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THE SOCIAL COSTS OF GUN OWNERSHIP

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ABSTRACT

This paper provides new estimates of the effect of household gun prevalence on homicide rates, and infers the marginal external cost of handgun ownership. The estimates utilize a superior proxy for gun prevalence, the percentage of suicides committed with a gun, which we validate. Using county- and state-level panels for 20 years, we estimate the elasticity of homicide with respect to gun prevalence as between +.1 and +.3. All of the effect of gun prevalence is on gun homicide rates. Under certain reasonable assumptions, the average annual marginal social cost of household gun ownership is in the range \$100 to \$600.

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I. INTRODUCTION

One in three American households currently keep at least one gun within the home, often to protect members of the household against criminal predation (Cook and Ludwig 1996).[1] Like many other private decisions about health and safety, such as getting vaccinated or driving a sport utility vehicle, private gun ownership may impose externalities. Widespread gun ownership in a community could provide a general deterrent to criminal predation, lowering the risk to owners and non-owners alike. But widespread gun ownership could also lead to increased risks of various sorts, including the possibility that guns will be misused by the owners or transferred to dangerous people through theft or unregulated sale. Whether the social costs of gun ownership are positive or negative is arguably the most fundamental question for the regulation of firearms in the United States.

This paper develops an estimate of the marginal social costs of gun ownership based on our new estimates of the effect of household gun prevalence on homicide rates. Previous research yields contradictory findings about whether the social costs of gun ownership are positive, as argued by Duggan (2001), or negative, as argued by Lott (2000). The present paper moves beyond earlier studies in three respects. First, our new estimates utilize a better, well-validated proxy for county and state gun ownership rates, the percentage of suicides committed with a gun (FSS).

Second, we demonstrate that our results are robust to a variety of reasonable specifications and methods. In the absence of a randomized experiment or even a convincing natural experiment, ours and previous studies are necessarily correlational in nature. However it is reassuring that we obtain similar estimates using different sources

[1] The General Social Survey for 2002 provides a recent estimate; 33.6 percent of all households possessed at least one gun in that year.

of variation in gun prevalence. Lott (2000) essentially uses cross-sectional variation in gun ownership rates across states and finds that gun prevalence exerts an enormous deterrent effect on homicide, with an elasticity estimate of -3.3 (p. 114). Duggan (2001) uses across-state or –county, over-time variation, which yields an elasticity estimate equal to +.2 in state data and +.15 in county data. Using the same source of variation in gun prevalence in our panel (but a different proxy and estimation methods) we estimate that the elasticity of homicides with respect to gun ownership is on the order of +.1 to +.3. We also estimate separate regressions for gun and non-gun homicides and show that it is only the gun homicide rate that responds positively to gun prevalence.

Our findings are not sensitive to how we control for observable county or state characteristics, including the robbery or burglary rate, or to conditioning on region/year or division/year fixed effects. For our estimates to be driven by unobserved variables, these would have to be orthogonal to all of the observed covariates included in our model. We also show that our results are not sensitive to the influence of whatever factors caused the dramatic increase and decline in homicide rates during the 1980's and 1990's, because we obtain similar findings when we estimate a long-difference estimator comparing the beginning and end points of our panel. As a further check on whether our results are driven by unmeasured variables, we replicate the panel regressions with a variety of other outcome variables that have little logical connection to gun prevalence (but are related to the homicide rate), demonstrating that FSS is not related to other types of crime besides homicide, or (in the state data) with other indicators of risky or anti-social behavior such as motor vehicle death rates.

The third contribution of the present paper is to develop what we believe to be the first estimates for the *magnitude* (rather than just sign) of the social costs of gun ownership. At an average homicide rate of 10/100,000, our baseline elasticity estimate implies that an additional 10,000 handgun-owning households is associated with one additional homicide per year. Translating these results into a dollar-denominated cost raises a number of additional complications. One approach is to assign a value per statistical life for each homicide victim, adjusted in some way for the fact that those at highest risk for homicide victimization appear to have below-average aversion to the risk of death (Levitt and Venkatesh 2002). If the average value per statistical life for homicide victims is only $1 million, far below the $3 to $9 million per statistical life estimated from workplace samples (Viscusi 1998), then at mean values our baseline estimate of +.1 implies that the social cost per gun owning household is on the order of $100.

That sort of calculation is standard in the literature on risks to life and limb, but perhaps not adequate to assessing the social costs of gun violence. Gun violence engenders fear, concern about other members of the family and community, and a variety of costly avoidance and prevention activities (Cook and Ludwig 2000). Most of these effects do not have a clear counterpart in the literature on compensating wage differentials. Previous contingent-valuation (CV) estimates suggest that the complete social costs of each assault-related gun injury in the U.S. may be on the order of $1 million. If we assume that the ratio of fatal to non-fatal assault-related gunshot injuries is stable across time and space, these CV estimates would imply a social cost per gun owning household on the order of $600. Using the largest reasonable elasticity estimate suggests a figure of around $1,800.

The remainder of the paper is organized as follows. The next section reviews the mechanisms through which gun prevalence could increase or decrease homicide rates.

Section three discusses our proxy for gun prevalence, and four describes other variables used in the analysis. Section 5 develops the empirical strategy, which is implemented in Section 6. Section 7 considers the implications for social cost, and the final section concludes.

II. GUNS AND VIOLENCE[2]

America has at least 200 million firearms in private circulation, enough for every adult to have one (Cook and Ludwig 1996). But only one-quarter of all adults own a gun, the great majority of them men. Most people who have guns own several: three-quarters of all guns are owned by those who own four or more guns, amounting to just 10 percent of adults.

Around 65 million of America's 200 million privately held firearms are handguns, which are more likely than long guns to be kept for defense against crime (Cook and Ludwig 1996). In the 1970s one-third of new guns were handguns (pistols or revolvers), a figure that grew to nearly half by the early 1990s and then fell back to around 40 percent (ATF 2000). Despite the long-term increase in the relative importance of handgun sales, a mere 17 percent of gun-owning households have only handguns; 43 percent have both handguns and long guns, and 40 percent long guns only, reflecting the fact that most people who have acquired guns for self-protection are also hunters and target shooters.[3]

Given the importance of hunting and sport shooting it is not surprising that gun ownership is concentrated in rural areas and small towns, and among middle-aged, middle-income households (Cook and Ludwig 1996). These attributes are associated

[2] The discussion in this section borrows in part from Cook and Ludwig (2003a).
[3] From the 2002 General Social Survey (www.icpsr.umich.edu, dataset 3728).

with relatively low involvement in criminal violence, suggesting that most guns may be in the hands of people who are unlikely to misuse them. Some support for this view comes from the fact that most of the people arrested for gun homicide, unlike most gun owners, have prior criminal records (Cook and Ludwig 1996; Kates and Polsby 2000). More generally, gun homicide offending and victimization is disproportionately concentrated among low-income young men living in urban areas (Cook and Laub 1998; Blumstein 2000).

More guns, more crime. There are both direct and indirect connections between household gun prevalence and the availability of guns for criminal use. The direct connection is simply that a household gun, no matter why it was acquired, may at some point be misused by a member of the household, although this does not seem to account for the bulk of lethal violence.[4]

We suspect that the indirect connections are more important. Youths and criminals will find it easier and cheaper to obtain a gun in a community with widespread ownership than in a community where only a relatively few households have them. The prevalence of ownership would not be relevant if most criminals obtained their guns directly from retailers, but in fact that is quite rare (Cook and Braga 2001). More commonly, crime guns are obtained by a variety of transactions involving guns already in private hands – borrowing, renting, buying, or stealing. Theft in particular is a common source of crime guns: more than 500,000 are stolen each year nationwide (Cook and Ludwig 1996; Kleck 1997), and household burglaries are more likely to yield a firearm as

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[4] Recent research using case control methods demonstrates that gun possession is a strong positive correlate of the likelihood that a batterer will eventually kill his intimate partner (Campbell et al 2003). More generally, a gun in the home has been shown to be a risk factor for homicide victimization, but only for gun homicide, after controlling for several other household characteristics (Wiebe 2003; Kellermann et al 1993). Whether these studies have identified a direct causal relationship is not clear, but the logic of availability for misuse is compelling.

part of the haul in communities where guns are more common (Cook and Ludwig 2003b). Search times in the informal or "secondary" market for guns should be less, and prices quite possibly lower, where guns are plentiful.[5] One piece of evidence for this view comes from the fact that crime guns confiscated in low-prevalence jurisdictions are much more likely to have been first purchased from an out-of-state dealer and then (illegally) imported compared to crime guns confiscated in high-prevalence jurisdictions (Cook and Braga 2001; Braga et al. 2003).

From a social welfare perspective, variation across areas in gun availability to criminals is only important if the type of weapon matters in influencing the likelihood of crime or its seriousness. At one time, criminologists generally ignored the issue of weapon choice as a determinant of homicide, focusing instead on more "fundamental" causes, or even argued that guns themselves had little effect on the outcome of a violent encounter (Wolfgang 1958). Beginning in the late 1960's a growing body of empirical evidence has documented the common-sense view that the type of weapon does matter in the outcome of a criminal assault. Compared with other commonly used weapons, guns kill more quickly and easily, with little skill or strength required, and with less chance of effective self-defense (Zimring 1968; Cook, 1991; Wolfgang 1995). Because guns make killing easier, it is reasonable to believe that the presence of a gun in a violent encounter will increase the chance that it will end in death, controlling for the motivations of the assailant – a hypothesis that has been supported by a variety of empirical evidence.

This "instrumentality" effect of guns to increase the lethality of violent behavior may influence the volume of violent behavior in a variety of conflicting ways. Guns

[5] The secondary market in guns consists of all gun transfers that do not involve licensed dealers (Cook, Molliconi and Cole 1995), and accounts for 30 to 40 percent of all gun transfers and for most guns used in crime (Wright and Rossi 1994; Beck and Gilliard 1993; Sheley and Wright 1995; Cook and Braga 2001).

increase the costs of losing a contest that involves violence, which all else equal should reduce the volume of violent behavior (Donohue and Levitt 1998). On the other hand, relative to knives and fists, guns make the outcome of a violent encounter less predictable and so may lead to more violence (Donohue and Levitt 1998). Guns could also lead to more crime because their use appears to increase the "payoff" to robbery, by enabling perpetrators to rob more lucrative targets with a lower risk of victim resistance (Cook 1987).

More crime, more guns. Crime may in principle be cause as well as consequence of local gun ownership rates. While the majority of gun owners have guns for recreational purposes, nearly half of gun owners say that their primary motivation for having a gun is self-protection against crime (Cook and Ludwig 1996; Glaeser and Glendon 1998). Previous research provides some support for the idea that local-area crime rates are positively related to the prevalence of gun ownership, although personal victimization risk or experience appears to have little association with keeping a gun (Kleck 1997; Glaeser and Glendon 1998).

More guns, less crime. The same features of guns that make them valuable to criminals may also make guns useful in self-defense. Just how often guns are used in defense against criminal attack has been hotly debated and remains unclear. Estimates from the National Crime Victimization Survey (NCVS), a large government-sponsored in-person survey that is generally considered the most reliable source of information on predatory crime, suggests that guns are used in defense against criminal predation around 100,000 times per year (Cook, Ludwig and Hemenway 1997). In contrast are the results of several one-time telephone surveys, which provide a basis for asserting that there are millions of defensive gun uses per year (Kleck and Gertz, 1995; Cook, Ludwig and

Hemenway 1997), although the internal inconsistencies and likely biases of these estimates have been well documented (Hemenway 2004).

Whatever the actual number of defensive gun uses, the mere threat of encountering an armed victim may exert a deterrent effect on the behavior of criminals. A growing body of research within criminology and economics supports the notion that some criminals are sensitive to the threat of punishment (Nagin 1998; Levitt 2002). It is therefore not surprising that the threat of armed victim response may also figure in a criminal's decision: around 40 percent of prisoners in one survey indicated that they had decided against committing a crime at least once because they feared that the potential victim was carrying a gun (Wright and Rossi 1994).

In sum, our reading of the research suggests that the claims of both gun control opponents and proponents are plausible: widespread gun ownership could in principle make crime less common by deterring criminal activity, and the reverse outcome could arise if the effective price of guns to criminals is lower in areas where legal gun ownership is more common. Previous research yields conflicting conclusions regarding which of these effects dominate.

One prominent estimate for the effects of gun prevalence on homicide is by Lott (2000), who relates state-level estimates of gun ownership rates from voter exit polls in 1988 and 1996 to state crime rates, conditioning on the state's overall arrest rate, income, population density, percent black, region dummies and a year dummy. Lott (2000, p. 114) estimates an elasticity of homicide with respect to state gun ownership rates equal to -3.3 , with equally large elasticity estimates for other crimes such as aggravated assault (-4.3), robbery (-4.3), burglary (-1.6), and auto theft (-3.2). One concern is that these estimates are essentially cross-sectional, given that most of the variation in gun ownership rates in a state-level panel that does not include state-specific fixed-effects will be across states rather than over time (Azrael, Cook and Miller 2004), and so are susceptible to bias from other across-state differences in social conditions, culture or public policies.[6]

A more fundamental problem with Lott's estimates is that there are serious problems with his survey data. While Lott's voter exit poll data suggest that from 1988 to 1996 gun ownership rates increased for the U.S. as a whole from 27.4 to 37.0 percent (p. 36), the best source of national data on gun ownership trends – the General Social Survey – indicates that individual gun ownership trends were essentially flat during this period (Kleck 1997, pp. 98-99). The difference is not too surprising, since voters are by no means a representative sample of the adult public.

The best previous study of the relationship between gun prevalence and homicide is Duggan (2001), that identifies the relationship between guns and crime using over-time variation in panels of states and also counties. Duggan's elasticity estimate of +.2 is not only of the opposite sign from Lott's, but also more than an order of magnitude smaller in absolute value. One concern about Duggan's study is his proxy for gun prevalence, the subscription rate to *Guns and Ammo* magazine.

[6] Kleck and Patterson (1993) analyze a cross-section of city-level data and find no statistically significant relationship between gun ownership rates and homicide or other crime rates. However, rather than relying on a simple cross-section regression-adjusted comparison of crime rates across areas with different rates of gun ownership, they attempt to isolate variation in gun ownership rates that will be arguably unrelated to the unmeasured determinants of local crime rates. Their choice of instrumental variable to explain variation in gun prevalence – per capita rates of hunting licenses, and subscriptions to gun magazines – are likely to be correlated with unmeasured variables that affect crime. The problem with the Kleck and Patterson instruments can be seen by noting that Duggan (2001) actually uses gun magazine subscriptions as a proxy (rather than instrument) for local gun prevalence.

III. FSS AS A PROXY FOR GUN PREVALENCE

Since most states lack any sort of registration or licensing system that would generate administrative data on firearms ownership, household surveys provide the only direct source of information on this matter. But survey data are not always available, so analysts have employed a variety of proxy variables. Two independent inquiries have recently identified one such proxy as superior to all others for the purpose of estimating the cross-section structure of gun prevalence across large geographic entities (Azrael, Cook, and Miller 2004; Kleck 2004). That proxy is the fraction of suicides committed with a firearm (FSS).

Table 1 reports validity tests of FSS and four other proxies that have been used in the literature: the fraction of homicides with a firearm, the rate of fatal firearms accident, and two measures of interest in guns and gun sports – the subscription rate to *Guns & Ammo,* and membership in the National Rifle Association. FSS performs very well indeed, and better than the other four in each of three tests: correlation with state-level gun ownership estimated from the Behavioral Risk Factor Surveys in 21 states, correlation with state-level ownership rates estimated from two national surveys (combined), and correlation across the nine Census divisions with gun ownership rates estimated from the General Social Survey (Azrael, Cook, and Miller 2004).

Our use of FSS is primarily to estimate variation over time rather than in the cross section. To validate this use requires consistent estimates of gun prevalence over time, preferably at a sub-national level. The "gold standard" for national surveys of gun ownership is the General Social Survey, conducted by the National Opinion Research Center most years from 1972 to 1993 and biennially since 1994 (Davis and Smith 1998). In its current form the GSS is conducted in person with a national area-probability sample of 3,000 non-institutionalized adults. The response rate has been quite high (for example, 78% in 1994, 76% in 1996, 76% in 1998). The sample is chosen to be representative of the nation and of each of the nine Census divisions, but not of individual states.

"Prevalence of gun ownership" may be usefully defined with respect to individuals or households, and with respect to all types of guns or just handguns. Handguns, including revolvers and pistols, are of particular interest because they are vastly over represented in crime and suicide in comparison with long guns.[7] The GSS provides enough detail in recent years to estimate all four variants: the percent of households with some type of gun, the percent of households with a handgun, the percent of adult individuals who possess a gun, and the percent of adult individuals who possess a handgun. These four prevalence measures are highly correlated across the nine Census divisions. The inter-division correlations for these four measures are in every case above .90, based on estimates from the GSS for 1994, 1996, and 1998 combined (Azrael, Cook and Miller 2004). Hence a proxy that provides a good approximation to the geographic structure of, say, household handgun prevalence, likely also provides a good approximation of other measures of prevalence. On the other hand, the four measures have followed somewhat different trajectories over time at the national level.[8]

The proxy variable, FSS, is computed from the U.S. Vital Statistics mortality data. These data have the virtues of being consistent across time and space, of high quality, and readily available for annual estimates at the national, state, or county level, though only counties with large populations are identified in the public-use data files.

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[7] The long-gun category includes rifles and shotguns. While handguns make up only about one-third of the total guns in private hands, they account for over 80% of gun crimes and injuries (Cook 1991).

[8] Since 1980 the household gun prevalence in the United States has trended down, while the prevalence of individual ownership has been close to constant. The explanation for the difference in trends is in the downward trend in the size of households, and in particular the declining percentage of households that include a man.

In Appendix Table 1 we provide estimates from the General Social Survey of the household prevalence of handgun ownership for all available years, together with an estimate of the sampling error associated with each estimate. The variation over the period 1973-2000 is quite muted; a small increase during the 1970s, and a drop during the 1990s, with the estimated prevalence in 2000 very close to that of 1974. Table A1 also reports the national percentage of suicides with firearms, FSS, which follows a similar pattern. (A plot of the time trends for both variables over time is included in Appendix Figure A1, with FSS re-scaled so that the two series have the same mean averaged over the entire time period). The correlation between the two measures for the 18 annual observations is +.63.

$$\text{Handgun Prevalence (t)} = \underset{(.0958)}{-.1012} + \underset{(.1645)}{.5578}\ \text{FSS(t)} \qquad (1)$$

The 95% confidence intervals for all 18 of the GSS estimates for handgun prevalence overlap with this regression line.

Table 2 reports the results of panel regressions of GSS-based estimates of gun prevalence against two proxies, FSS and the subscription rate to *Guns & Ammo*. The latter was singled out because it was used in the study of gun prevalence and crime reported in Duggan (2001). The panel is defined over the nine Census divisions and the years in which GSS fielded gun questions, since 1980. (That was the first year that GSS included a question on individual ownership.) The estimated coefficients on FSS are in every case significantly positive, and especially strong when year fixed effects are omitted. (Fixed effects for census division are included in all cases.) The subscription rate for *Guns & Ammo* performs less well, and in three cases the estimated coefficients are negative.

FSS has been used as a proxy for gun prevalence in a number of recent studies of crime and violence: see, for example, Cook and Ludwig (2002; 2004; Miller, Azrael and Hemenway 2002a,b). Our study of gun prevalence on residential-burglary rates and the likelihood of "hot" burglaries (Cook and Ludwig 2003) utilizes the same general approach as the current study.

IV. DATA

The estimates presented below are based on panel data for 200 counties that had the largest population in 1990,[9] or a subset of those counties. We also present estimates based on state-level panel data, but have some preference for county-level data because of a larger sample size and the belief that local gun ownership is more closely related to local gun availability. While it would have been possible to expand the set of counties (the public-use data from Vital Statistics identifies over 400), the smaller counties do not provide much additional information, since they tend to have very small counts of homicides and suicides. The 200 largest counties accounted for 74 percent of all homicides in the United States in 1990.

Suicide and homicide counts are taken from Vital Statistics Program mortality data, based on reports of coroners and medical examiners and compiled by the National Center for Health Statistics. The alternative source for homicide is the FBI's Supplementary Homicide Reports, which are based on voluntary reports by law-

[9] Kelly (2000) used this sample of counties in studying the determinants of crime rates. The 5 counties of New York City are combined in our analysis due to data limitations. Oklahoma City was dropped in 1995 due to the large homicide count associated with the bombing of the federal building there.

enforcement agencies. The Vital Statistics data are generally more complete, reliable and consistent (Wiersema, Loftin, and McDowall 2000).

The FBI's Uniform Crime Reports are our source for data on robbery, burglary, and other types of crime besides homicide. Detailed information about the sources of data used in our analyses are presented in Appendix 2.

Finally, we would also like to control for other changes over time in county socio-demographic characteristics that could affect both crime and gun prevalence. Unfortunately such data are available at the county level only from the decennial census, from which we interpolate data for the intercensal years. Our measures capture the socio-economic status of county residents, including the percent black and percent households headed by a female. The percent county living in an urban area is included in our set of census variables given evidence on the relationship between cities and crime rates (Glaeser and Sacerdote 1999). We also control for the percent of county or state residents who were living in the same house 5 years ago, in part because of criminological arguments about the effects of community instability on informal social control. The movement of people across areas may affect gun ownership rates given the strong regional component to gun prevalence (Cook and Ludwig 1996).

V. EMPIRICAL STRATEGY

The basic empirical approach here is to estimate the relationship between gun prevalence and homicide by exploiting the substantial across-area differences in trends in gun ownership over a 20-year period. Our baseline estimates come from estimating a model as in equation (2), which relates the natural log of jurisdiction (i)'s homicide rate (or, alternatively, the gun- or non-gun homicide rate) in year t against FSS, the proxy for the jurisdiction's gun ownership rate, in year (t-1). FSS is lagged by one period out of concern for reverse causation -- gun ownership may be consequence as well as cause of a county's crime rate – although the lag can also be justified for substantive reasons: the thefts and secondary-market transfers that move guns from households to use by criminals will ordinarily take some time. To further control for the possibility of reverse causation, we condition on the natural log of the area's burglary and robbery rates, which are the kinds of crimes that seem likely to motivate the acquisition of a firearm for self defense. These crime variables also are a good reflection of criminogenic factors in the community that influence homicide rates (Blumstein 2000). To account for other county or state characteristics that affect homicide, the regression model includes year and county/state fixed effects, as well as the logs of the detailed set of county socio-demographic characteristics described in the previous section. The regression estimates are weighted by each county or state's population to account for heteroskedasticity in the error term.

$$\log Y_{it} = \beta_0 + \beta_1 \log FSS_{it-1} + \beta_2 X_{it} + d_i + d_t + \varepsilon_{it} \qquad (2)$$

Another concern is serial correlation in the error structure of equation (2), given that FSS changes only slowly over time within counties and other unmeasured determinants of county crime rates may also have jurisdiction-specific trends. Testing for the presence of serial correlation in fixed-effects models is complicated in applications where the time dimension is fairly short compared to the number of observational units. Following Solon (1984) we test for serial correlation by first-differencing the data, and then keep the residuals from a regression of the log change in homicides against the log change in FSS and year effects. A regression of these residuals against their one-year lag yields a coefficient of -.4, close to the value of -.5 we would expect if the error structure was serially uncorrelated in log levels.[10] The data are a bit more serially correlated with the state-level data.[11] In any case, we address this problem when estimating equation (2) by calculating Huber-White standard errors that are robust to an arbitrary autocorrelation pattern in the errors over time within counties. Bertrand et al. (2004) show this approach works better than more parametric strategies in panels with a short time dimension.

A final concern in estimating equation (2) is that the proxy for gun prevalence, FSS, is subject to measurement error, of two types. First, because it is only a proxy, the correlation between FSS and the "true" prevalence is presumably less than one. Judging the quality of the proxy in that sense is difficult, given that there are no error-free measures of the criterion variable. In particular, survey-based estimates are subject to sampling error and other sorts of error. Based on the analysis of the GSS estimates over time reported above, the hypothesis that FSS is a "perfect" proxy cannot be rejected, but that is not the same thing as demonstrating that it is perfect.

Second, and probably more important, is that the reliability of FSS will depend on the number of suicides used to compute it. For the 21 years of data on 200 large counties, the 10th and 90th percentiles have 27 and 142 suicides respectively, with a median of 52. If the choice of weapon in suicide follows a binomial process, then a jurisdiction with 50 suicides a year would generate an observed FSS that is subject to a standard error of 7

[10] When we regress the residuals against three year's worth of lags, the coefficients equal -.536 (.028) for the one year lag, -.334 (.031) for the two year lag and -.153 (.021) for the three year lag. If the data were serially uncorrelated in log levels we would expect the two and three year lag coefficients to be equal to zero, although our data still appear to be closer to being serially uncorrelated in level than in change form. Similar results hold for the residual from log gun homicides, and when we include our full set of covariates in the initial homicide or gun homicide equation. The estimated autocorrelation coefficients tend to be somewhat larger in absolute value for the log of non-gun homicides.

[11] With our state data the coefficients from regressing the first-differenced residual against three years of lagged residuals equals -.35, -.06 and -.07, a pattern that is close to what we would expect for an AR(1) process for the error structure in levels with ρ=.30 (see Solon 1984, Table 3).

percentage points. The effect of this measurement error will be to bias the coefficient estimate of FSS toward zero. We attempt to limit its effects in several ways. First, all regressions are weighted by population, which will give greater leverage to the larger counties that have less "noisy" observed values of FSS. Second, we experiment with limiting the data to the 100 largest counties, or the 50 largest counties, which shifts the distribution of suicide counts upward. Third, we replicate our estimates using the average value of FSS over several years. Finally, we also produce estimates using state-level data.

VI. RESULTS

Descriptive Statistics. Table 3 presents descriptive statistics for the full panel data set assembled from annual data for the 200 largest counties for the years 1980-1999. (All calculations are weighted by county population.) Data for the year 1979 are not included in these calculations since all analyses presented below begin with 1980; 1979 data are only used to compute the lagged value of FSS in the first year for those analyses.

Over the entire sample period the average homicide rate is 11 per 100,000 residents, with half of all suicides having been committed with a firearm. On average our gun proxy – the share of suicides committed with a firearm – is calculated from 196 suicides per county per year.

Table 3 also provides some sense for the nature of the variation in gun ownership that identifies the panel-data estimates shown below. Table 3 shows that there has been convergence in gun prevalence rates between high- and low-gun ownership areas over time (see also Azrael, Cook and Miller 2004). The second and third columns of Table 3 present data for the top and bottom quartiles for our 200 counties ranked according to their gun ownership rates at the start of our panel, in 1980. The (disproportionately Southern) counties where guns are most common in 1980 experience a persistent and pronounced reduction in household gun ownership rates during the 20 years of our panel, as reflected by the nearly 20 percent decline in FSS over this period. At the same time, counties where guns were least common in 1980 (disproportionately in the Northeast and Midwest regions) experienced an increase in FSS of 20 percent from 1980 to 1999.

The source of this convergence remains something of a mystery. Gun ownership rates tend to be much higher for men than women and for whites than for blacks, and tend to be lowest in cities (Cook and Ludwig 1996). Yet the descriptive statistics in Table 3 show that the two sets of counties experienced relatively little change in these population characteristics over time either absolutely or in relation to one another. If whatever drove this convergence between high- and low-gun ownership areas was orthogonal to the determinants of homicide trends, then a difference-in-differences estimate of the effect of FSS on homicide (Y) would be unbiased. In particular, expression (3) is an estimate of the elasticity of Y with respect to FSS, where Δ indicates the difference between 1999 and 1980, and the subscripts t and b refer to "top quartile" and "bottom quartile" respectively.

$$\{\Delta \ln Y_t - \Delta \ln Y_b\} / \{\Delta \ln FSS_t - \Delta \ln FSS_b\} \qquad (3)$$

As seen in Table 3, the high-FSS counties experience a decline in overall homicide rates of about 49% over this period, compared to a decline of 45% for the low-FSS counties. Converting all figures to log form (consistent with a constant-elasticity assumption), and dividing as in expression (3), the estimated elasticity of homicide with respect to FSS is +.18. A similar calculation for gun homicides implies an estimated elasticity with respect to gun ownership rates of +.35. These simple estimates, which use just the variation in gun prevalence that arises from convergence between the highest- and lowest-gun counties, turn out to be compatible with those derived from the panel analysis below that uses all of the variation across counties over time.

Main Findings. The first column of Table 4 presents the results for our most parsimonious model, which regresses log homicide rates against log FSS and county and year fixed effects. The estimated elasticity of homicide with respect to the lagged value of log FSS equals +.100, which is statistically significant at the 5 percent cutoff. The final three columns of Table 4 show that our point estimate is not sensitive to controlling for several powerful covariates: the log of the contemporaneous UCR burglary and robbery rates, as well as percent black, urbanicity, a measure of residential stability and the proportion of households headed by a female. These other coefficients generally have the expected signs. While previous research typically finds that crime rates are higher in cities (for example Glaeser and Sacerdote 1999), the negative coefficient on percent urban in the fixed-effects model is not necessarily in contradiction, since it is not identified off of cross-section variation, but rather off of the relative growth in the urban portion of counties that were not entirely within city limits in 1980. In any case, with our full set of covariates included the estimated elasticity of homicides with respect to FSS equals +.086.

Suppose that the predominant causal mechanism linking gun prevalence to homicide is that increased prevalence induces substitution of guns for other weapons in assaults, with a consequent increase in lethality. Then only the gun homicide rate will increase in response to an increase in FSS. The estimates shown in the first row of Table 5 demonstrate that this is the case. The estimated elasticity of gun homicides with respect to FSS is equal to around +.17, while the elasticity of non-gun homicides with respect to FSS equals -.03 and is not statistically significant.

Extensions. The rest of Table 5 demonstrates that the results are robust to a variety of modifications to our basic estimation approach. The second row shows that adding in a series of additional county-level characteristics from the Census – namely, percent living in poverty, percent county residents born outside of the U.S., and the percent residents in different age groupings – has almost no effect on the point estimates. Our preferred regression model relates homicide rates to lagged values of FSS in order to address the problem of reverse causation, although this specification comes at the cost of omitting any additional causal effect that current gun ownership rates may exert on homicide rates. The results reported in the third row of Table 4 suggest that the cost of using lagged rather than contemporaneous values of our gun proxy is modest. The fourth row of Table 5 shows that ignoring the problem of serial correlation changes our standard errors by only around 25 percent, consistent with the finding reported above that there is only a modest degree of serial correlation in the process determining county homicide rates.

Re-calculating the estimates without weighting by county population produces an elasticity estimate for homicide with respect to guns that is about two-thirds as large as the weighted estimate (Table 5, row 5). We prefer the weighted estimates because they assign greater importance to larger counties, which should have regression residuals that have smaller variances compared to less-populous counties, and should also have less measurement error in FSS given the larger suicide count in these counties. Estimating a model where all variables are included in linear rather than log form (row 6) also reduces the magnitude of the point estimates in relation to their standard errors. This change appears to be due to the decision to log or not log our dependent variable, because regressing FSS in linear form (row 7) against logged homicide rates yields results that are as strong as in the baseline model. In our view, models focusing on proportional rather than absolute changes in homicide rates per capita are more sensible – if for example the effect of increasing gun availability is simply to increase the probability of any given assault ending in death, then the relevant effects will be proportional.

As expected, other efforts to reduce the problem of measurement error in FSS lead to larger elasticity estimates, as shown in rows 8 through 10 of Table 5. Replacing log lag FSS with the value averaged over the past two years nearly doubles the absolute value of our point estimates. Limiting the sample to the 100 or 50 largest counties, for which FSS values are calculated using larger numbers of suicides, produces a qualitatively similar change.

Given that we do not have a clearly exogenous source of identifying variation in gun prevalence, there necessarily remains some concern that our estimates confound the causal effects of guns on homicide with those of other variables not included as covariates in any specification that may influence crime rates. However it is reassuring that our results are not much affected by conditioning on region/year or division/year fixed effects (rows 11 and 12, Table 5), which provides a flexible, non-parametric way of accounting for regional trends in both homicides and gun prevalence. The fact that the results are not sensitive to adjusting for region/year or division/year effects would also seem to rule out bias from the influence of an unmeasured trend in the "Southern subculture of violence" (Butterfield 1997). Another way to account for the possibility of unmeasured variables is to condition on the lagged value of the dependent measure, as in row 13, which yields a point estimate of around +.06. Interestingly, the point estimate for this dynamic model implies a steady-state elasticity that is quite close to our preferred baseline model.

The final four rows of Table 5 shows what happens when we use state-level data, which has the advantage over county-level data of reducing measurement error in FSS, but perhaps at the cost of measuring gun prevalence at some level larger than the relevant local gun market. In any case, replicating the baseline model using state data yields an estimated elasticity of homicide with respect to gun prevalence of +.41. Further accounting for the possibility of omitted variables bias by conditioning on either region/year or division/year fixed effects, or the lagged value of the dependent variable, implies a steady-state elasticity on the order of +.3.[12]

Concern about unmeasured variables is particularly salient with our application because homicide rates in America exhibit a powerful non-linear trend over our sample period, with dramatic increases from the mid-1980's through early 1990's followed by an equally dramatic decline. The leading explanations for this pattern focus on violence associated with crack-cocaine markets, police and prison spending, and even the legalization of abortion in the early 1970's (see for example Cook and Laub 2002; Blumstein and Wallman 2000; Levitt 2004). Our baseline regression model attempts to indirectly control for the influence of most of these factors, for example by conditioning on year or region-year fixed effects as well as each county's robbery and burglary rates. But there remains the possibility that difficult-to-measure factors that drive the pronounced trends in crime during the time of our panel are influencing our estimates for the effects of guns on homicide.

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[12] The last row is closest in specification to Duggan's (2001) preferred specification. But he used a different proxy for gun prevalence (as explained earlier in the text) and only reports the results of estimates when all substantive variables are in first difference form.

In Table 6 we attempt to circumvent the problem of trying to model or control for whatever is driving the increase and decline in homicide over our sample period by using just the long-term variation in gun ownership rates and homicide from the early 80's to the late 90's. The first row of Table 6 presents estimates from a long-difference model that shows the changes in log homicides (or log gun or non-gun homicides) from 1980 to 1999, regressed against the change in log FSS over the same period, conditioning on the log changes in the other explanatory variables included in our baseline model (as shown in the final column of Table 4). This long-difference estimator yields an elasticity of homicide with respect to gun prevalence of +.3. Pooling data from multiple years to reduce attenuation from measurement error with FSS serves to increase the magnitude of the point estimates as expected.

A final way to test for the possibility of bias from unmeasured variables is to determine whether FSS predicts outcomes that logically have little relationship to gun prevalence, in the spirit of Altonji, Elder and Taber (2000, 2002). Table 7 reports the results of estimating the baseline model (final column, Table 4) on rates of other types of crime from the UCR, and on the fatality rate from falls and from motor-vehicle accidents. The estimated coefficients on FSS are not significantly different from zero in any of these regressions.[13] (Note that another implication from Table 7 is that the results are not sensitive to measuring homicides using data from the UCR rather than our preferred source, the Vital Statistics).

Finally, Table 8 provides suggestive evidence that gun prevalence leads to elevated rates of homicide through the transfer of guns from "legal" to "illegal" owners,

[13] The one exception is the arson rate, not shown, although we are not inclined to put much stock on this outcome because arson is so poorly measured in the UCR.

rather than through increased gun misuse by otherwise legal owners. In this exercise we focus on homicide rates to victims 15 to 19, a relatively high percentage of whom are killed in gang- and felony-related attacks by youthful criminals – with guns that are typically obtained from the secondary market (Cook and Ludwig 2004). That this market is closely tied to the prevalence of gun ownership is suggested by the large coefficient on FSS.

VII. SOCIAL COSTS OF GUN OWNERSHIP

In sum, gun prevalence is positively associated with overall homicide rates but not systematically related to assault or other types of crime. Together, these results suggest that an increase in gun prevalence may cause an *intensification* of criminal violence – a shift toward greater lethality, and hence greater harm to the community. Gun ownership also confers benefits to the owners and possibly other members of the household. The benefits are associated with the various private uses of guns – gun sports, collecting, protection of self and household against people and varmints. But the net external effects are negative.

The magnitude of these net external costs is suggested by the elasticity estimates of homicide with respect to FSS. The baseline model applied to county-level data yields an elasticity of +.09 or +.10, with a range of reasonable estimates from +.07 (from conditioning on region/year or division/year fixed effects with the baseline county model) to +.3 (from either long-differencing the county data or applying the baseline model to state-level data with region/year or division/year fixed effects). All of these have the feature that the associated estimates for gun and non-gun homicide indicate that the effect on overall homicide is due to changes in gun use, with the possibility of some substitution.

These elasticity estimates with respect to FSS also serve as estimated elasticities with respect to the household prevalence of gun ownership, if FSS is proportional to prevalence. Based on cross-section data, FSS does not appear to be strictly proportional – the best-fit line between FSS and survey-based gun-ownership rates is linear with a significantly negative intercept (Azrael, Cook and Miller 2004). But proportionality is a defensible assumption for time-series data: the regression of national handgun prevalence rates on FSS reported in the text above finds an intercept with a t-statistic of about –1. In what follows we treat the elasticity with respect to FSS as equal to the elasticity with respect to the prevalence of gun ownership.

The positive elasticity estimates imply that an increase in the prevalence of gun ownership has positive marginal social cost. It is relevant to translate the elasticity into a ratio: the annual change in the homicide count associated with a change in the number of households with guns. That ratio is related to the elasticity by this formula:

$$\text{Ratio of changes in homicides to gun-owning households} = [e \times h \times n]/g \qquad (4)$$

where

e = elasticity of homicide rate to prevalence of guns

h = homicide rate per capita

g = household prevalence of gun ownership

n = number of people per household

This ratio is proportional to the marginal social cost of an additional gun homicide. The formula implies that the marginal social cost of acquiring a gun increases with the homicide rate. For a given homicide rate, the marginal social cost is lower for high-prevalence jurisdictions than low-prevalence – an algebraic result of the log-log specification.

It is important to distinguish between gun types. While handguns make up only about one-third of the private inventory of guns, they account for 80 percent of all gun homicides and a still-higher percentage of gun robberies. Handguns are also used in most gun suicides. Hence the social costs of handgun ownership are much higher than ownership of rifles and shotguns. Unfortunately it is difficult to distinguish between the prevalence of long-gun ownership and handgun ownership in aggregate data, since they are very highly correlated across jurisdictions. There is some divergence over time, as overall gun ownership has had a strong downward trend that is not so evident for handgun ownership. FSS is a better proxy over time for handgun ownership.

If the marginal social cost of gun prevalence is entirely attributable to handguns, then the relevant national average is about 20 percent (see Table A1). Using that value, together with a homicide rate of 10/100,000 (which is close to the average for the 200 counties), an elasticity of +0.10, and 2 people per household, then the formula indicates one additional homicide per year for every 10,000 additional handgun-owning households.

Table 9 offers similar calculations for other values of the baseline homicide rate and handgun-prevalence rate, assuming throughout an elasticity of 0.1 and 2 people per household. Each entry is the number of additional homicides per year resulting from a change in the number of handgun-owning households. If the true elasticity is closer to +.3 instead of +.1, then the figures reported in Table 9 should be tripled.

Two additional questions relevant to calculating marginal social cost cannot be resolved satisfactorily from our empirical results: which margin, and what geographic unit?

Which margin? Most households that own one gun own several. About three-quarters of all guns are owned by the one-third of gun-owning households that own at least four (Cook and Ludwig 1996). FSS is a valid proxy for the prevalence of gun ownership, but much of the "action" is at the intensive margin. With respect to providing the right attribution of marginal social cost, it is important to determine whether the acquisition of the n^{th} gun by a gun-owning household has the same cost on average as the acquisition of the first gun. Of course it is only the latter acquisition that will change prevalence.

What geographic unit? While our focus has been on county-level ownership, we note that guns often travel across county lines. For that reason, household gun ownership in nearby counties may affect gun availability to local criminals. If true, then "gun prevalence in nearby counties" is a variable that belongs in the homicide regressions, since it is substantively relevant and quite possibly correlated with within-county prevalence. We experimented with specifications that included rest-of-state FSS in addition to the usual within-county FSS, but unfortunately the results were not very sensible. At this point, it is necessary to be guided by other sorts of evidence regarding the importance of diffuse sources of guns outside of the immediate county. If one is inclined to believe that there are few frictions in the flow of guns to criminals within a state, then our state-level estimates are a better guide to the social costs than the county estimates.

Translated into the policy domain, the answers to these questions should influence the nature of regulation adopted in response to the cost argument, and also the geographic scope of the regulatory system. If the number of households with guns, as opposed to the number of guns, is the main concern, then a licensing system may be the preferred form of regulation.[14]

What would be the optimal license fee per household? Answering this question requires monetizing the social costs of the additional homicides that appear to be generated by widespread gun prevalence. One possibility would be to assign each homicide the value per statistical life that has been estimated in previous research, which come primarily from studies of workplace wage-risk tradeoffs and suggest a range of $3 to $9 million per life (Viscusi 1998). But even the lower end of this range may overstate the dollar value required to compensate the average homicide victim for a relatively higher risk of death, given that (as noted above) such a large proportion of homicide victims are engaged in criminal activity that entails a high risk of death. For example, a study of the wage premium paid to gang members engaged in selling drugs suggests a value per statistical life on the order of $8,000 to $127,000 (Levitt and Venkatesh 2000).

Suppose that given local conditions with respect violence and gun ownership we estimate a ratio of 10,000 handgun-owning households per annual homicide (approximately what holds at the national average for gun prevalence and homicide). Given a conservative value of life, $1 million, then the appropriate license fee for a household would be $100 per year. That license fee would increase with the homicide rate, and in some jurisdictions, such as Washington DC, would become so high that as to

[14] If it is the number of guns that matters, as opposed to the number of households, then an annual tax per gun could be assessed. But our estimates are not directly relevant to estimating the appropriate fee in that case.

be the practical equivalent of a ban on ownership. (A ban on handgun acquisition is currently in place in Washington, Chicago, and some other cities.) Of course, this calculation ignores the problem of compliance.

This calculation will understate the optimal license fee per gun-owning household if our assumption about the average value per statistical life for homicide victims is too low, or if, as seems likely, gun violence imposes costs on society that are not well captured by any study of the value per statistical life. Figures for the value per statistical life typically come from estimating the additional wage premium required to induce workers to accept jobs with higher risks of death, and thus reflect the valuation an individual places on an increase in the risk of death to himself of, say, 1/100,000. Multiplying this figure by 100,000 then yields the value per statistical life. But a given individual's willingness to pay to reduce gun homicides in his community by 1/100,000 is almost surely likely to exceed his willingness to pay to reduce his own risk of death by 1/100,000. The reason is that a community-wide reduction in gun homicide rates reduces the risk of death to others whom the individual cares about beyond himself, including others in the household and the community at large, and may reduce the individual's expenditures on measures designed to reduce his own risk of victimization.

Contingent-valuation estimates intended to capture the complete social costs of gun violence indicate value of around $1 million per assault-related gunshot injury (Cook and Ludwig 2000; Ludwig and Cook 2001). On average one in six assault-related gunshot injuries results in death (Cook 1985; Cook and Ludwig 2000). Under the assumption that this case-fatality rate is stable across time and space, then at the national averages for gun prevalence and homicide our baseline estimate of a guns/homicide elasticity of +.10 implies that each additional 10,000 gun-owning households leads to around 6 additional crime-related gunshot injuries. One potential concern with this calculation is that a decline in gun homicides and non-fatal injuries may be accompanied by an increase in overall crime-related injuries, since previous research suggests that gun assaults and robberies are less likely to lead to injury than non-gun crimes (Kleck 1997, pp. 225-6; Cook 1976). In principle this substitution effect will be reflected in the willingness to pay figures reported by CV respondents.[15] In any case, if these contingent valuation estimates are approximately correct, the optimal license fee per gun-owning household would be on the order of $600. If the true elasticity of homicide with respect to gun prevalence is on the order of +.30 rather than +.10, the optimal license fee may be as high as $1,800 per household.

VII. DISCUSSION

A fundamental issue for the regulation of firearms in America is understanding whether the social cost of gun ownership is on net positive or negative. Answering this question is complicated by the difficulty of measuring gun prevalence at the local level, and in isolating the causal effects of gun prevalence on crime rates in the absence of a randomized or convincing natural experiment. Our empirical analysis of county- and state-level data finds that gun prevalence is positively related to overall homicide rates. We are confident that our proxy for gun prevalence dominates what has been used in

[15] The willingness to pay values used here come in response to the following question, asked as part of a 1998 nationally representative telephone survey on firearm-related issues conducted by the National Opinion Research Center at the University of Chicago: "Suppose that you were asked to vote for or against a new program in your state to reduce gun thefts and illegal gun dealers. This program would make it more difficult for criminals and delinquents to obtain guns. It would reduce gun injuries by 30% but taxes would have to be increased to pay for it. If it would cost you an extra [$50 / $100 / $200] in annual taxes would you vote for or against this new program?" The dollar value included in this question was randomly assigned across respondents. Survey participants were than asked a follow-up question about their support at twice or half the initial value, depending on whether their initial response was positive or not (Cook and Ludwig 2000, p. 103).

previous research. While there necessarily remains some uncertainty about whether we have successfully identified a causal effect of guns on crime, our main result is robust to a variety of specification experiments.

These panel-data estimates are identified in part by the convergence over time in gun prevalence between areas that initially have quite different levels of gun ownership. Much of this over-time within-county variation seems to come from reductions in gun ownership rates in the South. We attempt to rule out the confounding effects of other changes within the South over time by showing that the estimates are not much affected by also conditioning on region/year or division/year fixed effects. We also show that our results are not driven by whatever factors caused the homicide rate in the U.S. to increase dramatically from the mid-1980's to the early-1990's and decline thereafter, because we obtain qualitatively similar findings from a long-difference estimator that compares changes in gun prevalence and homicide from the early 1980's to the late 1990's.

In principle, our panel data estimates could still be confounding the effects of gun prevalence with those of other unmeasured factors. Given this concern, it is noteworthy that gun prevalence is not related to other types of crime as measured by data from the FBI's Uniform Crime Reporting system. Our estimates identify a factor that influences the lethality but not the overall volume of crime within an area. Firearm ownership is presumably the leading candidate as an explanation for this relationship. That conclusion is strengthened by the fact that the estimated effect of FSS on gun homicide is larger than overall homicide, while the effect on non-gun homicide is nil or negative.

These estimates imply a positive marginal external social cost of gun ownership; the lethality of criminal violence increases with gun prevalence. The cost per gun-owning household will increase with the homicide rate and be inversely related to the level of gun ownership. (Interestingly, public opposition to gun control is strongest in rural areas, which typically have high rates of gun ownership and low rates of homicide – exactly where our estimates suggest that the regulation on gun ownership should be lightest, unless, of course, there is a free flow of guns across county or state lines.) In general, effective law enforcement reduces the cost of gun ownership,[16] and would reduce the appropriate degree of regulation -- although in some cases enforcement may be complemented by rather than substitute for particular regulatory measures. In any event, the dramatic reduction in homicide rates nationwide during the 1990s made guns less socially burdensome.

Most of the harm is probably associated with handguns rather than rifles or shotguns, but our analysis does not allow the relative contribution to be identified directly. Nor can we determine the geographic scope of the relevant market for arming criminals, or distinguish whether it is the number of gun-owning households, or the number of guns, that is important in determining the availability to criminals.

Assuming average levels of gun prevalence and homicide, and a low value of human life, we make a very conservative estimate of $100 per handgun-owning household per year. Given our various elasticity estimates and alternative valuations of reducing gun violence, the correct cost could range as high as $1,800 per year.[17]

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[16] The growing body of empirical evidence on the deterrent effects of punishment on crime provides one reason for suspecting that enforcement activity may reduce gun crimes as well (Nagin 1998; Levitt 2002). Previous evaluations of interventions that target gun crime specifically yield suggestive evidence for the effectiveness of police patrols against illegal gun carrying (Cohen and Ludwig, 2003). On the other hand, a recent evaluation of Richmond, Virginia's Project Exile sentence-enhancement program, one model for the national Project Safe Neighborhoods intervention, does not find any detectable effect on crime (Raphael and Ludwig 2003), perhaps in part because of the scale of the program's operations (Levitt 2003).

[17] By way of comparison, Lott's (2000) estimated elasticity of homicide with respect to gun ownership of -3.3 implies (using equation 4) one homicide is averted for each additional 303 gun-owning households. Assuming a value per life of just $1 million per homicide victim, Lott's estimate implies that a licensing fee set to the external costs of gun ownership would involve a subsidy of $3,300 per year to each gun owning household. If instead we use the contingent valuation estimates presented in Cook and Ludwig (2000) of

While the empirical estimates are largely silent on the mechanisms through which household gun ownership affects homicide, in our view the most likely mechanism is through influencing the supply of guns to prohibited individuals rather than through increasing gun misuse among otherwise law-abiding people. If this interpretation is correct, then interventions capable of reducing the flow of guns from legal to illegal owners, or reducing gun misuse by prohibited people, will reduce the social costs of legal gun ownership. Personalized gun technology that is currently under development could in principle substantially reduce the social costs of private gun ownership by making stolen guns inoperable to criminals, and by complementing regulatory efforts to reduce secondary-market transfers to prohibited people (Cook and Leitzel 2002).

$1 million per gunshot injury, Lott's estimates imply that each gun owning household should receive a subsidy of nearly $20,000 per year.

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TABLE 1

VALIDITY TESTS OF PROXIES FOR HOUSEHOLD GUN PREVALENCE

Correlation Coefficients, Cross-Section Data

	HICRC[a] N=48 States	BRFSS[b] N=21 States	GSS[c] N=9 Divisions
FSS % Suicides with Firearms	0.81	0.90	0.93
FHH % homicides with firearms	0.02	0.19	0.52
UFDR Unintentional firearms deaths	0.61	0.68	0.85
G&A Guns&Ammo Subscription rate	0.75	0.67	0.51
NRA Membership Rate	0.67	0.55	-0.06

[a] State estimates based on two Harvard Injury Control Research Center (HICRC) surveys, which were conducted using the random-digit-dial technique in 1996 and 1999, with sample sizes of 1,900 and 2,500 respectively.

[b] Between 1992 and 1995 the Behavioral Risk Factor Surveillance System (BRFSS) included gun-ownership items in surveys conducted in 21 states. These surveys were conducted under the auspices of state health departments using the random-digit-dial telephone technique. The median sample size of adults ages 18 and over was 2,061

[c]General Social Survey prevalence estimates are here based on pooled data from surveys conducted in 1994,1996, and 1998.

Source: Azrael, Cook and Miller 2004

TABLE 2
VALIDITY OF TWO PROXIES WITH RESPECT TO 4 MEASURES
Data from GSS Surveys, 1980-1998, for 9 Census Divisions
Panel regression results with Census Division fixed effects
Linear Regression coefficients on proxy (FSS or *G&A*), and standard errors

Which proxy?	GSS % of individuals who own a gun	GSS % of individuals who own a handgun	GSS % of households with a gun	GSS % of households with a handgun
With "year" fixed effects				
FSS	.850[b]	.485[c]	1.108[a]	.737[b]
	(.354)	(.250)	(.417)	(.305)
G&A	.358	.713	-.055	.861
	(1.196)	(.835)	(1.419)	(1.027)
No "year" fixed effects				
FSS	.812[a]	.554[a]	.905[a]	.742[a]
	(.285)	(.201)	(.355)	(.246)
G&A	-.762	.403	-2.219[c]	.201
	(1.013)	(.713)	(1.240)	(.878)

Notes: Each cell contains the key coefficient estimate and standard error from a different regression. Each regression includes divisional dummies; the coefficients are not reported in this table. N = 126, annual observations from the General Social Survey for the following 14 years: 1980-1982, 1984, 1985, 1987- 1991, 1993, 1994, 1996, and 1998. (The GSS was not fielded or did not include the relevant items during the missing years.)

The two proxies for gun ownership are:

- FSS = % of suicides in the Census Division committed with a gun, from Vital Statistics data
- G&A= Subscriptions to *Guns&Ammo* per 10,000 residents of Census Division

a. Significantly different from zero at the 1% level
b. Significantly different from zero at the 5% level
c. Significantly different from zero at the 10% level

TABLE 3
DESCRIPTIVE STATISTICS FOR COUNTY DATA

	Full sample (largest 200)	Bottom quartile 1980 FSS	Top quartile 1980 FSS
Full period (1980-1999)			
Northeast	26.8	67.1	0
South	25.4	2.4	71.0
Midwest	19.6	23.9	14.6
West	26.2	6.6	14.4
FSS	49.9	34.6	66.9
Homicide rate	11.0	10.9	14.4
Gun homicide rate	7.3	6.9	10.1
% Urban	92.6	94.7	91.8
% Black	14.0	13.5	19.5
% Female Household Head	18.0	20.1	18.5
# suicides	195.8	192.5	120.0
1980			
FSS	48.0	29.2	73.3
Homicide rate	12.9	12.6	17.2
Gun homicide rate	8.4	7.5	12.2
% Urban	89.5	93.1	88.5
% Black	13.0	12.8	18.9
% Female Household Head	16.6	18.7	16.3
1990			
FSS	52.8	37.2	69.1
Homicide rate	12.5	13.2	16.4
Gun homicide rate	8.5	9.3	11.5
% Urban	93.0	94.6	92.4
% Black	14.2	13.6	19.5
% Female Household Head	18.1	20.3	18.7
1999			
FSS	48.0	34.9	59.8
Homicide rate	6.7	6.9	8.8
Gun homicide rate	4.8	4.7	6.7
% Urban	95.0	96.6	94.0
% Black	14.7	14.3	20.2
% Female Household Head	19.2	21.2	20.3

Notes: Descriptive statistics calculated from county level data for the 200 largest counties in the U.S., weighting by county population.

TABLE 4
BASELINE RESULTS, COUNTY-LEVEL DATA 1980-1999

	Ln(Hom)	Ln(Hom)	Ln(Hom)	Ln(Hom)
Ln FSS(t-1)	.100 [b]	.107[a]	.085 [c]	.086[b]
	(.044)	(.037)	(.044)	(.038)
Ln Rob(t)		.139[a]		.149[a]
		(.043)		(.042)
Ln Burg(t)		.258[a]		.226[a]
		(.068)		(.072)
Ln % Black(t)			.233	.278[c]
			(.166)	(.164)
Ln % Urb(t)			-.389[b]	-.537[a]
			(.161)	(.157)
Ln % Same House 5 yrs			-1.209[a]	-.690
ago			(.430)	(.419)
Ln % Female Headed			.790[c]	-.303
House			(.460)	(.413)
Year fixed effects?	Yes	Yes	Yes	Yes
County fixed effects?	Yes	Yes	Yes	Yes
R-squared	.915	.921	.918	.923
N	3822	3822	3822	3822

Notes Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight. Analytic sample consists of annual observations for 200 largest counties in U.S. over the period 1980-1999.

a. Significantly different from zero at the 1% level
b. Significantly different from zero at the 5% level
c. Significantly different from zero at the 10% level

TABLE 5
SENSITIVITY ANALYSIS

	Ln(Homicide)	Ln(Gun Homicide)	Ln(Non-gun Homicide)
Baseline model, final column, Table 4	.086[b] (.038)	. 173[a] (.049)	-.033 (.040)
Additional Covariates (Age, Poverty, Immigrants)	.086[b] (.036)	. 173[a] (.043)	-.020 (.040)
Use contemporaneous (not lagged) FSS	.097[a] (.035)	.250[a] (.056)	-.089[b] (.042)
Without serial correlation correction	. 086[a] (.030)	.173[a] (.040)	-.033 (.037)
Baseline Model, Unweighted	.051 (.043)	.167[a] (.043)	-.061 (.042)
Baseline Model, Linear in all vars.	.016 (.012)	.015 (.011)	-.004 (.004)
Baseline Model, Semi-Log (FSS linear)	.002[b] (.00087)	.0043[a] (.001)	-.001 (.00095)
Average FSS over 2 years	.148[b] (.059)	.317[a] (.089)	-.054 (.061)
Limit sample to largest 100 counties	.131[a] (.047)	.207[a] (.066)	.026 (.051)
Limit sample to largest 50 counties	.223[a] (.076)	.252[b] (.101)	.114 (.078)
Add region/year Fixed Effects	.067[c] (.037)	.152[a] (.046)	-.038 (.040)
Add Census Division/year Fixed Effects	.068[c] (.035)	.162[a] (.044)	-.047 (.038)
Condition on lag dependent variable	.061[c] (.033)	.108[b] (.046)	-.032 (.040)
State-level data, baseline model	.407[a] (.142)	.562[a] (.180)	.106 (.130)
State data, add region/year fixed effects	.270[b] (.119)	.421[a] (.158)	-.017 (.109)
State data, add division/year fixed effects	.335[a] (.114)	.534[a] (.167)	-.066 (.099)
State data, condition on lag dependent variable	.208[b] (.081)	.272[b] (.110)	.103 (.116)

Notes: Unless otherwise noted, analytic sample consists of 200 largest counties in US using data from 1980 to 1999. Each cell in table presents the coefficient estimate and standard error for the log of FSS(t-1) (except for the last row), with the robbery rate, burglary rate, indicators for missing values for robbery and burglary, and % black as covariates. Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight.

a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

TABLE 6
LONG-DIFFERENCE ESTIMATES, COUNTY-LEVEL DATA

	Ln(homicides)	Ln(gun homicides)	Ln(non-gun homicides)
1999 versus 1980	.304[c]	.782[b]	-.300
	(.154)	(.191)	(.183)
1998-99 vs 1980-81	.311[c]	.822[b]	-.167
	(.168)	(.217)	(.173)
1997-99 vs 1980-82	.471[b]	.988[b]	-.091
	(.166)	(.216)	(.160)
1996-99 vs 1980-83	.496[b]	.954[b]	-.019
	(.169)	(.225)	(.152)

Notes: Estimates derived from a first-difference equation for the variables listed in the baseline model (final column, Table 4) averaged over the years shown in the first column in the present table, calculated using weighted least squares with each county's 1999 population as a weighting variable.
a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

TABLE 7
SPECIFICATION CHECKS FOR COUNTY AND STATE RESULTS
1980-1999

Outcome:	200 Largest County data	State data
Ln(UCR murder)	.073 [c]	.645 [a]
	(.043)	(.200)
Ln(UCR rape)	-.012	-.201
	(.048)	(.382)
Ln(UCR aggravated asslt)	-.040	.275
	(.038)	(.168)
Ln(UCR larceny)	.004	.096
	(.015)	(.074)
Ln(UCR MV theft)	.041	.046
	(.038)	(.189)
Ln(Fatality rate from falls)	N/A	.058
		(.158)
Ln(MV crash fatality rate)	N/A	.081
		(.068)

Notes: Each cell present the coefficient and standard errors (adjusted for serial correlation) for a separate regression of the log of lagged FSS against the outcome measure described in the first column, controlling for the log of the robbery and burglary rates as well as the other covariates described in the final column of Table 4. The county-level regressions condition on county and year fixed effects and weight by county population, using a sample of the 200 largest counties in the U.S.; the state-level regressions condition on year and state fixed effects, as well as the weight by state population.
a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

TABLE 8
EFFECTS OF GUN OWNERSHIP ON YOUTH HOMICIDES
STATE DATA, 1980-1999

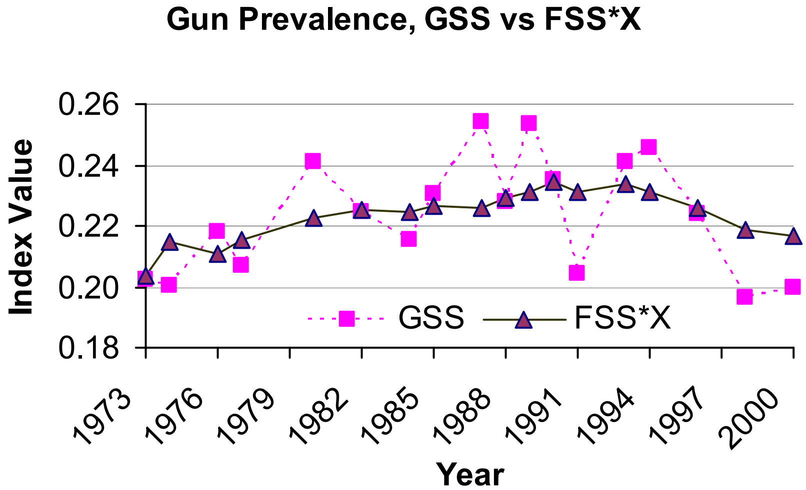
	Ln(Hom 15-19)	Ln(Gun hom 15-19)	Ln(Nongun 15-19)
State data	.593[a]	.458[b]	-.053
Ln(State FSS)	(.194)	(.205)	(.373)

Notes: Each cell presents a coefficient and standard error (adjusted for serial correlation) from a separate regression. Each regression controls for the log of the state's burglary and robbery rate and percent black, log state alcohol consumption per capita, and year and state fixed effects. Estimates are calculated using state populations as weights.
a = Statistically significant at 1 percent.

TABLE 9
INCREASE IN HOMICIDES RESULTING FROM 10,000 ADDITIONAL HANDGUN-OWNING HOUSEHOLDS

Homicide Rate/ 100,000	10% prevalence	20% prevalence	30% prevalence
5	1.0	0.5	0.3
10	2.0	1.0	0.7
15	3.0	1.5	1.0
20	4.0	2.0	1.3

Appendix Figure A1



APPENDIX 1

National Household Prevalence of Handgun Ownership, and FSS

Year	Handgun Prevalence General Social Survey	FSS	Est. Standard Error (Sample error)
1973	0.2023	0.5302	0.0128
1974	0.2001	0.5595	0.0128
1976	0.2181	0.5489	0.0132
1977	0.2066	0.5608	0.0127
1980	0.2407	0.5790	0.0137
1982	0.2245	0.5864	0.0133
1984	0.2155	0.5843	0.0132
1985	0.2303	0.5895	0.0132
1987	0.2541	0.5889	0.0140
1988	0.2281	0.5975	0.0166
1989	0.2536	0.6013	0.0166
1990	0.2348	0.6110	0.0172
1991	0.2045	0.6013	0.0158
1993	0.2411	0.6090	0.0160
1994	0.2458	0.6026	0.0119
1996	0.2237	0.5878	0.0117
1998	0.1961	0.5699	0.0113
2000	0.1998	0.5651	0.0115

Notes: FSS is computed as the ratio of firearms suicides to all suicides recorded in the U.S. Vital Statistics mortality data. Handgun Prevalence is the estimated percentage of households that have at least one handgun. The Estimated Standard Error is computed as $[P/(1-P)/(N*2/3)]^{1/2}$*, where P is estimated handgun prevalence and N is the sample size. The adjustment factor 2/3 is included at the suggestion of GSS documentation to correct for geographic clustering in the sample:*
http://www.icpsr.umich.edu:8080/GSS/rnd1998/appendix/apdx_a.htm

Source: For GSS: web: http://www.icpsr.umich.edu:8080/GSS/homepage.htm
For FS/S: 1979-2000 WONDER http://wonder.cdc.gov FSS data from 1973-1977
Monthly vital statistics report (Hyattsville, Md.) ; Hyattsville, MD: U.S. Dept. of Health, Education, and Welfare, Public Health Service, National Center for Health Statistics

APPENDIX 2
Data Sources for Analyses

County population data-Data for years 1979 to 2000. Downloaded from U.S. Census Bureau data for each county available through the CDC Wonder website: http://wonder.cdc.gov/census.html.

State population data-Data for years 1981 to 2000. Downloaded from state population numbers used to calculate homicide rates in Fatal Injury Mortality reports available from CDC's National Center for Injury Prevention and Control's Web-based Injury Statistics Query and Reporting System (WISQARS) website, http://webappa.cdc.gov/sasweb/ncipc/mortrate.html.

Homicide data-Data was obtained from three sources: Vital Statistics-United States Department of Health and Human Services Mortality Detail Files 1979-1992, obtained via ICPSR Study Datasets: 7632 (1979-1991) and 6798 (1992); National Center for Health Statistics' Multiple Causes of Death data 1993-1999, obtained via ICPSR Study Datasets 6799 (1993), 2201 (1994), 2392 (1995), 2702 (1996), 3085 (1997), 3306 (1998), and 3473 (1999); Uniform Crime Reports County Offense Data, 1979-2000, obtained via ICPSR Study Datasets: 8703 (1979-83), 8714 (1984), 9252 (1985 and 1987), 9119 (1986), 9335 (1988), 9573 (1989), 9785 (1990), 6036 (1991), 6316 (1992), 6545 (1993), 6669 (1994), 6850 (1995), 2389 (1996), 2764 (1997), 2910 (1998), 3167 (1999), and 3451 (2000); and the FBI's Uniform Crime Reports Supplementary Homicide Reports (SHR) 1976-1999, obtained from ICPSR Study No. 3180. Separate state data was obtained for homicide from CDC WISQARS dataset for 1981-2000.

Suicide data- From Vital Statistics-United States Department of Health and Human Services Mortality Detail Files 1979-1992, obtained via ICPSR Study Datasets: 7632 (1979-1991) and 6798 (1992); and National Center for Health Statistics' Multiple Causes of Death data 1993-1999, obtained via ICPSR Study Datasets 6799 (1993), 2201 (1994), 2392 (1995), 2702 (1996), 3085 (1997), 3306 (1998), and 3473 (1999). Separate state data was obtained for suicide from CDC WISQARS dataset for 1981-2000.

Other Crime data (Robbery, Burglary)-From the FBI's Uniform Crime Reports, County-Level Offense Data, 1979-2000. Obtained via ICPSR Study Datasets: 8703 (1979-83), 8714 (1984), 9252 (1985 and 1987), 9119 (1986), 9335 (1988), 9573 (1989), 9785 (1990), 6036 (1991), 6316 (1992), 6545 (1993), 6669 (1994), 6850 (1995), 2389 (1996), 2764 (1997), 2910 (1998), 3167 (1999), and 3451 (2000).

Urban Population Data-Calculated for 1990 and 2000 from Decennial Census data from US Census Bureau. 1990 Data from Census of Population and Housing, Summary Tape File 3C, downloaded from ICPSR Study No. 6054. Data for 2000 was for US Census Bureau's Census 2000 Gateway website, Summary Tape File 3, downloaded from: http://www.census.gov/Press-Release/www/2002/sumfile3.html on March 3, 2004.