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VIOLENCE AND U.S. PROHIBITIONS
OF DRUGS AND ALCOHOL

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ABSTRACT

Among the many unresolved questions regarding the determinants of violence is the role of prohibitions against drugs and alcohol. Conventional wisdom holds that consumption of these goods encourages violence and that prohibitions discourage such consumption; thus, prohibitions reduce violence. An alternative view, however, is that prohibitions create black markets, and in black markets participants use violence to resolve commercial disputes. Thus, prohibitions potentially increase violence.

This paper examines the relation between prohibitions and violence using the historical behavior of the homicide rate in the United States. The results document that increases in enforcement of drug and alcohol prohibition have been associated with increases in the homicide rate, and auxiliary evidence suggests this positive correlation reflects a causal effect of prohibition enforcement on homicide. Controlling for other potential determinants of the homicide rate -- the age composition of the population, the incarceration rate, economic conditions, gun availability, and the death penalty -- does not alter the conclusion that drug and alcohol prohibition have substantially raised the homicide rate in the United States over much of the past 100 years.

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1 Introduction

The control and reduction of violent behavior is one of the primary concerns of modern societies. Virtually all countries proscribe most types of violence, and they expend considerable resources to deter, control and punish violent acts. At the same time, many academic disciplines have labored long and hard to understand both the reasons for violence and the ability of different policies to control it. Yet there is so far little consensus regarding the causes of violence, and debates about appropriate policies rage in academic and non-academic circles around the world.

One question that arises routinely in discussions of violent behavior and appropriate policy responses is the role of drugs and alcohol. A widely held view is that consumption of these commodities causes violence because such consumption induces aggression or releases inhibitions. Thus, economies routinely adopt polices aimed at reducing consumption of drugs and alcohol, in part to reduce the incidence of violent acts. In the most extreme cases, societies prohibit the production, sale and possession of these commodities, with the purported goal of eliminating the violence induced by their consumption.

Yet on *a priori* grounds it is not obvious whether prohibitions of alcohol and drugs decrease or increase violent behavior. Prohibitions often give rise to black markets, and in black markets participants cannot easily use the courts to resolve commerical disputes. One obvious alternative is violence, and anecdotal evidence suggests that black market participants often use violence to resolve disputes. This possibility does not mean prohibitions necessarily increase violence, since drug or alcohol consumption might induce violence, and prohibitions might reduce such consumption. But the fact that prohibitions can also encourage violence means their net effect could be small or even positive. Thus, it is necessary to evaluate the relation between prohibitions and violence empirically.

This paper considers the relation between violence and prohibitions of drugs and alcohol by examining the historical behavior of the homicide rate in the United States. The paper demonstrates that prohibitions of drugs and alcohol have been associated with increased rather than decreased homicide rates over the past century, and it argues that at least some of this correlation reflects a causal influence of prohibition enforcement on violence. Assuming the estimated relation is causal, the estimates presented here suggest the homicide rate is currently 25-75% higher than it would be in the absence of drug prohibition. Controlling for other potential determinants of the

1

homicide rate – the age composition of the population, the incarceration rate, economic conditions, gun availability, or the death penalty – does not alter this conclusion, although certain of these factors – particularly the age structure and the incarceration rate – appear to play important roles.

The remainder of the paper is organized as follows. Section 2 presents the theoretical framework that guides the empirical work and then discusses the estimation issues that arise in attempting to confront this framework with data. The framework incorporates three ideas that have not previously been formalized in the literature: that violence results from a demand for dispute resolution; that the degree to which a prohibition is enforced determines the impact of that prohibition on violence; and that expenditure for enforcement of prohibition is a good measure of the degree to which a prohibition is enforced. The key issue that arises in attempting to examine this framework empirically is that enforcement expenditure is potentially endogenous with respect to the homicide rate. I explain that the nature of the federal budget process in the United States allows creation of an enforcement series that is at least predetermined relative to the homicide rate, which reduces the possibility of bias in estimating the effect of enforcement on homicide. I also explain that any remaining bias is not necessarily large and might even cause the estimates to understate the role of prohibition enforcement in increasing violence.

Section 3 presents the empirical part of the paper. I start simply by examining the time series data on the homicide rate in the United States, since this examination suggests a number of the main conclusions of the paper. I then consider explicitly the framework suggested by the theoretical discussion and present estimates of this model. The results show that expenditure for enforcement of alcohol and drug prohibition have been positively associated with the homicide rate in the United States, consistent with the view that increased prohibition enforcement encourages the substitution of violent for non-violent dispute resolution mechanisms. The result is robust with respect to controls for a broad range of other factors, including demographics, economic conditions, incarceration effects, gun availability, the death penalty, and support for drug legalization.

I conclude the paper by discussing implications of the results for the understanding of violence generally. The fact that enforcement of drug and alcohol prohibition appear to have affected violence rates over time in the United States suggests reconsideration of cross-sectional differences in rates of violent behavior. These differences are often dramatic, yet existing research has not explained these differences in terms of observable variables.

2

The analysis here suggests that differences across locations in enforcement of drug prohibition – or of prohibitions against prostitution, gambling, or even guns – might be an important omitted factor in much previous research. Examination of this hypothesis is outside the scope of this paper, but casual observation is consistent with the hypothesis.

More generally, the view that violence is a dispute resolution mechanism suggests an economic approach to studying violence that has received little previous attention. In the economics-of-crime literature, the motivations for violence are not explored in detail; most research examines the deterrence of violence (or crime generally), assuming a supply of violent acts exists. The work here suggests putting greater emphasis on understanding violence in the first place: what determines the number and severity of disputes, and what determines the methods used to resolve them. This perspective provides a unified framework for analyzing many kinds of violence and suggests a number of questions for future research.

2 Framework for Analysis

This section presents a simple theory of violence and then discusses estimation of the key effect suggested by the theoretical discussion. The model focuses on one particular hypothesis, but the empirical work below allows for a broader range of determinants of violence.

2.1 Prohibitions and the Demand for Violence

The critical idea of this paper is the following. The activities of any market give rise to commerical disputes, and these disputes give rise to a demand for dispute resolution. In general there are many methods for resolving disputes – negotiations, lawsuits, arbitrations, and the like – each of which relies ultimately on an official dispute resolution system – courts or government regulatory agencies. Alternatively, participants in any market can attempt to "resolve" disputes with violence. Evidently, participants in legal markets generally find it cheaper to use "lawyers" than to use violence, since violence is rarely observed as a dispute resolution system in the markets for non-prohibited commodities. Under prohibition, however, it is more difficult to use the official dispute resolution mechanisms, since courts will typically not enforce contracts involving prohibited commodities, and disputants cannot easily use these mechanisms without incriminating themselves. Thus, black

market participants use violence rather than lawyers to settle commerical disputes.

Taken in its simplest form, this perspective suggests that the critical factor determining whether violence is used in lieu of lawyers is whether a particular commodity is prohibited. Further consideration, however, suggests that the level of enforcement of the prohibition is also critical, and that the level of violence will generally be increasing in the degree of enforcement.

To understand this point, one must begin by thinking about the nature of prohibitions. On the one hand, prohibitions consist of laws that forbid or restrict the production, sale, transportation, purchase, or possession of the good in question. On the other hand, prohibitions consist of the activities governments conduct to enforce these laws, including arrests, fines, asset seizures and the like. Further, an important feature of prohibitions is that they are rarely absolute; instead, they contain provisions for the legal production and consumption of the good under certain circumstances. In the case of Alcohol Prohibition in the United States, for example, it was legal to produce small quantities of alcohol for one's personal use, to produce certain kinds of low alcohol wine and beer, to put alcohol in medicines and sacramental wines, and to use alcohol in industrial products. In the case of drug prohibition in the United States and elsewhere, some or all prohibited drugs can be prescribed by doctors, and in several cases governments operate treatment programs that provide prohibited drugs (or close substitutes) to consumers that meet certain conditions.

With this perspective, one can imagine several reasons why increased enforcement of a prohibition tends to increase the use of violence to settle disputes. One aspect of increased enforcement is new laws or regulations that decrease the scope for legal production or consumption of the good. Such changes place some additional transactions outside the realm of official dispute resolution mechanisms and thus encourage the use of violence. The most obvious example of such a change is the initial creation of a prohibition, but even once a prohibition has commenced societies often expand the prohibition in ways that further limit the scope for legal trade in the prohibited commodity or close substitutes. The United States did not treat the maintenance of opiate users by physicians as proscribed until several years after prohibition took effect (Musto 1973), and it did not criminalize marijuana (in 1937) until many years after it criminalized cocaine and opiates (in 1914). Similarly, England allowed doctors relatively free reign in the dispensation of heroin for the first several decades of its drug prohibition, but since the 1960s it has imposed greater limits on heroin maintenance.

4

Even holding the legal regime constant, increases in activities to enforce prohibition tend to increase the use of violence. To begin, a prohibition law with literally zero enforcement might not cause violence at all, since in this case no one would be aware of the prohibition, and courts sometimes treat as obsolete a law that is rarely or never enforced. More generally, many enforcement activites shift supply from the legal to the illegal sector for any given set of laws, thereby increasing the likelihood of violence. For example, increased monitoring or surveillance of doctors or clinics that legally dispense prohibited drugs might cause these sources to limit the quantities supplied, thereby implying more demand for the illegal sector. Alternatively, suppliers of a prohibited commodity might be able to disguise their supply of that commodity as something legal when monitoring is lax and thereby still use the official dispute resolution mechanisms (e.g., industrial alcohol in the case of Alcohol Prohibition). Likewise, in the absence of monitoring, medical supplies of prohibited commodities can be easily diverted, but in this case their production, importation, and distribution can be carried out legally.

A third reason that enforcement activites might encourage the use of violence to resolve disputes is that certain kinds of enforcement tend to disrupt arrangements that have been made within a black market to avoid disputes or the use of violence to resolve them. For example, when enforcement is used to arrest suppliers, increased violence might result from attempts by remaining suppliers to capture the market share freed up by this increased enforcement. Alternatively, black market suppliers might create private mechanisms for resolving disputes without the use of violence, but enforcement that creates turnover in the set of suppliers (and destroys reputational capital) makes such arrangements more difficult to maintain. Still another mechanism is that given higher dispute resolution costs, participants in a black market will use production and distribution methods that minimize transactions (e.g., home production), but heightened levels of enforcement make this more difficult.

A different way in which enforcement encourages the use of violence rather than lawyers arises from the fact that in a prohibited market, disputes between the government and market participants are more likely to be resolved with violence. The government itself often uses violence in the enforcement of prohibition (e.g., breaking down doors in the middle of the night, raiding production and distribution facilities, forcibly stopping boats on the high seas or planes in the air, destroying crops with aerial spraying), and black market participants cannot sue the government or use the political process for redress of grievances and are therefore more likely to

5

use violence (e.g., attacks on police, judges, prosecutors, and politicians). And the greater scope for bribes creates an additional type of contract that cannot be adjudicated with lawyers.

Still a final mechanism by which enforcement encourages violence is that enforcement of prohibition is likely to cause a redistibrution of criminal justice resources from general crime deterrence toward drug prohibition, implying that enforcement of laws against violence will tend to diminish and thereby lower the costs of using violence not just relatively but absolutely. This effect is not necessarly important at low levels of enforcement, since increased drug enforcement and increased general crime deterrence might go hand in hand. But as the level of prohibition enforcement increases, society's willingness to fund criminal justice as opposed to other objectives is likely to diminish, implying increased drug enforcement will occur at the expense of other kinds of enforcement.[1]

For all these reasons, it is natural to assume that the costs of using lawyers increases relative to the costs of using violence as prohibition enforcement increases. This does not necessarily mean enforcement raises violence on net, but it suggests the possibility of that outcome.

2.2   A Model

To make this slightly more formal, assume that the good in question is produced using two inputs, lawsuits and homicides, each of which has some price that suppliers take as exogenous. The price of lawsuits, however, is increasing in e, the enforcement of prohibition. Given enforcement, suppliers choose the number of lawsuits and homicides to minimize costs for any given level of output. I assume constant returns to scale throughout, implying industry input demand functions of the form

$$L^d = L(w_l(e)/w_h)Q \tag{1}$$

and

$$H^d = H(w_l(e)/w_h)Q \tag{2}$$

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[1]Benson, Rasmussen, and Kim (1998) examine the relation between the Index I crime rate and the rate of drug arrests using panel data for a cross-section of Florida counties during the mid-1980s. They find a marginally significant positive association between drug arrests and the Index I crime rate, controlling for a variety of demographic factors and the levels of police resources. They do not address the breakdown of their results for different kinds of Index I crimes, so the results shed no direct light on the effect of drug prohibition on homicide. Benson et al. (1992) and Sollars et al. (1994) provide evidence that increased drug enforcement diverts police resources from deterring property crime.

where w_l and w_h are the prices of lawsuits and homicides, L and H are the numbers of lawsuits and homicides, e is enforcement, and Q is industry output. The discussion above implies that increased enforcment leads to increased costs of hiring lawyers,

$$w_l'(e) > 0, \tag{3}$$

so increased enforcement leads to a substitution from lawyers to homicides. The equilibrium price of the good under these conditions is

$$P(e) = w_l(e)L(w_l(e)/w_h) + w_h H(w_l(e)/w_h), \tag{4}$$

and cost minimization means that $P(e)$ is increasing in e. Assuming a standard demand curve,

$$Q = D(P), \tag{5}$$

it follows that increased enforcement raises the costs of lawyers, which raises the cost and price of the good, thereby reducing equilibrium quantity. The net effect on the number of homicides depends on which of two effects dominates: the reduction in output due to higher goods prices, or the substitution of homicides for lawyers due to higher lawsuit prices. The less elastic the demand for the good, the more likely that the substitution effect dominates.

2.3 Estimation and Identification

Estimation of the effect of enforcement on homicide is complicated by the fact that for prohibited goods, data on both price and quantity are generally unavailable. Under certain assumptions, however, it is still possible to determine the reduced form effect of enforcement on homicide. To begin, rewrite (2) and (4) as

$$H = H(e)Q \tag{6}$$

and

$$P = P(e), \tag{7}$$

and substitute these equations into (5) to get

$$H/H(e) = D(P(e)). \tag{8}$$

This equation contains only the observable variables enforcement and homicide; given specific functional forms one can estimate this relation and determine empirically whether the net effect of enforcement on homicides is positive or negative.

7

The empirical work below, therefore, examines regressions of the form

$$h_t = \beta e_t + \gamma x_t + \epsilon_t \tag{9}$$

where h_t is the homicide rate, e_t is a measure of prohibition enforcement, x_t is a vector of other variables that might affect the homicide rate (including lags of the dependent and independent variables), and ϵ_t is the error term. Assuming the level of enforcement, e_t, is exogenous and the variables in x_t are known and observable, OLS estimation of this equation provides consistent estimates of the reduced form impact of enforcement on the homicide rate. If enforcement substantially reduces drug or alcohol consumption, and if such consumption directly induces violent behavior, the estimated impact will tend to be negative; if enforcement substantially encourages the substitution of violence for lawsuits in the resolution of commercial disputes, the estimated impact will tend to be positive. The two possibilities are not mutually exclusive, and the estimation cannot disentangle the magnitude of the two effects. Under the stated assumptions, however, it can indicate which effect is empirically more important.

The difficulty in interpreting estimates of equation (9) is that neither of the critical assumptions is likely to be satisfied precisely, so the estimated effect of enforcement on homicide is potentially biased. I discuss next the nature of this bias and the conditions under which it is likely to be small or in a direction that allows clean interpretation of the results.

The problem with treating enforcement as exogenous is that a society experiencing increased violence might choose to increase its enforcement of drug or alcohol prohibition, implying enforcement depends on the level of violence. In this case violence and enforcement will tend to be positively correlated even if enforcement's true effect on violence is zero or negative, and a positive estimated impact of enforcement on violence would not necessarily indicate that enforcement *per se* plays a direct role in producing violence. This possibility can not be dismissed on *a priori* grounds, but a number of considerations suggest it is not the whole story and perhaps not even a substantial source of bias.

To begin, the empirical work presented below employs enforcement *expenditures* as the measure of enforcement, rather than standard alternatives such as arrests for violation of prohibition laws (see below for a justification of this approach). Most changes in expenditure, and particularly changes carried out at the federal level, require actions by a legislature, and such actions take effect only after delays of months or even years. In addition,

8

decisions about the level of expenditure are based on *lagged* information about the level of violence, since it takes time for the collection and publication of relevant data. Thus, the level of enforcement chosen for any year might well depend on the level of violence in earlier periods, but it cannot easily reflect the contemporaneous levels of such variables.[2] Moreover, the federal budgetary process in the United States provides a measure of enforcement that by construction is predetermined relative to the contemporaneous homicide rate; in each year, the government constructs budget *projections* for the following year, and these projections are (except in rare cases) completed and published *before* that year begins.

Thus, the two measures of enforcement considered here (actual expenditure, and the projection of expenditure for a given year made and recorded the previous year) can reasonably be taken as predetermined relative to any given period's homicide rate (i.e., these measures are functions only of time $t-1$ information). Assuming the error term in the estimation equation is uncorrelated with all time $t-1$ information, these measures of enforcement are uncorrelated with the error term and can therefore be used to obtain consistent estimates of the equation. For example, if all the relevant variables in x_t were known and accurately measured, the only remaining error term would be a rational expectations forecast error arising out of a forward looking model of drug or alcohol consumption; such a forecast error is by nature orthogonal to variables dated $t-1$ and earlier.

This brings the discussion to the question of x_t. In practice, some components of x_t are difficult to measure and are potentially correlated with the level of expenditure or correlated over time. This means OLS estimates of equation (8) might be biased even if the measure of enforcement is predetermined relative to all time t variables.

What is less clear is the direction of this bias. Consider, for example, a society in which tolerance for drug use increases. Such a society might choose to adjust its level of expenditure on drug prohibition, but arguably the adjustment would be negative (or would take the form of increased expenditure for treatment). Thus, assuming the increased tolerance for drug use implies increased consumption of drugs, which in turn expands the size of the black market and the associated use of violence to resolve commercial disputes, the estimation would be biased toward finding a negative corre-

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[2]According to Riedel (1990), the data on UCR homicide data are available approximately six months after the end of the previous year, while NCHS homicide data are available approximately three years after completion of the year to which the data correspond (p.191).

9

lation between violence and expenditure even if the true causal effect were positive.

Even if the estimates are upward biased, this bias is not necessarily large because a number of factors have provided substantial exogenous variation in both alcohol and drug prohibition enforcement over the past century. A key factor leading to the onset of Alcohol Prohibition was U.S. involvement in World War I, which legitimized the view that turning grain into alcohol was wasteful (Merz 1932), created an air of moral certainty that facilitated passage of prohibition (Sinclair 1962), and produced a distaste for anything German (i.e., beer). Similarly, one factor usually credited with precipitating Prohibition's demise is the Great Depression (Levine and Reinarman 1991), which invalidated dry claims that Prohibition promoted prosperity and produced a need for tax revenue. Thus, at least some of the major variation in spending for prohibition of alcohol reflected factors orthogonal to the homicide rate.

Regarding drug prohibition, a number of similar exogenous effects are present. Ronald Reagan's escalation of the war on drugs seems to have reflected his strong, ideological conservatism more than any underyling changes in the economy or society, and his victory over Jimmy Carter resulted from the state of the economy and the Iran hostage crisis, not concern over violence or drug use. Alternatively, incidents like the death of Len Bias tend to crystalize public attention and lead to major shifts in funding, entirely out of proportion to any underlying facts. Bill Clinton's de-escalation of the war on drugs is again a fixed factor of his personality more than a reflection of conditions in society. Richard Nixon's expansion of the drug enforcement budget probably did reflect society's concern over drug use and violence among the baby-boomers; but since the analysis below controls for the age distribution of the population, these effects are largely captured in the regressions.

None of this means the estimated effect of enforcement on homicide is immune from upward bias; for example, the emergence of a new, violence-inducing drug could simultaneously lead to more violence and cause law-abiding voters to support increased drug enforcement, thus producing a positive relation between homicide and enforcement even when drug use causes violence. But this possibility is only one of many (the increased concern could also cause increased expenditure on treatment), implying the overall bias is not necessarily large or even positive. Similarly, the existence of exogenous influences on expenditure does not eliminate the possibility of positive bias, but the magnitude of any bias is decreasing in the fraction of

10

variation explained by exogenous factors. Thus, again, any bias that exists is not necessarily large.

3 Empirical Results

I begin the empirical work by considering plots of the key variables, since these suggest the main conclusions of the paper. I then examine regressions of violence rates on enforcement and other variables.

3.1 Overview of the Data

The sole measure of violence employed in this paper is the homicide rate, for several reasons. First, homicide is one serious kind of violence, from many perspectives the most serious kind. Second, in the specific context considered here, homicide may be the preferred form of violence, since using homicide rather than mere assault helps eliminate a key witness against the perpetrator of the violent act. Third, data on homicide rates are available for a longer time span than are other measures of violence, which allows examination of Alcohol Prohibition. And finally, the homicide rate considered here, which is from vital statistics sources, is not subject to the same degree of measurement error as are other measures of violence, most of which derive from criminal justice sources.[3]

Figure 1 presents data on the homicide rate in the United States for the period 1900-1995, the longest period for which consistent and reliable data are available.[4] The numbers at the beginning of the sample are sufficiently unusual as to raise questions of data comparability; the change in the homicide rate from 1.3 in 1904 to 2.1 in 1905 to 3.9 in 1906 is large in comparison to virtually all subsequent fluctuations. One plausible explanation for these unusual observations is that the set of states reporting data on homicide rates accounted for only 26 percent of the population in 1900,

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[3]O'Brien (1985) discusses many of the pitfalls that arise with Uniform Crime Report, National Crime Survey, and Self-Report Survey data on assault or rape; see also the collection of papers in MacKenzie, Baunach, and Roberg (1990). Riedel (1990) compares the UCR and Vital Statistics measures of homicide; he concludes that both data sources provide reasonable measures of the homicide rate but suggests that the vital statistics data may be slightly more accurate overall. This conclusion confirms a similar one reached by Cantor and Cohen (1980) a decade earlier.

[4]The data are from the *Vital Statistics of the United States*, as reported in Bureau of the Census (1975) and *Statistical Abstracts of the United States*, various issues.

but 50 percent in 1910, 80 percent in 1920, and 100 percent in 1933 (Bureau of the Census (1975), p.408); moreover, the states in the initial sample had relatively low homicide rates (Eckberg 1995). Thus, especially for the first decade of the century, data from additional states might have raised the overall homicide rate simply because of differences across states. By 1910, this problem is necessarily diminishing in importance – since further additions make a smaller marginal difference to the aggregate – so there is a case for excluding, say, the first ten years of data from the analysis. I choose instead to include all the data, but I show that the key empirical results are generally strengthened by omission of these potentially problematic observations. Also, the use of Eckberg's (1995) "corrected" homicide rate series provides results that are similar – if anything, stronger – than those presented below.

Beginning around 1910, the homicide rate rises steadily through 1933, when it begins a general decline until approximately 1960, interrupted by a spike during World War II. Begining in the early 1960's the homicide rate rises steadily until the mid-1970's – to a level slightly above the previous peak in 1933 – and then fluctuates around a relatively high value for the remainder of the sample.[5]

Consideration of these data is instructive on several counts. Roughly speaking there have been two periods with high homicide rates in U.S. history, the 1920-1933 period and the 1970-1990 period. Both before the first episode and between these two episodes, homicide rates were relatively low or clearly declining. This pattern appears inconsistent with a number of hypotheses about the causes of violence. For example, restrictions on handguns and other weapons have been more extensive in the 1965-1995 period than during the 1945-1965 period, yet homicide rates have consistently been higher during the period with greater controls. Urbanization has proceeded, if not at a steady rate, at least always in an upward direction over this sample, yet violence rates have gone up and come down dramatically. The incarceration rate, a crude measure of the overall degree of criminal justice deterrence, fluctuated within a narrow range from about 1930 to 1970, during which time the homicide rate both declined substantially and rose substantially; the incarceration rate then increased by a factor of more than four between 1970 and 1995, during which time the homicide rate exhibited

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[5]The recent, widely noted declines in violent crime rates are not fully apparent in these data because publication of the Vital Statistics lags several years behind publication of the Uniform Crime Reports, the source for crime statistics usually cited in the media.

several significant ups and downs. And the use of the death penalty fell substantially from the mid-1930s to the mid-1960s, precisely the period over which the homicide rate declined markedly as well. None of these simple correlations demonstrates that the variables in question do not play a role in explaining fluctuations in the homicide rate, but they fail to make a *prima facie* case in that direction.

By contrast, the hypothesis that drug and alcohol prohibition cause violence appears consistent with these data (Friedman 1991). The homicide rate was high in the 1920-1933 period, when constitutional prohibition of alcohol was in effect, as it was in the 1970-1990 period, when drug prohibition was enforced to a relatively stringent degree. After repeal of alcohol prohibition, the homicide rate dropped quickly and remained low during a period when drug prohibition, although in existence, was not vigorously enforced. And the homicide rate was lowest at the beginning of the sample, when neither alcohol nor drug prohibition existed at the federal level and only in a minor way at the state level.

To see this more carefully, consider Figure 2, which plots the two measures of prohibition enforcement considered in this paper. Both series consist of annual estimates of the real (1992 dollars), per capita expenditures by the federal government for enforcment of alcohol and drug prohibition by the one or two agencies devoted solely to that mission.[6] The difference between the two series is that the first (the solid line) consists of the actual expenditure for each year, as reported approximately a half-year after the end of that

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[6]The details of the construction of these two series are as follows. Actual expenditure series: for 1900-1919, the series is equal to zero; for 1920-1929, the series is equal to the obligations incurred under the National Prohibition Act; for 1930-1934, the series is equal to obligations incurred by the Bureau of Prohibition of the Department of Justice, the Bureau of Industrial Alcohol of the Department of Treasury, and the Bureau of Naracotics of the Department of Treasury; for 1935-1995, the series is equal to obligations incurred by the Bureau of Narcotics and its successor organizations: the Bureau of Narcotics and Dangerous Drugs (1968-1972) and the Drug Enforcement Administration (1973-1995). Projected expenditure series: for 1900-1919, the series is equal to zero; for 1920-1931, the series is equal to the obligations projected (as of a year earlier) to be incurred under the National Prohibition Act; for 1932-1935, the series is equal to obligations projected to be incurred by the Bureau of Prohibition of the Department of Justice, by the Bureau of Industrial Alcohol of the Department of Treasury, and by the Bureau of Naracotics of the Department of Treasury; for 1936, the series is equal to the obligations projected to be incurred by the Bureau of Prohibition and by the Bureau of Narcotics; for 1937-1995, the series is equal to obligations projected to be incurred by the Bureau of Narcotics and its successor organizations: the Bureau of Narcotics and Dangerous Drugs (1968-1972) and the Drug Enforcement Administration (1973-1995). All data are from the Budgets of the United States, annual issues.

year, while the second (the dotted line) consists of projected expenditure for each year, as estimated approximately a half-year before the beginning of that year.

The use of expenditure as the measure of enforcement does involve several simplifying assumptions. The discussion above suggests considering a complicated vector of variables to measure enforcement, with these variables reflecting both the detailed characteristics of the law, the kind and amount of actions taken to enforce the law, and interaction effects between these factors. No doubt this approach is the most defensible one from a theoretical perspective, and with a sufficiently detailed data set it would be desirable to implement it.

Given available data, however, it is far easier to construct a consistent series on expenditure than on the precise nature of prohibition laws or on the full range of enforcement activites, and this parsimony helps conserve degrees of freedom. Further, as discussed above, the mere existence of laws prohibiting a commodity is unlikely to have a substantial effect on the use of violence to resolve disputes, and for any given set of laws, increased enforcement (which requires additional expenditure) will tend to increase violence. Still further, changes in expenditure might often coincide with changes in the law that limit the avenues for legal circumvention of the prohibition, or with changes in enforcement activites given expenditure, so expenditure will tend to proxy for changes in the law and for changes in other aspects of enforcement that suggest effects in the same direction. Indeed, the other available measures of enforcement (arrests for violation of drug laws, removals of illegal drugs from the market, seizures of illegal drug laboratories) are strongly, positively correlated with the two expenditure series examined here for the sample periods in which both exist. Finally, so long as expenditure is predetermined or exogenous, it is interesting to see the effects of this variable even if it is an imperfect measure of the overall prohibition regime.

Even granting the use of expenditure to measure enforcement, however, there are potential problems with using the particular expenditure series considered here. They do not account for expenditure by agencies other than the key alcohol or drug enforcement units; they do not account for enforcement expenditure by states; and they do not account for the potentially offsetting effects of government provision of drugs and/or the effects of government treatment and prevention policies.

Despite these possible problems, however, the measures of expenditure considered here are likely to be the best available measures of expenditure for prohibition enforcement. The allocation of other agencies' budgets to

enforcement is, with minor exception, not readily available and in many cases highly suspect; focusing on the central agency in charge of prohibition has the virtue of greater consistency.[7] For the limited sample period for which one can attempt to account for all drug prohibition expenditures (1986-1995), the correlation between this series and those employed here is extremely high. Similarly, for the period for which a measure of state-level enforcement efforts is available (total drug arrests, 1932-1995), this measure correlates well with the federal expenditure series considered here. The government provision of prohibited drugs (e.g., heroin in Switzerland) or close substitutes (e.g., methadone in the United States) probably does diminish the size of the black market and thus the incidence of violence, but the total magnitude of such efforts has been modest in the United States, and treatment for drug addiction appears to have small or relatively short-term effects in many cases (Apsler and Harding, 1991). And as argued above, any problems with the measures of expenditure used here creates measurement error, which seems likely to bias the results against finding a strong effect of prohibition enforcement on the homicide rate.

The data presented in Figure 2 suggest a strong, positive relation between expenditure on enforcement of prohibition and the homicide rate. Expenditure climbs along with the homicide rate during alcohol prohibition and then falls at the end of this prohibition, as does the homicide rate. The level of expenditure is relatively low during the 1940s and 1950s, as is the homicide rate for the most part. And expenditure on prohibition rises along with the homicide rate beginning after 1960. A regression of the homicide rate on the actual or projected measure of enforcement produces a strongly significant and positive coefficient in both cases, and R^2's in excess of 0.50 (with or without a trend).

Despite this *prima facie* consistency, however, there are challenges for the prohibition-causes-violence hypothesis. The homicide rate began increasing as early as 1906, and increases to some degree throughout the next decade, even though constitutional prohibition did not take effect until 1920.[8] Sim-

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[7]Consider, for example, the activities of customs and immigration officers. Under prohibition, one duty of such personnel might be to check for smuggling of alcohol or drugs. From one perspective, this could imply that a large fraction of each such person's time was devoted to enforcement of prohibition, since every shipment or passenger is potentially involved in illegal activity. From a different perspective, however, only a smaller fraction is involved, since only some cargo or persons appear at all suspicious, and from a still different perspective only a few cases actually involve substantial expenditure of resources for detecting and/or prosecuting cases of drug smuggling.

[8]This statement refers to the official, *Vital Statistics* series on homicides presented in

ilarly, the homicide rate increases in the mid-1960s, several years before Lyndon Johnson and especially Richard Nixon escalated the war on drugs. And despite enormous additional escalation of the war on drugs under the Reagan and Bush administrations, the average value of the homicide rate in the 1980s is not much different than in the 1970s. Thus, factors other than prohibition have almost certainly played a significant role in producing fluctuations in the homicide rate.

3.2 The Age Composition of the Population

One potentially important determinant of the homicide rate is fluctuations in the age distribution of the population, since abundant evidence suggests that age plays a large role in determining the propensity to commit violent acts (e.g., Cohen and Land (1987), Steffensmeier, Allan, Harer, and Streifel (1989), Sourcebook of Criminal Justice Statisics (1995, Table 4.7, pp.404-405)). Most obviously, violent acts are committed at a disproportionate rate by individuals in their late teens and early twenties. The fact that propensities for violence appear to decline starting in the early to mid 20s might overstate the underlying "causal" relation, since some of those who commit such acts are subsequently incarcerated. But incarceration effects are not the whole story, since they cannot explain the increase in violent propensities observed between early and later teenage years.

To assess the importance of age composition effects for an overall understanding of the homicide rate, Table 1 reports regressions of the homicide rate on the fraction of the population in the age categories 5-14, 15-24, 25-34, 35-44, 45-54, 55-64 and 65 and over.[9] The regression in column (1) has an R^2 of more than 70 per cent, which taken at face value means almost three-quarters of the fluctuations in the homicide rate over the past century are "explained" simply by fluctuations in the age composition of the population. Columns (2) and (3) make this point even more dramatically, by limiting the sample to the periods beginning in 1910 or 1920. The year 1910 is the first in which consistent reporting of death certificates exists in states

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Figure 1. There is less of an anomaly in the alternate series offered by Eckberg (1995), which does not display such large increases during the first two decades of the century.

[9]This particular breakdown is not necessarily ideal; one can construct less detailed breakdowns for the entire sample and more detailed breakdowns for part of the sample. But this breakdown is available on a consistent basis for the entire sample, and it seems to represent a reasonable tradeoff between more detailed breakdowns, which would quickly exhaust degrees of freedom, and less detailed breakdowns, which might fail to capture significant differences across age groups.

accounting for at least 50 percent of the population, and the year 1920 is the first in which annual breakdowns of the population by age are computed using data on annual births, deaths, and immigration, rather than just interpolations between census years (Bureau of the Census, 1965). In these two cases the R^2s exceed 0.80 and 0.86, respectively.

The pattern of coefficients in these regressions is plausible although not without difficulties. The coefficient is negative for the youngest age group but becomes positive and tends to rise with age through the 25-34 group. The coefficients then tend to fall, except for an increase for the oldest age category. Since there are multiple effects at work in producing any one coefficient, it is not obvious one should get a perfectly smooth result: for example, the age-specific propensity to commit a violent act is not necessarily the same as the age-specific propensity to be the victim of a violent act, yet the coefficients reported incorporate both effects. Alternatively, the age groups that tend to commit violent acts might become incarcerated at a disproportionate rate, leaving mainly the non-violent members of that group in the population at risk of committing such acts in the future. Plus, these propensities might have changed over time (e.g., Steffensmeier et al. (1989), Blumstein and Cork (1996), Levitt (1997)), which would tend to contaminate the coefficients on any one age group, as seems plausible in a few cases. Overall, however, the pattern of coefficients makes rough sense.

Although these results are suggestive and tend to confirm a substantial role for demographics in explaining fluctuations in the homicide rate, the magnitude of the R^2 needs to be interpreted with caution. As noted above, the homicide rate is a highly persistent series, as are the age variables used as regressors. Thus, the true "degrees of freedom" of this regression is far lower than it might appear; there are in fact relatively few independent observations.

This point can be seen easily in Figure 3, which graphs the homicide rate and the fraction of the population in the age range 15-24. There is a high correlation between the two series, consistent with widespread cross-sectional evidence that homicides are disproportionately committed by and against people in this age range. It is also clear, however, that the age variables are not explaining the high frequency variation in the homicide rate but instead the long-term swings. These long term movements coincide well with movements in the age distribution, but there are only 2-3 such movements in the sample. Levitt (1999) makes a related point, noting that while changes in the age structure clearly play a role in aggregate crime changes (including homicide) over long periods, the impact in any given

17

year is limited. Similarly, Blumstein and Rosenfeld (1998) document major changes in age-specific homicide rates during the period since 1985. Thus, it is almost certainly important to control for age composition effects in analyzing the time-series behavior of the homicide rate, but there is also much remaining variation to be explained.

3.3 Regression Analysis

I now turn to examining more formally the relation between prohibition enforcement and the homicide rate. Table 2 presents OLS regressions of the homicide rate on the two alternative measures of enforcement. All regressions include a constant and a time trend in addition to the variables listed in the table, with Newey and West (1987) t-statistics in parentheses. The top half of the table gives results that exclude the seven age composition variables discussed above, while the bottom half gives results that include these variables. The last line of each section gives the "impact" of the enforcement variables, evaluated as the coefficient estimate multiplied by the maximum value of enforcement in the relevant sample. Thus, the estimated impact shows the effect of prohibition on the homicide rate.

The results in the table show that enforcement of alcohol and drug prohibition have been positively correlated with the homicide rate. This result obtains for both measures of prohibition enforcement, and in most cases, for subsamples of the overall time period. The results are not especially sensitive to inclusion of the age distribution variables, but the range of estimated impacts narrows and becomes uniformly positive when these variables are included. Additional regressions not displayed here show that these results, including the R^2's in the top half of the table, are only slightly affected by exclusion of the trend term. Thus, the enforcement variables by themselves can "account" for more than half the variation in the homicide rate, and enforcement plus the age distribution can account for more than 80 percent of the variation in the homicide rate.

The estimated impacts of the enforcement variables -- measured at the maximum values in the sample – are not only positive but often large. In the more extreme cases, the estimated impacts are close to half of the maximum value attained by the homicide rate in the sample, and in many other cases the estimated impact is more than 25 percent of this value. This approach to evaluating the impact of prohibition enforcement assumes that enforcement is fully exogenous, not just predetermined, an assumption that is not necessarily correct. This summary statistic nevertheless provides a useful

18

way of comparing estimates across different specifications.

Interestingly, these estimates suggest a fraction of all homicides due to disputes arising in the illegal drug trade that is consistent with cross-sectional, individual level data. Goldstein et al. (1989), using police reports and police evaluations, examine the causes of all homicides in a sample of New York City precincts during part of the year 1988. They determine that more than half of the homicides were due to drug-related factors, but of these almost three quarters were due to "systemic" factors (Goldstein 1985), meaning disputes over drug territory, drug debts, and other drug-trade related issues. Thus, approximately 39 percent of the homicides resulted from the inability of drug market participants to settle disputes using the official dispute resolution system; only 7.5 percent resulted from the psychopharmacological effects of drugs or alcohol.

The regressions reported in Table 2 make a *prima facie* case that enforcement of alcohol and drug prohibition has, on net, increased rather than decreased violence. If the effect of enforcement in reducing consumption were strong, and if such consumption had a substantial effect on the propensity to commit violent acts, it would be surprising to find a strong positive relation between enforcement and homicide. These simple regressions are not conclusive, however, because they omit potentially relevant variables. The remainder of this section presents additional regressions that attempt to address this issue.

One potentially key omission from the regressions in Table 2 is lags of the dependent and independent variables. According to the model presented above, the relation between homicide and enforcement should reflect the main features of the relation between drug or alcohol consumption and price; thus, since such consumption is potentially addictive, this relation should allow for both lagged endogenous and lagged exogenous variables, as implied by models of addiction (e.g., Chaloupka (1991), Becker, Grossman, and Murphy (1994)). Further, enforcement might have both contemporaneous and lagged effects on the price of a prohibited commodity, or on the incentive to substitute guns for lawyers, further suggesting the presence of lagged exogenous variables on the right hand side.

To address this issue, Table 3 presents regressions of the homicide rate on lagged homicide rates and current and lagged enforcement (plus current and lagged values of the age distribution variables, a constant, and trend). I present results only for the projected expenditure variable, but the actual expenditure series provides similar results. The results confirm that the homicide rate is persistent, as would be predicted by any model of addictive

19

consumption, and they suggest that lagged values of enforcement are relevant for explaining the homicide rate as well. The overall estimated impact of enforcement based on these regressions, however, is still strongly positive and generally similar to that estimated off the purely contemporaneous relation. In most cases, the estimated impact increases as the number of included lags increases.

In addition to lags of homicide and enforcement, the theoretical framework employed here suggests that any variable with a substantial influence on the supply of or demand for alcohol and drugs should be included in the estimation equation, as should any variable that affects the tradeoff between guns and lawyers. For example, the unemployment rate or the level of per capita income might affect the demand for alcohol or drugs, and the unemployment rate might affect the relative cost of hiring hit-men as opposed to lawyers. Alternatively, the incarceration rate in an economy could affect the cost of lawyers relative to hit-men, assuming those with the highest proclivity toward violence are locked up first, and the execution rate might affect the expected costs of engaging in violent acts. Similarly, differences over time in the cost or availability of guns might induce substitution from one method of resolving disputes to the other. And changes in social attitudes towards drugs or alcohol could affect the frequency of violence either directly or through induced changes in the size of the drug or alcohol markets. Moreover, to the extent violence results from totally separate mechanisms (e.g., income inequality) there will be still other factors to be included in a complete examination of the homicide rate.

Table 4 addresses some of these issues by including, singly or jointly, several variables that are available on a consistent basis over long periods of time; these are the unemployment rate, the level of per capita income, the execution rate (measured as the number of executions per homicide), and the incarceration rate (measured as the number of prisoners per population at the end of the previous year).[10] The execution rate and the incarceration rate are available for only a subset of the overall sample, so I report estimates over the relevant sub-sample without these variables for comparison. All the regressions reported in this table include the seven age distribution variables in addition to a constant and trend.

The results in the tables again suggest that enforcement of alcohol and

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[10]The data on per capita income are from Bureau of the Census (1975) and Council of Economic Advisers (1998); the data on unemployment are from Romer (1986) and Citibase (provided by Romer); and the data on incarceration rates and execution rates are from U.S. Department of Justice (1997).

drug prohibition have been strongly associated with higher homicide rates, even after controlling for the other variables considered in Table 4. Results that use actual enforcement rather than projected enforcement are similar to those presented in the table, and adding these four variables (plus relevant lags) to the specification in Table 3 typically has small and often positive effects on the estimated impacts of enforcement.

The estimated coefficients on the control variables are consistent with existing results in the literature. Neither the unemployment rate nor the level of income per capita enters significantly; some earlier papers using time-series data report a significant estimated association between the homicide rate and measures of aggregate economic conditions (e.g., Cantor and Land (1985), Devine, Sheley, and Smith (1988)) but others contain weak and inconsistent results on these correlations (e.g., Cook and Zarkin (1985)). Similarly, cross-sectional evidence does not typically find stable correlations between economic variables and the homicide rate (e.g., Land, McCall, and Cohen (1990)).

The execution rate, which is a crude proxy for the effect of capital punishment, enters negatively but insignificantly. This result is not directly comparable to previous estimates of the deterrent effect of capital punishment, since those estimates are typically based on regressions that separately include the ratio of arrests to homicides, the ratio of convictions to arrests, and the ratio of executions to convictions, in addition to differing from the specification here in terms of sample period, functional form and control variables. Nonetheless, the result here is broadly consistent with the earlier literature, which finds strong negative effects in some cases (e.g., Ehrlich 1975, 1977) but smaller, less significant effects in others (e.g., Bowers and Pierce (1975), Passell and Taylor (1977)). The coefficient reported in the table implies that the increase in the ratio of executions to homicides from its level in the early 1970s (zero executions per year) to levels of the mid-1990s (30-50 executions per year), has decreased the homicide rate by at most 0.12 homicides per 100,000, roughly 1 percent of the overall maximum value.

The incarceration rate enters negatively and close to significantly, which is what should occur if the more violent members of society have a higher likelihood of being incarcerated first. The estimated coefficient implies that if the approximate tripling of the incarceration rate between 1981 and 1995 had not occured, the homicide rate in 1995 would have been higher by about 5.0 homicides per 100,000 population, or 50 percent. This strong, negative relation between the incarceration rate and the homicide rate potentially

21

explains the fact that although the level of enforcement in the 1980s was far greater than in the 1970s, the homicide rate fluctuated within a similar range. Using state level data and an instrumental variables procedure based on prison overcrowding litigation, Levitt (1996) finds that the UCR violent crime rate would have been about 70 percent higher in 1993 as compared to 1971 if the observed increase in prisoners per capita had not occurred.

Table 5 attempts to address a different issue by including two alternative measures of gun availability as a control variable (U.S. Department of Justice 1997). The data that permit such an examination are limited and potentially quite noisy; they are from the General Social Survey and the Gallup poll, both of which ask respondents whether a gun is present in their home. These surveys have been conducted only in the post-WWII period, and the General Social Survey only since 1972. Moreover, in many years neither survey posed this particular question, so the usable sample sizes are even smaller than implied by the start dates of the series. Thus, to conserve degrees of freedom, I include as other control variables only a time trend and the fraction of the population aged 15-24.

The results suggest only a small role for guns as a determinant of violence; one of the two measures enters positively and significantly, while the other enters negatively but insignificantly. Given the caveats mentioned above, neither result can be taken as definitive, but they are consistent with the previous literature, which fails to make a compelling case that gun control laws or gun availability affects violence rates and even suggests that certain restrictions on gun availability might increase violence (Ohsfeldt and Morrisey (1992), Kleck and Patterson (1993), Lott and Mustard (1997)).[11] The more important result here is that even in the case where gun ownership appears to increase the homicide rate, the net effect of prohibition enforcement is still strongly positive. Thus, this first-pass attempt to account for the role of gun availability does not affect the overall conclusion that prohibition enforcement plays a strong positive role in generating violence.

The final issue explicitly addressed here is the possibility that unobserved shifts in the demand for drugs, or in tolerance toward drug use, play a role in determining the homicide rate. Table 6 addresses this issue by in-

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[11]The Lott and Mustard paper has been criticized along a number of dimensions (see, e.g., Zimring and Hawkins (1997), Dezhbakhsh and Rubin (1998) Black and Nagin (1998)), and key empirical results appear sensitive to some aspects of the econometric specification. Even the alternative estimates presented by the paper's critics, however, suggest a negative effect of right-to-carry laws on homicide rates, albeit a small and insignificant one (Lott 1998).

cluding alternative measures of public support for legalization of marijuana. The direct violence-inducing effects of marijuana are widely agreed to be infinitessimal (Duke and Gross 1993), but shifts in attitudes toward marijuana probably correlate with the overall demand for drugs and with societal tolerance of drug use. The two measures of support for legalization are from the General Social Survey, which polls respondents of all ages, and from the Higher Education Research Institute, which polls only college freshmen (U.S. Department of Justice 1997). As with the variables considered in Table 5, these series are available only on a limited basis, so I again include only a trend and the fraction of the population aged 15-24 as controls.

The results fail to demonstrate any consistent effect of "legalization support" on the homicide rate; in one case the variable enters negatively but insignificantly, while in the other it enters positively but insignificantly. In both cases, the enforcement variable continues to enter positively and significantly, and the magnitude of the coefficient is not changed materially by addition of either legalization variable. This result does not rule out effects of social attitudes towards drugs or drug use, but it provides one example in which controlling for such attitudes does not change the key result presented here.

Beyond the variables considered explicitly above, there are numerous other factors that might be quantitatively important determinants of the homicide rate but that are not easily measured and included as regressors. Thus, it is possible the results here are biased in favor of finding a positive effect of enforcement on homicide. A final reason to think this bias is not necessarily large, however, is the consistency of the results across different time periods. The quantitative importance of any omitted factor, such as social attitudes towards drug use, likely varies substantially over a period of a hundred years, yet the relation between enforcement and homicide is relatively stable. For example, omitting the 1980s and 1990s, the period during which crack has been available, has only minor effects on the results, and separating the sample into the part characterized primarily by Alcohol Prohibition (1900-1947) versus that characterized solely by drug prohibition (1948-1995) does not change the overall result. This subsample stability does not eliminate the possibility of omitted variable bias, but it is not obvious what that omitted variable might be.

4 Conclusions

The empirical results presented above document that over the past century, the major fluctuations in the U.S. homicide rate have been positively associated with fluctuations in the enforcement of alcohol and drug prohibition. At a minimum, this evidence fails to make a *prima facie* case that such prohibitions reduce violence by reducing alcohol or drug consumption; such an effect might exist, but it is apparently not strong enough to overcome the forces that make the observed correlation strongly positive. The empirical work reported here cannot eliminate the possibility that enforcement of alcohol and drug prohibition are positively associated with the homicide rate because of unobserved shifts in attitudes toward or demands for alcohol and drugs, but *a priori* arguments and auxilliary evidence suggest this explanation is not the entire story. The hypothesis that enforcement of alcohol and drug prohibition plays an independent and substantial role in increasing violence by encouraging its use in the resolution of commerical disputes is entirely consistent with the evidence reported here.

I conclude by discussing the implications of the results for the understanding of violence more generally. To begin, the results suggest that the cross-sectional differences in violence rates may be more easily explained than previously believed. Innumerable papers examine these differences (across countries, states, cities, counties and the like), but as emphasized by Land, McCall, and Cohen (1990) or Glaeser, Sacerdote, and Scheinkman (1996), the most common outcome of these efforts is their failure to produce consistent, understandable empirical regularities. Moreover, Glaeser, Sacerdote, and Scheinkman (1996) suggest that any attempt to relate spatial differences in violence rates to fundamentals is doomed, since these differences appear large in comparison to any plausible differences in fundamentals.

The analysis in the paper, however, suggests that previous work has omitted a potentially important determinant of violence rates, the degree to which drug prohibition and similar policies are enforced (see Brumm and Cloninger (1995) for an exception). Most countries have drug prohibitions that are broadly similar to that in the United States, but enforcement of these prohibitions varies dramatically, and some countries administer highly subsidized treatment programs or provide illegal drugs directly to certain users. Likewise, all states have drug prohibitions that are broadly similar, and federal prohibition applies in all states. But states differ substantially in the degree to which they enforce their own laws, and federal prohibition is not applied evenly across states. Thus, differences across locations in the

24

enforcement of drug prohibition potentially explain some of the previously unexplained cross-sectional variation. Moreover, to the extent drug production, importation, and distribution activities are moveable from one location to another, differences in enforcement can easily change where such activity occurs without affecting the amount to a substantial degree.

Careful examination of this possibility is outside the scope of this paper, but casual observation lends credibility to the hypothesis. Homicide rates in Western Europe are only 10-20 percent of those in the United States (United Nations 1998), consistent with the fact that these countries' attempts to control drug use focus more on demand side policies (needle exchange, narcotic maintenance, treatment) than on prohibition. Columbia, by contrast, where both domestic and international efforts to prohibit drugs are considerable, has a homicide rate approximately 8-10 times that of the United States (United Nations 1998).

A related implication of the results here is that other prohibitions -- such as those against prostitution, gambling, or guns -- may play a part in explaining the use of violence. These prohibitions likely have small effects in expanding violence in many cases, since exceptions to these prohibitions are common and expenditure for enforcement is often lax. But casual observation suggests that violence is often used by pimps and johns in settling disputes with prostitutes, and that violence was common in the illegal gambling industry before government provision and the legalization of private gambling led to shrinkage of the black market. Analysis of these prohibitions is especially interesting because the commodities in question differ dramatically in their inherent ability to cause or facilitate violence.

Finally, the analysis presented here is interesting because it provides support for an economic theory of violence that has received relatively little attention in previous work on the economics of crime. The standard economic model (Becker 1968) does not provide a theory of violence *per se*, but instead a theory of income-generating crimes such as robbery, burglary, or prostitution, with violent acts viewed as arising incidentally in the commission of income-generating activities. Of course, the fact that violence might occur incidentally during the commission of other criminal acts already suggests the notion of violence as a dispute resolution mechanism; presumably perpetrators use violence mainly when their efforts to steal, say, without violence have been unsuccesful. And certain components of the previous literature, such as that on right-to-carry laws, are complementary to the perspective adopted here. But the violence-as-dispute-resolution perspective has usually been implicit rather than explicit in the existing literature,

and earlier work has not explored the numerous implications of this perspective. Given the results presented above, the violence-as-dispute-resolution hypothesis seems worthy of further examination.

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Table 1
Regressions of the Homicide Rate on Age Composition Variables

	1900-1995	1910-1995	1920-1995
Constant	-170	-119	-43
	(9.38)	(8.49)	(3.47)
Fraction 5-14	263	190	75
	(7.88)	(7.64)	(2.66)
Fraction 15-24	113	102	72
	(7.12)	(9.44)	(6.85)
Fraction 25-34	272	189	97
	(7.20)	(6.82)	(3.44)
Fraction 35-44	133	99	4
	(4.05)	(4.37)	(0.15)
Fraction 45-54	208	117	70
	(4.92)	(3.88)	(2.59)
Fraction 55-64	156	80	-104
	(3.49)	(2.46)	(2.57)
Fraction 65+	194	160	120
	(9.29)	(10.8)	(8.47)
R^2	.72	.81	.87

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in all regressions.

Table 2: Regressions of the Homicide Rate on Prohibition Enforcement

	Age Variables Excluded									
	Actual Expenditure					Projected Expenditure				
	1900-95	1900-47	1948-95	1910-95	1900-80	1900-95	1900-47	1948-95	1910-95	1900-80
Enforcement	139.2	299.8	1.3	168.9	311.6	146.1	291.4	-4.4	170.9	309.5
	(3.44)	(5.22)	(0.02)	(5.20)	(8.01)	(3.53)	(5.20)	(0.09)	(5.06)	(7.83)
R^2	.53	.64	.73	.60	.62	.55	.67	.73	.61	.63
Impact	5.0	4.6	0.0	6.1	5.2	5.2	4.7	-0.2	6.1	5.1

	Age Variables Included									
	Actual Expenditure					Projected Expenditure				
	1900-95	1900-47	1948-95	1910-95	1900-80	1900-95	1900-47	1948-95	1910-95	1900-80
Enforcement	90.1	7.8	81.0	133.9	79.9	101.9	138.0	77.5	131.1	97.8
	(3.08)	(0.11)	(1.00)	(4.45)	(2.35)	(3.78)	(2.46)	(1.48)	(4.76)	(2.73)
R^2	.94	.94	.96	.93	.94	.95	.95	.96	.94	.94
Impact	3.2	0.1	2.9	4.8	1.3	3.6	2.2	2.8	4.7	1.6

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in all regressions.
2. Fraction of the population in each of seven age categories included in all regressions in bottom half of table; see text for details.

Table 3: Regressions of the Homicide Rate on Prohibition Enforcement

	1900-95	1900-47	1948-95	1910-95	1900-80	1900-95	1900-47	1948-95	1910-95	1900-80
Homicide$_{t-1}$	0.70	0.45	0.67	0.60	0.70	0.83	0.58	0.77	0.54	0.74
	(6.02)	(2.72)	(4.92)	(4.43)	(3.88)	(4.64)	(1.50)	(4.11)	(2.91)	(3.66)
Homicide$_{t-2}$						-.35	-.49	-.37	-.22	-.32
						(1.90)	(1.45)	(2.99)	(1.68)	(1.32)
Enforcement$_t$	52.2	87.4	50.7	81.1	59.7	56.7	87.7	85.6	101.2	69.3
	(2.07)	(1.79)	(1.43)	(3.25)	(1.79)	(1.63)	(0.74)	(2.22)	(2.91)	(1.31)
Enforcement$_{t-1}$	-14.4	-46.1	42.5	-1.8	-19.0	-16.9	-12.3	60.5	-8.4	4.74
	(0.46)	(0.80)	(0.80)	(0.06)	(0.52)	(0.33)	(0.09)	(1.20)	(0.18)	(0.06)
Enforcement$_{t-2}$						41.0	76.5	107.1	62.5	67.3
						(1.14)	(0.70)	(2.01)	(2.01)	(1.39)
R^2	.97	.98	.98	.97	.97	.97	.98	.99	.97	.98
Impact	4.5	2.7	10.0	7.1	4.8	5.6	2.7	15.0	8.0	8.7

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in all regressions.
2. Fraction of the population in each of seven age categories, plus first or first and second lags, included in all regressions; see text for details.

Table 4: Regressions of the Homicide Rate on Prohibition Enforcement

	1900-95	1900-95	1900-95	1930-95	1930-95	1925-95	1925-95	1930-95
Enforcement	101.9	100.9	102.1	162.6	152.2	154.9	150.6	141.1
	(3.78)	(3.56)	(3.78)	(5.19)	(4.50)	(5.71)	(7.15)	(4.02)
Unemployment Rate		0.01						0.02
		(0.31)						(0.31)
Per Capita Income			37.2					3.87
			(0.41)					(0.02)
Execution Rate					-47.2			-51.0
					(0.96)			(0.92)
Incarceration Rate							-0.03	-0.03
							(3.36)	(3.22)
R^2	.95	.95	.95	.94	.94	.94	.95	.95
Impact	3.6	3.6	3.7	5.8	5.4	5.6	5.3	5.0

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in all regressions.
2. Fraction of the population in each of seven age categories included in all regressions; see text for details.

Table 5: Regressions of the Homicide Rate
on Prohibition Enforcement and Gun Ownership

	1973-94	1973-94	1959-93	1959-93
Enforcement	96.4	96.3	182.3	176.9
	(1.27)	(1.23)	(2.92)	(3.31)
Gun Ownshership (NORC)		-0.02 (0.28)		
Gun Ownshership (Gallup)				0.13 (2.42)
R^2	.22	.22	.92	.94
Sample Size	15	15	12	12
Impact of Prohibiton	3.44	3.44	6.51	6.32

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in the regression.
2. Fraction of the population aged 15-24 included in all regressions.
3. The NORC gun ownership variable is missing for the years 1975, 1978-1979, 1981, 1983, 1986, and 1992.
4. The Gallup gun ownership variable is missing for the years 1960-1964, 1966-1967, 1969-1971, 1973-1974, 1976-1978, 1981-1982, 1984, 1986-1988, and 1992.

Table 6: Regressions of the Homicide Rate on Prohibition Enforcement and Attitudes Toward Drug Policy

	1973-94	1973-94	1968-95	1968-95
Enforcement	118.6 (3.28)	136.3 (2.90)	124.8 (2.79)	110.4 (2.29)
Legalize Marijuana (NORC)		-0.03 (0.50)		
Legalize Marijuana (HERI)				0.01 (0.54)
R^2	.38	.39	.53	.54
Sample Size	15	15	28	28
Impact of Prohibiton	4.2	4.9	4.5	3.9

1. Newey and West (1987) t-statistics in parentheses; constant and trend term included in the regression.

2. Fraction of the population aged 15-24 included in all regressions.

3. The NORC legalization variable is missing for the years 1974, 1977, 1979, 1981-1982, 1985, and 1992.

2. The measure of support for spending to combat drug addiction is missing for the years 1979-1980, 1982, and 1992.
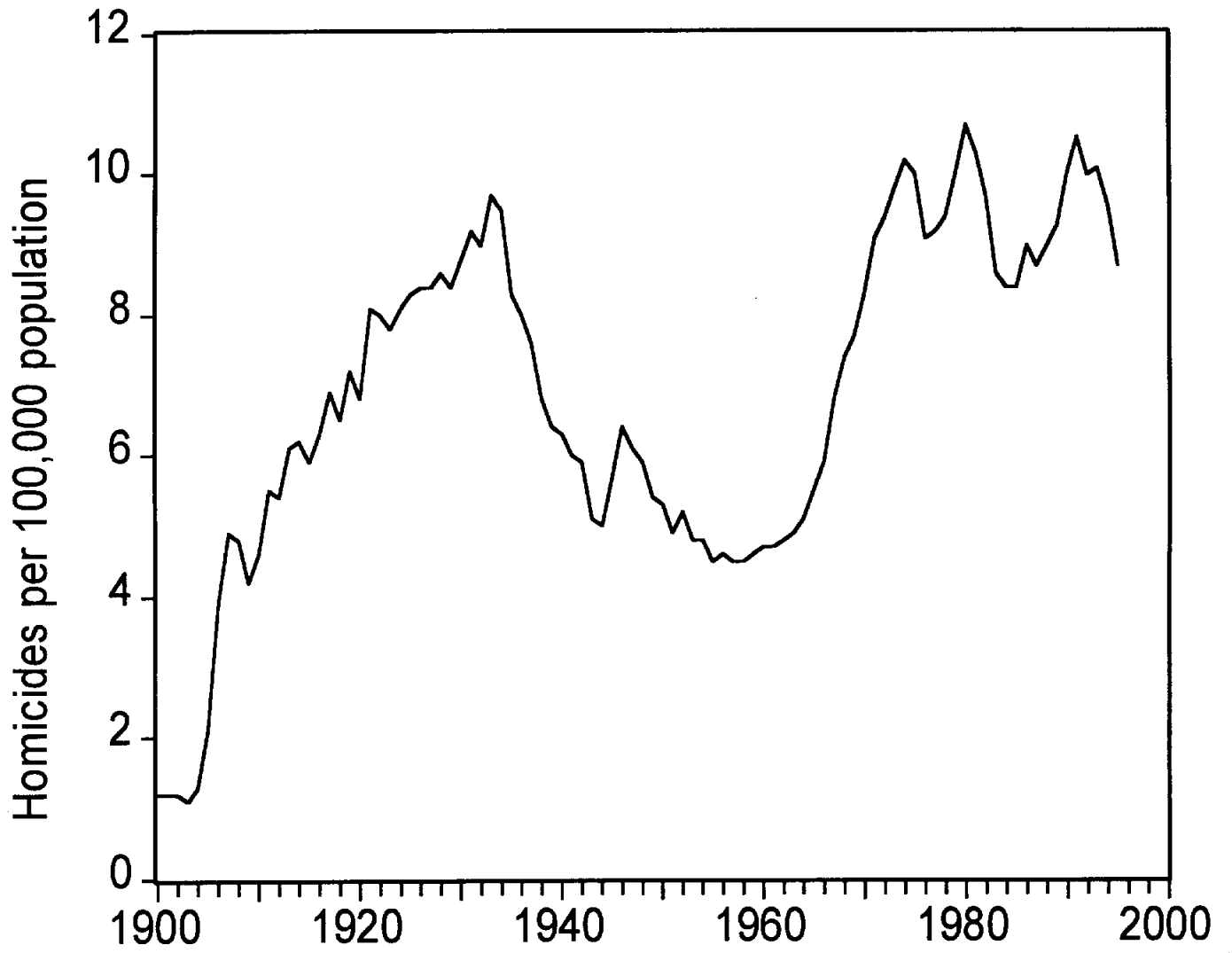
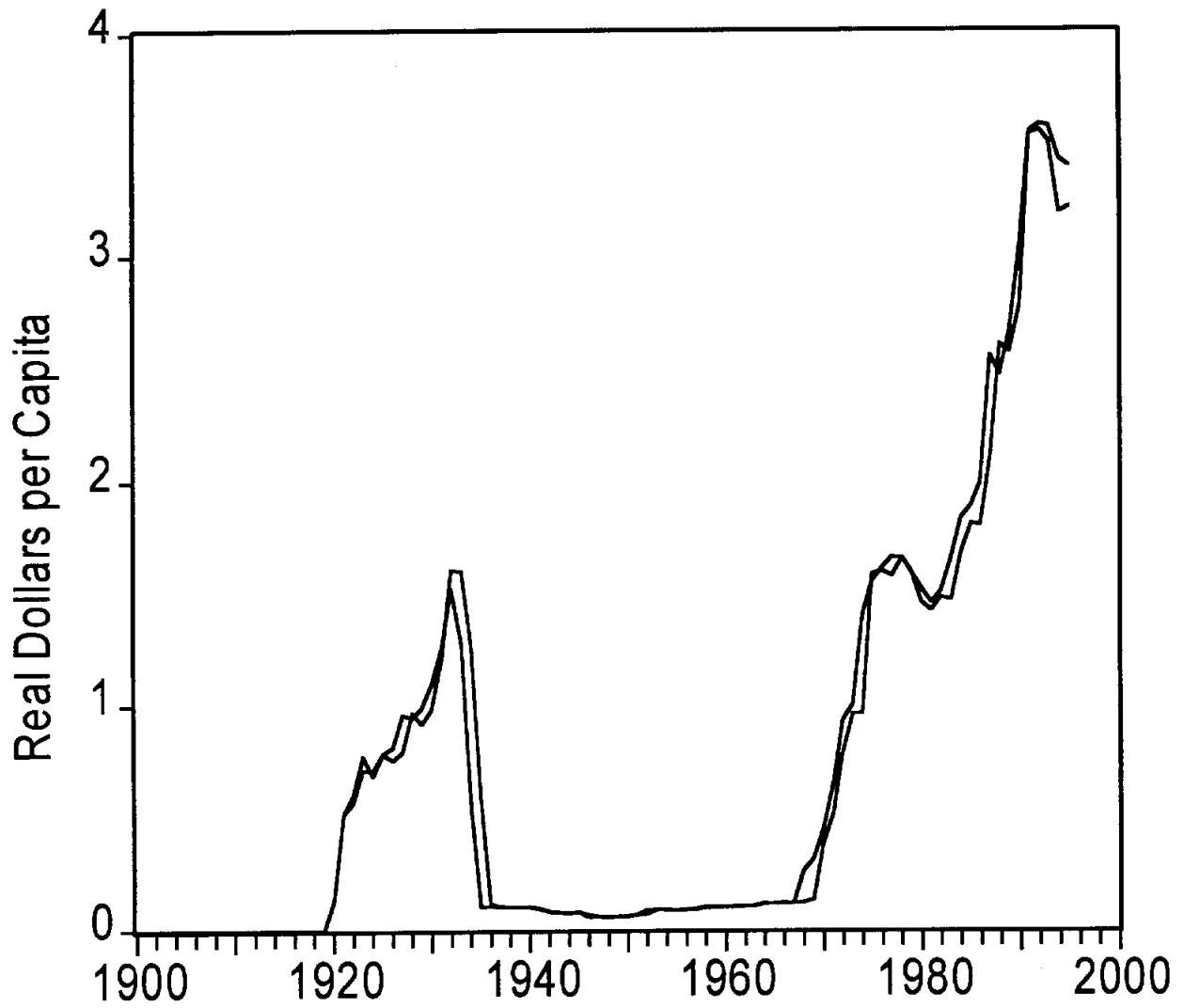Figure 1: Homicide Rate in the United States

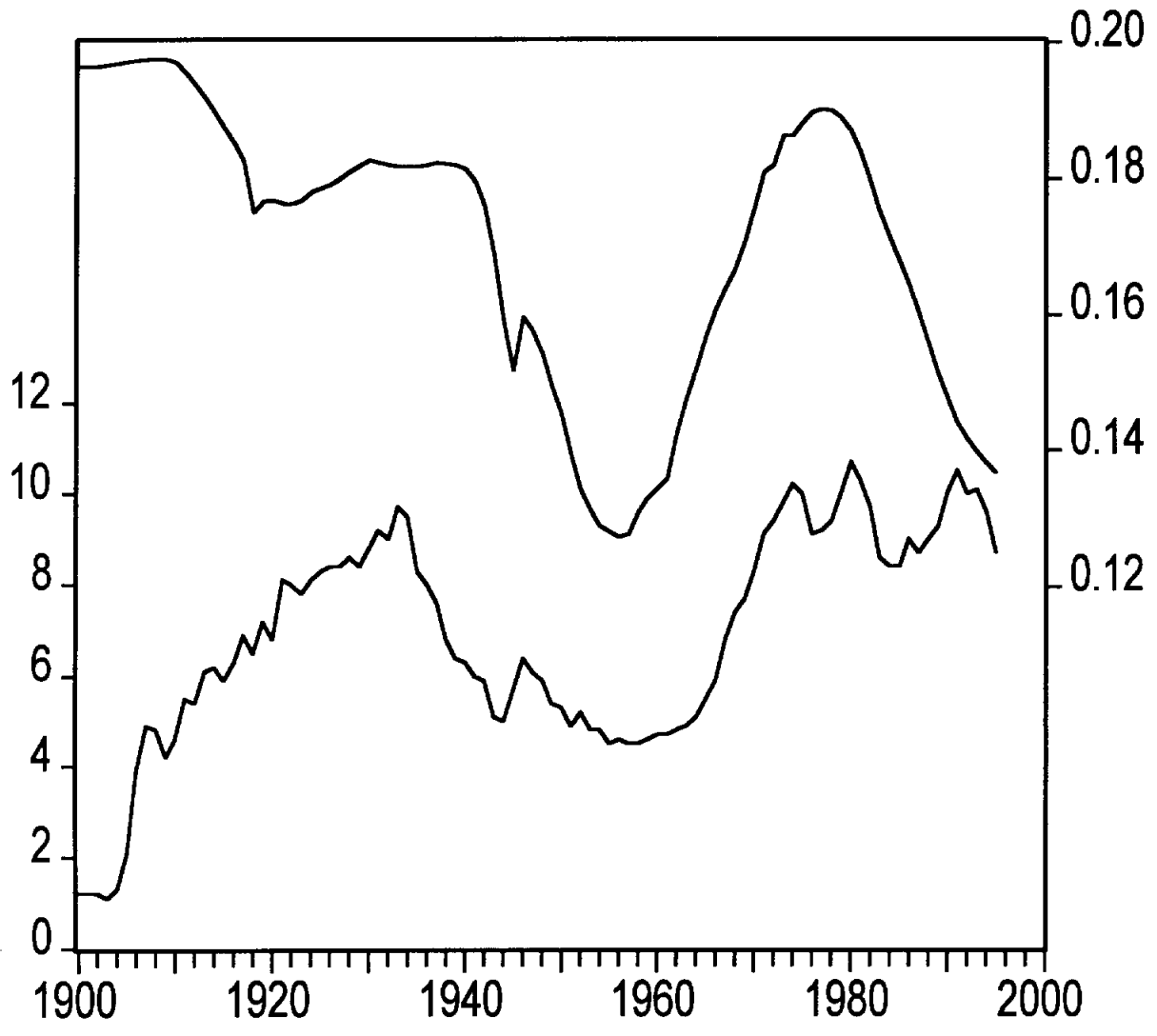Figure 2: Expenditure for Enforcement of Alcohol and Drug Prohibition

Figure 3: Homicide Rate and the Fraction of the Population Aged 15-24