationship between gun ownership and crime.

A. *The Relationship between Changes in Homicide and Gun Ownership*

I first use the annual, state-level sales data from gun magazines described above to investigate whether changes in gun ownership are positively related to changes in homicide rates by running specifications of the following form:

$$\Delta \log (\text{homicides}_{it}) = \alpha + \beta \Delta \log (\text{guns}_{it}) + \rho \Delta X_{it} + \lambda_t + \mu_i + \epsilon_{it}. \quad (1)$$

In this regression guns_{it} equals the gun magazine's sales rate in state i during year t. I obtain homicide data from two different sources: the Federal Bureau of Investigation (FBI) and the NCHS. The NCHS data are a more accurate source of homicide data since 5–8 percent of homicides are not reported in the FBI state-level data each year. The variables X_{it} include control variables for the log of per capita personal income, the unemployment rate, and the fraction of state residents that are between the ages of 18 and 24. Throughout this section, the measures of crime and gun ownership are defined in per capita terms, and summary statistics for these variables are included in column 1 of table 7 below.

The coefficient estimates presented in table 4 demonstrate that changes in state-level homicide rates are significantly positively related to changes in gun ownership. The first three specifications use FBI homicide data when calculating the left-hand-side variable, and the latter

[10] Using instrumental variables, Kleck and Patterson (1993) use cross-sectional data to estimate this relationship. To be valid, these instruments must be related to crime only through their relationship with gun ownership. Their instruments, which include city-level gun control laws, are likely to respond to criminal activity and thus fail to meet this test.

TABLE 4
Relationship between Changes in the Homicide Rate and Changes in Gun Ownership

	ΔLog(FBI Homicides$_{it}$)			ΔLog(NCHS Homicides$_{it}$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Δlog(guns$_{it}$)	.226***	.187**	.194**	.226***	.191**	.178**
	(.079)	(.083)	(.085)	(.072)	(.077)	(.078)
State trend dummies?	no	yes	yes	no	yes	yes
R^2	.159	.173	.174	.222	.237	.240

Note.—The dependent variable in specifications 1–3, Δlog(FBI homicides$_{it}$), is the change in the log of the state-level homicide rate as reported to the FBI. The dependent variable in specifications 4–6, Δlog(NCHS homicides$_{it}$), is equal to the change in the log of the state-level homicide rate using data from the National Center for Health Statistics. The variable Δlog(guns$_{it}$) equals the change in the log of the state-level sales rate of gun magazines. The sample includes state-year observations for 1980–98. The number of observations is 918 in all regressions. Each regression includes year fixed effects and is weighted by state population. White standard errors are in parentheses.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

three employ the corresponding data from the NCHS.[11] The coefficient estimates are not significantly affected by the inclusion of state trend dummies or by state-level economic and demographic controls, as the second and third specifications reveal. The coefficient estimates suggest that a 10 percent increase in the rate of gun ownership is associated with approximately a 2 percent increase in the homicide rate.

This finding is consistent with the theory that increases in gun ownership lead to a rise in criminal activity but also supports the hypothesis that an increase in crime leads more individuals to purchase a gun for self-defense purposes. The set of regressions summarized in table 5 aims to differentiate between these alternative stories by examining whether lagged increases in gun ownership are associated with increases in crime or whether the opposite is true. Columns 1–4 summarize the results from specifications of the following form:

$$\begin{aligned}\Delta \log(\text{homicides}_{it}) = \alpha &+ \sum_{\tau=1}^{2} \beta_\tau \Delta \log(\text{guns}_{i,t-\tau}) \\ &+ \sum_{\tau=1}^{2} \gamma_\tau \Delta \log(\text{homicides}_{i,t-\tau}) \\ &+ \rho \Delta X_{it} + \lambda_t + \mu_i + \epsilon_{it}. \qquad (2)\end{aligned}$$

The coefficient estimate on Δlog(guns$_{i,t-1}$) in column 1 implies that a 10 percent increase in gun ownership in the current year is associated with a 2.14 percent increase in the homicide rate in the following year. The significantly negative estimate of $-.356$ on the Δlog(FBI

[11] Like the suicide data used above, the NCHS homicide data are tabulated from death certificates. The NCHS homicides include those with an ICD9 code beginning in 96 and therefore exclude deaths from legal executions or other legal interventions.

TABLE 5

State-Level Estimates of the Relationship between Changes in Rates of Homicide and Gun Ownership

	ΔLog(FBI Homicides$_{it}$)				ΔLog(Guns$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δlog(guns$_{i,t-1}$)	.214**	.210**	.180*	.190*	.100*	.102*	.032	.011
	(.099)	(.100)	(.103)	(.103)	(.054)	(.054)	(.050)	(.050)
Δlog(guns$_{i,t-2}$)		.243**	.210**	.214**		.134***	.080**	.070**
		(.092)	(.094)	(.095)		(.038)	(.035)	(.034)
Δlog(FBI homicides$_{i,t-1}$)	−.356***	−.386***	−.428***	−.427***	.013	.025**	.019*	.019*
	(.050)	(.050)	(.049)	(.050)	(.010)	(.011)	(.011)	(.011)
Δlog(FBI homicides$_{i,t-2}$)		−.065*	−.102***	−.102**		.032***	.027**	.029**
		(.048)	(.050)	(.050)		(.012)	(.012)	(.012)
Δlog(per capita income$_{it}$)				−.276				.694***
				(.390)				(.134)
Δunemployment rate$_{it}$				−.300				.588**
				(.749)				(.276)
Δ% aged 18–24$_{it}$				−.673				1.946
				(3.957)				(1.788)
State trend dummies?	no	no	yes	yes	no	no	yes	yes
R^2	.270	.281	.317	.317	.600	.628	.661	.677

Note.—The dependent variable in specifications 1–4, Δlog(FBI homicides$_{it}$), is the change in the log of the state-level homicide rate (from FBI data). The dependent variable in specifications 5–8, Δlog(guns$_{it}$), is equal to the change in the log of the state-level sales rate of gun magazines. The sample includes state-year observations for 1980–98. The number of observations is 816 in all regressions except 1 and 5, which have 867 each. Each regression includes year fixed effects and is weighted by state population. White standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

homicides$_{i,t-1}$) coefficient demonstrates that there is substantial regression to the mean in state-level homicide rates. Results from the second specification show that this relationship between lagged changes in gun ownership and current changes in the homicide rate continues into the subsequent year as well. To control for differences in homicide trends across states during the time period of interest, I next include state trend dummies in specification 3. The estimates for $\Delta\log(\text{guns}_{i,t-1})$ and $\Delta\log(\text{guns}_{i,t-2})$ decline slightly but remain significantly positive. Adding state-level control variables to this regression has virtually no impact on the coefficient estimates for $\Delta\log(\text{guns}_{i,t-1})$ and $\Delta\log(\text{guns}_{i,t-2})$.

In columns 5–8, I summarize the results from analogous regressions with $\Delta\log(\text{guns}_{i,t})$ as the dependent variable. In most cases, the estimated relationships between lagged changes in homicide rates and current changes in gun ownership are significantly positive, providing support for the hypothesis that individuals purchase firearms in response to an increase in criminal activity. However, the estimated effect is much smaller in magnitude than in the previous four regressions: a 10 percent increase in the homicide rate is associated with only a 0.2–0.3 percent increase in gun ownership in the subsequent year. If these dynamic specifications are accurately capturing a causal relationship, then it appears that gun ownership has a much greater impact on murder rates than murder rates have on gun ownership.

B. Gun versus Nongun Homicides

One factor not addressed above is that individuals may purchase guns in response to expected future increases in criminal activity. Rather than demonstrating a causal effect of gun ownership on crime, the observed relationship in columns 1–4 of table 5 may instead represent a causal effect of expected increases in crime on current gun ownership. Heckman (2000) points out, for example, that future Y_t often determines current X_t in dynamic economic models.

One way to differentiate between these two hypotheses is to divide homicides into two categories: those committed with a firearm and those committed with some other weapon. If changes in gun ownership have a similar relationship with both types of homicide, then one could reasonably conclude that individuals are purchasing more firearms in response to expected increases in crime or that increases in gun ownership simply proxy for increases in the average criminal tendencies of the population. Alternatively, if current increases in gun ownership are more strongly related to future increases in gun homicides, then the theory that a rise in gun ownership is causing an increase in the homicide rate would be much more plausible.

The regression results summarized in table 6 use the NCHS data to

TABLE 6

Relationship between Lagged Changes in Gun Ownership and Current Changes in Gun and Nongun Homicides

	ΔLog(Gun Homicides$_{it}$)				ΔLog(Nongun Homicides$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δlog(guns$_{i,t-1}$)	.316***	.302***	.306***	.291**	.117	.118	.020	.022
	(.111)	(.112)	(.117)	(.118)	(.087)	(.088)	(.092)	(.093)
Δlog(guns$_{i,t-2}$)		.236**	.223**	.215*		.111	.040	.037
		(.107)	(.112)	(.114)		(.096)	(.099)	(.100)
Δlog(gun homicides$_{i,t-1}$)	−.322***	−.337***	−.372***	−.376***				
	(.039)	(.044)	(.046)	(.047)				
Δlog(gun homicides$_{i,t-2}$)		−.038	−.071*	−.071*				
		(.040)	(.042)	(.042)				
Δlog(nongun homicides$_{i,t-1}$)					−.453***	−.557***	−.589***	−.588***
					(.042)	(.044)	(.044)	(.045)
Δlog(nongun homicides$_{i,t-2}$)						−.222***	−.253***	−.253***
						(.040)	(.042)	(.042)
Δlog(per capita income$_{it}$)				.197				.000
				(.424)				(.421)
Δunemployment rate$_{it}$				.691				−.453
				(.905)				(.798)
Δ% aged 18–24$_{it}$				.045				3.474
				(4.693)				(4.620)
State trend dummies?	no	no	yes	yes	no	no	yes	yes
R^2	.294	.299	.328	.329	.257	.303	.336	.337

Note.—The dependent variable in specifications 1–4, Δlog(gun homicides$_{it}$), is the change in the log of the state-level gun homicide rate. The dependent variable in specifications 5–8, Δlog(nongun homicides$_{it}$), is equal to the change in the log of the state-level nongun homicide rate. These data are obtained from the NCHS. The sample includes state-year observations for 1980–98. The number of observations is 816 in all regressions except 1 and 5, which have 867 each. Each regression includes year fixed effects and is weighted by state population. White standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

run specifications analogous to those presented in table 5. A comparison of the coefficient estimates for $\Delta\log(\text{guns}_{i,t-1})$ and $\Delta\log(\text{guns}_{i,t-2})$ in the $\Delta\log(\text{gun homicides}_{i,t-1})$ and $\Delta\log(\text{nongun homicides}_{i,t-2})$ specifications reveals that increases in gun ownership are significantly positively related to increases in gun homicides but bear no corresponding relationship with nongun homicides. This finding strongly supports the hypothesis that increases in gun ownership lead to future increases in homicides, since it is not plausible that individuals would purchase firearms in response to predictable increases in gun homicides but be unresponsive to expected increases in nongun homicides.

C. *Other Crime Categories*

The results from previous research suggest that guns influence criminal activity primarily by increasing the likelihood that a victim will be murdered and by raising the probability that an individual criminal will be successful (Cook and Moore 1995), but that changes in gun ownership have a smaller impact on the number of other crimes committed. If this hypothesis is true, then one would expect to find a much weaker relationship between changes in gun ownership rates and future changes in other crime rates.

To test this hypothesis, I run specifications similar to the ones described above for every individual crime category. In each specification, I include lagged changes both for the gun magazine sales and for the appropriate crime category. I use data from the FBI's *Uniform Crime Reports,* which provides annual state-level data on the number of homicides, robberies, aggravated assaults, rapes, burglaries, larcenies, and auto thefts. The coefficient estimates for $\Delta\log(\text{guns}_{i,t-1})$ and $\Delta\log(\text{guns}_{i,t-2})$ are displayed in table 7, which also provides summary statistics for each of the variables of interest.

As is clear from the coefficient estimates, the relationship between state-level changes in gun ownership and future increases in robberies, aggravated assaults, and rapes is much smaller than the corresponding one with gun homicides. In all three cases, the two coefficient estimates of interest are statistically insignificant. Similarly small estimates are found for the three specifications relating to property crimes (burglary, larceny, and theft), although two of the six coefficient estimates are significantly positive. Given that nearly 500,000 guns are reported stolen annually, guns are apparently considered a valuable commodity to criminals. It is therefore plausible that increases in firearm ownership may increase the payoff to crimes of theft. In any case, the estimated effect is much smaller than the corresponding one for homicides, which is driven entirely by a relationship between changes in gun ownership and gun homicides. This set of findings strongly suggests that increases in

TABLE 7
RELATIONSHIP BETWEEN CHANGES IN GUN OWNERSHIP AND FUTURE CHANGES IN CRIME

Dependent Variable	Mean and Standard Deviation of Dependent Variable (1)	ΔLog(Guns) Coefficient Estimates: $t-1$ (2)	$t-2$ (3)
$\Delta\log(\text{gun homicide}_{it})$	−.026	.306***	.223**
	(.166)	(.117)	(.112)
$\Delta\log(\text{nongun homicide}_{it})$	−.028	.020	.040
	(.163)	(.092)	(.099)
$\Delta\log(\text{FBI homicide}_{it})$	−.027	.180*	.210**
	(.123)	(.103)	(.094)
$\Delta\log(\text{FBI robbery}_{it})$	−.017	−.016	.069
	(.104)	(.097)	(.069)
$\Delta\log(\text{FBI assault}_{it})$	.010	−.007	−.013
	(.091)	(.085)	(.061)
$\Delta\log(\text{FBI rape}_{it})$	−.003	−.052	−.092
	(.085)	(.073)	(.060)
$\Delta\log(\text{FBI burglary}_{it})$	−.038	−.002	.094*
	(.065)	(.054)	(.049)
$\Delta\log(\text{FBI larceny}_{it})$	−.009	.081**	.032
	(.048)	(.036)	(.035)
$\Delta\log(\text{FBI auto theft}_{it})$	−.004	.043	.019
	(.096)	(.077)	(.073)

NOTE.—The coefficient estimates for $\Delta\log(\text{guns}_{i,t-1})$ and $\Delta\log(\text{guns}_{i,t-2})$ (from a specification analogous to specification 3 in table 6) are displayed in cols. 2 and 3. Each specification includes lagged changes of the appropriate crime category. The summary statistics for each of the dependent variables are provided in col. 1. The mean and standard deviation for $\Delta\log(\text{guns}_{it})$ are .003 and .074, respectively. For all specifications, the sample includes state-year observations for 1980–98, resulting in 816 observations. Each regression includes year fixed effects and is weighted by state population. White standard errors are in parentheses.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

gun ownership lead to increases in the number of homicides, but the evidence for effects on other crime categories is much weaker. The next subsection uses county-level FBI crime data to further probe these results.

D. *County-Level Estimates of the Relationship between Gun Ownership and Crime*

The FBI's *Uniform Crime Reports* data provide annual county-level information on the number of crimes in each of the seven categories described above. An examination of these data reveals that there is substantial underreporting, with many states neglecting to provide county-level crime data for multiple years.[12] In constructing the state-level crime data utilized above, the FBI attempts to account for underreporting or changes in the reporting practices of police precincts. Thus state-level data are likely to be a more reliable guide to changes in crime patterns. Despite the problems associated with the county-level crime data, I use them here to probe the state-level results described above. I include in the empirical analysis in this section all counties with populations of 100,000 or more and collapse the remaining counties within each state into 48 "rest of state" observations.[13]

Table 8 summarizes a set of specifications analogous to the ones presented in table 5 that examine the dynamic relationship between changes in gun ownership and changes in homicide rates. The statistically significant estimate of .142 in column 1 implies that a 10 percent increase in gun ownership is associated with a 1.42 percent increase in the homicide rate in the following year. This result is similar to the state-level estimate, although it is slightly smaller in magnitude. The coefficient estimate for the second lagged change in gun ownership is also significantly positive, as column 2 shows. Including county trend dummies in column 3 does not appreciably affect the two coefficient estimates, although the second one becomes insignificantly positive. In specification 4, I adjust the standard errors to account for the possibility that county-level changes in homicides or in gun ownership within a state during a year may not be independent.[14] Columns 5–8 reveal that, as was true in the state-level regressions, the relationship between lagged changes in homicide rates and current changes in rates of gun ownership are much smaller in magnitude.

In table 9, I present the results from regressions analogous to specification 4 in table 8 that examine the relationship between lagged

[12] See Maltz (1999) for a detailed description of the reporting problems with county-level crime data. The states that are especially bad at reporting at the county level are Vermont, Illinois, Montana, and Mississippi. Many other states, including Florida, Georgia, Iowa, and Kentucky, fail to report any crime data for one or more years. A comparison of the summary statistics at the state and county levels reveals that county-level data are substantially noisier, with standard deviations that are typically two to three times as large as the corresponding ones from state-level data (tables 7 and 9). In approximately one out of five cases, the sum of county crimes within a state deviates by more than 20 percent from the statewide total reported by the FBI.

[13] While this accounts for less than 14 percent of all counties, those with a population of 100,000 or more in 1990 account for almost 75 percent of the U.S. population in 1990. Without this adjustment, approximately half of the county-year observations will have zero murders. After the adjustment, virtually none do. Owing to reporting inconsistencies with the magazine sales data, St. Louis County and St. Louis City are combined into one observation, as are Baltimore County and Baltimore City and the five counties in New York City. For the same reason, there is only one observation annually for the state of Alaska and one annually for the state of Hawaii.

[14] For example, there may be state-level changes in legislation that similarly affect county-level rates of gun ownership within a state, or the police within a state may change the accuracy with which they report homicides in particular years.

TABLE 8

County-Level Estimates of the Relationship between Changes in Rates of Homicide and Gun Ownership

	ΔLog(FBI Homicides$_{it}$)				ΔLog(Guns$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta\log(\text{guns}_{i,t-1})$	.142***	.152***	.145**	.145**	−.030	−.028	−.081***	−.081**
	(.054)	(.055)	(.055)	(.063)	(.032)	(.033)	(.034)	(.039)
$\Delta\log(\text{guns}_{i,t-2})$		.098*	.087	.087		−.078***	−.123***	−.123***
		(.055)	(.057)	(.057)		(.017)	(.017)	(.025)
$\Delta\log(\text{FBI homicides}_{i,t-1})$	−.425***	−.548***	−.570***	−.570***	.001	.003	.003	.003
	(.024)	(.025)	(.023)	(.026)	(.003)	(.003)	(.003)	(.003)
$\Delta\log(\text{FBI homicides}_{i,t-2})$		−.255***	−.276***	−.276***		.004	.004	.004
		(.021)	(.020)	(.019)		(.003)	(.003)	(.003)
County trend dummies?	no	no	yes	yes	no	no	yes	yes
Observations	7,766	7,181	7,181	7,181	7,963	7,359	7,359	7,359
R^2	.201	.260	.287	.287	.260	.267	.318	.318

Note.—The dependent variable in specifications 1–4, $\Delta\log(\text{FBI homicides}_{it})$, is the change in the log of the county-level homicide rate. The dependent variable in specifications 5–8, $\Delta\log(\text{guns}_{it})$, is equal to the change in the log of the county-level sales rate of gun magazines. The sample includes county-year observations for 1980–98. Counties with a population of 100,000 or more in 1990 are included, with all other counties collapsed into rest of state categories for each state. Certain county-year observations are missing because of missing crime data. Each regression includes year fixed effects and is weighted by county population. White standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

TABLE 9
Relationship between Changes in Gun Ownership and Future Changes in Crime

Dependent Variable	Mean and Standard Deviation of Dependent Variable (1)	ΔLog(Guns) Coefficient Estimates $t-1$ (2)	$t-2$ (3)
Δlog(FBI homicide$_{it}$)	−.028	.145**	.087
	(.499)	(.063)	(.057)
Δlog(FBI robbery$_{it}$)	−.006	.069*	−.026
	(.376)	(.041)	(.046)
Δlog(FBI assault$_{it}$)	.006	.035	−.062
	(.270)	(.037)	(.055)
Δlog(FBI rape$_{it}$)	.000	.011	−.059
	(.313)	(.042)	(.051)
Δlog(FBI burglary$_{it}$)	−.042	.019	−.023
	(.226)	(.031)	(.037)
Δlog(FBI larceny$_{it}$)	−.014	.023	−.038
	(.298)	(.049)	(.044)
Δlog(FBI auto theft$_{it}$)	.000	.071*	.007
	(.333)	(.037)	(.045)

Note.—The coefficient estimates for Δlog(guns$_{i,t-1}$) and Δlog(guns$_{i,t-2}$) (from a specification analogous to specification 3 in table 6) are displayed in cols. 2 and 3. Each specification includes two lagged changes for the appropriate crime category. Summary statistics for each of the dependent variables are provided in col. 4. The mean and standard deviation of Δlog(guns) are .003 and .114, respectively. For all specifications, the sample includes county-year observations for 1980–98. Each regression includes year fixed effects and is weighted by county population. White standard errors are in parentheses.
* Significant at the 10 percent level.
** Significant at the 5 percent level.

changes in gun ownership and current changes in the other six types of crime. Consistent with the state-level analyses summarized in table 7, changes in gun ownership are more strongly related with future changes in the homicide rate than with changes in the other crime categories.

Taken together, the results in this section provide strong support for the hypothesis that increases in gun ownership lead to future increases in the homicide rate. From 1993 to 1998, the number of gun homicides fell by more than 36 percent, whereas the number of homicides in which some other weapon was used fell by only 18 percent.[15] The GSS estimates suggest that the share of households with at least one gun fell from 42.4 percent to 34.9 percent during that same time period. From the coefficient estimates from table 6 and the GSS estimates of gun ownership, my findings suggest that approximately one-third of the *differential* decline in gun homicides, relative to nongun homicides, can be explained by reductions in the fraction of households owning a gun.[16] These gains

[15] See Donohue and Levitt (2001) for a discussion of other factors leading to recent reductions in the overall crime rate.

[16] Errors-in-variables problems with the proxy variable will tend to bias downward my estimates of the relationship between changes in gun ownership and changes in crime, suggesting that the true impact of the reduction in gun ownership may be even greater.

have been concentrated in the states with the largest relative reductions in gun ownership.

I also find some support for the theory that gun ownership increases other crime rates, but in all cases the estimated effects are much smaller than the corresponding one for the homicide rate.[17] This set of findings conflicts with recent research suggesting that the increases in gun ownership caused by the passage of CCW legislation led to reductions in rates of violent crime (Lott and Mustard 1997; Lott 1998). In the next section, I use the sales data for gun magazines to examine the reason for this discrepancy.

IV. Testing the Impact of CCW Laws

Recent work has explored whether states that enacted CCW laws experienced significant declines in crime relative to other states. The first paper in this literature argued that the passage of this legislation in 10 states between 1985 and 1991 led to a substantial reduction in violent crimes (and a corresponding increase in property crimes), as criminals were deterred by the greater likelihood that potential victims would be armed and therefore able to defend themselves (Lott and Mustard 1997). A series of studies probed the robustness of the Lott-Mustard results (Black and Nagin 1998; Ludwig 1998; Ayres and Donohue 1999; Moody 2000), with some finding support for their central conclusions and others finding that the estimated effects were quite fragile and sensitive to the precise specification used.

Despite the abundance of recent papers in this literature, none has examined whether CCW legislation increased the likelihood that potential victims would be armed. This probability could change if (1) the fraction of individuals owning a gun increased or if (2) the frequency with which existing owners carried their guns increased. Using the sales data for gun magazines, I test whether CCW legislation had an impact through either of these causal pathways. Finding no evidence of this, I then probe the Lott-Mustard findings and determine that their central results are inaccurate. Taken together, the three sets of findings in this section cast considerable doubt on the hypothesis that CCW legislation could plausibly have affected the rate of violent crime.

[17] If county trends are not included in the first-difference specifications, changes in county-level rates of gun ownership are significantly positively related to subsequent changes in other types of violent crime (Duggan 2000).

TABLE 10
IMPACT OF CCW LEGISLATION ON ESTIMATED CHANGES IN GUN OWNERSHIP

	Δ Log(Guns$_{it}$) (N=765)		
	(1)	(2)	(3)
CCW$_{it}$	.0038	.0009	.0038
	(.0077)	(.0099)	(.0099)
R^2	.701	.717	.743
State trend dummies?	no	yes	yes
State controls?	no	no	yes

NOTE.—The observations include annual state-level data for 1978–92 (first change includes 1977 data). CCW$_t$ is set equal to one in the year that legislation regarding carrying concealed weapons is passed within a state and equals one in all subsequent years as well. All regressions include year fixed effects and are weighted by state population. White standard errors are in parentheses.

A. *The Impact of CCW Legislation on Gun Ownership*

Ten states[18] passed legislation between 1985 and 1991 that allowed individuals to carry concealed handguns. If the option to carry a firearm increased the perceived benefit associated with owning a gun, then one would expect to find an increase in the fraction of individuals owning one. To test whether gun ownership did increase in CCW states following the passage of this legislation, I run specifications of the following form:

$$\Delta \log(\text{guns}_{it}) = \alpha + \rho \text{CCW}_{it} + \beta \Delta X_{it} + \mu_t + \lambda_i + \epsilon_{it}. \tag{3}$$

In this regression, i and t index states and years, respectively; guns$_{it}$ equals the number of gun magazines sold in state i in year t; and CCW$_{it}$ is a dummy variable set equal to one if a state allows individuals to carry concealed weapons and zero otherwise. If CCW legislation led to substantial increases in the fraction of individuals owning a gun,[19] then one would expect to find a significantly positive coefficient estimate on CCW$_{it}$.

The set of regressions summarized in table 10, however, suggests that gun ownership did not increase significantly in the states that passed CCW legislation relative to other states. In the first specification I regress the change in the state-level estimates of gun ownership on a CCW dummy variable. Year fixed effects are included to control for changes in gun ownership that are occurring at the national level. The insignificant estimate of .0038 in the first specification suggests that gun ownership did not increase substantially more in those states that en-

[18] The 10 states are Maine (1985); Florida (1987); Virginia (1988); Georgia, Pennsylvania, and West Virginia (1989); Idaho, Mississippi, and Oregon (1990); and Montana (1991). The Pennsylvania law did not include the county of Philadelphia.

[19] In analogous studies of the effect of changes in the prison population and the police force on the crime rate, Levitt (1996, 1997) first demonstrates that his instruments (legislation on prison overcrowding and the timing of mayoral elections) had a significant impact on the number of prisoners and on the number of police. He then uses this plausibly exogenous variation to examine the impact on crime.

acted CCW legislation. This estimate remains small and statistically insignificant if I include state trend dummies or control variables for states' economic indicators and demographic characteristics, as the second and third specifications demonstrate. The results are similar if I run these regressions instead at the county level with a similar CCW dummy variable, run the regressions in levels of gun ownership with state and year fixed effects, or extend the time period under consideration. Thus it does not appear that the CCW legislation reduced crime by significantly increasing the rate of gun ownership, suggesting that the benefits of carrying a firearm were not sufficiently large to induce many individuals who did not already own a firearm to purchase one. This finding weakens the evidence for one of the two causal pathways described above.

B. Did Crime Decline More in High-Ownership Counties?

Despite the apparent absence of an effect on gun ownership, the passage of CCW laws could still have caused a reduction in crime rates. Criminals in CCW states may have been deterred from committing crimes because of a perception that the existing set of gun owners would now carry their guns with them more frequently. If CCW legislation did lead to a reduction in crime through this channel, one would expect to detect the greatest change in those counties with relatively higher rates of gun ownership within CCW states.

The logic of this argument is straightforward. Suppose that there are two types of counties: those with high gun ownership and those with low ownership. If the frequency with which any individual owner carried her gun increased uniformly across counties, then the probability that a potential victim would be armed would increase more significantly in high-ownership areas. As long as criminals did, on average, accurately perceive differences in gun ownership rates across areas, one would expect to see the largest declines in crime in those counties with the highest gun ownership when the legislation was enacted. Although there is no perfect way to test whether criminals accurately estimate gun ownership rates, it seems implausible that criminals would systematically mistake high-ownership areas for low-ownership ones and vice versa. The fact that guns are frequently stolen and that criminals often purchase their firearms at gun shows would provide them with two direct sources of information regarding the rate of gun ownership within an area.

One potential problem with this line of reasoning is that the increase in the propensity of carrying a firearm after the passage of CCW legislation may vary systematically with the rate of gun ownership. It seems likely, however, that this carrying probability for an individual gun owner would increase most in areas with the highest rates of ownership, be-

TABLE 11
Effect of Pre-CCW Gun Ownership on Subsequent Changes in Crime

	ΔLog(Violent Crime$_{it}$) (N=629)		ΔLog(Property Crime$_{it}$) (N=613)	
	(1)	(2)	(3)	(4)
Log(guns$_{it}$)	−.037	.031	−.007	.031
	(.051)	(.061)	(.040)	(.045)
Δlog(violent crime$_{i,t-1}$)		−.433***		
		(.148)		
Δlog(property crime$_{i,t-1}$)				−.249
				(.214)
R^2	.137	.243	.281	.306
State effects?	yes	yes	yes	yes

Note.—Observations include those counties in the 10 CCW states with nonmissing crime data. Δviolent crime$_{it}$ is defined to be the change in the log of the violent crime rate from one year before the CCW legislation was passed to two years after. The property crime dependent variable is defined similarly. Regressions are weighted by county population. White standard errors are in parentheses.
*** Significant at the 1 percent level.

cause these individuals are likely to have a greater taste for gun ownership. This would therefore strengthen the claim that the probability of carrying a firearm would increase most in high-ownership counties after CCW legislation is passed.

In table 11, I explore whether, within CCW states, those counties with the highest rate of pre-CCW gun ownership had the largest changes in their rates of violent and property crimes. I include only those counties located in the 10 states that passed CCW legislation during the time period of interest. In these regressions, the gun ownership measure is simply the log of the sales rate of gun magazines in a county in the year before the CCW legislation was passed. The left-hand-side variable Δlog(violent crime$_{it}$) is the log difference between the violent crime rate two years after the passage of CCW legislation and one year before. The variable Δlog(property crime$_{it}$) is defined similarly.

The first specification suggests that there were not significantly greater reductions in rates of violent crime in those counties with high rates of pre-CCW gun ownership. The estimate on the log(guns$_{it}$) coefficient is not affected much by the inclusion of preexisting trends in violent crime rates, as the second specification shows. The corresponding regressions for changes in property crime rates, summarized in columns 3 and 4, are not consistent with the hypothesis that property crime changed differentially in places with more gun ownership following the passage of CCW legislation. Similar specifications for each of the individual crime categories demonstrate that there is no significant relationship between the pre-CCW level of gun ownership and the subsequent change in any type of violent or property crime.

Thus it appears that violent crime did not decline significantly more in counties within CCW states that had high rates of gun ownership, suggesting either that existing gun owners did not increase the frequency with which they carried their guns or that this carrying had a negligible impact on the behavior of criminals. In either case, this finding weakens the evidence for the second channel through which CCW legislation could have affected the crime rate.

C. *Robustness Checks of the Lott-Mustard Results*

Given that CCW legislation did not lead to significant increases in gun ownership, nor reduce crime relatively more in places with high pre-CCW rates of gun ownership, it does not appear that CCW legislation could plausibly have caused a reduction in crime rates. Therefore, it would be surprising if one did find a strong reduction in violent crime rates following the passage of CCW legislation. After all, what would the causal pathway be? If such a relationship did exist, then possible explanations would be that (1) CCW states were simultaneously taking other, more effective, measures to reduce violent crime; (2) states pass CCW legislation when crime is peaking and crime rates exhibit regression to the mean; or (3) there is an omitted variable that is leading to a spurious relationship.

In this subsection I probe the Lott-Mustard results to investigate whether there is, in fact, a significant relationship between the passage of CCW legislation and criminal activity. The first set of specifications, summarized in column 1 of table 12, replicate the Lott-Mustard results and represent regressions of the following form:

$$\log(\text{crime}_{ijt}) = \alpha + \beta X_{ijt} + \rho \text{CCW}_{jt} + \lambda_t + \mu_i + \epsilon_{ijt}. \tag{4}$$

In this equation, i, j, and t index counties, states, and years, respectively. The coefficient estimates in column 1 suggest that violent crime rates are significantly lower after states pass CCW legislation and that property crime rates are correspondingly higher.

One limitation of this analysis is that it implicitly assumes that CCW laws are varying at the county-year level, when in fact they are varying only at the state-year level.[20] Therefore, one must adjust the standard errors appropriately to account for the fact that county-level disturbances may be correlated within a state during a particular year.[21] Column 2 presents coefficient estimates for ρ from nine analogous regressions that properly account for this. In all cases, the standard errors increase substantially, and several of the estimates become statistically

[20] One exception to this is Philadelphia, which was exempt from Pennsylvania's CCW legislation during the time period under consideration.

[21] See Moulton (1990) for a discussion of this issue. In essence, Lott and Mustard are assuming that there are 700 independent "natural experiments" when in fact there are only 10.

TABLE 12
Robustness Checks on the Lott-Mustard Results

	Coefficient Estimates on CCW Dummy Variable					
	(1)	(2)	(3)	(4)	(5)	(6)
Log(murder$_{it}$)	−.0733***	−.0733**	−.0531*	−.0639*	−.0093	.0004
	(.0157)	(.0295)	(.0304)	(.0356)	(.0351)	(.0529)
	26,458	26,458	26,458	26,458	46,979	46,979
Log(rape$_{it}$)	−.0520***	−.0520	−.0426**	.0344	.0563	−.0401
	(.0122)	(.0232)	(.0217)	(.0354)	(.0388)	(.0433)
	33,865	33,865	33,865	33,865	46,161	46,161
Log(assault$_{it}$)	−.0699***	−.0699	−.0582**	−.0558*	−.0460	.0129
	(.0144)	(.0277)	(.0263)	(.0286)	(.0294)	(.0432)
	43,445	43,445	43,445	43,445	46,893	46,893
Log(robbery$_{it}$)	−.0225*	−.0225	.0234	.0417	. 0995*	−.0073
	(.0133)	(.0334)	(.0364)	(.0509)	(.0562)	(.0926)
	34,949	34,949	34,949	34,949	46,974	46,974
Log(violent crime$_{it}$)	−.0488***	−.0488**	−.0250	−.0072	.0029	−.0076
	(.0098)	(.0213)	(.0213)	(.0258)	(.0254)	(.0400)
	43,451	43,451	43,451	43,451	46,070	46,070
Log(burglary$_{it}$)	.0005	.0005	.0320	.0758**	.0897**	.0200
	(.0076)	(.0229)	(.0244)	(.0337)	(.0336)	(.0537)
	45,769	45,769	45,769	45,769	46,970	46,970
Log(larceny$_{it}$)	.0334***	.0334	.0298	.0487	.0483*	.0132
	(.0089)	(.0227)	(.0259)	(.0303)	(.0294)	(.0497)
	45,743	45,743	45,743	45,753	46,973	46,973
Log(auto theft$_{it}$)	.0701***	.0701**	.0998***	.0919**	.1114***	.0545
	(.0113)	(.0259)	(.0259)	(.0443)	(.0436)	(.0570)
	44,360	44,360	44,360	44,360	46,978	46,978
Log(property crime$_{it}$)	.0267***	.0267	.0452**	.0647**	.0686**	.0219
	(.0072)	(.0189)	(.0190)	(.0266)	(.0261)	(.0455)
	45,940	45,940	45,940	45,940	46,963	46,963

Note.—Each cell contains a coefficient estimate for the CCW dummy variable from specifications analogous to eq. (4) in the text. The dependent variable in each case is the log of the crime rate (per 100,000 county residents). Col. 1 replicates the results from Lott-Mustard (1997). Col. 2 contains the same coefficient estimates with the proper adjustment to the standard errors, accounting for the fact that the dependent variable of interest varies only at the state-year level. The specifications summarized in col. 3 correct the CCW dummy variable so that it is consistently defined across states. Col. 4 includes only county and year fixed effects, in addition to the CCW dummy variable, but retains the same sample that is included in cols. 1–3. The fifth specifications summarized in col. 5 include those counties with nonmissing crime rate data. The coefficients displayed in col. 6 equal the difference between the CCW dummy variable and the estimate for the dummy variable that equals one in the year before a state passed CCW legislation.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

insignificant.[22] The regressions summarized in column 3 adjust the Lott-

[22] As Bertrand, Duflo, and Mullainathan (2001) point out, the Moulton correction alone is typically not sufficient for differences-in-differences estimators that utilize more than two periods of data because of autocorrelation in both the intervention variable and the economic variable of interest. A simple application of their randomization inference test to the Lott-Mustard data reveals that the true standard errors are approximately twice as large as those listed in col. 2. For example, the standard errors in the log(murder) and log(rape) specifications approximately double from .030 to .057 and from .023 to .048, respectively. As a result, none of the coefficient estimates on the CCW dummy variable remain statistically significant, and all the t-statistics have a magnitude of less than 1.3.

Mustard CCW dummy variable to be consistently defined across states.[23] For all five violent crime categories, the coefficient estimates become less negative, suggesting that the point estimates from the previous columns are systematically biased.

The coefficient estimates displayed in column 4 are obtained from specifications that utilize the same sample of counties but include only year and county fixed effects with the CCW dummy variable. This robustness check is motivated by the fact that the previous specifications include dozens of control variables that are quite imprecisely estimated at the county level on an annual basis. More important, some of these controls are mechanically related to the dependent variables in each specification,[24] which could bias the CCW coefficient estimates. A comparison of the estimates listed in columns 3 and 4 indicates that this adjustment weakens the CCW findings in all but one of the specifications of violent crime.

Because a substantial share of all county-year observations are excluded from these regressions, one important issue concerns the sensitivity of the coefficient estimates to the resulting sample selection. Column 5 provides results from regressions that include all county-year observations with nonmissing crime data and demonstrates that the CCW dummy variable is not significantly negatively related to any of the five violent crime variables.[25] It therefore appears that the sample selection leads to an overestimate of the impact of CCW legislation on violent crime.

While the column 5 estimates do not suggest a significant relationship between CCW legislation and violent crime, there is a significantly positive relationship between the passage of CCW legislation and the rate of property crimes. The results summarized in column 6 probe the robustness of this result by estimating specifications of the following form:

$$\log(\text{crime}_{ijt}) = \alpha + \beta X_{ijt} + \gamma \text{CCWPRE}_{jt} + \rho \text{CCW}_{jt} + \lambda_t + \mu_i + \epsilon_{ijt}. \tag{5}$$

The dummy variable CCWPRE equals one in the year *prior* to the passage of CCW legislation and in every year thereafter. Thus the estimate for the CCW variable essentially represents the difference between average crime rates following the passage of CCW legislation and in the year

[23] In their regressions, the CCW dummy is set equal to one in the year that legislation was passed and in all years thereafter for eight of the 10 states. For Florida and Georgia the dummy variable is set equal to one in the year after the legislation was passed.

[24] For example, the number of crimes is in the numerator of the dependent variable and in the denominator of one of the explanatory variables—the arrest rate.

[25] If one runs similar specifications using annual state-level data, the results also do not support the Lott-Mustard findings.

immediately before. In all specifications, the coefficient estimate for the CCW dummy variable is statistically insignificant. This finding suggests that property crime rates were trending up differentially in CCW states prior to the passage of the legislation and that the passage of the law was not associated with a significant change in property crime relative to its pre-CCW rate.

Carrying concealed weapons legislation could plausibly have affected the crime rate through two channels. Using the magazine sales data, I find no evidence to suggest that CCW laws caused a reduction in the violent crime rate through either of these two pathways. Consistent with this, robustness checks of the Lott-Mustard findings cast considerable doubt on the hypothesis that CCW legislation had any effect on crime rates.

V. Conclusion

This paper uses a unique data set to demonstrate that increases in gun ownership lead to substantial increases in the overall homicide rate. This is driven entirely by a relationship between firearms and homicides in which a gun is used, implying that the results are not driven by reverse causation or by omitted variables. The relationship between changes in gun ownership and changes in all other crime categories is weaker and typically insignificant, suggesting that guns influence crime primarily by increasing the homicide rate.

The data employed in this paper should allow researchers to answer other important questions regarding the impact of alternative gun control policies and the effect of gun ownership on other outcomes of interest. After peaking at more than 39,000 in 1993, the number of individuals dying from gun-inflicted injuries fell by 23 percent during the next five years. While much of this decline is due to a reduction in gun homicides, the number of deaths from gun suicides has also fallen substantially. Whether recent reductions in firearm ownership have caused the reduction in the nation's suicide rate is an important topic for future research.

More generally, the magazine sales data employed in this paper suggest an alternative strategy for estimating variables that have previously been considered unobservable. Similar applications of these data in other settings may allow researchers to answer other important empirical questions more convincingly.

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