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ABSTRACT

This paper develops an estimate of the marginal social costs of gun ownership based on new estimates of the effect of household gun prevalence on homicide rates. Using a panel dataset of 20 annual observations on the 200 largest counties, we estimate an elasticity of homicide with respect to gun prevalence equal to +.10, conditioning on county fixed effects, year fixed effects, burglary and robbery rates, and percent black. Using the same estimation procedure for gun and non-gun homicides separately, we find that all of the effect of gun prevalence is on gun homicide rates. These results are robust to a variety of alternative specifications, including models that condition on year fixed effects defined separately for each region, or the lagged value of the dependent variable. We also apply the same set of procedures to state-level data for the same period, with qualitatively similar results. The elasticity estimates from state-level data are larger and less robust than for county-level data.

All estimates utilize as a proxy for gun prevalence the percentage of suicides committed with a gun. Earlier research has demonstrated that it is superior to other proxies in common use for cross-section analysis. New results presented here provide confirmation of its validity in time-series analysis of repeated cross-sections.

Given that more guns cause more homicides and have little effect on other types of crime, it appears that the marginal external social cost of private gun ownership is positive. The magnitude of this cost increases with the level of crime and violence in the community. While it is not possible to make separate estimates of the effects of different types of guns, it is relevant that handguns, which constitute about one-third of the guns in private hands, account for 80 percent of all homicides. At mean values, an increase of 10,000 handgun-owning households in a county is associated with 1 additional homicide per year. If these lives are valued at just $1 million, the average annual marginal social cost of household handgun ownership is $100. If we instead monetize the harm from gun violence using contingent-valuation estimates, our estimates imply that the average annual social cost per household is on the order of $600.

I. INTRODUCTION

Around one-third of all 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. Understanding the net impact of guns on the public's health and safety is relevant to evaluating the regulation of gun transactions, possession, and use.

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 or negative. One major challenge in developing such estimates is the lack of a direct measure of gun ownership at the level of the state or smaller areas. The most prominent competing studies of how gun prevalence affects homicide, by Lott (1998) and Duggan (2001), have both been subject to criticism for how they measure gun ownership rates. Our new estimates utilize an improved proxy for county and state gun ownership rates, the percentage of suicides committed with a gun (FSS). A second econometric challenge is to identify the effect of gun prevalence on homicide given the possibility of reverse causation and of confounding by unobserved variables. The first objective of our study is to simply establish whether guns impose net benefits or costs on society as a whole by improving on previous research along both of these key dimensions. Moving beyond previous studies, we then attempt to assess the marginal social cost of gun ownership under various circumstances.

Our baseline estimates identify the relationship between guns and homicide using across-county over-time variation, as in Duggan (2001), and imply an elasticity of homicide with respect to gun prevalence equal to around +.10 with county-level data. Separate regressions for gun and nongun homicides yield elasticity estimates of +.19 and -.03 respectively. To address the problem of reverse causation, this baseline specification incorporates the lagged value of FSS. Our findings are quite robust to changes in specification, including attempts to control for unmeasured criminogenic factors by conditioning on the county's robbery and burglary rates, fixed year effects for each census division, and even the lagged value of the county's homicide rate. Evidence that our results are not driven by unmeasured variables that affect an area's crime rate more generally comes from our finding that FSS is not related to other types of crime besides homicide, or (in the state data) with other measures of risky or anti-social behavior such as motor vehicle death rates.

[1] The General Social Survey for 2001 provides a recent estimate; 34.9 percent of all households possessed at least one gun in that year.

As an additional check, we also identify the relationship between guns and homicide using a very different source of variation in gun prevalence. Rather than rely on over-time across-county or -state variation in gun prevalence, we present instrumental variables (IV) estimates that use only the cross-sectional variation in gun prevalence during the 1980's and 1990's that can be explained by across -state variation in the percent of the population that lives in rural areas in 1950. The similarity between these IV estimates and our panel estimates helps establish that the latter are not driven by reverse causation.

The second objective is to determine the marginal social costs of gun prevalence. One implication of our findings is that the cost increases with the homicide rate. At an average homicide rate of 10/100,000, the 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, 2000). 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 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. The costs of gun violence include concern about the risk of injury or death to others within their household or community, as well as the avoidance costs that people undertake to reduce their risk of victimization. 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.

Our approach is a "black box" that does not provide insight on the mechanisms by which the prevalence of gun ownership affects the use of guns in violence. Both direct and indirect mechanisms are relevant. Because they have a gun readily available in case of "need," gun owners are at greater risk of shooting someone than non-owners, other things equal. Gun owners are also a potential source of guns to others who may be at risk of becoming a shooter, with transfers occurring by theft, loan, or informal sale. That is, gun owners form a large, diffuse source of guns to criminals. The relevant markets are not limited to the local jurisdiction. Our county estimates are limited to the effects of within-county ownership, although we recognize the possibility that the prevalence of guns in neighboring counties or even states may also affect the supply of guns to local criminals. That possibility is relevant econometrically, and also with respect to evaluating the appropriate level of gun regulation. If a rural county with low violence rates is a source of supply of guns to criminals in a neighboring urban area, then the case for regulating ownership in such areas is much stronger. Unfortunately we have been unable to derive sensible estimates of the effects of gun prevalence in neighboring counties on homicide rates.

We are able to provide evidence that suggests the importance of the indirect mechanism by which guns reach criminals. The killers of adolescents are quite likely to be gang members or otherwise caught up in criminal activity. Even for this group, homicide rates are strongly influenced by county gun prevalence in our baseline specification.

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 20 percent of gun-owning individuals have only handguns; 44 percent have both handguns and long guns, reflecting the fact that most people who have acquired guns for self-protection are also hunters and target shooters.

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 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

[2] The discussion in this section borrows in part from Cook and Ludwig (2003a).

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.[3]

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 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.[4] 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). Because guns make killing easier, it is reasonable to believe that the presence of a gun in a violent encounter will increase the

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[3] 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.

[4] 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).

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 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 interestingly enough 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 smaller one-time telephone surveys, which provide a basis for asserting that there are millions of defensive gun uses per year (Kleck and Gertz, 1995).

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, although 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 (1998), 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 estimates an elasticity of homicide with respect to state gun ownership rates equal to -3.3 (p. 114). 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 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.[5]

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), who estimates an elasticity of homicide with respect to local gun prevalence equal to +2, while finding that other types of crime are not systematically related to gun ownership rates. Duggan identifies the relationship between guns and crime using over-time variation in panels of states and also counties. One critique of Duggan's study has focused on his proxy for gun prevalence, which is the subscription rate to *Guns and Ammo* magazine – a proxy that has not been well validated.

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,

[5] 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 unlikely to be orthogonal to 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.

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 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.[6] 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.[7]

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

[6] 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).
[7] 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.

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.

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.

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

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. ("Division" fixed effects are included in all cases.) The subscription rate for *G&A* 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); Miller, Azrael and Hemenway 2002a and 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,[8] 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

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[8] 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.

(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 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-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 3.

V. EMPIRICAL STRATEGY

Our basic empirical approach 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 (1), 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 the pool of guns in 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 county'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 are also likely to influence homicide rates (Blumstein 2000). To account for other county characteristics that affect homicide the regression model includes year and county fixed effects, as well as the log of the percent of the county that is black. Our regression estimates are weighted by each county's population to account for heteroskedasticity in the error term.

(1) $$\log Y_{it} = \beta_0 + \beta_1 \log FSS_{it-1} + \beta_2 \log Rob_{it} + \beta_3 \log Black_{it} + d_i + d_t + \varepsilon_{it}$$

Serial correlation. It is reasonable to expect some degree of serial correlation in the error term; for one thing, causal variables that are not fully reflected in this specification may well have jurisdiction-specific trends.

Testing for the presence of serial correlation in fixed-effects models is complicated in applications such as ours, where the time dimension is fairly short compared to the number of observational units (Solon, 1984). One way to determine whether the error structure is serially correlated is to first-difference the data and 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 lagged values results in a coefficient on the one-year lagged residual of -.536 (with a standard error, adjusted for serial correlation, of .028); coefficients on the two- and three-year lagged residual values equal -.334 (.031) and -.153 (.021), respectively.[9] If the error term in level form in our fixed-effects model was serially uncorrelated we would expect the regression of first-differenced residuals against lagged values to yield a coefficient estimate of -.50 for the one-year lag residual with coefficients on higher-order lags equal to zero. While the error structure in our preferred county-level data does not quite fit this pattern, at the very least serial correlation appears to be less of a problem working in level rather than change form. We note that the story is a bit different with the state-level data, in which we find stronger evidence for serial correlation of the errors in level form.[10]

To address the problem of serial correlation in our preferred fixed-effects levels model as in equation (1), we use the approach suggested by Bertrand, Duflo and Mullainathan (2002). Specifically, we estimate Huber-White standard errors that are robust to an arbitrary autocorrelation pattern in the errors over time within counties. Bertrand et al. show that this approach works better than more parametric strategies (such as the standard GLS correction for a first-order autoregressive error process), given that most panels -- including ours -- have no more than 20 years' worth of data.

Measurement error. The proxy for gun prevalence, FSS, is subject to two types of measurement error. 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.

Probably a greater concern 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 10^{th} and 90^{th} 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 percentage points.

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[9] 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. If we simply regress the residual against only its one-year lag value (excluding the two and three year lag values), the coefficients for homicide and gun homicide are on the order of -.40. The estimated autocorrelation coefficients tend to be somewhat larger in absolute value for the log of non-gun homicides.

[10] 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).

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 produce estimates based on state-level data.

Reverse causation. A particular version of the omitted variables problem stems from the possibility of reverse causation, in which unmeasured local-area factors cause crime rates to increase, which then spurs the demand for guns by local residents. Our baseline model attempts to address this problem by using the lagged rather than contemporaneous value of each county's gun ownership rate, and conditioning on the county's current robbery and burglary rates. As an additional check on the problem of reverse causation, we re-estimate our model using a very different source of identifying variation. Rather than rely on across-county, over-time variation in gun ownership rates, we use the cross-sectional variation in gun ownership that can be explained by variation across states in the proportion of the population that lives in a rural area in 1950. This variable is in fact highly correlated with contemporary gun prevalence (Azrael, Cook and Miller 2004; Cook and Ludwig 2003).

Finally, we note that while we estimate levels models with fixed effects, other authors have chosen to estimate first-difference models in similar circumstances – including Duggan (2001). In our case, a first-difference model introduces strong negative serial-correlation. In addition, measurement error with our gun proxy appears to produce greater attenuation of the estimated effect of gun prevalence on homicides in the first-difference model compared to a fixed-effects model estimated in levels.[11] A final reason to prefer the levels model is greater efficiency given a strongly trended independent variable – first differencing the data greatly reduces the range of the independent variable.

[11] Hsiao (1986, p. 64-65) shows that the first-difference estimate for the coefficient on FSS will be attenuated by a factor equal to twice the variance of measurement error in FSS divided by the variance of the first difference in FSS, while the fixed-effects (within) estimator will be attenuated by (T-1)/T times the variance of the measurement error in FSS divided by the variance of FSS after subtracting each observational unit's mean value over our panel period. In our data the first-difference attenuation factor is about 20 percent greater than for the fixed-effects estimator.

VI. RESULTS

Table 3 presents descriptive statistics for the full panel data set assembled from annual data for the 200 largest counties for the years 1979-1999 (all calculations are weighted by county population). Over our entire sample period the average homicide rate is around 11 per 100,000 residents, with around 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 approximately 180 suicides per county per year.

Table 3 also provides some sense for the nature of the variation in gun ownership that identifies our panel-data estimates shown below. The second and third columns of the table present descriptive statistics for the 50 counties with the lowest FSS value in 1980 (that is, where guns are least prevalent at the start of our panel), as well as for the 50 counties with the highest value of FSS in 1980. Table 3 shows that there is substantial variation over time, with somewhat different regional trends. In particular, 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 – which were disproportionately located in the Northeast and Midwest regions of the country – experience an increase in gun prevalence equal to about 25 percent from 1980 to 1990. Gun ownership rates held steady or even declined somewhat in these areas during the 1990's. Table 3 suggests that the identifying variation in FSS within our panel is driven in part by a convergence in gun ownership rates over time between initially low-gun Northeastern and Midwestern counties and the high-gun counties located in the South.

Comparing the high- and low-gun counties in Table 3 also provides some useful intuition about the instrumental variables (IV) strategy that we use as a complement to the panel-data analysis. The IV strategy uses cross-sectional variation in FSS that is driven by variation across states in the proportion of the population living in rural areas in 1950. Table 3 shows that those areas with the strongest rural tradition in 1950 also experienced the largest declines in FSS during the sample period, while those counties that experienced an increase in FSS (at least from 1980 to 1990) had a much lower proportion of their populations living in rural areas in 1950. If guns increase homicide rates, our IV analysis would indicate that in the cross-section homicide levels are higher in the Southern counties where guns are more common in 1980, while the panel analysis would suggest that such counties also experience the most pronounced decline in homicide rates during our observational period.

Baseline results. The top panel of Table 4 presents findings from the county-level panel-data regression analysis, all of which control for fixed county and year effects. The first column of Table 2 reports an elasticity of homicide with respect to the lagged value of FSS equal to +.100. As shown in the second column, this point estimate is not much affected by controlling for several powerful covariates: the log of the contemporaneous UCR burglary and robbery rates, and the log of the county population that is black. One necessary condition for interpreting the estimates in columns 1 and 2 as a causal effect of gun prevalence is for the guns-homicide relationship to be driven by a link between guns and gun homicides specifically. The final four columns of Table 4 demonstrate that this is the case. The estimated elasticity of gun homicides with respect to FSS is equal to around +.19 (regardless of whether or not we condition on the county's robbery and burglary rates), while the elasticity of non-gun homicides with respect to FSS equals -.03 and is not statistically significant.

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 bottom panel of Table 4 suggests that the cost of using lagged rather than contemporaneous values of our gun proxy is modest, at least for our estimates for the effects of gun prevalence on overall homicide: the estimated elasticity of homicides with respect to current FSS is only about twenty percent higher than that estimated for lagged FSS. In contrast the estimated elasticities for the association between FSS and gun- and non-gun homicide rates are each substantially larger in absolute value when using contemporaneous rather than lagged FSS. Because the estimated effects of gun prevalence on gun- and non-gun homicides are of the opposite sign, the increase in the magnitudes of these coefficients largely offset one another when we examine the association of FSS with the overall homicide rate.

Robustness checks. These results are quite robust to alterations in the baseline approach, as shown in Table 5. The first row replicates baseline estimates from Table 4. Comparing the estimates in the first and second row demonstrates that the Huber-White standard errors that are robust to arbitrary forms of autocorrelation are only about one-quarter larger than OLS standard errors that make no adjustments for autocorrelation. This difference is consistent with the finding, reported above, that there is only a modest degree of serial correlation in the process determining county homicide rates.

To check for the possibility of omitted variables related to geographic region, we experiment with introducing separate vectors of "year" fixed effects for each Census Region, and then for each Census Division. These additions have little effect on the estimated elasticity of homicide with respect to gun prevalence (as shown in Table 5). Evidence that the results are not much affected by conditioning on region/year or division/year fixed effects would also seem to rule out bias from the influence of an unmeasured trend in the 'Southern subculture of violence" (Butterfield 1997).

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). 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 relatively larger number of suicides used to compute our proxy in these counties. Estimating a model where all variables are included in linear rather than log form also reduces the magnitude of our 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 Table 5 shows that regressing FSS in linear form against logged homicide rates yields results that are as strong as in our 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.

The final row of Table 5 shows what happens when we re-estimate the baseline model using the proxy for gun prevalence used by Duggan (2001), which is the *Guns and Ammo* magazine subscription rate per 100,000 residents. Because this proxy is only available through 1998, for purposes of comparison we replicate in the next to last row of Table 5 our baseline model estimated using data through 1998. The estimated elasticity of homicide with respect to the *Guns and Ammo* subscription rate is nearly twice as large as what we estimate using FSS. However, unlike with FSS, the *Guns and Ammo* subscription rate also has a strong positive correlation with *non-gun* as well as gun homicides, which suggests that gun magazine subscriptions may also be picking up in part the effects of other unmeasured local-area population characteristics. Our preference for FSS follows from that concern, and from the fact that *Guns & Ammo* subscriptions do not appear to be a valid proxy for the prevalence of gun ownership in the current context (Table 2).[12]

Introducing lagged values. In order to further address the possible dynamic relationship between guns and crime, Table 6 re-estimates the baseline model conditioning on lagged as well as contemporaneous values of burglary and robbery rates, and incorporating one or two additional lags in FSS. This model assumes that homicide rates are responsive to a change in general criminal activity that is captured by burglary and robbery rates. For example, homicides may respond in part to a change in robberies committed by members of one gang against another, or a surge in robbery or burglary rates within an area may reflect an increase in overall gang activity. The data provide some support for the idea that homicide may be part of a dynamic process for violent behavior in general, given that lagged robbery (but not burglary) rates are predictive of future homicide and gun-homicide rates. Note that including these lagged robbery and burglary rates does not have much impact on our estimate for the effects of lagged FSS on homicide.

Dynamic models. The models shown in Table 7 allows for the possibility that homicide itself is to some extent a self-generating process by conditioning on the lagged value of the log of our dependent variable. It would not be surprising if past homicide rates had some direct influence on current homicide rates, and empirical analysis of national homicide rates supports this view (McDowall 2002). Among the processes that would produce this dynamic effect is retaliation. This type of dynamic model will improve upon those presented in Table 6 if homicide is more responsive to previous values of

[12] In Duggan's (2001) analysis of state-level data, the estimated elasticity of nongun homicides with respect to Guns and Ammo subscription rates is about one-third the elasticity for gun homicides without state-specific linear trends included in the model; with such trends included, the elasticity of nongun homicides with respect to G&A subscriptions is very close to zero (Table 6, p. 1099). Separate estimates for gun versus nongun homicides are not presented for Duggan's county-level data analysis.

homicide than other crimes, if for example gang killings occur in retaliation for prior gang killings but not other forms of gang rivalry. The cost of this conceptual refinement is that the coefficient on the lagged value of the dependent variable will be biased in a fixed-effects model, which in principle could also bias our estimate for the effects of gun prevalence on homicide.

The top panel of Table 7 shows that in such a dynamic model FSS still has a positive and statistically significant association with the homicide rates, equal in size to about two-thirds the elasticity found in our baseline model. It is interesting to note that the coefficient of .07 on FSS in this dynamic model implies a steady state effect of +.10, the same as estimated in the baseline model.

To establish reasonable bounds on the size of the bias, we experiment (bottom panel of Table 7) with a sequence of estimates in which the coefficient of the lagged dependent variables is assigned different values (rather than letting the data estimate this relationship). A clear pattern emerges: as the coefficient on lagged Y increases from zero to one, the coefficient on FSS shrinks to zero. But it remains significantly positive for values that somewhat exceed the estimated value. Even if the estimated relationship between lagged and current homicide shown in the top panel of Table 7 is biased downward by up to 40 percent, the relationship between FSS and homicide is still statistically significant at the 10 percent level. If the estimated coefficient on lagged Y is larger than the true value then the coefficient on FSS in the dynamic model is close to that in the baseline model.

Reducing measurement error. Table 8 reports results of several efforts to reduce the problem of measurement error in FSS, which introduce negative bias in the baseline estimates of the effect on homicide. As expected, improving the signal-to-noise ratio of FSS by either focusing on the largest 100 or 50 counties or using the proportion of suicides that involve a firearm averaged over two years increases the size of the association between guns and homicide rates.

Table 9 presents estimates using the baseline specification with state-level data. The pattern of coefficient estimates is qualitatively similar to those derived from county data, although the elasticity estimates are much larger. Note that adjusting for serial correlation in the state data roughly doubles the size of the standard errors, while correcting for serial correlation in our preferred county data increased the standard errors by only around one-quarter. These findings are consistent with the results reported earlier that serial correlation is more pronounced in the state data compared to the county data.

Unmeasured covariates. 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 unmeasured variables that may influence crime rates. In the spirit of Altonji, Elder and Taber (2000, 2002), one test of this possibility is determining whether FSS predicts outcomes that logically have little relationship to gun prevalence. Table 10 reports the results of running the baseline regression on rates of other types of crime, 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] (The final row of Table 10 reports the result of the baseline regression with the UCR measure of homicide used in place of our preferred Vital Statistics measure.)

Reverse causation. As a further specification experiment, we identify the effects of gun prevalence on homicides using a very different source of variation – cross-sectional differences in gun ownership rates across counties that are explained by differences in the proportion of each county's surrounding state that lived in rural areas in 1950 (Azrael, Cook and Miller 2004; Cook and Ludwig 2003). Previous survey research indicates that gun ownership is strongly associated with rural life, and that growing up with guns is a strong predictor of having a gun during adulthood (Cook and Ludwig, 1996). The proportion of a state's population that lived in a rural area in 1950 strongly predicts current gun ownership; regressing the log of FSS at the county level against the log of the state's percent population in rural areas in 1950, controlling for our other covariates and year (but not county) fixed effects, yields an F statistic for percent rural 1950 equal to 18.5. The partial R-squared of percent rural 1950 in this equation equals .184. We note that an area's rural tradition is highly predictive of current gun ownership even after controlling for current urbanicity, as captured by county-level census data from 1990 and 2000. (Standard errors in these first-stage equations and our second-stage IV estimates presented below are adjusted to account for the clustering of counties within states).

Table 11 presents the second-stage estimates from the instrumental variables (IV) analysis that uses the log of percent rural in the state in 1950 as the instrument. The IV estimates imply an elasticity of homicide with respect to the gun prevalence equal to +.56.

Given the nature of the instrument, it is safe to say this estimate is not driven by reverse causation. Whether the IV estimates are affected by unmeasured variables is more difficult to determine, since an area's rural tradition may influence homicide rates through a variety of social, cultural or policy factors that are not explicitly modeled in our estimating equation. Unlike with our panel-data analysis, the IV estimates suggest a statistically significant, positive relationship of gun prevalence with other crime rates beyond homicide, in particular larceny and rape (although not motor vehicle theft or aggravated assault). While we cannot rule out the possibility of omitted variables bias with our IV estimates for homicide, this problem may lead us to understate rather than overstate the effects of gun prevalence to increase homicides. Denote the omitted variable or variables as "rural culture." If rural culture tends to be less violent than its converse (as suggested by the fact that rural areas have lower violent crime rates on average than urban areas) then the omitted variable, presumably positively correlated with the instrument, is negatively correlated with homicide.[14]

[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.

[14] See Cook and Ludwig (2003b) for a formalization of this argument. As in that paper, our IV estimates suggest a positive elasticity of homicide with respect to guns even with no other covariates included in the

Finally, Table 12 provides suggestive evidence that gun prevalence leads to elevated rates of homicide through the transfer of guns from "legal" to "illegal" owners, 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. 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

The results suggest that 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 those costs is suggested by the elasticity estimates of homicide with respect to FSS. The following table lists some estimates. (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.)

Elasticity of homicide with respect to FSS	Source and comments
.10	Baseline-specification estimate using data on 200 counties
.07	200-county estimate incorporating lagged dependent variable into baseline specification. The implied steady-state elasticity is .10.
.20	200-county estimate from baseline specification when FSS is estimated from a moving 3-year average
.36	Estimate based on state data with baseline specification plus division/year fixed effects

These elasticity estimates with respect to FSS also serve as estimated elasiticities 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

regression model. This is relevant because signing the bias with our IV estimates is straightforward with no other conditioning variables in the first or second stage equation, but more complicated when we control for other factors.

appears to be 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 indicate 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:

(2) Ratio of changes in homicides to gun-owning households = ehn/g

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), a conservative choice of elasticity of 0.1, and 2 people per household, then the formula indicates one additional homicide per year for every 10,000 additional handgun-owning households.

The following table 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:

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

Based on our results we believe that an elasticity as high as 0.2 or 0.3 is plausible, in which case all of these estimates should be doubled or 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 nth 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.[15]

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

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 suggest a value of around $1 million per assault-related gunshot injury (Cook and Ludwig, 2000, Ludwig and Cook, 2001). Previous research suggests that on average around one in six assault-related gunshot injuries results in death (Cook, 1985, Cook and

[15] 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.

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). In principle this substitution effect will be reflected in the willingness to pay figures reported by CV respondents.[16] 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 +.20 or +.30 rather than +.10, the optimal license fee may be as high as $1,200 or $1,800 per household.

VII. DISCUSSION

Our empirical analysis of county- and state-level data finds that gun prevalence is positively related to overall homicide rates. This 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-and-year specific fixed effects. These results are unlikely to be due to reverse causation because we regress homicide rates against the lagged value of gun prevalence, and because we derive qualitatively similar findings when we identify the gun-homicide relationship using cross-sectional variation in each state's rural tradition (as captured by the percent of the state's population living in rural areas in 1950).

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 ident ify a factor that influences the lethality but not the overall volume of crime within an area. Firearm ownership is presumably the leading candidate explanation for this relationship. That conclusion is

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[16] 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).

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 regulatory costs 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,[17] 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.

Given 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.

While our 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.

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[17] 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).

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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] (.354)	.485[c] (.250)	1.108[a] (.417)	.737[b] (.305)
G&A	.358 (1.196)	.713 (.835)	-.055 (1.419)	.861 (1.027)
No "year" fixed effects				
FSS	.812[a] (.285)	.554[a] (.201)	.905[a] (.355)	.742[a] (.246)
G&A	-.762 (1.013)	.403 (.713)	-2.219[c] (1.240)	.201 (.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)	50 counties w/ lowest FSS in '80	50 counties w/ highest FSS in '80
Full period (1980-2000)			
% rural 1950	29.4	21.3	42.0
Northeast	26.8	59.1	0
South	25.4	2.8	71.3
Midwest	19.6	28.7	14.2
West	26.2	9.5	14.6
FSS	49.9	37.5	66.3
Homicide rate	11.0	8.9	14.2
Gun homicide rate	7.3	5.4	9.9
# suicides	195.8	132.0	116.8
Robbery rate	334.5	335.5	309.7
G&A/100,000	180.7	153.7	208.6
1980			
FSS	48.0	32.4	73.0
Homicide rate	12.9	10.1	16.4
Gun homicide rate	8.4	5.8	11.8
Robbery rate	302.0	331.1	303.5
G&A/100,000 (81)	184.3	138.5	226.5
1990			
FSS	52.8	40.3	68.5
Homicide rate	12.5	10.6	16.1
Gun homicide rate	8.5	6.8	11.2
Robbery rate	382.00	373.3	360.7
G&A/100,000	202.7	181.8	219.8
1999			
FSS	48.0	38.5	59.5
Homicide rate	6.7	6.1	8.8
Gun homicide rate	4.8	4.5	6.7
Robbery rate	227.7	280.2	246.2
G&A/100,000 (98)	162.2	136.9	191.3

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
200 LARGE COUNTIES, 1980-1999

Panel regression results with county and year fixed effects
Coefficients and standard errors

	Ln(Homicide)	Ln(Homicide)	Ln(Gun Homicide)	Ln(Gun Homicide)	Ln(Nongun Homicide)	Ln(Nongun Homicide)
1. lagged FSS						
Ln FSS(t-1)	.100 [b] (.044)	.102 [a] (.037)	.191 [a] (.065)	.192 [a] (.051)	-.036 (.043)	-.030 (.040)
Ln Rob(t)		.138 [a] (.042)		.215 [a] (.050)		.005 (.046)
Ln Burg(t)		.258 [a] (.067)		.270 [a] (.091)		.286 [a] (.058)
Ln % Black(t)		.286 [c] (.162)		.357 (.246)		.538 [a] (.174)
R-squared	.915	.921	.896	.903	.836	.843
2. contemp FSS						
Ln FSS(t)	.123[a] (.039)	.118[a] (.035)	.289 [a] (.058)	.277 [a] (.053)	-.089 [c] (.045)	-.087 [b] (.041)
Ln Rob(t)		.129 [a] (.041)		.210 [a] (.049)		-.008 (.043)
Ln Burg(t)		.258 [a] (.066)		.257 [a] (.091)		.302 [a] (.055)
Ln % Black(t)		.309 [b] (.149)		.359 (.226)		.525 [a] (.157)
R-Squared	.914	.921	.895	.902	.836	.843
N	3,822	3,822	3,771	3,771	3,785	3,785

Notes Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight.

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
200 LARGE COUNTIES, 1980-1999

Panel regression results with county and year fixed effects
Coefficients on ln FSS(t-1) and standard errors

	Ln(Homicide)	Ln(Gun Homicide)	Ln(Non-gun Homicide)
Baseline model, from Table 4	.102[a] (.037)	.192[a] (.051)	-.030 (.040)
Without serial correlation correction	.102[a] (.030)	.192[a] (.040)	-.030 (.037)
Add region/year Fixed Effects	.085[b] (.037)	.171[a] (.046)	-.036 (.040)
Add Census Division/year Fixed Effects	.083[b] (.035)	.179[a] (.044)	-.047 (.039)
Baseline Model, Unweighted	.063 (.044)	.182 [a] (.044)	-.059 (.041)
Baseline Model, Linear in all vars.	.017 (.014)	.016 (.013)	-.004 (.004)
Baseline Model, Semi-Log (FSS linear)	.00243[a] (.00088)	.00478[a] (.00111)	-.00103 (.00095)
Baseline through '98	.106[a] (.037)	.203[a] (.053)	-.034 (.038)
Guns & Ammo proxy through '98	.193[a] (.064)	.219[b] (.098)	.130[b] (.055)

Notes: 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
INCORPORATING LAGS IN FSS, BURGLARY AND ROBBERY
200 LARGE COUNTIES, 1980-1999

Panel regression results with county and year fixed effects
Coefficients and standard errors

	Ln(Hom)	Ln(hom)	Ln(Gun Hom)	Ln (Gun Hom)	Ln(Non-gun Hom)	Ln(Non-Gun Hom)
FSS(t-1)	.106 [a] (.039)	.077 [c] (.040)	.203 [a] (.049)	.151 [a] (.056)	-.033 (.042)	-.033 (.045)
FSS(t-2)		.100 [b] (.041)		.218 [a] (.068)		-.036 (.050)
Rob(t)	.101 [b] (.048)	.102 [b] (.049)	.177 [a] (.057)	.182 [a] (.058)	-.006 (.059)	-.009 (.064)
Rob(t-1)	.134 [b] (.060)	.163 [a] (.053)	.160 [b] (.080)	.147 [c] (.078)	.057 (.065)	.139 [b] (.064)
Rob(t-2)		-.048 (.056)		.001 (.081)		-.106 [c] (.060)
Burglary(t)	.170 [a] (.064)	.204 [a] (.071)	.126 (.084)	.171 [c] (.093)	.242 [a] (.067)	.248 [a] (.071)
Burg(t-1)	.092 (.072)	.040 (.062)	.148 (.103)	.105 (.086)	.061 (.080)	-.052 (.080)
Burg(t-2)		.052 (.086)		.046 (.113)		.111 (.078)

Notes: Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight. Specifications include county and year fixed effects as well as % black and indicators of missing values for robbery and burglary.

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

TABLE 7
DYNAMIC MODELS
200 LARGE COUNTIES, 1980-1999

A. Baseline regression with lag of dependent variable as covariate

Panel regression results with county and year fixed effects
Coefficients and standard errors

	Ln(Homicide)	Ln(Gun Homicide)	Ln(Nongun Homicide)
Ln FSS(t-1)	.070 [b] (.031)	.119 [a] (.047)	-.029 (.040)
Ln Y(t-1)	.324[a] (.029)	.330[a] (.032)	.061[a] (.022)
Ln Rob(t)	.096 [a] (.034)	.150 [a] (.039)	.003 (.045)
Ln Burg(t)	.197 [a] (.048)	.210 [a] (.064)	.278 [a] (.056)
Ln % Black(t)	.204 [c] (.112)	.281 (.173)	.530[a] (.164)

Notes: Parentheses contain standard errors adjusted for serial correlation (see text). Indicators for missing values for burglary and robbery included but not shown. Estimates utilize county population as weight.

B. Experiments with alternative weights on lagged Y

Panel regression results with county and year fixed effects
Coefficients and standard errors for ln(FSS(t-1))

Dependent Variable	Y = Homicide	Y = Gun Homicide	Y = Nongun Homicide
Ln Y(t)	.103[a] (.037)	.197[a] (.052)	-.034 (.040)
Ln Y(t) – Ln [.1*Y(t-1)]	.092[a] (.035)	.174[a] (.049)	-.027 (.039)
Ln Y(t) – Ln [.324*Y(t-1)]	.070[b] (.031)	.119[a] (.047)	-.029 (.040)
Ln Y(t) – Ln [.5*Y(t-1)]	.053[c] (.030)	.078[c] (.043)	.002 (.039)
Ln Y(t) – Ln [.7*Y(t-1)]	.033 (.030)	.031 (.045)	.016 (.042)
Ln Y(t) – Ln [Y(t-1)]	.003 (.032)	-.040 (.052)	.038 (.049)

Notes: Each cell in table presents the coefficient estimate and standard error for ln FSS(t-1), using the baseline specification. Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight.
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 8
EFFECTS OF REDUCING MEASUREMENT ERROR
LARGE COUNTIES, 1980-1999

Panel regression results with county and year fixed effects, Baseline specification
Coefficients on lagged ln(FSS) (as defined) and standard errors

	Ln(Homicide)	Ln(Gun Homicide)	Ln(Non-gun Homicide)
Baseline model Largest 200 counties	.102 [a] (.037)	.192 [a] (.051)	-.030 (.040)
Sample restricted to Largest 100 counties	.139 [a] (.047)	.209 [a] (.069)	.034 (.049)
Sample restricted to Largest 50 counties	.226 [a] (.073)	.253 [b] (.104)	.111 (.077)
FSS averaged over 2 years	.174 [a] (.059)	.347 [a] (.092)	-.050 (.059)
FSS averaged over 3 years	.203 [b] (.079)	.394 [a] (.127)	-.044 (.066)

Notes: Each cell in table presents the coefficient estimate and standard error for ln FSS(t-1), with the baseline covariates: robbery rate, burglary rate, % black . Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weights.
a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

TABLE 9
RESULTS FROM STATE DATA, 1981-1999
Panel regression results with state and year fixed effects
Coefficients and standard errors (corrected for serial correlation)

	Ln(Homicide)	Ln(Homicide)	Ln(Gun Homicide)	Ln(Gun Homicide)	Ln(Nongun Homicide)	Ln(Nongun Homicide)
Ln FSS(t-1)	.598[a] (.238)	.477[a] (.160)	.883[a] (.300)	.720[a] (.223)	.167 (.193)	.069 (.127)
Ln Rob(t)		.254[a] (.065)		.331[a] (.093)		.106[a] (.036)
Ln Burg(t)		.304[a] (.076)		.296[a] (.108)		.309[a] (.062)
Ln Alc(t)		-.072 (.267)		-.493 (.337)		.657[a] (.128)
Ln % Black(t)		.503[a] (.184)		.552[b] (.228)		.605[a] (.147)
R-squared	.927	.952	.919	.939	.878	.916

NOTES: Parentheses contain standard errors adjusted for serial correlation (see text). All regressions include, in addition to the variables shown, indicators for missing data on burglary and robbery. Estimates utilize state population as weight. Data are for 50 states and the District of Columbia, N=913.

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

TABLE 10
SPECIFICATION CHECKS FOR COUNTY AND STATE RESULTS
1980-1999

Panel regression results with county or state and year fixed effects
Coefficients on ln FSS(t-1) and standard errors

Outcome:	200 Largest County data	State data
Ln(UCR murder)	.087[c] (.047)	.758 [a] (.238)
Ln(UCR rape)	-.027 (.047)	-.330 (.406)
Ln(UCR aggravated asslt)	-.026 (.041)	.300 (.181)
Ln(UCR larceny)	.008 (.017)	.090 (.081)
Ln(UCR MV theft)	.030 (.038)	.052 (.173)
Ln(Fatality rate from falls)	N/A	-.114 (.172)
Ln(MV crash fatality rate)	N/A	.099 (.064)

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 percent black. The county-level regressions condition on year and county 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 log of alcohol consumption per capita, and weight by state population.
a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

TABLE 11
2-STAGE LEAST SQUARES RESULTS
200 LARGE COUNTIES, 1980-1999

A. 2nd Stage with % Rural 1950 as instrument

Panel regression results with year fixed effects
Coefficients and standard errors

	Ln(Homicide)	Ln(Gun Homicide)	Ln(Nongun Homicide)
FSS(t-1) (from 1st stage)	.558[b] (.215)	.966[a] (.312)	.148 (.153)
Ln Rob(t)	.803[a] (.092)	1.019[a] (.146)	.554[a] (.056)
Ln Burg(t)	.218[b] (.109)	.116 (.137)	.317[a] (.100)
Ln % Black(t)	.039 (.073)	-.003 (.109)	.048 (.042)
Ln % Urban 1990	.239 (.227)	.373 (.279)	.213 (.220)

Notes Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight.

B. 2nd Stage Results for 5-Year Intervals

Panel regression results with year fixed effects
Coefficients and standard errors on fitted value of log FSS(t-1), Homicide

Interval	Dependent Var. = Ln Homicide Coefficient and SE on FSS(t-1) (fitted)
1980-1984	.696[a] (.172)
1985-89	.637[b] (.311)
1990-94	.743[a] (.212)
1995-1999	.418 (.284)

Notes Parentheses contain standard errors adjusted for serial correlation (see text). Estimates utilize county population as weight.

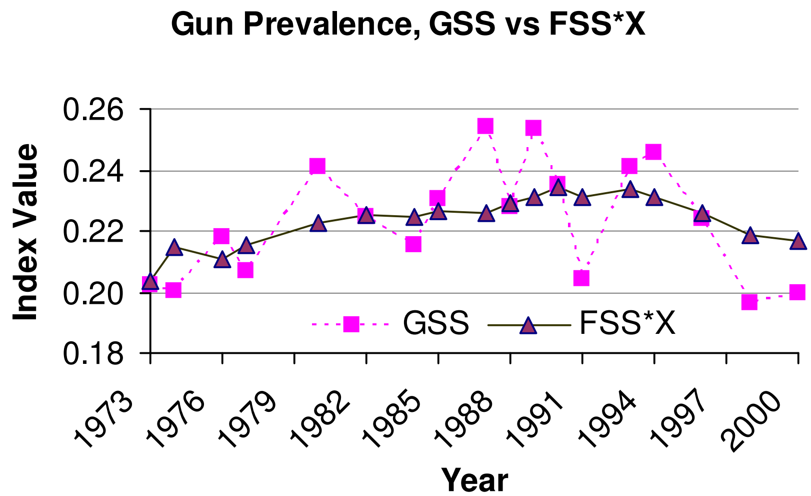
a. Significantly different from zero at the 1% level
b. Significantly different from zero at the 5% level
c. Significantly different from zero at the 10% level

TABLE 12
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			
Ln(State FSS)	.688[a]	.624[a]	-.069
	(.239)	(.273)	(.348)

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.

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
SENSITIVITY ANALYSES
STATE DATA, 1980-1999

Panel regression results with state and year fixed effects
Coefficients on lagged FSS and standard errors

	Ln(Homicide)	Ln(Gun homicide)	Ln(Non-gun homicide)
No covariates other than state / year FE's	.598 [b] (.238)	.883 [a] (.300)	.167 (.193)
Baseline model, from Table 9	.477 [a] (.160)	.720 [a] (.223)	.069 (.127)
Baseline model, no serial correlation correction	.477 [a] (.084)	.720 [a] (.114)	.069 (.089)
Add region/year FE's	.320 [a] (.119)	.515 [a] (.165)	-.015 (.107)
Add division/year FE's	.362 [a] (.116)	.585 [a] (.177)	-.068 (.102)
Baseline, Unweighted	.330 [b] (.138)	.539 [a] (.163)	.028 (.132)
Baseline, Linear	.111 [a] (.046)	.093 [b] (.038)	.018 [c] (.011)
Add log Y(t-1) as covariate	.236 [a] (.081)	.332 [a] (.121)	.069 (.113)
Add ln rob(t-1), & ln burg(t-1)	.436 [a] (.147)	.634 [a] (.204)	.085 (.125)

Notes: Sample consists of data for each state for 1980-1999. Each cell in table presents coefficient estimate and standard errors (adjusted for serial correlation) for the log of the lagged value of FSS (gun prevalence), conditional on log burglary, robbery, percent black, and alcohol consumption per capita, as well as state and year fixed effects. Estimates weighted by state population.
a = Statistically significant at 1 percent.
b = Statistically significant at 5 percent.
c = Statistically significant at 10 percent

APPENDIX 3
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.