An Evaluation of the 1977 Canadian Firearms Legislation: Robbery Involving a Firearm[1]

Concern about firearms violence has led many countries around the world to introduce increasingly restrictive firearms control regimes. Australia recently prohibited semi-automatic and pump rifles and shotguns; Canada introduced universal firearms registration and banned about half of all handguns; and the United Kingdom banned all handguns. Since restrictive firearms control regimes involve complex and expensive governmental bureaucracy, it is important to empirically evaluate gun control laws. If such laws cannot be shown to be effective in reducing criminal violence, it might be reasonable to explore more effective approaches to improving public safety.

The theoretical argument for restrictive gun control laws and regulations is straight forward. Firearms are potentially dangerous and are viewed as a "contributing cause" of lethal violence (Zimring and Hawkins 1997). It is argued that criminal violence can be reduced by restricting access to firearms. Thus, a variety of legal restrictions on firearms are introduced that encompass the general public with an eye to restricting firearms availability to potential criminals, and thus acting to reduce criminal violence (Zimring and Hawkins 1997, pp 121-125).

Alternatively, firearms may be seen as being morally ambiguous in that firearms may be used to prevent criminal violence as well as to commit

crimes (Kleck 1997; Lott 1998). For example, a woman who stops a rape by brandishing a revolver is using her firearm to prevent criminal violence not cause it. Second, the general public may not be homogeneous with respect to how they react to firearms ownership. People with a life-long record of obeying the law would be expected to respond differently to firearms regulations than would those with a criminal record. For example, a store owner will be discouraged from keeping a handgun at his store if he is unable to obtain the proper permit, but it is unlikely that a criminal would be similarly discouraged from getting a pistol if he wanted one. Studies of firearms recovered from Canadian criminals have found that the vast majority of them had never been registered as required by the Canadian criminal code (Axon and Moyer, 1994; Francis, 1995).

An alternative theoretical framework for examining gun control laws, utility theory, comes from economics. In this framework, criminals are seen as motivated by self-preservation, and so they can be deterred by threat of violence. Because they are afraid of getting hurt, they pick other targets, or give up.[2] Criminals are afraid of burglarizing homes or businesses where they suspect the home owner is armed (Wright and Rossi 1986, pp. 141 ff.). Thus, to the extent that such laws remove the deterrent of widespread citizen firearm ownership, stricter gun control regimes may result in more, not less, criminal violence. Criminals, as they become aware that their victims are less likely to be armed, due to the stricter gun laws, will be motivated to rob

or to attack targets they would have been afraid to tackle had they believed their victims been armed (Lott and Mustard 1997).

But what is the empirical support for restrictive firearms control legislation? A recent review of American studies concluded that "the most technically sound evidence indicates that most types of gun control have no measurable net effect, for good or ill, on rates of most types of crime and violence (Kleck 1997, page 377). Outside of the United States, the situation is no better. Surprisingly, only a very few government reports are available that evaluate the effectiveness of gun control regimes from those democratic countries with the most restrictive gun control regimes (e.g., Australia, Canada, New Zealand, the United Kingdom). In all, there are very few studies of the effectiveness of gun control regimes that have been published in the criminological literature, and most of the studies that are available are not supportive.

Canada's is one of the few countries outside of the United States where empirical studies of their gun control regime have been published. In 1977 Canada introduced a firearm law that may be seen as a precursor of the wave of subsequent firearm legislation that has since swept around the world. The 1977 legislation, and its associated regulatory changes, was an omnibus piece of legislation that introduced a five-year police permit to purchase any firearm (the Firearms Acquisition Certificate), banned certain types of firearms, centralized the registration requirements for "restricted weapons" (which are mostly handguns), eliminated the option of keeping a handgun

in a place of business, and eliminated "protection of property" as a legitimate reason for owning handguns.[3] At that time, universal firearm registration was rejected by the government, partially because handguns had already been the subject of strict registration and tight control since 1934.[4]

The results of empirical studies of the 1977 Canadian legislation have been mixed. Researchers have almost exclusively limited themselves to examining the impact of this legislation upon homicide (Scarff 1983; Sproule and Kennett 1988; Mundt 1990; Mauser and Holmes 1992; Department of Justice 1996). Three of these studies did not find a significant impact of the 1977 legislation on homicide (Sproule and Kennett 1988; Mundt 1990; Mauser and Holmes 1992), while the two government studies did (Scarff 1983; Department of Justice 1996).

This is the first paper to empirically examine the effect of the 1977 Canadian firearms legislation on robbery. It will examine robbery, armed robbery, and robbery involving a firearm. A Department of Justice study included an analysis of robbery (and robbery involving a firearm) in its evaluations of the 1977 firearms legislation, but curiously no results were reported. The authors comment laconically that this omission was due to "excessive autocorrelation" (Department of Justice 1996, p 94). Few econometricians would accept autocorrelation as a sufficient reason to stop analysis. To my knowledge, the only previous papers that have examined the impact of firearm legislation upon robbery have looked at the impact of

concealed handgun legislation in the United States (Lott and Mustard 1997; Lott 1998).

Robberies, and especially armed robberies, constitute an important threat to the peace and security of modern citizens. Robberies in Canada cost residents an estimated $90 million in 1996 (Brantingham and Easton 1998). Statistics Canada reported that there were 29,590 robberies in Canada in 1997, in about half of these (15,411) the perpetrator was armed with a weapon of some sort. Over one-third of armed robberies (5,478) involved a firearm (Kong 1998). Armed robbery statistics are reported on an annual basis using a Uniform Crime Reporting system; although detailed information on the type of weapon involved in a robbery is only available after 1974. Even though there are many more robberies than homicides, police are much less successful in dealing with robbery than they are with homicide. Out of the 596 homicides known to police in Canada in 1994, the police "cleared" 80% of them, while only 33% of the 28,888 robberies that year were cleared by the police (Brantingham and Easton 1998). Figure 1 shows the trend in robberies in Canada since 1974.

Figure 1 about here

Methodology

A pooled cross-section, time-series model is used to estimate the statistical importance of the independent variables including the 1977 firearms legislation (Kmenta 1986). Building upon the independent variables one of the authors used in an earlier study, we included as wide a set of relevant independent variables in this time-series model as insight and data availability allowed (Mauser and Holmes 1992). We believe that the breadth of this set will increase the power of tests designed to isolate the effects of GUNLAW while simultaneously reducing the probability of erroneously attributing to GUNLAW the effects of other variables.

Three classes of independent variables are included in the model: (a) criminological variables (eg, clearance rates), (b) sociological variables (eg, percentage male youth), and (c) index variables (eg, effect of the 1977 firearms legislation). Previous studies have demonstrated the importance of criminological variables such as arrest and conviction rates (Erlich 1975; Lott and Mustard, 1997). The clearance rate is a useful index of the probability of a perpetrator being arrested and convicted (Lott 1998). We also included the number of police effectives as a proxy for the probability of a perpetrator being caught, or for the differences in the proportion of crimes that are committed which are reported.

A number of researchers have argued that sociological variables, principally sex or ethnic differences, are important factor in crime rates (Lenton 1989; Williams 1984). In this model, we have included percent male

youth in the population, various indices of immigration, as well as the aboriginal share of the population. Aboriginal status has been found to be strongly linked with homicide (Silverman and Kennedy, 1993). A few unpublished Canadian studies have looked at immigration and crime rates (Samuel and Santos 1990; Thomas 1990). One study found that both immigration and ethnicity were important factors in determining Canadian homicide rates (Mauser and Holmes 1992). Ethnicity has been found to be an important factor in identifying who commits robbery in both Canada and the United States (Desroches 1995).

The independent variables also include a broad set of socio-economic and legal indices measured at the provincial level for all ten Canadian provinces. A dummy variable was used to evaluate the 1977 gun-control legislation ('0' before its introduction; '1' afterwards). In Canada, gun-control legislation is part of the national criminal code, and it was, in principle, introduced in all provinces at the same time. However, there are important provincial differences in how this legislation was introduced that stem from the provinces having the constitutional responsibility for administering the criminal code. There are three dependent variables: the total robbery rate, the 'armed robbery' rate, and the 'robbery involving a firearm' rate. All dependent variables are 'actual crimes' calculated per 100,000 provincial population.

In sum, there are nine independent variables included in this model: (1) population per serving police officer in the province; (2) unemployment

rate (for both sexes), (3) weeks of Unemployment Insurance (UI) benefits paid per capita; (4) percentage Status Indian, (5) percentage male youth (between 15 and 24); (6) percentage of the population that immigrated to Canada and settled in a province over the past three years; (7) interprovincial migration rates over the past five-years; (8) the percentage of the population that are non-permanent residents; and (9) the clearance rate, which is the percentage of known crimes "cleared" by bringing charges or resolved in an acceptable manner. Finally, linear time trends were included for each province as well as provincial dummy variables. We are of course limited by the availability of data. No information is available concerning firearm involvement in robbery before 1974. (See Table 1 for more details.)

Some researchers have argued that it is necessary to lag the clearance rate. The argument is that perpetrators' decisions are influenced by the chance of being caught and convicted in the past. Even if we accept the validity of lagging, there is still the question of choosing the proper time frame. We argue that criminals are more influenced by last month's probability of being caught than by the previous year's value.[5] Hence, we believe the current value of the clearance rate is more important than the previous year's value. In this paper, we will investigate both lagged and unlagged versions of the clearance rate to determine if this difference is important empirically.

Ideally, our goal was to get complete information on all variables for all ten provinces and for both territories. This proved possible for almost all

of the ten provinces. Unfortunately, the territories had to be excluded because neither unemployment rates nor immigration data were available before the mid-1980's. It was necessary to interpolate the number of Status Indians for Newfoundland for 7 out of the 18 years included in the data set.

Tables 1 & 2 about here

The authors recognize the difficulties in using provinces as the unit of analysis. Ideally, neighborhoods or census tracts should be used because they would provide a closer link between social indices and criminality. Provinces were used here because they are the smallest units for which such a wide range of information is available over the entire time period since 1974. Despite the methodological limits of this study, the authors believe that the results will shed light on important social questions. Policy decision makers cannot always wait for perfect data; decisions must be made on the best data available. (The data set is described more fully in Table 2.)

One of the more intractable problems in econometric modeling is the problem of specification error. The results of a model are highly dependent upon the variables specified as important enough to include. But, since only a few variables may be included, researchers never know if the addition or deletion of another variable would radically alter the results. This problem is

particularly pernicious in criminology because there are so many variables that might be included, and because researchers differ so widely about which variables are theoretically important. Despite the large number of independent variables included in this model, we have not included all variables that have been theoretically hypothesized as important. Nor have we even included all of the variables we would like to have included: eg, arrest rates, conviction rates, the expected length of prison sentence, or the recidivism rates.

Consequently, in this paper all possible subsets of independent variables are analyzed for each dependent variable to ensure that the results are not simply due to a unique combination of independent variables. Since there were nine IVs, this gives 512 equations (one with no independent variables, nine with only one variable, 36 with two variables, 84 with three variables, 126 4-tuples, 126 5-tuples, 84 6-tuples, 36 7-tuples, nine 8-tuples, and one with all nine variables).

Figure 2 about here

Results

The data for FR (robbery involving a firearm) are plotted in Figure 2, with the sequence shown being time series of 19 annual observations (1974-

1992) within each province; provinces are arranged from east-to-west. Quebec's spike juts up boldly near the center. Three broad points are apparent from the data: (1) the mean of FR varies greatly among provinces, with Quebec having a much higher mean than any other province; (2) the trends in FR vary among provinces, with the Atlantic provinces displaying virtually no trend, Quebec displaying a strong negative trend, and the western provinces displaying positive trends; and (3) the variance of FR, even adjusted for trend, is noticeably higher in some provinces than in others. Each of these data characteristics has implications for the estimation of equations.

It is of course possible that the three characteristics of the raw data apparent in Figure 2 could be "explained" by the set of independent variables introduced into the estimated equations. This happy circumstance usually does not occur, due to the very large number of factors which cause these differences among provinces coupled with either the lack of insight on the part of the researchers specifying the equations and/or lack of available data to measure some factors which might be deemed relevant. A common approach to this problem is to introduce provincial dummy variables (intercept shifts) to deal with variation in means, province-specific time trend variables to capture variation in trends, and some form of estimated generalized least squares estimation to deal with heteroscedasticity (Gujarati 1995). Tests are available to help determine whether these adjustments to the estimation procedure are necessary.

In the preliminary analysis, we used OLS to estimate the most appropriate pooled regression model. Testing to see whether provincial dummy variables are necessary, given the set of nine independent variables plus GUNLAW, produces an F-test value of 118.26, with (9,170) degrees of freedom, easily significant at the .001 level. Hence, the data indicate provincial dummy variables are necessary. Testing to see whether province-specific time trends are necessary, given the set of nine independent variables, GUNLAW and the provincial dummy variables, produces an F-test value of 13.38, with (10,160) degrees of freedom. This is again easily significant at the .001 level. Hence, provincial dummy variables and province specific trend variables are included in all estimations reported and discussed in this paper.

Table 3 shows the complete pooled regression model on all three dependent variables: (1) FR - robbery involving a firearm, (2) AR - armed robbery, and (3) TR - total robbery. These models include all independent variables considered in this paper.[6] The effect of the legislation on FR is positive but not significant. The direction is somewhat surprising, as the effect of the legislation was hypothesized to decrease, not increase, firearms crime. However, the direction is unimportant as the effect is statistically insignificant.

Table 3 about here

In order to investigate the sensitivity of the model to specification error, all possible combinations of the nine independent variables were run. Significance (for both GUNLAW and the nine independent variables) was based on an absolute t-value of 1.65, which approximates a 5 per cent significance level for one-tailed tests. Thus, of 512 possible runs, if a variable (e.g. GUNLAW) had no real effect, one would expect the coefficient to obtain a t-value less than -1.65 five per cent of the time, or approximately 26 times out of 512. Similarly a t-value greater than 1.65 would obtain in approximately 26 estimations out of 512 under the same assumption of no real effect.

The results (see Table 4) for OLS estimation using robberies involving firearms (FR) as the dependent variable and CR unlagged produced 192 of 512 runs where the t-value of GUNLAW was greater than 1.65 in absolute value. Of these, 101 were negative values and 91 were positive. Note that each of these latter numbers is almost four times as large as the number of significant values expected if there is no effect of GUNLAW on FR. This would lead to the conclusion that there is a significant effect of GUNLAW on robbery rates involving firearms, but that this effect is approximately as likely to be positive as it is to be negative. Our initial conclusion is that the 1977 firearms legislation had no significant effect on robberies involving firearms .

The results noted above mean that persons who want to show that GUNLAW had a significant negative effect on FR (or have the expectation

that this is true) because they believe the legislation makes it more difficult for potential robbers to obtain access to firearms or makes them less likely to use available firearms can find a large number of empirical specifications to support their position. Similarly persons who want to show a significant positive effect on FR (or have the expectation that this is true) because they believe the legislation makes it more difficult for potential victims to defend themselves, or makes potential robbers believe potential victims are less able to defend themselves, can find a large number of empirical specifications to support their position. Since these specifications differ only as to the set of independent variables included, it becomes important to examine: (i) which independent variables appear to "matter," and (ii) any patterns among which groups of independent variables lead to positive versus negative significance for the coefficient of GUNLAW.

Dealing with the first question, YOUTH, tested one-tailed with a positive expectation is significant in 58.8 per cent of the 512 equations using FR as the dependent variable, OLS as the estimation technique, and no lag on the clearance rate. Using the same convention, UNEMP is significant 37.9 per cent of the time, INDIANR 37.5 per cent of the time, TYIMMR 23.0 per cent of the time, and FYIPMR and NPRR are never significant. Testing one-tailed with a negative expectation, CRFR is significant 11.5 per cent of the time and WPPC 5.5 per cent of the time. POPPOL is tested two- tailed and is significant 15.4 per cent of the time (negative 10.9 per cent of the time and positive 4.5 per cent of the time). It thus appears that FYIPMR, NPRR and

WPPC are not variables which "matter" in explaining FR. There are two reasons why this would be a premature conclusion. First, FYIPMR and NPRR are sometimes significant if province-specific time trends are not included, i.e., these variables may actually "matter," but their effect is picked up by the time trends.

Second, in the case of WPPC, the variable is only interpretable with a negative expected sign if UNEMP is also included in the equation, i.e. the positive effect of high unemployment on robbery rates is ameliorated by the negative effect of unemployment insurance payments. Since WPPC and UNEMP are strongly positively correlated (r=0.924), in specifications where WPPC is included while UNEMP is excluded, WPPC will have a strong positive coefficient (which we do not "count" as significant since we do one-tailed tests for negative significance). In the 130 estimations where UNEMP and WPPC occur together, WPPC is negative and significant 28 times, or 21.5 per cent of the time. There is thus strong evidence that WPPC "matters."

Turning to the second question, which deals with patterns of independent variables, in the estimations using OLS, FR as the dependent variable and no lag on CR, the following generalizations hold:

- TYIMMR is never present in any estimations where GUNLAW is negative and significant,

- YOUTH is never present in any estimations where GUNLAW is positive and significant, and

- when TYIMMR and YOUTH are both included GUNLAW is never significant.

Since any given independent variable is included in 187 estimations and there are "only" 101 significant negative coefficients and 91 significant positive coefficients for GUNLAW, it is obvious that the *presence* of any independent variable is not sufficient to guarantee significance. WPPC, FYIPMR and NPRR are relatively weak variables, and it appears not to matter for purposes of significance of GUNLAW whether these variables are included. Finally, INDIANR, UNEMP, CRFR and POPPOL are found in some specifications where GUNLAW is significant and negative and some specifications where GUNLAW is significant and positive. Hence, in order to make a case that the FCR has reduced rates of robberies involving firearms, it is necessary to argue that TYIMMR does not belong in the equation. Symmetrically, in order to make a case that the FCR has increased rates of robberies involving firearms, it is necessary to argue that YOUTH does not belong in the equation. Since we find both of these variables to be reasonable on a priori grounds, we conclude on the basis of the OLS runs that the 1977 Firearms Act has had no effect on robbery rates involving firearms.

If firearm legislation has no significant effect on robberies involving firearms (FR), one would hardly expect the legislation to have an impact upon armed robberies (AR), or upon total robberies (TR). As may be seen in Table 3, no significant effect was found for the firearm legislation on either armed robbery or total robbery.

Analyzing the full set of 512 OLS equations for both total robbery or armed robbery gave results similar to that of robbery involving a firearm, i.e., that the results were highly sensitive to which variables were included in (or excluded from) the model. For armed robbery, 115 models where GUNLAW is negative vs. 88 where it is positive; and for total robbery, there were 66 models where GUNLAW is negative and 123 where it is positive. Thus, we concluded, on the basis of the OLS estimates, that there was no significant effect of the 1977 firearms legislation upon either armed robberies or total robberies.

Table 4 about here

The original intent was to examine possible substitution between firearm robberies and other types of robberies by comparing the coefficients of GUNLAW in equations with different dependent variables. Since GUNLAW is generally non-significant, this is not a useful exercise. The three dependent variables are quite similar to each other: the simple correlations between FR and AR is 0.957, between FR and TR is 0.872 and between AR and TR is 0.965. In the 6,144 runs reported, the largest negative value obtained for GUNLAW was -2.8614. The largest positive value was 4.7491.

We also examined the effect of lagging the clearance rate on the result. Lagging made no important changes in the interpretation on any of the three dependent variables. (See Table 4). For robberies involving a firearm (FR), the proportion shifted from 101 negative; 91 positive, when the clearance rate was unlagged, to 101 negative: 93 positive, for the lagged clearance rate. For armed robberies (AR), the changes were similarly trivial: from 115 negative, 88 positive, when the clearance rate was unlagged, to 119 negative: 95 positive, for the lagged situation. For total robberies (TR), the ratio went from 66 negative: 123 positive, when unlagged, to 76 negative: 117 positive, when lagged.

Tables 5 and 6 about here

While OLS estimation of the model provides unbiased coefficient estimators, these estimators are not efficient due to the simultaneous presence of hetroscedasticity and autocorrelation. These problems may be dealt with by using a pooled data estimation technique, called generalized least squares (GLS),[7] which simultaneously corrects for first order autocorrelation in the time series within each province (allowing for different estimated rho values for each province), as well as heteroscedasticity of the form where variances differ among provinces. The nature of these corrections can be illustrated by noting the estimated rho

values and variances (diagonal values of the phi matrix) for the GLS estimation using FR as the dependent variable and including provincial dummies, province-specific time trends, all nine independent variables plus GUNLAW. The rho values are: 0.28, 0.04, 0.31, 0.18, 0.17, 0.31, -0.28, 0.50, 0.02 and 0.23 (in east-to-west province order). The variances are: 1.18, 8.63, 3.16, 2.20, 106.75, 4.20, 9.98, 7.19, 8.64 and 12.11 (in the same order). The autocorrelation is thus generally minor, and the main effect of the GLS estimation is to reduce the importance of the Quebec observations, due to the large relative variance for that province.

Table 5 shows the complete pooled regression model using estimated generalized least squares (GLS) on all three dependent variables: (1) FR - robbery involving a firearm, (2) AR - armed robbery, and (3) TR - total robbery. These models include all independent variables considered in this paper. The effect of the legislation on both FR and TR is positive and significant, but it is not significant for AR. This implies that the 1977 firearm legislation acted to increase the numbers of robberies and robberies involving a firearm, but was not found to have an effect on armed robberies in general.

Table 6 shows that using estimated generalized least squares (GLS) estimation a large number of specifications yield positive significant coefficients for GUNLAW with almost no specifications yielding negative significance. This holds for all three dependent variables.

Analyzing the GLS estimations for the FR dependent variable with CR used unlagged further, the smallest t-value for GUNLAW was -1.57, so there

were no cases of negative significance at 5 per cent, but there are some cases of "near significance." The patterns of significance for the nine independent variables are very similar to those reported for the OLS estimations. The preponderance of the specifications where positive significance occurs for GUNLAW include the TYIMMR variable, but since only 187 specifications include this variable and there are 236 cases of positive significance, it is obvious that the presence of TYIMMR is not necessary to obtain positive significance for GUNLAW. The main difference in the OLS results compared to the GLS results is that in the former, when TYIMMR was included with YOUTH the result was nonsignificance for the coefficient of GUNLAW, while now the result is positive significance. We interpret these results as providing evidence in favour of the hypothesis that gun control legislation can lead to an increase in robbery rates, presumably due to a perception on the part of potential robbers of greater vulnerability among potential victims.

Accepting that the results support a positive effect of GUNLAW on all three dependent variables, it is then relevant to look at patterns of substitution. Results for the coefficient (t-value) of GUNLAW in estimations using GLS, provincial dummies, province-specific time trends and all nine independent variables (full estimation results available from the authors upon request) are: FR dependent, 1.58 (1.81); AR dependent, 1.56 (0.99) and TR dependent 4.52 (2.11). Using the fact that the coefficient of GUNLAW in the TR equation is over twice as large as it is in the FR equation, one could argue that since total robberies increased more than robberies involving

firearms, there was a substitution away from firearms. Since even the "relatively large" coefficient of GUNLAW in the TR equation is about twice its own standard error, any conclusion about substitution is based on very weak evidence.

The effect of lagging the clearance rate on the result was also examined for the GLS estimates. As with the OLS, lagging made no important changes in the interpretation on any of the three dependent variables. (See Table 6). For robberies involving a firearm (FR), the results shifted from 0 negative; 236 positive, when the clearance rate was unlagged, to 10 negative: 158 positive, for the lagged clearance rate. For armed robberies (AR), the changes were even more trivial: from 0 negative, 144 positive, when the clearance rate was unlagged, to 0 negative: 183 positive, for the lagged situation. For total robberies (TR), the ratio went from 0 negative: 246 positive, when unlagged, to 0 negative: 239 positive, when lagged.

Conclusions

This is the first paper to empirically examine the effect of the 1977 Canadian firearms legislation on robbery, armed robbery, and robbery involving a firearm. Previous researchers have almost exclusively limited themselves to examining the impact of this legislation upon homicide or suicide. An earlier paper by the Department of Justice of Canada study attempted an analysis of armed robbery in their evaluations of the 1977 firearms legislation, but no results were reported due to “excessive

autocorrelation." This excuse is most curious because only relatively minor problems with autocorrelation were found in our analysis.

A pooled cross-section, time-series model was used to estimate the statistical importance of the independent variables including the 1977 firearms legislation (Kmenta 1986). The results of the OLS (ordinary least squares) estimation show that the 1977 Canadian firearms legislation did not act to reduce robbery involving a firearm. Logically, given these results, one would not expect to find a significant effect on either the total robbery or armed robbery rates. That is what this analysis found: the 1977 legislation did not have a significant effect on either the total robbery or armed robbery rates. These results are consistent with previous published findings but contrast with two unpublished government studies. Not one of the independent empirical studies of the 1977 Canadian firearms legislation found that the legislation had a significant effect on firearms crime. The only studies reporting finding a significant link have been reports issued by the Canadian Department of Justice.

On the other hand, the GLS (generalized least squares) estimates indicate that the 1977 Canadian firearms legislation may have acted perversely to increase robbery involving a firearm, as well as increasing both total robbery and armed robbery rates. The primary difference between the OLS estimation and the GLS estimation is how the model treats the Quebec data. The Quebec robbery rates are dramatically higher and more variable than the rest of Canada; this means that Quebec has a tremendous impact

upon the results. When generalized least squares (GLS) estimation is used, the impact of Quebec is reduced, the firearm law is found to be positively related to all three dependent variables: total robberies, armed robberies, and robberies involving firearms.

Thus, the GLS estimation implies that the 1977 firearms legislation acted to increase both robberies involving firearms and armed robberies by 1.6 points, and increasing the total robbery rate by 4.5 points. This implies that this legislation not only did not reduce armed robberies, but is estimated to have increased the numbers of all classes of robberies. Between 1978 and 1992 this translates into an increase of 3,322 armed robberies, and an increase in the number of total robberies of 17,069. Based upon the estimates of Brantingham and Easton (1998) each robbery costs Canadian residents around $3,000. Using this approach, the 1977 firearms legislation cost Canadian residents an estimated $51 million between 1974 and 1992.

How could such a thing happen? The goal of a firearms control regime is to reduce, not increase, violent crime, so to find the converse is somewhat surprising. However, it should be unsurprising to say that human intentions are not always translated into the expected results. The inevitable corollary is that government policy occasionally has unexpected consequences. It may be instructive to examine some examples where government policy has had unexpected consequences. Studies have shown that widening public roads may cause drivers to increase driving speeds and to take more risks (Adams, 1985, 1995). The Endangered Species Act is

argued to emperil the very species that it is supposed to protect (Schrock, 1998). Though perverse, it is not unreasonable to discover that the firearms legislation, that had been introduced to reduce firearms crime, actually increased armed robberies.

The first explanation for the failure of the 1977 firearms law to reduce armed robbery is that this law was not likely to disarm criminals who commit armed robbery. The vast majority of firearms used by Canadian armed robbers have been found to have never been registered as required (Axon and Moyer, 1994; Francis, 1995). This is not unusual as the British home office has found the same result (Home Office, 1997).

We may be able to further understand how the firearms control could have acted perversely by hypothesizing that this legislation removed the deterrent of widespread citizen firearm ownership. This could have happened in two ways. First, the Canadian media may have advertised the defenseless state of many Canadian businesses. It is not uncommon to read in newspapers, or to hear on the radio, a government official asserting that Canadians do not use firearms in self defence (e.g., Rock, 1995; McLellan 1998). According to utility theory from economics, robberies would be expected to increase as more criminals discover that their intended victims are disarmed. The second way in which this legislation may have removed the deterrent effect of firearms ownership is that the firearm control regime might have disarmed a number of individuals (or small businesses) who previously had kept a firearm for protection. As mentioned earlier, the 1977

legislation eliminated the protection of property as a legitimate reason for owning a handgun, and the associated regulations made it difficult if not impossible to keep a handgun at a place of business. Thus, because fewer businesses could legally keep handguns, and robbers were not likely to be disarmed, this legislation may have increased the number of successful armed robberies (with or without firearms). Utility theory from economics then provides two arguments to help us understand how gun control laws might act perversely by removing the threat of civilian force as a deterrent to armed robbery.

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Notes

[1] We would like to acknowledge the helpful contribution of colleagues who have read this paper and have made critical comments, particularly C.B. Kates and John Lott.

[2] This theory has been criticized as relying overly much upon the "rationality" of common criminals, who as a rule are not very intelligent (Wilson and Herrnstein, 1985). However, economists reply that the level of rationality being assumed here is not particularly higher than the "pain avoidance" frequently observed in the behavior of dogs or young children.

[3] Almost all "restricted weapons" are handguns and their registration requires a "legitimate" reason as well as a location. Thus, these changes effectively removed the option business people had of keeping a handgun to defend themselves and their businesses against armed criminals.

[4] The 1977 legislation also introduced penalties for the criminal use of a firearm, but this section has been applied very infrequently (Meredith et al 1994).

[5] Research shows that criminals have a limited time frame (Wilson and Herrnstein, 1985).

[6] In Table 3, since no dummy variable is defined for British Columbia, the Constant is the intercept for that province. The intercept for Newfoundland is the Constant plus the coefficient of DNFLD, and similarly for other provinces. The coefficient of TIME is the estimated change in the dependent variable per year in British Columbia. The analagous concept for Newfoundland is the sum of the coefficient of TIME and TNFLD. Again, time trends for other provinces are interpreted similarly.

[7] The POOL command in SHAZAM.