Guns and Crime*

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Abstract.

Guns, especially handguns, can be used both by criminals and citizens. We estimate several models of handguns and crime based on state-level panel data for 1981-1998 using both General Social Survey data on handgun prevalence and a new proxy for gun prevalence. We find that handguns have a negligible effect on crime. Apparently, there is no causation in either direction, or a rough balance between criminals who use handguns in the commission of crime and citizens who use handguns to defend themselves and deter crime.

1. Introduction.

A criminal with a gun is a more efficient criminal. He or she is more likely to encourage victim cooperation with a gun. An increase in the supply of guns, could reduce the cost of entry into the violent crime industry. On the other hand, people also acquire guns for self-defense. Potential victims who are armed can protect themselves more efficiently against criminals. There is substantial evidence that criminals do not want to confront armed victims; that defensive gun use is an effective crime prevention strategy; and that unarmed citizens benefit from the possession of guns by others. (Kleck, 1997, 166-174.) The net effect on crime could be positive or negative, or simply wash out. So, do more guns mean more or less crime?

Homeowners who fear that their homes might be broken in to might decide to acquire a gun and, because guns are valuable loot, the presence of guns in homes might encourage burglary (Cook and Ludwig, 2002). We therefore investigate the relationship between guns and major crime (violent crime plus burglary) and the relationship between guns and each of the major crime categories (murder, rape, robbery, assault, and burglary).

There has been considerable econometric research on the relationship between guns and crime (Lott, 2000, Lott and Mustard, 1997, Moody 2001, Marvell and Moody 1995, Duggan, 2001, Cook and Ludwig, 2002). In this paper, we investigate a model of guns, crime and sanctions using a panel of annual data on 50 states from 1981-1998.

In the next section we outline a simultaneous equation model of crime, guns, and sanctions. Section 3 discusses the data and data problems associated with gun prevalence, and describes the development of a new proxy for guns. Section 4 describes the econometric methods employed in the analysis of dynamic panel data models. The results of the analyses are reported in Section 5 and Section 6 summarizes and concludes the paper.

2. Theory.

A basic sketch of the theoretical relations among the three target variables is that crime is a function of guns and sanctions. The theoretical signs on the partial derivatives are subject to some dispute. It is generally recognized that prison incarceration reduces crime due to a combination of incarceration and deterrent effects (e.g., Moody and Marvell, 1994, 1997, 1998, Levitt, 1996). We would therefore expect a negative partial derivative on prison in the crime equation. The theoretical sign on guns in the crime equation is much more controversial. Guns could cause crime because the more guns there are, the more will fall into the hands of criminals through loss, theft, etc. (Duggan 2000) and more guns in the home or in public will lead to an escalation of violence, making crime more likely and more serious. On the other hand, Lott and Mustard (1997) find that the passage of right-to-carry concealed weapons laws, which presumably increases the number of guns both at home and in public, causes crime to decrease. The theory is that if a potential criminal is not sure which of a distribution of potential victims can defend themselves efficiently with a gun, the criminal is likely to be deterred from committing any crime involving face to face contact with such a potentially armed populace. It is entirely possible that both arguments are correct and the net effect of guns on crime could be positive, negative, or zero. Similarly, both arguments could be wrong and guns are completely independent of crime, yielding a zero coefficient on guns in the crime equation. Thus, the sign of the partial derivative of guns in the crime equation remains an empirical question. See Kleck (1997) for a thorough review of the literature.

Crime can also cause ordinary citizens to acquire guns for self-defense. Thus we would expect that the partial derivative of crime in the gun equation is nonnegative. Society attempts to control crime through the use of sanctions. The criminal justice system includes the police who investigate crimes and make arrests, prosecutors and judges who try the accused and sentence the guilty, and prisons where convicted felons are incarcerated. We expect that crime is negatively related to sanctions and that prisons are positively related to arrests. We assume that arrests and the prison population are independent of the level of guns except indirectly through possible impacts on crime[1]. Assuming a linear model, the specification is as follows.

$$\begin{aligned} C &= \alpha_0 + \alpha_1 P + \alpha_2 G + \alpha_3 A + \alpha_4 Z_C \\ P &= \beta_0 + \beta_1 C + \beta_2 A + \beta_3 Z_P \\ G &= \gamma_0 + \gamma_1 C + \gamma_2 Z_G \\ A &= \delta_0 + \delta_1 C + \delta_2 Z_A \end{aligned} \tag{1}$$

where C is crime, P is prison incarceration, G is gun availability, A is arrests; Z_C, Z_P, Z_G and Z_A are vectors of exogenous variables, and $\alpha_1 < 0, \alpha_3 < 0, \beta_1 > 0, \beta_2 > 0, \gamma_1 \geq 0, \delta_1 > 0$. The sign on α_2 is not known *a priori*.

Because it is difficult to identify the coefficient on arrests in the crime equation (Fisher and Nagin, 1978), we solve for arrests, yielding the following set of equations.

[1] An anonymous referee has suggested that guns might by substituted for sanctions by people who are substituting private deterrence for social sanctions. We can find no empirical support for this interesting hypothesis.

$$C=\left(\frac{\alpha_0+\alpha_3\delta_0}{1-\alpha_3\delta_1}\right)+\left(\frac{\alpha_1}{1-\alpha_3\delta_1}\right)P+\left(\frac{\alpha_2}{1-\alpha_3\delta_1}\right)G+\left(\frac{\alpha_3\delta_4}{1-\alpha_3\delta_1}\right)Z_A+\left(\frac{\alpha_4}{1-\alpha_3\delta_1}\right)Z_C$$
$$P=(\beta_0+\beta_1\delta_0)+\beta_1\delta_1 C+\beta_1\delta_1 Z_A+\beta_3 Z_P \tag{2}$$
$$G=\gamma_0+\gamma_1 C+\gamma_2 Z_G$$

As equation system (2) shows, the coefficients on prison and guns in the crime equation depend on the value of the product $\alpha_3\delta_1$. Since we expect that $\alpha_3 < 0$ and $\delta_1 > 0$, $1-\alpha_3\delta_1 > 1$, the crime equation will have a nontrivial solution and the signs of the coefficients will be preserved. The resulting equation system is

$$C=a_0+a_1P+a_2G+a_3(Z_A+Z_C)$$
$$P=b_0+b_1C+b_2(Z_A+Z_P) \tag{3}$$
$$G=\gamma_0+\gamma_1 C+\gamma_2 Z_G$$

where $a_1 < 0, b_1 > 0, \gamma_1 > 0$ and the sign of a_2 is undetermined. The coefficient on prison in the crime equation, $a_1 = \alpha_1/(1-\alpha_3\delta_1)$, includes the effect of arrests on crime (α_3) and the effect of crime on arrests (δ_1). It is therefore properly interpreted as the effect of arrest and imprisonment on crime. Note that we could have solved for prison as a function of arrests, in which case arrests would appear in the crime equation in place of prison. The coefficient would then measure the effect of arrest and incarceration on crime and would include both the deterrent effect and the incapacitation effect of prisons on crime as well as the effect of arrests. Thus, we have our choice of including arrests and prisons either one, or both. We choose to include prison and exclude arrests in our primary model because of the difficulties associated with finding instruments that are highly correlated with arrests, but uncorrelated with crime (Fisher and Nagin, 1978)[2]. However, we also estimated the crime equation with arrests included as a robustness check and we include lagged arrests in the Granger causality tests because simultaneity is not a problem in that case.

3. Data.

The data set consists of observations on 50 states from 1981-1998. Crime is measured as the major crime categories from the FBI Uniform Crime Reports (FBI) (murder, rape, robbery, assault and burglary), the kind for which one is usually sentenced to prison. Arrests are the total number of arrests for the relevant crime definition, from the Uniform Crime Reports, divided by population. We do not use arrests divided by crime because of potential ratio bias due to measurement error in crime.

Prison population is the average of the year-end census of the current and previous year, including prisoners temporarily housed in local jails, divided by population (Marvell and Moody, 1994).

State-level observations on gun ownership are not readily available. The only direct measure gun ownership at the state level is the General Social Survey (GSS) from the National Opinion Research Center (NORC). It is the "gold standard" for survey measures of gun ownership according to Azrael, Cook, and Miller (2001, p. 4). NORC conducts in-person surveys of 3000 adults biennially. Between 1980 and 1998 over 18,000 individuals have responded to the gun ownership question. The GSS yields the proportion of households reporting ownership of firearms at the state level. If the household reports owning a firearm, the interviewer asks a follow-up question concerning

[2] Strictly speaking, we have also rolled the effect of prosecution, conviction, and sentencing into the coefficient on prisons. The argument is analogous to that for arrests. The coefficient on prison measures the

whether it is a pistol, rifle, or shotgun. Since handguns are more likely to be used in crime and most handguns are owned for defensive reasons (Kleck, 1995, p. 13), we concentrate primarily on the proportion of households reporting ownership of handguns, although we replicate all our analyses using the percentage of houses reporting gun ownership[3].

Because the GSS survey is not conducted in every state in every year, there are not a sufficient number of observations to conduct some analyses. We therefore need a proxy for gun ownership. Several different proxies have been employed in previous studies including gun magazine subscriptions (Duggan 2001), the proportion of suicides committed with guns, and the proportion of various gun crimes among all crimes. (See Kleck, 1997, pp. 248-261 for a review.) Of these proxies we discount percent gun crimes because of the obvious simultaneity between the denominator and the dependent variable in the crime equation and resulting identification problems. That leaves percent gun suicide and gun magazine subscriptions as potential proxies. Percent gun suicide was taken from the Center of Disease Control website (http://wonder.cdc.gov/). Magazine circulation data were provided by the Audit Bureau of Circulation. Using the proportion of households reporting handgun ownership (HG) as the true measure of handgun availability and the proportion of households reporting gun ownership (GUN) as the true measure of ownership of all types of guns, we correlated HG and GUN with percent gun suicide (PGS) and sales of *American Rifleman*, *American Hunter*, *American Handgunner*, and *Guns & Ammo*, the four gun magazines with the largest circulations. The results are reported in Table 1 below.

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effect of arrest, prosecution, conviction, sentencing, and prison incapacitation on crime.

[3] The GSS survey is weighted to reflect national, not state, demographics. However race and age can vary substantially across states. For example, the proportion of African-Americans is 10.6 for the US as a whole,

Insert Table 1 about here.

Percent gun suicide is much more closely correlated with the GSS measures than any of the gun magazines. This is also the result found by Azrael, Cook, and Miller (2001). Consequently, we construct a proxy for handgun ownership by regressing HG on PGS and use the predicted values to impute the missing values of HG. As a robustness check, we also imputed the missing values of GUN using the same methodology. The regressions are reported in Table 2 below.

Insert Table 2 about here.

This process produces proxies (PHG, imputed handguns and PGUN, imputed guns) whose correlations with the true measures are weighted averages of the perfect correlation between the survey and itself for those 498 observations for which GSS data are available and the correlation between percent gun suicide and the survey results for the remaining 573 observations. These correlation coefficients are reported in the last two rows of Table 1 above.

Having both the true value of gun prevalence (HG) and the proxy variable (PHG) allows us to estimate each of the relevant equations using the actual HG data then re-estimate the same equation using the imputed value of HG. The advantage of this process is that we can get an estimate of the order of magnitude of the coefficient using the relatively small number of observations on HG. We can verify these estimates, with presumably greater accuracy, using the larger number of observations on the imputed values (PHG). If the signs and numerical values of the coefficients are similar between

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but it varies from 0.3 (Vermont) to 35.5 (Mississippi) across states. We use a re-weighted version of the GSS, kindly provided by John Whitley, that reflects the individual state's race and age.

the two equations, we can be more confident that we have a good estimates than if we had used either one in isolation.

With a proxy for guns that is closely related to the GSS, we can identify the crime equation in part by using gun magazine sales as instruments for gun availability on the assumption that gun magazine sales will be related to guns but generally independent of crime. Some individuals might purchase gun magazines if they are considering acquiring a gun in response to an increase in the crime rate. However, we suspect that this group constitutes a small proportion of gun magazine sales, and we test the hypothesis when we estimate the simultaneous equation models below. We can also identify the gun equation through the use of prison and arrests as instruments which are highly correlated with crime but independent of gun ownership. We make no attempt to estimate the prison or the arrest equation.

In addition, we include the usual control variables (e.g., the proportion of the population in various age groups, unemployment, income, etc.) which comprise the vector of exogenous variables in each equation. The variable names, descriptions, and means are presented in Table 3 below.

Insert Table 3 about here.

4. Econometric Method.

Because we are pooling time series and cross section data, we have to determine the level of integration of the panels. Im, Pesaran, and Shin (1997) suggest estimating the following ADF test equation for each state

$$\Delta y_{it} = \alpha_i + (\rho_i - 1) y_{i,t-1} + \sum_{j=1}^{p} \Delta y_{i,t-j} + \varepsilon_{it}$$

The test for a unit root consists of testing the coefficient on the lagged level with a t-test. To test the null of a unit root across all states, we take the average of the t-ratios ("t-bar test"). When the errors are serially uncorrelated and independently and normally distributed across states, the resulting test statistic is distributed as standard normal for large N (number of states) and finite T (number of time periods). When the errors are serially correlated and heterogeneous across individuals, the test statistics are valid as T and N go to infinity, as long as N/T goes to some finite positive constant. The tests are consistent under the alternative hypothesis that the fraction of the individual processes that are stationary is non-zero. We apply this test to all the variables in the model allowing the number of lags to vary from zero to three with and without a linear trend term.

The tests indicate that, of the endogenous variables, guns, crime, and prison are nonstationary I(1) variables. Arrests appear to be stationary. Of the exogenous control variables, only the percent metropolitan, and percent African-American are definitely stationary. The remaining variables are almost certainly I(1) except for the proportion of the population between 35 and 44 where the results depend on whether one includes a trend, and the unemployment rate, which depends crucially on the number of lags. Overall, the variables appear to be nonstationary random walks.

According to Phillips and Moon (1999), pooled time series and cross section panel models are ideal for estimating the long run average relationships among I(1) variables. The pooled panel estimator yields consistent estimates with a normal limit distribution. These results hold in the presence of individual fixed (or random) effects (Phillips and Moon, 1999, 1088-1091). These regressions reflect long run average cross-section relationships (i.e., averaged over the time periods). This makes sense in that the cross section is usually assumed to reflect long run equilibrium relationships. They are analogous to the population (not sample) regression coefficients in conventional cross section regressions.

The problem with estimating a long run regression model is that the direction of causation cannot be easily determined. Therefore, a positive relationship between guns and crime could reflect that guns cause crime or that crime causes guns. However, a negative long run relationship between guns and crime would be indicative of crime reduction by guns.

Determining the direction of causation is somewhat easier in the short run. In a time series or panel of time series, lags can be used to identify causal relationships. If the lags are long enough, their existence could even eliminate the need for simultaneous equation modeling. In the following section, we estimate both long run static and short run dynamic panel data models of guns and crime.

5. Results: guns and crime.

The long run static results are presented in Table 4 below where we regress the variables in levels, using a fixed effects model to control for unobserved heterogeneity..

Insert Table 4 about here.

The coefficients on guns in all the crime equations are numerically very small and most are insignificantly different from zero. This indicates that the net effect of guns on crime in the long run is approximately zero. With respect to the individual crime categories, we find that handguns appear to have a positive effect on rape and a negative effect on robbery. However, the coefficients are numerically very small, so that a doubling of the proportion of households reporting handgun ownership will increase rape by 2.5% and decrease robberies by the same amount.

With respect to the sanctions, prison incarceration is significantly negatively associated with major crime, but apparently has no significant effect on murder. The reverse is true of arrests, which have no significant effect on major crime but a strong deterrent effect on murder.[4]

We now transform to stationary, I(0) variables by taking first differences. This allows us to model the short run fixed effects model of crime and guns using standard testing procedures[5]. An additional advantage of first differences is that the independent variables are close to orthogonal, minimizing the effects of multicollinearity and possible omitted variables.

In Table 5 below we show the result of a series of Granger causality tests (Granger, 1969) relating guns to crime.

Insert Table 5 about here.

The individual coefficients show whether the estimated effect occurs in the first or second year. The long run effect is a function of the sum of the two lag effects. We therefore report the sum of the two lag coefficients and the F-test on the null hypothesis that the sum of the coefficients is zero. We find that there is some evidence that handguns deter major crime in general and murder, robbery, and burglary in particular. However, the estimated coefficients are numerically small again indicating a small deterrent effect.

[4] Executions were never significant in any of our murder equations.
[5] Taking first differences of nonstationary I(1) variables transforms them to stationary I(0) variables so that all the usual tests are relevant. First differences of I(0) variables remain stationary.

Prison is found to have a significant lagged deterrent effect on murder and robbery. Arrests apparently have no lagged effect on crime.

These results differ from those of Duggan (2001) where he reports regressions of the first differences of the log of murders on the lagged first differences of the log of guns, measured as the circulation of *Guns & Ammo* magazine. We attribute the difference between Duggan's results and ours to our use of a better proxy. As Table 1 above shows, the percent gun suicide is much more highly correlated with GSS survey data than *Guns & Ammo*, and our proxy, which uses actual GSS data where it is available, is even more highly correlated with GSS data.

In Table 6 below we test whether crime granger-causes handgun prevalence.

Insert Table 6 about here.

Examination of Table 6 indicates that, with the possible exception of murder, lagged crime has no significant effect on the prevalence of handguns. Although not reported in Table 6, the primary cause of handguns is lagged handguns. Therefore, there is some possibility that handguns are exogenous in the crime equation. These findings also turn out to be useful in identifying the simultaneous equation model below.

Granger causality is neither necessary nor sufficient for simultaneous equation bias. Even if independent in a Granger causality sense, there could be contemporaneous causality running in either direction between guns and crime or prison and crime. In Table 7 below we report estimates of the short run crime equation based on first differences. We apply the Hausman-Wu test (Hausman 1978, Wu 1973) reported as T(HW) in Table 7, to determine if there is significant simultaneous equation bias present in the crime equation. We find evidence of simultaneous equation bias only in the robbery equation. This implies that we are justified in employing ordinary least squares to estimate the crime equation. However, the Hausman-Wu test is notoriously weak, so we estimate both two-stage least squares and OLS versions of all equations. We rely on the (realistic) assumption that there is a significant lag between the commission of a crime and the eventual incarceration of the criminal. Consequently, we assume that prison population is predetermined in the crime equation.[6] We also make no attempt to identify the arrest equation. Because we could not identify equations with the arrest variable included, we omit the arrest variable from the equations reported in Table 7. The coefficients on prison therefore incorporate the effects of arrests, and other law enforcement activities.[7]

Insert Table 7 about here.

We find numerically small, but significantly positive coefficients on handguns for HG in the two stage least squares versions of the major crime and burglary equations and for major crime and robbery in the PHG equations. The numerical values are again very small, even when significantly different from zero. So, for example, a doubling of the number of households reporting handgun ownership would cause major crime to increase by 1.5 percent. Prison, reflecting law enforcement in general, is found to have a significant deterrent effect on major crime in general and rape, robbery, and burglary in particular. The Basmann (1969) test for overidentification is significant in the rape and

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[6] We used the Hausman-Wu procedure to test for exogeneity of the prison population using Levitt's (1996) prison overcrowding litigation dummies as identifying variables. Predicted prison was not significant in the test equation, indicating that it is exogenous for the crime equation.

[7] Including the arrest variable produced coefficient estimates on guns and prison that are essentially the same as those reported in Table 7. All equations include lagged crime, percent urban, percent black, the change in population, income, lagged income, age groups, and year dummies.

burglary equations, indicating that these models may not be valid reductions from the reduced form equations. However, since the Hausman-Wu test is insignificant for both of these models, the OLS versions are preferred for these equations.

The ordinary least squares estimates are numerically very close to the two stage least squares estimates for all the crime categories. Again, we find numerically small, occasionally significantly positive contemporaneous effect of handguns on crime. Using the ordinary least squares estimate of the major crime equation, a hundred percent increase in the reported ownership of handguns would increase major crime by less than one percent. Prison is again found to have a significant deterrent effect on major crime, rape, robbery, and burglary.

In Table 8 below we report the results of our estimates of the handgun equation. The Hausman-Wu test was significant only for the assault equation. The Basmann test for overidentification was not significant for any of the two stage least squares estimates, indicating that these models are correctly identified. We find some evidence that major crime causes handgun prevalence. There is also some evidence that rape and assault causes handgun ownership.

Insert Table 8 about here.

6. Summary and conclusions.

We estimate several models of handguns and crime. Since handguns can be used by criminals to cause crime and by citizens to deter crime, we are estimating in the crime equation the net effect of guns on crime. Our overall conclusion is that guns have a numerically very small net effect on crime. Perhaps because the net effect is small, the evidence is mixed as to whether handguns have a positive or negative net effect. The evidence from the Granger causality analysis is that there is generally a small deterrent effect. However, the long run analysis showed no significant effect on overall crime with a positive effect on rape and a deterrent effect on robbery. Because of the difficulty of identifying the long run effect, the positive effect on rape might be the result of reverse causation running from rape to guns. The results of the short run analysis indicates generally numerically small but positive effects of handguns on crime. We also find some evidence that crime, especially rape, causes handgun prevalence. The latter reinforces the supposition that the positive relationship between rape and handguns found in the long run analysis reflects reverse causation running from crime to guns.

It is possible that the numerically small coefficients are the result of errors in variables, which tends to bias the estimated coefficients toward zero. However, this would imply that the ordinary least squares estimates would be much smaller than the two stage least squares estimates, which are consistent under errors in variables. Since we find that the coefficients are very similar for both OLS and 2SLS, and the Hausman-Wu tests indicate no significant difference between them, we are confident that the estimates are not underestimated because of errors in variables.

. We have replicated the analyses using overall gun prevalence (including long guns as well as handguns), imputed values of gun prevalence, and using the proxy variable percent gun suicide. Our results are consistent throughout. The estimated net effect of guns on crime, however guns are measured or modeled, is numerically very small, sometimes positive, sometimes negative.

In conclusion, we find that handguns have a negligible effect on crime. This result can be interpreted in two ways. The fact that we do not find much significant causation in either direction could be the result of criminals, as opposed to guns, causing crime. In other words, criminals simply use the tools available to them to commit crime. If guns are readily available, they use guns, if not, they use other weapons, the rate of crime being unaffected by the presence or absence of guns. On the other hand, criminals certainly acquire guns to commit crimes and citizens also acquire guns to protect themselves from crime. There may be a rough balance between criminals who use guns in the commission of crime and citizens who use guns to defend themselves and deter crime.

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Table 1
Correlation of proxy variables to General Social Survey responses

Proxies		*HG*	*GUN*
Percent Gun Suicide PGS	(n=1071)	.608	.668
Am. Rifleman Magazine	AMRMS (n=969)	.195	.254
Am. Hunter Magazine	AMHMS (n=969)	.228	.381
Am. Handgunner Magazine	AMHGS (n=1122)	.203	.189
Guns & Ammo Magazine	G&A (n=1173)	.297	.342
Imputed HG	PHG (n=1104)	.777	
Imputed GUN	PGUN (n=1104)		.812

Note: HG is the number of households reporting a handgun, GUN is the number of households reporting a gun, including handguns, rifles, or shotguns. Variables measured in their natural units.

Table 2
Regressions of HG and GUN on percent gun suicide.

Dependent Variable	*PGS*	*Intercept*	*R-square*	*n*
HG	.681	-15.78	.35	498
	(16.38)	(6.15)		
GUN	.951	-10.81	.42	498
	(19.10)	(3.52)		

Note: t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Variables measured in their natural units.

Table 3
Variable names, definitions, and means.

CRMAJ	major crime per million population	16202.37
CRMUR	murder per million population	72.979
CRRAP	rape per million population	356.10
CRROB	robbery per million population	1560.26
CRASS	assault per million population	2992.59
CRBUR	burglary per million population	11220.44
HG	GSS handgun prevalence (%)	25.911
PHG	imputed handgun prevalence (%)	25.342
GUN	GSS gun prevalence (%)	48.443
PGUN	imputed gun prevalence (%)	47.094
PGS	percent gun suicide	60.214
GUNSAMM	Guns & Ammo circulation	2415.64
AMHGS	American Handgunner circulation	592.832
AMHMS	American Hunter circulation	6504.58
AMRMMS	American Rifleman circulation	6602.79
PRISON	prison population per capita	2360.08
AOMAJ	arrest rate, major crime	110.973
AOMUR	arrest rate, murder	44.588
AORAP	arrest rate, rape	18.838
AOROB	arrest rate, robbery	16.916
AOASS	arrest rate, assault	23.001
AOBUR	arrest rate, burglary	7.100
METPCT	percent urban	64.201
AMPCT	percent black	10.212
MILITARY	military employment	5.061
EMPLOY	total employment	26.860
UNRATE	unemployment rate	6.217
RPCPI	real per capita personal income	13.229
RPCIM	real per capita income maintenance	0.171
RPCUI	real p.c. unemployment insurance	0.068
RPCRPO	real p.c. retirement payments	0.484
P1517	percent population 15-17	4.636
P1824	percent population 18-24	11.232
P2534	percent population 25-34	16.198
P3544	percent population 35-44	14.251
P4554	percent population 45-54	10.567
P5564	percent population 55-64	8.689
POP	population	4963.10

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Table 4
Long Run Models

crime	*HG*	*PHG*	*Prison*	*Arrests*	R^2
Major Crime	-.005	-.013	-.127**	.002	.98
	(0.52)	(1.44)	(4.36)	(0.14)	
Murder	-.020	-.014	-.085*	-.051**	.91
	(1.22)	(.91)	(1.71)	(2.60)	
Rape	.017	.025**	-.131**	-.096**	.89
	(1.52)	(2.24)	(3.65)	(5.54)	
Robbery	-.016	-.025*	-.169**	-.061**	.96
	(1.05)	(1.66)	(3.54)	(2.99)	
Assault	.000	-.005	-.098**	-.090**	.94
	(0.03)	(.40)	(2.46)	(5.10)	
Burglary	.001	-.005	-.248**	-.061**	.92
	(.06)	(.57)	(9.24)	(4.45)	

Notes: All variables measured in logs, t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Each equation also contains percent urban, percent African-American, employment, the unemployment rate, the six age groups, the four income categories, and state dummy variables as control variables.

Table 5
Granger Causality Analyses

	Major crime	*Murder*	*Rape*	*Robbery*	*Assault*	*Burglary*
HG(-1)	-.009	.004	-.016	-.010	.026	-.015
	(0.80)	(0.15)	(0.74)	(0.47)	(1.57)	(1.15)
HG(-2)	-.000	.031	-.010	-.001	.022	-.007
	(0.04)	(1.30)	(0.59)	(0.08)	(1.63)	(0.67)
Sum	-.009	.035	-.026	-.011	.048	-.022
F	0.26	0.57	0.62	0.11	3.05*	1.08
PHG(-1)	-.009**	-.020*	-.013**	-.015**	-.007	-.008*
	(2.38)	(1.73)	(2.04)	(2.33)	(1.32)	(1.92)
PHG(-2)	-.004	-.032**	-.004	-.004	-.007	-.002
	(1.02)	(2.72)	(0.56)	(0.62)	(1.27)	(0.59)
Sum	-.013**	-.052**	-.017	-.019**	-.014	-.010
F	3.84	6.60	2.23	2.85	2.22	2.09
Prison(-1)	-.346**	-.746**	-.277	-.658**	-.178	-.459**
	(3.35)	(2.36)	(1.52)	(2.33)	(1.15)	(4.04)
Prison(-2)	.233**	.187	.399**	.290	.100	.300**
	(2.28)	(0.61)	(2.27)	(1.64)	(0.67)	(2.67)
Sum	-.113	-.559**	.122	-.368**	-.278	-.159
F	1.50	4.09	0.60	5.23	0.32	2.40
Arrests(-1)	.010	-.001	.017	.014	.006	-.002
	(1.53)	(0.06)	(1.39)	(1.14)	(0.84)	(0.21)
Arrests(-2)	-.001	.019	.011	.002	.011	-.029**
	(0.10)	(1.13)	(0.87)	(0.16)	(0.84)	(2.70)
Sum	.009	.018	.028	.016	.017	-.031*
F	0.63	0.40	1.83	0.60	0.68	3.36
R^2	.12	.27	.10	.11	.09	.13

Notes: All variables measured as first differences of logs, t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Each equation also contains first differences (logs) of percent urban, percent African-American, employment, the unemployment rate, the six age groups, and the four income categories. Year dummy variables were not significant.

Table 6
Does Crime Granger-Cause Handguns?

Variable	*Handguns*		*Imputed Handguns*	
	Coefficient	T-Ratio	Coefficient	T-Ratio
Crime(-1)	-1.037	0.42	-.168	0.46
Crime(-2)	2.415	0.82	.400	1.10
Sum	1.378		.232	
F(sum)	0.15		0.21	
Murder(-1)	-1.004	1.27	.072	0.66
Murder(-2)	1.170	1.55	.223**	2.04
Sum	.166		.295	
F(sum)	0.90`		2.49**	
Rape(-1)	-2.704	1.51	-.031	0.16
Rape(-2)	.094	0.05	.118	0.59
Sum	-.610		.087	
F(sum)	0.90		0.08	
Robbery(-1)	-.337	0.24	.090	0.44
Robbery(-2)	-1.310	0.97	.239	1.21
Sum	-1.647		.329	
F(sum)	0.68		0.19	
Assault(-1)	1.506	0.86	.125	0.57
Assault(-2)	.596	0.33	.008	0.04
Sum	2.102		..133	
F(sum)	0.68		0.19	
Burglary(-1)	-.572	0.28	-.288	0.85
Burglary(-2)	2.439	0.97	.382	1.17
Sum	1.867		.094	
F(sum)	0.37		0.04	

Notes: All variables measured as first differences of logs, t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Each equation also contains first differences (logs) of percent urban, percent African-American, employment, the unemployment rate, the six age groups, and the four income categories. Year dummy variables were not significant.

Table 7
Short Run Analyses

	Major Crime	*Murder*	*Rape*	*Robbery*	*Assault*	*Burglar*
2SLS						
HG	.015**	-.010	-.010	.008	.007	.020**
	(2.37)	(0.52)	(0.84)	(0.69)	(0.62)	(2.59)
PHG	.008*	.025	.014	.016*	.006	.006
	(1.70)	(1.51)	(1.57)	(1.89)	(0.87)	(1.22)
Prison	-.364**	-.300	-.355	-.713**	-.009	-.465**
	(4.80)	(1.10)	(2.44)	(4.97)	(0.08)	(5.48)
R^2	.28	.27	.21	.23	.20	.28
F(overid)	1.14	1.17	1.41**	1.20	1.05	1.21*
T(HW)	1.22	1.22	0.03	1.99**	0.19	1.00
OLS						
HG	.006*	-.001	.018**	.008	.013**	.005
	(1.72)	(0.10)	(2.43)	(1.22)	(2.26)	(1.18)
PHG	.001	.004	.012**	.004	.005	.000
	(0.45)	(0.46)	(2.28)	(0.66)	(1.10)	(0.09)
Prison	-.464**	-.302	-.404**	-.737**	-.057	-.568**
	(6.97)	(1.32)	(3.31)	(5.94)	(0.52)	(7.72)
R^2	.37	.26	.19	.25	.17	.37

Notes: All variables measured as first differences of logs, t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Each equation includes as control variables first differences (logs) of percent urban, percent African-American, population, six age groups, four income categories, lagged crime, four lagged income categories, and year dummies. The list of instruments consists of all of the above plus circulation of the four gun magazines, total employment, military employment, the unemployment rate, all of the above variables lagged twice, and crime, prisons and handguns lagged twice. F(overid) is the Basmann test for overidentifying restrictions. T(HW) is the Hausman -Wu test for specification error.

*

Table Eight

Short Run Handgun Equation

	2SLS		*OLS*	
	HG	PHG	HG	PHG
Major crime	2.923**	0.673	2.237*	0.433
	(1.98)	(1.16)	(1.76)	(1.42)
Murder	.005	.064	.054	-.050
	(0.01)	(0.37)	(0.61)	(0.13)
Rape	-.233	.061	-.452	.398**
	(0.27)	(0.15)	(0.57)	(2.34)
Robbery	1.058	.251	0.591	.093
	(1.12)	(0.75)	(0.75)	(0.55)
Assault	1.594*	.050	1.024	.340*
	(1.71)	(1.07)	(1.24)	(1.76)
Burglary	2.008	0.418	1.679	.196
	(1.60)	(0.77)	(1.54)	(0.69)

Notes: All variables measured as first differences of logs, t-ratios in parentheses; * indicates significant at the .10 level (two-tailed), ** indicates significant at .05 level (two-tailed). Each equation includes as control variables first differences (logs) of lagged handguns, *Guns & Ammo* circulation, *American Rifleman* circulation, and the proportion of the population between 35-44 and 45-54. The Basmann test for overidentifying restrictions indicated that all equations were correctly specified. The Hausman-Wu test for specification error was not significant for any crime category, indicating that the OLS estimates are consistent.