Document Title: Impact of Alcohol Control Policies on the Incidence of Violent Crime, Final Report

Author(s): Bruce L. Benson ; David W. Rasmussen ; Paul R. Zimmerman

Document No.: 191199

Date Received: 11/29/2001

Award Number: 1999-IJ-CX-0041

THE IMPACT OF ALCOHOL CONTROL POLICIES ON THE INCIDENCE OF VIOLENT CRIME

by

Bruce L. Benson
DeVoe L. Moore and Distinguished Research Professor of Economics
Department of Economics
Florida State University
Tallahassee, FL 32306-2220
USA
Phone: (850) 644-7094
e-mail: bbenson@coss.fsu.edu

David W. Rasmussen
Director, DeVoe L. Moore Center
Professor of Economics
Florida State University
Tallahassee, FL 32306-2220
USA
Phone: (850) 644-7649
e-mail: drasmuss@coss.fsu.edu

and

Paul R. Zimmerman.
Economist
Competitive Pricing Division
Common Carrier Bureau
Federal Communication Commission
Washington, D.C. 20554
USA
Phone: (202) 418-7285
e-mail: pzimmerm@fee.gov

with additional research assistance from
Brent D. Mast, Ph.D.
Florida State University

September 2001

A report prepared upon completion of a research project supported by a grant from The National Institute of Justice, Grant No. 1999-IJ-CX-0041. The opinions expressed herein are those of the authors and not necessarily those of the Federal Communications Commission, its Commissioners, or its staff.

TABLE OF CONTENTS

Page

INTRODUCTION

The consumption of alcohol is a complement to a wide variety of social and recreational activities in the United States and other countries. However, probably no other legally transacted good has ever been associated as many adverse individual or social outcomes as alcohol. The definitive expression of alcohol as a "bad" instead of a "good" was the enactment of Prohibition in 1920, which made selling and producing alcohol illegal activities in the Unites States until 1933. Today, nearly seventy years after the repeal of Prohibition, policy makers, social commentators, and academics still debate the relative merits of regulating this commodity as a tool for conducting social policy.

The purpose of this study was to examine the efficacy of alcohol control policies in mitigating the incidence of Index I violent crime. This investigation attempts to address and overcome the conceptual and empirical limitations found in the existing studies of the alcohol consumption, regulation, and crime relationship.[1] Our focus on this issue does not detract from the fact that the alcohol control policies analyzed here may be important to serve other public policy objectives.

In Chapter 1 the theoretical literature on the potential relationships between alcohol consumption, alcohol regulation, and crime is discussed, and a more formal model than those that currently exist is proposed. The implication drawn from the chapter is that the alcohol-crime relationship cannot be unambiguously predicted based on theory alone. Therefore, the question must be addressed empirically.

[1] A Ph.D. Dissertation (Zimmerman 2000) was also written in conjunction with this project using a subset of one of the data samples. Parts of this report draw from that dissertation, as noted in various places below, including parts of this Introduction.

Chapter 2 contains a critical review of existing empirical studies of the alcohol-crime relationship and of the efficacy of alcohol policy in a crime control strategy. Concerns with previous studies and possible remedies for their solution are discussed. Several studies have examined the impact of beer excise taxes (and/or drinking age laws) on violent crime in a reduced-form estimation framework while ignoring non-tax determinants of the retail price and level of consumption of beer. This approach is likely to be problematic for several reasons. First, beer excise taxes comprise only a small portion of the good's retail price and may thus be picking up the variation in other omitted price determinants. That is, such estimates may suffer from missing-variable bias. Second, given the high degree of product differentiation in alcohol markets higher taxes may lead to changes in consumption patterns without reducing the overall level of alcohol consumption. Third, reduced form specifications ignore potentially significant simultaneous relations that are likely to be relevant in guiding policy proscriptions. Thus, they suffer from simultaneity bias. In addition, the apparent efficacy of excise taxes found in previous studies is somewhat surprising given the apparent inelastic demand for alcohol among young male drinkers, the segment of the population that commits the most crime.

Chapter 3 provides estimates of per-capita beer, liquor, and wine consumption equations and examines the effects of a wider range of alcohol regulations than are found in previous studies. In addition, unlike previous studies of alcohol and crime, the estimated beer consumption equation controls for other regulatory/market factors besides excise taxes and drinking age that are hypothesized to influence the retail price of beer. Given the apparent effectiveness of some alcohol control policies in reducing alcohol consumption, Chapter 4 reports attempts to empirically determine the effect of alcohol on the incidence of violent criminal activity using crime data collected from the Federal Bureau of Investigations Uniform Crime Reports, using state level data. Unlike earlier studies of the alcohol-crime relationship, other determinants of criminal participation as motivated by the vast violent crime literature (e.g., deterrence, deprivation, and opportunity cost of crime variables) are controlled for in the structural equations. It is shown that per-capita beer and/or liquor consumption measures appear significant positive determinants of some per-capita crime rates, but the type of alcohol that matters tends to differ across crime types. Finally, endogeneity tests conducted on all structural crime equations show that in most instances (all crime categories except murder) deterrence and consumption variables are endogenous factors. As such, estimation of crime equations with these factors entered directly as crime determinants, as is typically done in the literature, must be treated with considerable caution, as the relationships should be considered with simultaneous equation estimation techniques.

Given the results of the previous two chapters, Chapter 5 reports efforts to determine the effects of alcohol control policies on the incidence of crime using state level data. Due to endogeneity concerns, a simultaneous equations framework is specified and estimated for the purpose of deriving consistent policy estimates. For purposes of comparison, reduced form models also are presented. The results of this Chapter are simultaneously interesting and disappointing. They suggest that the relationship between alcohol and violence is quite complex. Beer consumption appears to have an influence on assaults, for instance, but liquor appears to be the important source of alcohol in murder, rape and robbery. Wine tends to be significantly and negatively related to crimes, suggesting that it may not be alcohol consumption that matters, but rather the circumstances of, or people involved in, alcohol consumption. This is reinforced by an examination of policy variable relationships. Much of what might appear to be a policy-to-consumption-to-violence causal relationship in a reduced form specification turns out to be far more complex, and perhaps spurious, as a substantial portion of the policy variable impacts do not arise though their influence on consumption. Instead, the alcohol policy variables tend to influence the probability of arrest, the level of policing, and/or imprisonment. Alcohol taxes are sources of revenue, of course, so this could be one factor, but a more likely explanation is that alcohol policy variables influence the location and/or circumstances in which alcohol is consumed, in turn making crimes and/or arrests more or less likely. However, all policy estimates must be considered biased upwards given tests that suggest weakness in the instruments used in estimating the simultaneous system.

The complex simultaneity problems that arise using state level data simply cannot be overcome. After all, the alcohol control and taxing policies chosen in a state are likely to be a function of the perceived severity of alcohol-related problems, perhaps including the level of consumption and of crime. Endogeneity of state level policies may not be as problematic if crime and consumption data are drawn from local jurisdictions, however. Therefore, an attempt is made to re-examine the key relationships explored in Chapters 3, 4, and 5 using state level data. Results are reported in Chapter 6 using data from a sample of metropolitan areas. Regrettably, missing variables force us to consider a relatively small sample (and therefore, limited degrees of freedom), and as a consequence, the number of issues we can consider is severely limited. The implication is that while we are able to better control for simultaneity bias, we are less able to control for missing variable bias. Again, results are interesting but must be considered as tentative. A summary and conclusions appear in Chapter 7.

CHAPTER 1

ALCOHOL AND THE INCIDENCE OF CRIME: AN OVERVIEW OF THE THEORETICAL LITERATURE AND AN ALTERNATIVE MODEL

1.1 Introduction

The relationship between the consumption of alcohol and crime has been of interest to academics, policy makers, and health practitioners for quite some time.[2] The general consensus among most parties is that alcohol consumption and aggressive behavior (whether it be criminal or otherwise) are inexorably related. However, whether the relationship between alcohol consumption and aggression or crime is truly causal in nature remains a controversial issue among experts. For instance, consuming alcohol might make an individual act more aggressively (and thus increase their tendency for criminality) through some physiological, psychological, or sociological process (or some combination therein). Alternatively, the acts of consuming alcohol and committing crime may simply be behaviors arising simultaneously from a latent behavioral factor (such as an individual's preference for risk). As such, the precise nature of the alcohol-crime relationship has serious bearings on the efficacy of economic policy towards mitigating the incidence of crime.

The purpose of this chapter is to review the literature concerning the relationships between alcohol consumption, alcohol control policies, and the incidence of crime, and to offer a relatively rigorous alternative. Therefore, Section 1.2 presents a survey of the existing literature on the various theories regarding the nature of the alcohol-crime relationship, Section 1.3 reviews

[2]Much of the material in this Chapter is drawn from Chapters 1 and 2 of Zimmerman (2000).

theoretical models of criminal decision making that are relevant to the formal theory presented in Section 1.4, and Section 1.5 contains a formal model of alcohol's impact on decision making by potential victims. Concluding remarks appear in 1.6.

1.2 Existing Theoretical Hypotheses Regarding the Alcohol-Crime Relationship

A central tenant of research into the etiology of the relationship between alcohol consumption and the incidence of criminal activity has been primarily conducted outside of the economics profession is the literature's cautious interpretation of the observed positive correlation between the consumption of alcohol and crime as one of causation. In fact, the pervasive view held by most researchers is that if there is an alcohol relationship with criminal activity it arises from complex interactions between psychopharmacological, contextual, and societal forces (among others). A number of non-formal theoretical frameworks or hypotheses have been developed within this literature [see Cordilia (1985); Fagan (1990, 1993a, 1993b); Gelles and Cornell (1990); Light (1995); Lipsey et al. (1997); National Research Council (1993); Pernanen (1976, 1981, 1991); Reiss and Roth (1993)]. Each of these theories is capable of generating a positive relationship between alcohol consumption and criminal activity. However, several of the conceptualizations render the positive association as entirely spurious. If these models are accurate, then the efficacy in using alcohol control policies to mitigate the incidence of crime becomes greatly (if not entirely) diminished. This section reviews several of the more widely known frameworks.[3]

The simplest theory of the observed alcohol-crime relationship is the *direct cause* model. In this framework alcohol, through some causal pathway, directly affects the probability that an

[3] It should be noted that the categorization of frameworks presented in this section is not necessarily canon within the literature. The nomenclature employed here is taken from Light (1995).

individual engages in criminal activity. For instance, the psychopharmacological properties of alcohol may induce changes in individual levels of aggressiveness (e.g. by altering brain chemistry, by allowing individuals to feel braver, etc.) and thus increase the tendency for participation in criminal acts. However, this framework does not explicitly consider the effects of other factors that may mitigate or exacerbate this tendency. Of course, since the vast majority of drinking occasions do not result in the commission of criminal acts this theory has generally been rejected as a guide for the development of policy.

In contrast to the direct-causal model, the *common cause* model, both alcohol consumption and violent crime are associated with some third variable that may create the incorrect inference that a causal relation exists when in fact the relationship is merely statistical. For instance, prolonged periods of unemployment brought about by economic recessions may cause individuals to consume alcohol (for its perceived euphoric effects) while at the same time leading to greater occurrences of criminal behavior (since the probability of attaining a legitimate job is reduced). It has also been argued that consuming alcohol and committing crime may be behaviors associated with individuals who have preferences towards engaging in risky activities (e.g. a 'thrill-seeker mentality" held by younger individuals). Thus, a high correlation between the two factors is not actually one of causation even though such an inference may seem to be a reasonable interpretation of the observed data.

In the *conjunctive* model, alcohol may be consumed prior to the violent criminal activity but the nature of the relationship is purely coincidental. That is, perhaps alcohol tends to be consumed during periods just prior to the violent act but after the decision to commit the crime had *already* been made. Upon apprehension of the suspect a criminal investigator might inquire into whether alcohol had been consumed prior to the criminal act. If the suspect were to answer in the affirmative, the incident could be potentially classified as an alcohol-related crime (Light, 1995). However, the crime would have been committed even if the suspect had not consumed any alcohol. Thus, any inference of a causal relationship is, once again, rendered invalid.

Other models contradict both the direct-causal model's strong inferences and the common-cause and conjunctive models' spurious correlation conclusions. Within the *conditional* model, for instance, alcohol consumption may lead to violent behavior, but *if and only if* some other factor is also present.[4] For instance, individuals may consume alcohol as mechanism to justify their participation in illegal activities, ones that would otherwise not be "acceptable" (i.e. the most socially repugnant criminal offenses) for individuals who had not been drinking.[5] It is in this sense that alcohol's involvement with violence depends on the presence of factors and can therefore be deemed conditional. In this case alcohol can at best be interpreted as a contributing factor if in the absence of alcohol consumption the crimes would not also have been committed.

A somewhat stricter version of the conditional model is the *interactive model.* In this framework, alcohol only serves to increase the probability that an individual will engage in violence given that other factors that also influence the probability of engaging in crime are also present. For instance, assume that without any consumption of alcohol the probability that a youth will invoke (or participate in) a bar room fight is on average thirty-percent. However, if the

---

[4]Pernanen (1981) gives temporal lobe dysfunction, hypoglycemia, sleep deprivation, and the development of alcoholism through chronic alcohol abuse as examples of conditional/intervening variables.

[5]These individuals would choose to commit the criminal act in question only if they were also able to consume alcohol beforehand. One example that is consistent with this framework is where the consumption of alcohol serves as a mechanism for deflecting personal responsibility for committing a crime. That is, if before the commission of a crime an individual drinks and if after committing the crime they are apprehended, he/she may blame the alcohol for his/her actions. Note that the primary difference between this framework and the direct-cause mode is the issue of timing regarding when an individual chooses to commit the violent crime. In the direct cause model, the decision is made ex-post (after the individual has consumed alcohol, they are more willing to act violently) whereas in the conjunctive model the decision is made ex-ante (the a priori desire to commit the crime leads them to drink). In this sense the conjunctive model can be thought of as reverse causation: the desire to commit crimes causes one to drink.

youth had been consuming alcohol either prior to arriving at the bar or at the bar itself, then the probability of getting involved in a fight increases.[6] Likewise, mental illness may lead to some individuals randomly engaging in violent outbursts, but this in conjunction with the consumption of alcohol increases the relative frequency with which this will occur.

Of the theories on the alcohol-crime relationship mentioned above, the *conditional model* and the *interactive model* probably come closest to recognizing the true complexity of the nature of any realistic notion of causality in the alcohol-violent crime relationship. As such, it should be emphasized that only a *multidimensional* perspective to modeling the theoretical and empirical relationship between alcohol and violence is appropriate. This sentiment is expressed in the following quote from Lipsey et al. (1997, p. 247):

> "Many researchers believe that causal effects come essentially in the form of an *alcohol* × *person* × *situation* interaction. That is, *alcohol consumption increases the probability of violent behavior only for some persons in some situations*." (emphasis added)

1.3. Models of Decisions to Commit Crime

There are several theories about the nature of violence that do not consider the role that alcohol may play. For exposition, let us focus on three categories.[7] First, *deterrence theory*

6Note the difference between this conceptualization and that of the conditional model. Here, the probability that the youth will engage in violence is nonzero even if he has not consumed any alcoholic beverage. In some sense this framework is similar to the conditional model because it is generally presumed that consuming alcohol in and of itself does not increase the probability of engaging in violence for any reason (i.e. in the absence of expectation effects). It is the intervention of alcohol between the latent factor(s) and the violent act(s) that increases the probability that the former will lead to the latter. The consumption of alcohol and actually (in this example) being in a bar (conditional upon placing oneself in an environment where there is a thirty percent chance of getting into a fight) increases the probability that an individual will be involved in a violent interaction.

7This brief discussion draws heavily on Parker (1995) who also identified a fourth category, ***subculture of violence theory***, which sees violence as a socially acceptable mechanism within certain subcultures for resolving inter- or intra-personal conflicts or achieving various material objectives. This theory does not provide an explicit explanation stresses the disincentives for violent behavior created by threats of punishment. Second, *deprivation theory* hypothesizes that violence is a mechanism for individuals who perceive themselves to be disadvantaged in dealing with the stresses stemming from their inferior socio-economic position (perhaps due to limited educational attainment, discrimination, etc.). Finally, *routine activities theory* relates the incidence of violence to the victim's self-selection into circumstances or environments where violence is more likely to occur. The seminal article of Becker (1968) was the first formal model of criminal behavior within the economics literature, and this approach to modeling individual decision making allows us to incorporate each of these theoretical perspectives in an effort to see how alcohol might also influence the level of criminal activity. Within the economics literature on crime, the deprivation arguments are put somewhat differently. It is hypothesized that individuals with "low opportunity costs" (e.g., low paying legal opportunities) that may be sacrificed in order to become involved in crime (perhaps because of the punishment that may follow and/or because of the time and effort involved in criminal pursuits) find crime, including violent crime, to be relatively attractive. However, the empirical implications of explicit deprivation hypotheses and opportunity cost hypotheses cannot be distinguished, so the following presentation follows the economics conventions. In Becker's model crime arises from the optimizing behavior of a rational economic agent who maximizes an expected utility function that takes the expected return from criminal participation (both monetary and psychic rewards), the probability of being apprehended for a criminal offense, and the "monetary equivalent" of the severity of punishment as its arguments. The main implications

of why the subculture mentality arises among some groups but not others, however, so it lacks meaningful testable implications that can distinguish it from other theories (after all, a serious endogeneity bias may arise because other factors, such as poor income-earning opportunities could correlate with an individual belonging to the "subculture"). While some results of this study may bear on the theory, it is not a primary focus.

of the model are its unambiguous prediction that the equilibrium supply of offenses will be an increasing function of the return to criminal activity and a decreasing function in the level of deterrence factors. However, whether crime is reduced more by increases in the probability of apprehension or by increases in the expected severity of punishment (given apprehension) depends on the distribution of risk aversion across individuals in the economy.

Ehrlich (1973) extends the static framework of Becker's model by casting participation in the light of the theory of occupational choice. In Ehrlich's model, individuals derive income from time spent in either legitimate or illegitimate (i.e. criminal) activities. The difference between the two is that income generated from participation in illegitimate activities is subject to uncertainty in the presence of deterrence factors. Like Becker's model, Ehrlich's framework does not produce unambiguous comparative statics results with respect the relative effectiveness of the certainty versus severity of punishment. Assuming that agents are risk averse, Ehrlich's model does predict that an increase in the return from legitimate employment will decrease the time spent in illegitimate income-generating activities.

The theoretical ambiguities arising from Becker and Ehrlich's models lead to the development of several richer theoretical frameworks (Brown and Reynolds 1973; Block and Lind 1975a, 1975b; Block and Heineke 1975; Appelbaum and Erez 1984; Schmidt and Witte 1988; Witte 1980).[8] Brown and Reynolds (1973) and Block and Heineke (1975) show that the ambiguity associated with the relative effectiveness of the certainty versus severity of punishment in Becker's model arises from the particular notion of there existing a monetary equivalent of punishment, something the authors argue may not actually exist. As such, the distribution of risk

[8] Most theoretical models of crime in economics are static although a few dynamic models have been developed [see for example Myers (1983), Davis (1988) and for a more modern approach Mocan et al. (2000)].

across potential offenders does not allow one to make any inference about the relative effectiveness of deterrence factors. In addition, Block and Heineke (1975) show that if time spent in legitimate and illegitimate income-generating activities are allowed to enter into the utility function as arguments directly, then ambiguous comparative statics results with respect to deterrence factors are again generated despite assumptions on the degree of risk preference of the agents. These results are also evident in the model of Witte (1980) who incorporates both the time allocation problem of Ehrlich (1973) and the inclusion of time allocation directly into the utility function.

Appelbaum and Erez (1984) model a stochastic probability of arrest that is fully characterized by its first two moments. They show that under the assumption of expected utility being a linear function of the probability of arrest, a rise in the mean arrest probability unambiguously reduces criminal participation whereas a change in the variance has no effect. Schmidt and Witte's (1984) model allows for eight different realizations of the agent's problem with respect to employment and deterrence outcomes but also cannot derive unambiguous comparative statics results except with the assumption of decreasing absolute risk aversion.

Rasmussen et al. (1990) develop a formal model characterizing the utility-maximization problem of both the "users" and "producers" of illicit substances (e.g. cocaine). However, criminal participation in their models is not determined by the actual consumption of a commodity in any way. In fact, to date only one formal model exists that examines the effects of "alcohol" consumption on criminal choice. This has been developed by Markowitz and Grossman (1998a) and is based upon the formal models of domestic violence by Long et al. (1983) and Tauchen et al. (1991). Using their notation, the authors employ a utility function of the form:

$$U = u(Z, A, C)$$

where u denotes the utility function, Z a measure of control over a victim (or the "gain" to violence), and A the consumption of alcohol. The level of Z is assumed to be an increasing function of the level of violence chosen by the perpetrator, or $Z = z(V)$ where V denotes violence. As such violence enters the utility function in an indirect manner. The level of violence in turn is a function of the time spent pursing violent activities (Tv), the quantity of alcohol consumed, and an efficiency parameter (α) that includes the probability of facing non-monetary costs for committing violence. Thus

$$V = v(T_v, A, \alpha)$$

where V is strictly increasing in each of its arguments.

The probability of incurring monetary costs for committing violence is given by P. P is a function of the part of the probability unaffected by alcohol use (Π) and alcohol use:

$$P = p(\Pi, A)$$

It is assumed that alcohol reduces the probability of incurring monetary loses since alcohol serves as an excuse for violent behavior. Thus $\frac{\partial P}{\partial A} < 0$.

The agent's problem is to choose the levels of A and T_v to maximize the following expected utility function:

$$\begin{aligned} EU &= (1 - p(\Pi, A))U[v(T_v, A, \alpha), A, I - P_A A - wT_v] \\ &\quad + p(\Pi, A)U[v(T_v, A, \alpha), A, I - P_A A - wT_v - L(T_v)] \end{aligned}$$

where I denotes income, P_A the monetary price plus acquisition costs of alcohol, w the wage rate that represents the opportunity cost of spending time engaged in violence, and L(Tv) a loss function which governs the level of the costs incurred by the agent in the state of having to incur the monetary costs of violence (e.g. being apprehended). The solution to the agent's utility-maximization problem is given by the set of implicit functions for Tv and A:

$$T_v = t_v(P_A, I, w, \Pi, \alpha)$$

$$A = a(P_A, I, w, \Pi, \alpha)$$

where the first equation denotes the reduced form model of the level of violence chosen by the perpetrator and A the level of alcohol consumed in equilibrium. Markowitz and Grossman (1998a, 1998b) argue that by the law of downward-sloping demand, increases in the price of alcohol will decrease the quantity of alcohol consumed. This in turn will decrease the amount of violence chosen in equilibrium by the agent.[9]

While the above model adequately motivates the empirical specification of a reduced-form crime equation, it suffers from several shortcomings. First, it does not consider the effects of alcohol consumption on the behavior of a "victim" in addition to the offender, as suggested by routine activities theory. This may be particularly problematic to our purposes since most empirical studies of the alcohol-crime relationship conducted outside the economics literature have noted a strong tendency for consumption by both the offender and the victim (Wolfgang 1975; Johnson, et al. 1978; Lindquist 1986; Goodman, et al. 1986). If alcohol consumption increases the likelihood of violence it could do so by affecting the behavior of either or both parties. For instance, potential victims can take precautions that reduce their likelihood of being victimized (such as not leaving their house during the late evening hours) as well as engage in

---

[9]Markowitz and Grossman (1998b) note that whether or not violence itself is a choice function in the agent's utility-maximization problem does not alter the form of the reduced-form implicit functions that characterize the problem's solution. If it is not a choice variable, violence is considered to be a "by-product" of alcohol consumption. Higher alcohol prices lower the quantity of alcohol consumed and thus directly lower the incidence of the violent behavioral by-product. Alternatively, if violence is a choice variable and alcohol lowers the costs of engaging in violence, higher alcohol prices will still lead to less violence since they essentially correspond to a higher price of using the alcohol "excuse".

activities that increase their likelihood (such as consuming alcohol which lowers their capacity for defense and makes them "easier" targets). Therefore, the actual number of offenses that occur in equilibrium will be a function of both offender and victim actions with respect to the consumption of alcohol.

Second, the model of Markowitz and Grossman (1998a) relies on the assumption that there is a "preference" for violence, albeit in an indirect manner. Some individuals may indeed derive pleasure from committing criminal acts (e.g. mentally disturbed persons) but many crimes are committed by individuals who, in most instances, would not otherwise choose to do so. An analogous theoretical construct would be the standard microeconomic model of labor-leisure choice. It is generally not assumed that an individual has a preference for labor (in fact it is typically the case that the agent is modeled as deriving *disutility* from devoting time to labor activities). Instead, it is assumed that the income generated from working is a "good" and thus the relevant choice for the agent is a tradeoff between labor and income rather than between income and leisure. A person does not necessarily have to derive pleasure *directly* from behaving aggressively, but only from the benefits generated from acting aggressively/violently/criminally (such as the realization of higher income through committing a robbery).

Third, the model relies on the ex-ante assumption that higher (lower) alcohol prices will necessarily decrease (increase) the quantity consumed in equilibrium. However, from a theoretical standpoint a change in the price of a commodity induces both income and substitution effects. Under certain conditions an increase in the price of alcohol may lead to an increase in consumption (i.e. alcohol may be a Giffen good). The simple framework of Grossman and Markowitz does not allow for this theoretical possibility.

In light of these issues, we first develop (in Section 1.4) a theoretical model of the alcohol-crime relationship from the perspective of the offender where it is assumed that victim behavior is determined exogenously as in Markowitz and Grossman (1998a). It is shown that increases in the full monetary price of alcohol will generally have ambiguous effects on equilibrium crime rates. Section 1.5 then develops the model from the perspective of the victim where offender actions are treated as exogenous. Readers not interested in the derivation of the models in the next two sections may prefer to skip to Section 1.6 where the findings are summarized.

1.4 A Theoretical Model of the Effects of Alcohol Consumption and Alcohol Control Policies on the Decision to Participate in Criminal Activities

This section develops a static theoretical model of criminal choice where a particular "legal" consumption good, which we will regard as alcohol, is allowed to influence an individual's choice of participating in criminal activity.[10] We assume that the representative agent maximizes a strictly quasi-concave twice-differentiable utility function of the form:

$$U = U(C, A, L, X, \ell) \tag{1.1}$$

The first argument to the utility function, C, denotes the consumption of a composite (numeraire) commodity. The variable A denotes the quantity of alcohol consumed. Following Ehrlich (1973), we assume that the agent generates income from time spent in legitimate and illegitimate income-generating activities, denoted L and X, respectively. The variable ℓ denotes the amount of time spent in leisure activities. Following Block and Heineke (1975), we assume

[10] That is, we do not consider the cases of goods whose consumption is illegal (such as marijuana, crack, and heroin) but which may also affect criminal participation. The model presented here could be easily extended to consider such cases by defining the relevant punishment probabilities for the consumption of the illegal substance as well.

time spent in each activity enters the utility function directly. It is assumed $U_C > 0$, $U_A > 0$, $U_L < 0$, $U_X < 0$, $U_\ell > 0$ where subscripts denote partial derivatives.

It assumed that alcohol consumption affects the agent's labor allocation decision between income generating activities in legitimate versus illegitimate market sectors through the behavioral parameter Ψ, which may interpreted as an index of individual "aggressiveness" or propensity to choose illegitimate income generating means over legitimate ones.[11] This parameter is assumed to be a function of the agent's degree of risk-aversion, φ, and his/her "myopia" (the rate at which the agent discounts future rewards/punishments) which can be captured by the agent's subjective discount factor as well as "present-biased preferences" as described by O'Donoghue and Rabin (1999), δ. Alcohol in turn serves as an argument to each of these preference parameters so that $\delta = \delta(A)$)and $\varphi = \varphi(A)$)where $\frac{d\delta}{dA} > 0$ and $\frac{d\varphi}{dA} < 0$. That is, the higher the agent's level of alcohol consumption, the more risk-loving he/she becomes and the greater the rate at which he/she discounts future rewards/punishments. Therefore:

$$\Psi = \Psi(\delta(A), \phi(A)) \tag{1.2}$$

where $\frac{\partial\Psi}{\partial\delta} > 0$, $\frac{\partial\Psi}{\partial\varphi} > 0$. For simplicity, we shall consider the reduced-form version of equation (1.2) or $\Psi = \Psi(A)$. We assume that $\Psi(A) \in (0,1)$)where $\lim_{A\to\infty} \Psi(A) = 1$. This

11 Alternatively, Ψ could be viewed as an argument to the implicit expected costs of criminal participation conditional on the level of alcohol consumed. Higher levels of alcohol consumption would then increase the value of Ψ which in turn lowers the expected costs of criminal involvement (e.g. the severity of punishment conditional on apprehension). Thus, the individual is more likely to participate in illegal income-generating activities the higher their consumption of alcohol since consumption lowers the perceived "cost" of criminal behavior. Note that this particular interpretation does not alter the implications of the model presented here.

behavioral parameter may thus be regarded as a probability, which is monotonically increasing in the level of alcohol consumption: $\frac{d\Psi}{dA} > 0$.[12] That is, the greater the agent's consumption of alcohol, the greater the degree of his/her aggressiveness and therefore propensity for criminality. Note that this assumption taken by itself is, in a sense, consistent with the *direct-causal* hypothesis (consuming alcohol raises the propensity for violence), the theoretical perspective that provides the strongest implications for an alcohol-crime connection. Yet, we shall see that in the context of a model of individual decision making, this assumption does not necessarily imply that raising the price of alcohol necessarily reduces violent crime. That is because the decision to commit crimes is actually *conditional* or *interactive* as it depends on the individuals conditions (e.g., income, legitimate income earning possibilities, preferences including preferences for risk) and other constraints that the individual faces (e.g., probability and severity of punishment). Therefore, while alcohol is assumed to be a direct cause of the propensity for violence, it does not follow that it always leads to violence, and the model really fits within the *conditional/interactive* category discussed earlier. The utility function expressed in equation (1.1) is maximized subject to the following budget constraint:

$$p_C C + p_A A = I + [1 - \Psi(A)]wL + \Psi(A)[r - hF]X \qquad (1.3)$$

[12]These assumptions are made to keep the model tractable. Alcohol is of course a depressive substance where high levels of consumption will tend to render a person unconscious or so incapacitated that committing a crime becomes physically impossible. Thus, the function $\Psi(A)$) would tend to be strictly concave in its argument rather than monotonically increasing. Alcohol control policies are primarily concerned with the region of consumption over which proximate negative outcomes of alcohol consumption (such as crime) are more likely to occur than not to occur (such as at very high levels of consumption). As such, the assumption of monotonicity is not unreasonable. Note further that individuals may be highly "aggressive" in the sense employed here while not consuming any alcohol and vice-versa. An interesting source of heterogeneity across agents may be the rate at which the function Ψ increases with respect to the value of $\Psi_1(0)$). It seems reasonable to expect that if $\Psi(0) = a$ for individual one and $\Psi_2(0) = b$ for individual 2 where $a > b$, then $\frac{d\Psi_1}{dA} > \frac{d\Psi_2}{dA} \; \forall A$.

I denotes the agent's initial level of exogenously determined wealth. The variables p_C and p_A denote the full prices of the composite good and alcohol respectively, w denotes the equilibrium wage earned from time devoted to employment in the legitimate sector, r denotes the return from employment in the illegitimate sector, h the probability of apprehension, and F the monetary equivalent of the severity of punishment. Therefore, this model allows for consideration of the effect of both *deterrence* and *deprivation* theories, since *deterrence theory* emphasizes incentives created by threats of punishment while *deprivation theory* hypothesizes that violence is a mechanism for individuals who perceive themselves to be disadvantaged. A disadvantaged individual will probably have low income and limited legal earning opportunities (perhaps due to limited educational attainment, discrimination, etc.), and perhaps psychological characteristics that affect the utility function (e.g., low risk aversion, myopia). Note that *routine activities theory*, which attributes violence to the victim's self-selection into circumstances or environments where violence is more likely to occur, will be considered separately below, with a separate model of potential victim's decisions. With regard to the role of government policies aimed at controlling the consumption of alcohol (such as imposing taxes or minimum legal drinking ages), the assumption is that to the degree that such polices are effective , they raise the value of p_A. Again, note that the higher the agent's chosen level of alcohol consumption, the greater the probability that they will choose to devote time to the illegitimate sector as opposed to the legitimate sector for income-generating purposes, all else equal.

The agent's utility-maximization problem is also subject to the following time-resource constraint:

$$T = L + X + \ell \qquad (1.4)$$

where T denotes the total amount of time available to the agent to allocate among the three activities. Rewriting equation (1.4) as $L = T - X - \ell$ and substituting this expression into equation (1.3) allows the agent's utility-maximization problem to be written as:

$$\max_{\{C,A,X,\ell\}} U(C,A,X,\ell)$$

subject to:

$$p_C C + p_A A = I + [1 - \Psi(A)]w(T - X - \ell) + \Psi(A)[r - hF]X$$

The LaGrangian of the agent's utility-maximization problem is given by:

$$\begin{aligned} \Lambda = {} & U(C, A, L, X) \\ & + \lambda[I + [1 - \Psi(A)]w(T - X - \ell) + \Psi(A)[r - hF]X \\ & - p_C C - p_A A] \end{aligned} \tag{1.5}$$

The first order conditions for a maximum are given by:

$$\frac{\partial \Lambda}{\partial C} = U_C + \lambda(-p_C) = 0 \tag{1.6}$$

$$\frac{\partial \Lambda}{\partial A} = U_A + \lambda[\Psi_A((r - hF)X - w(T - X - \ell)) - p_A] = 0 \tag{1.7}$$

$$\frac{\partial \Lambda}{\partial X} = U_X + \lambda[\Psi(A)(r - hF) - (1 - \Psi(A))w] = 0 \tag{1.8}$$

$$\frac{\partial \Lambda}{\partial \ell} = U_\ell + \lambda[-w(1 - \Psi(A))] = 0 \tag{1.9}$$

$$\begin{aligned} \frac{\partial \Lambda}{\partial \lambda} &= I + [1 - \Psi(A)]w(T - X - \ell) + \Psi(A)[r - hF]X - p_C C - p_A] \\ &= 0 \end{aligned} \tag{1.10}$$

where $\Psi_A = \frac{\partial \Psi}{\partial A}$. The solution to the agent's utility-maximization problem requires solving the system of equations given by expressions (1.6)-(1.9) simultaneously. ~~Let $\Psi^* = \Psi(A^*)$ and~~ $\Psi_A^* = \frac{\partial \Psi^*}{\partial A^*}$. Assuming the second-order conditions are satisfied (i.e. no corner solutions occur in equilibrium), the solution to the offender's utility maximization problem can be characterized by the following set of implicit functions:

$$C^* = C^*(I, w, r, h, F, p_C, p_A, \Psi_A^*) \qquad (1.11)$$

$$A^* = A^*(I, w, r, h, F, p_C, p_A, \Psi_A^*) \qquad (1.12)$$

$$L^* = L^*(I, w, r, h, F, p_C, p_A, \Psi_A^*) \qquad (1.13)$$

$$X^* = X^*(I, w, r, h, F, p_C, p_A, \Psi_A^*) \qquad (1.14)$$

$$\lambda^* = \lambda^*(I, w, r, h, F, p_C, p_A, \Psi_A^*) \qquad (1.15)$$

1.4.1. Comparative Statics

In this section we use the method of comparative statics to derive implications of changes in the model's parameters on the time allocated to illegitimate activities in equilibrium. The complete derivation of the effect of a change in exogenously given income (e.g., public assistance payments, allowance from parents, income from inheritance) is presented first for reasons that will become apparent below. The effect of a change in the full price of alcohol on the equilibrium level of time devoted to crime is then derived. All other comparative statics results are then presented.

The primary comparative statics term of interest is that associated with the effect of an increase in the full price of alcohol on the equilibrium crime rate. First, consider the effect on the equilibrium crime allocation of an increase in the level of the agent's exogenous level of income, I. Substituting the equilibrium values in equations (1.11)-(1.15) into the first-order conditions transforms equations (1.6)-(1.10) into a system of identities. Totally differentiating this system of identities with respect to I yields:

$$U_{CC}\frac{\partial C^*}{\partial I} + U_{CA}\frac{\partial A^*}{\partial I} + U_{CX}\frac{\partial X^*}{\partial I} + U_{C\ell}\frac{\partial \ell^*}{\partial I} - p_C\frac{\partial \lambda^*}{\partial I} \equiv 0 \tag{1.16}$$

$$\begin{aligned} &U_{AC}\frac{\partial C^*}{\partial I} + U_{AA}\frac{\partial A^*}{\partial I} + [U_{AX} + \lambda^*\Psi_A^*(w+(r-hF))]\frac{\partial X^*}{\partial I} \\ &+[U_{A\ell} + \lambda^*\Psi_A^* w]\frac{\partial \ell^*}{\partial I} \\ &+[\Psi_A^*((r-hF)X^* - w(T-X^*-\ell^*)) - p_A]\frac{\partial \lambda^*}{\partial I} \equiv 0 \end{aligned} \tag{1.17}$$

$$\begin{aligned} &U_{XC}\frac{\partial C^*}{\partial I} + [U_{XA} + \lambda^*\Psi_A^*(w+(r-hF))]\frac{\partial A^*}{\partial I} + U_{XX}\frac{\partial X^*}{\partial I} \\ &+U_{X\ell}\frac{\partial \ell^*}{\partial I} + [\Psi^*(r-hF) - (1-\Psi^*)w]\frac{\partial \lambda^*}{\partial I} \equiv 0 \end{aligned} \tag{1.18}$$

$$\begin{aligned} &-p_C\frac{\partial C^*}{\partial I} + [\Psi_A^*((r-hF)X^* - w(T-X^*-\ell^*)) - p_A]\frac{\partial A^*}{\partial I} \\ &+[\Psi^*(r-hF) - (1-\Psi^*)w]\frac{\partial X^*}{\partial I} + [-(1-\Psi^*)w]\frac{\partial \ell^*}{\partial I} \equiv 1 \end{aligned} \tag{1.20}$$

Rewriting the system of equations represented by (1.16)-(1.20) in matrix form yields:

$$\begin{bmatrix} U_{CC} & U_{CA} & U_{CL} & U_{C\ell} & -p_C \\ U_{AC} & U_{AA} & [U_{AX} + \lambda^*\Psi_A^*[w + (r - hF)] & B & D \\ U_{LC} & E & U_{XX} & U_{X\ell} & G \\ U_{XC} & J & U_{\ell X} & U_{\ell\ell} & K \\ -p_C & D & G & K & 0 \end{bmatrix}$$

$$= \begin{bmatrix} \partial C^*/\partial I \\ \partial A^*/\partial I \\ \partial L^*/\partial I \\ \partial X^*/\partial I \\ \partial \lambda^*/\partial I \end{bmatrix} \begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \\ 1 \end{bmatrix}$$

where $B = U_{A\ell} + \lambda^*\Psi_A^* w$, and $D = -\Psi_A^*(w(T - X^* - \ell^*) - (r - hF)X^*) - p_A$,

$E = U_{XA} + \lambda^*\Psi_A^*(w + (r - hF))$, $G = \Psi^*(r-hF) - (1-\Psi^*)$, $K = -w(1-\Psi^*)$, and

$J = U_{\ell A} + \lambda^* \Psi_A^* w$. Let H denote the five by five matrix on the left hand side of the above expression. H is the bordered Hessian matrix of the agent's utility maximization problem. By appealing to Cramer's Rule we can determine the effect of a change in the agent's income on the equilibrium level of time he/she devotes to criminal activities as:

$$\frac{\partial X^*}{\partial I} = \frac{\begin{vmatrix} U_{CC} & U_{CA} & 0 & U_{C\ell} & -p_C \\ U_{AC} & U_{AA} & 0 & B & D \\ U_{LC} & E & 0 & U_{X\ell} & G \\ U_{XC} & J & 0 & U_{\ell\ell} & K \\ -p_C & D & 1 & K & 0 \end{vmatrix}}{|H|}$$

where the brackets | | denote the determinant of the matrix. Solving the above expression gives:

$$\begin{aligned} \frac{\partial X^*}{\partial I} = & \{U_{CC}[U_{AA}(KU_{X\ell} - GU_{\ell\ell}) + E(DU_{\ell\ell} - BK) - J(BG - DU_{X\ell}] \\ & + (U_{CA})^2(GU_{\ell\ell} - KU_{X\ell}) + U_{CA}[E(KU_{C\ell} + p_C U_{\ell\ell}) \\ & - J(GU_{C\ell} + p_C U_{X\ell}) + U_{XC}(BK - DU_{\ell\ell})\,] \\ & - U_{XC}[U_{AA}(KU_{C\ell} + p_C U_{\ell\ell}) - J(DU_{C\ell} - Bp_C)\,] \\ & + U_{C\ell}U_{CA}(DU_{X\ell} - BG) + U_{AA}(G(U_{C\ell})^2 + p_C U_{X\ell}) \\ & - E(D(U_{C\ell})^2 + EBp_C U_{C\ell})\,\} / |H| \end{aligned} \tag{1.21}$$

The sign of the above expression is indeterminate. The denominator of equation (1.21) is positive if the second order conditions for utility maximization are satisfied. The ambiguity of the sign of the numerator holds even under the assumption that preferences exhibit diminishing marginal utility of consumption ($U_{CC} < 0$, $U_{AA} < 0$) since economic theory does not provide clear insight into the sign of the cross-partial terms between the consumption goods and leisure. If leisure is a normal good then the sign of the above expression will tend to be negative.[13]

[13] For those who are not familiar with the economics jargon, various terms will be defined in footnotes. For instance, a "normal good" means that as income increases (decreases) demand for the good increases (decreases). That is, there is a positive relationship between demand for a good and the income of the consumer. In contrast, an "inferior

However, even this result is not certain given that the agent may substitute between time spent in legal and illegal activities as his/her income changes (Rasmussen, et al. 1990).

Next, consider the effects of a change in the return to illegitimate income-generating activities:

$$
\begin{aligned}
\frac{\partial X^*}{\partial r} &= -\Psi^* X^* \left(\frac{\partial X^*}{\partial I} \right) + \lambda^* [U_{CC}(U_{AA} K^2 \Psi - EK^2 \Psi_A^* X^* \\
&\quad + J(\Psi_A^* GKX^* - \Psi^* DK) + D(\Psi_A^* X^* (KU_{X\ell} - GU_{\ell\ell}) \\
&\quad + \Psi^* (DU_{\ell\ell} - BK)))] - (U_{CA})^2 \Psi^* K^2 \\
&\quad + U_{CA}[2D\Psi^* (KU_{C\ell} + p_C U_{\ell\ell}) - J\Psi^* K p_C] \\
&\quad + U_{XC}[\Psi_A^* X^* (K^2 U_{CA} + JKp_C - DU_{XC}(KU_{C\ell} + p_C U_{\ell\ell})] \\
&\quad + (U_{C\ell})^2 [D(\Psi_A^* GX^* - D\Psi^*)] + U_{C\ell}[D(\Psi_A^* p_C U_{X\ell} X^* - \Psi^* B p_C) \qquad (1.22) \\
&\quad + 2(\Psi^* K p_C U_{AA} - \Psi_A^* EKp_C X^*) - \Psi_A^* GKU_{CA} X^*] \\
&\quad + (p_C)^2 [\Psi^* U_{AA} U_{\ell\ell} + \Psi_A^* X^* (JU_{X\ell} - EU_{\ell\ell}) - \Psi^* BJ] \\
&\quad + p_C [U_{CA}(\Psi_A^* X^* (KU_{X\ell} - U_{\ell\ell}) - \Psi^* BK) \\
&\quad + JU_{C\ell}(\Psi_A^* GX^* - D\Psi^*)]] / |H| \\
&= -\Psi^* X^* \left(\frac{\partial X^*}{\partial I} \right) + \left. \frac{\partial X^*}{\partial r} \right|_{U = \bar{U}}
\end{aligned}
$$

Note that the sign of equation (1.22) depends partially on the sign of what is referred to as the income effect [the first term after the equal sign in (1.22)], and the sign of this term depends on the bracketed term which is given by equation (1.21). Therefore, the sign of the income effect depends on whether the good is normal or inferior (see the discussion in footnote 12), and it cannot be determined. Assume that the magnitude of the change in the first term of equation (1.22) is greater than that of the second. If the agent tends to increase the time devoted to criminal income-generating activities as his/her income rises, the sign of the above expression would tend to be negative. The opposite result holds if the agent would supply less time to

good is one for which demand decreases (increases) as income increases (decreases) so there is a negative relationship between income and demand for an inferior good.

criminal activities as income rose. The second term is the amount by which time devoted to criminal activity in equilibrium changes in response to a rise in the return to crime holding the agent's level of utility constant (i.e. the so-called substitution effect).[14] Again, this expression cannot be signed as it depends upon the signs of the various cross partial terms and the differences between potentially positive elements.

The slope of the agent's labor supply curve over legitimate income-generating activities is given by:

$$
\begin{aligned}
\frac{\partial X^*}{\partial w} = {} & -(1 - \Psi^*)L^*\left(\frac{\partial X^*}{\partial I}\right) + \lambda^*\{U_{CC}(U_{AA}(ZGK - ZG^2) + E\Psi_A^*LK^2 - EZDK \\
& - J\Psi_A^*LGK + JZDK + D\Psi_A^*(LGU_{\ell\ell} - LKU_{X\ell}) + DZBK \\
& - ZD^2U_{\ell\ell} - DZBG + ZD^2U_{X\ell}) + (U_{CA})^2(ZK(K - G)) \\
& + U_{CA}(JZp_CK - EZp_CK - 2DZ(KU_{C\ell} + p_CU_{\ell\ell} - p_CU_{X\ell}) \\
& + DZGU_{C\ell}) + U_{XC}(U_{CA}ZDK - U_{CA}\Psi_A^*LK^2 + Zp_CKU_{AA} \\
& - J\Psi_A^*Lp_CK + D\Psi_A^*L(U_{C\ell}K + Lp_CU_{\ell\ell}) - ZU_{C\ell}D^2 \\
& - DZp_CB) + (U_{C\ell})^2(ZD^2 - D\Psi_A^*LG) + U_{C\ell}(U_{CA}\Psi_A^*LGK \\
& - 2U_{AA}Zp_CK + 2E\Psi_A^*Lp_CK - D\Psi_A^*Lp_CU_{X\ell} + DZp_CB) \\
& + U_{AA}(Z(p_C)^2(U_{\ell\ell} + U_{X\ell})) + (p_C)^2(E\Psi_A^*LU_{\ell\ell} - EZB \\
& - J\Psi_A^*LU_{X\ell} + JZB) + p_C(U_{CA}(\Psi_A^*LGU_{\ell\ell} - \Psi_A^*LU_{X\ell}K \\
& + ZB(K - G)) + U_{AA}ZU_{C\ell}G - EZU_{C\ell}D - J\Psi_A^*LU_{C\ell}G \\
& - JZU_{C\ell}D)\} / |H| \qquad (1.23)
\end{aligned}
$$

where $Z = (1 - \Psi^*) > 0$. The above expression does not have an unambiguous sign. To the extent that increases in the agent's exogenously determined income lead to less time being devoted to crime, the sign of the equation (1.23) will tend to be positive. However, this result

[14] A change in a price, including a change in wage or the "price paid for" labor services (or the expected "payment" for an illegal act) produces two separate effects. A higher price creates incentives to "substitute" (e.g., look for lower priced goods that server similar purposes, substitute labor for leisure). However, the higher price also affects purchasing power so it has an "income effect" (e.g., a higher priced good reduces total purchasing power while a higher wage increases it). So the total effect of a price change includes an income effect and a substitution effect.

(and its converse) does not occur with certainty since the sign of the bracketed portion in the above expression is ambiguous.

The effects of deterrence on the equilibrium rate of crime are given by equations (1.24) and (1.25) below. Note that the signs of these expressions also cannot escape ambiguity. The sign of the first term in each equation depends on the sign of the income effect. The sign of the second term again depends on the sign of the bracketed portion. However, neither portion can be signed with certainty given that they include several cross-partial terms.

$$
\begin{aligned}
\frac{\partial x^*}{\partial h} = {} & \Psi^* X^* F\left(\frac{\partial x^*}{\partial I}\right) + F\lambda^* \{U_{CC}(E\Psi_A^* K^2 X^* - \Psi^* K^2 U_{AA} \\
& - J\Psi_A^* GKX^* + J\Psi^* DK - D\Psi_A^* KU_{X\ell} X^* + D\Psi_A^* GU_{\ell\ell} X^* \\
& + D\Psi^* BK - \Psi^* D^2 U_{\ell\ell}) + (U_{CA})^2 \Psi^* K^2 + U_{CA}(J\Psi p_C K \\
& - 2U_{CA} D\Psi(KU_{C\ell} - p_C U_{\ell\ell}) + U_{XC}(D\Psi_A^* X^*(KU_{C\ell} + p_C U_{\ell\ell}) \\
& - \Psi_A^* K^2 U_{CA} X^* - J\Psi_A^* p_C KX^*) + (U_{C\ell})^2 (D^2 \Psi^* - D\Psi_A^* GX^*) \qquad (1.24) \\
& + U_{C\ell}(U_{CA} \Psi_A^* GKX^* + 2U_{C\ell}(E\Psi_A^* p_C KX^* - \Psi^* K p_C U_{AA}) \\
& + D(\Psi^* p_C B - \Psi_A^* p_C U_{X\ell} X^*) + (p_C)^2 (\Psi_A^* X^*(EU_{\ell\ell} - JU_{X\ell}) \\
& + J\Psi^* B - \Psi^* U_{AA} U_{\ell\ell}) - p_C(\Psi_A^* U_{CA} X^*(GU_{\ell\ell} - KU_{X\ell}) \\
& + J\Psi^* B - \Psi^* BKU_{CA} - J\Psi_A^* GU_{C\ell} X^* + J\Psi^* DU_{C\ell}) \} / |H|
\end{aligned}
$$

$$\frac{\partial x^*}{\partial F} = \Psi^* X^* h\left(\frac{\partial x^*}{\partial I}\right) + h\lambda^*\{U_{CC}(E\Psi_A^* K^2 X^* - \Psi^* K^2 U_{AA}$$
$$- J\Psi_A^* GKX^* + J\Psi^* DK - D\Psi_A^* KU_{X\ell}X^* + D\Psi_A^* GU_{\ell\ell}X^*$$
$$+ D\Psi^* BK - \Psi^* D^2 U_{\ell\ell}) + (U_{CA})^2 \Psi^* K^2 + U_{CA}(J\Psi p_C K$$
$$- 2U_{CA}D\Psi(KU_{C\ell} - p_C U_{\ell\ell}) + U_{XC}(D\Psi_A^* X^*(KU_{C\ell} + p_C U_{\ell\ell})$$
$$- \Psi_A^* K^2 U_{CA} X^* - J\Psi_A^* p_C KX^*) + (U_{C\ell})^2(D^2\Psi^* - D\Psi_A^* GX^*) \tag{1.25}$$
$$+ U_{C\ell}(U_{CA}\Psi_A^* GKX^* + 2U_{C\ell}(E\Psi_A^* p_C KX^* - \Psi^* Kp_C U_{AA})$$
$$+ D(\Psi^* p_C B - \Psi_A^* p_C U_{X\ell}X^*) + (p_C)^2(\Psi_A^* X^*(EU_{\ell\ell} - JU_{X\ell})$$
$$+ J\Psi^* B - \Psi^* U_{AA}U_{\ell\ell}) - p_C(\Psi_A^* U_{CA}X^*(GU_{\ell\ell} - KU_{X\ell})$$
$$+ J\Psi^* B - \Psi^* BKU_{CA} - J\Psi_A^* GU_{C\ell}X^* + J\Psi^* DU_{C\ell})\} / |H|$$

Next, consider the effects of an increase in the (full) price of the numeraire commodity on the time allocated to criminal activity in equilibrium:

$$\frac{\partial x^*}{\partial p_C} = C^*\left(\frac{\partial x^*}{\partial I}\right) + \lambda^*\{U_{XC}(D^2 U_{\ell\ell} - JDK + KU_{AA})$$
$$+ U_{CA}(JGK + D(KU_{X\ell} - GU_{C\ell}) - EK^2) \tag{1.26}$$
$$+ p_C(U_{AA}(KU_{XC} - GU_{\ell\ell}) + E(DU_{\ell\ell} - BK) + J(BG - DU_{X\ell})$$
$$+ U_{C\ell}(EDK + DBG - D^2 U_{X\ell} - GKU_{AA})\} / |H|$$

The sign of the above expression cannot be determined due to the ambiguity of the income effect in the first term and differences in cross-partial terms in the second.

Now consider the effects of a change in the full (monetary-equivalent) price of alcohol on the equilibrium time allocation to criminal activity:

$$\frac{\partial X^*}{\partial p_A} = A^*\left(\frac{\partial X^*}{\partial I}\right) + \lambda^*\{U_{CC}[EK^2 - JGK - DKU_{X\ell} + DGU_{\ell\ell}]$$
$$+ U_{XC}[K^2U_{CA} + JKp_C - DKU_{C\ell} - Dp_CU_{\ell\ell}]$$
$$- DG(U_{C\ell})^2 + U_{C\ell}[GKU_{CA} + 2EKp_C - Dp_CU_{X\ell}] \tag{1.27}$$
$$+ (p_C)^2[EU_{\ell\ell} - JU_{X\ell}] - p_C[KU_{CA}U_{X\ell} - GU_{CA}U_{\ell\ell}$$
$$+ JGU_{C\ell}]\} /|H|$$

Note that the sign of the above expression cannot be determined unambiguously given that it relies partially on the income effect. If the income effect were positive, the agent would tend to reduce his income generating criminal activity in response to increase in alcohol prices. In other words, given that his/her purchasing power has fallen he/she would tend to commit less crime. On the other hand, if the income effect is negative the agent will tend to increase the time devoted to criminal income generating activities. Note further that the second term of equation (1.27) cannot be signed unambiguously as well. In short, *increasing the (full) price of alcohol does not necessarily decrease the equilibrium time devoted to criminal income-generating activities.* Some intuition for this result can be garnered by examining the effect of a change in the price of alcohol on the equilibrium quantity of alcohol consumed (again assuming that all other model parameters remain constant). This expression is given by:

$$\frac{\partial A^*}{\partial p_A} = A^*\left(\frac{\partial A^*}{\partial I}\right) + \lambda^*\{K^2(U_{CX})^2 + G^2(U_{C\ell})^2 + (p_C)^2(U_{X\ell})^2$$
$$+ U_{CC}(2GKU_{X\ell} - K^2U_{XX} - G^2U_{\ell\ell})$$
$$+ 2U_{CX}(Kp_CU_{X\ell} - G(KU_{C\ell} + p_CU_{\ell\ell})) \tag{1.28}$$
$$+ 2U_{C\ell}(p_C(GU_{X\ell} - KU_{XX}))$$
$$- (p_C)^2U_{XX}U_{\ell\ell}\} /|H|$$

Expression (1.28) gives the slope of the demand curve for alcohol. The parenthesized expression in the first term of the above expression is the income effect. This term cannot be signed *a priori* and depends on whether alcohol is a normal or inferior good. The second term corresponds to the substitution effect. Note that this expression cannot be signed either. The reason for this unusual result is relatively straightforward. Recall that alcohol consumption increases the agent's tendency to devote time to illegal income generating activities. Whether the agent tends to consume more or less alcohol (holding utility constant) will depend partially on the agent's preference structure over the two income generating activities. Indeed, examining equation (1.28) shows that the sign of the substitution effect depends on the sign of the own and cross-partial effects with respect to the marginal utility of each income-generating activity as well as the cross-partial effects between the marginal utility of the composite good and each activity. Note that these theoretical results stands in stark contrast to the model of Markowitz and Grossman (1998a), the validity of whose implications lies conditional on the demand for alcohol exhibiting non-Giffen behavior.[15] Introducing an income generating time allocation decision that is influenced by the consumption of alcohol may lead to the Giffen outcomes even if alcohol is a normal good.

Finally, consider the effects of an increase in the agent's marginal propensity for aggression from alcohol:

[15] A "Giffen Good" is one that violates the "law of demand." Empirically, the law of demand (that there is an inverse relationship between price and quantity demanded) holds for most people and most goods, but an economist named Giffen demonstrated that theoretically this need not be the case. If a good is inferior and the income effect is larger than the substitution effect, then quantity demanded can increase as price increases. In typical models of consumer choice the substitution effect always is consistent with the law of demand, and if the good is also normal then the income effect reinforces the substitution effect. In this case, the substitution effect also cannot be signed because we have added consideration of the allocation of time to the choice set, and the relationship between work (illegal and legal) and leisure is affected by the price change for a good.

$$\frac{\partial X^*}{\partial \Psi_A^*} = \lambda^* \{DQU_{CC}X^*(GU_{\ell\ell} - KU_{X\ell})$$
$$+ U_{XC}(DV(KU_{C\ell} + p_C U_{\ell\ell} - K^2V - Jp_C K))$$
$$+ U_{C\ell}(V(GKU_{CA} + 2Ep_C K - Dp_C U_{X\ell}) \qquad (1.29)$$
$$+ (p_C)^2(V(EU_{\ell\ell} - JU_{X\ell}))$$
$$+ p_C(V(GU_{\ell\ell} - KU_{X\ell} - GJU_{C\ell}))$$
$$+ U_{CC}(V(EK^2 - JGK) - DWL^*(GU_{\ell\ell} - KU_{X\ell}))\} / |H|$$

The sign of the above expression cannot be signed without further information regarding the offender's utility function.

1.5 A Theoretical Model of the Effects of Alcohol on the Tendency of Victimization to Criminal Activities

This section develops a model of individual behavior when the consumption of alcohol increases the probability of being victimized by crime, thus providing a basis for considering alcohol's effect under a *routine activities theory* that relates the incidence of violence to the victim's self-selection into circumstances or environments where violence is more likely to occur. Throughout the analysis it is assumed that offender behavior (i.e. the amount of time criminal devote to illegitimate income-generating activities) is constant. In other words, we abstract from the possibility that offenders will devote less time to criminal income-generating activities in response to victims lowering their exposure to crime. There is at least some evidence that this is a reasonable assumption, as criminals can shift targets when they find that a given target is investing in self-protection. For instance, Benson and Mast (2000) find that while employment of private security provides specific deterrence (i.e., crime against the employer of the security services is relatively low), it does not appear to provide general deterrence (i.e., overall crime rates are not reduced).

It is assumed that potential victims maximize a strictly quasi-concave utility function of the form

$$U(C, A, L, \ell) \tag{1.30}$$

where C, A, and L are again consumption of a numeraire commodity, consumption of alcohol, and time devoted to legal-income generating activity respectively. Once again, the variable denotes the amount of time spent in leisure activities. As such, the time resource constraint of the agent is

$$T = L + \ell \tag{1.31}$$

where) T denotes the total time available to the agent. By devoting time to legal employment activities the agent earns a return of w with certainty. It is assumed that by devoting time to the consumption of leisure the agent faces some positive probability of being a "victim" of crime. This probability is given by $\xi(A) \in (0,1) \; \forall A$. For instance, leisure may be time spent outside of the home (e.g. nighttime social activities) during which the agent faces the risk of being robbed. Further, it is assumed that the consumption of alcohol (A) increases the probability that the agent will be a victim of a criminal act (e.g. because individuals who have been drinking are "easier" targets for offenders) or $\frac{d\xi(A)}{dA} > 0$.

The utility function expressed in equation (1.30) is subject to the following budget constraint:

$$p_C C + p_A A = I_0 + w(T - \ell) - \xi(A) \cdot r \cdot \ell \tag{1.32}$$

Again, ξ denotes the probability of victimization per unit of time devoted by offenders to criminal activity. The variable r denotes the amount of wealth the agent loses given that they are victimized (r corresponds to the return earned by offenders from devoting time to criminal income-generating activities in the previous case). Note that the agent's utility maximization problem may be written in terms of the choice variables C, A, and ℓ exclusively by substituting the expression $L = T - \ell$ into equations (1.30) and (1.32). The agent's problem then becomes:

$$\max_{\{C,A,\ell\}} U(C,A,\ell)$$

subject to:

$$p_C C + p_A A = I_0 + w(T - \ell) - \xi(A) \cdot r \cdot \ell$$

The LaGrangian for the agent's utility-maximization problem is

$$\Lambda = U(C, A, \ell) + \lambda[I + w(T - \ell) - (\xi(A) \cdot r \cdot \ell) - p_C C - p_A A] \quad (1.33)$$

The first-order conditions for a maximum are given by:

$$\frac{\partial \Lambda}{\partial C} = U_c + \lambda(-p_c) = 0 \quad (1.34)$$

$$\frac{\partial \Lambda}{\partial A} = U_A + \lambda[(-\xi_A \cdot r \cdot \ell) - p_A] = 0 \quad (1.35)$$

$$\frac{\partial \Lambda}{\partial \ell} = U_\ell + \lambda(-w - \xi(A) \cdot r) = 0 \quad (1.36)$$

$$\frac{\partial \Lambda}{\partial \lambda} = I + w(T - \ell) - (\xi(A) \cdot r \cdot \ell) - p_C C - p_A A = 0 \quad (1.37)$$

where subscripts denote partial derivatives. It is assumed that all conditions for an interior solution are satisfied. The following set of implicit functions characterizes the solution to the agent's utility-maximization problem:

$$C^* = C^*(I, w, r, p_C, p_A, \xi_A^*) \quad (1.38)$$

$$A^* = A^*(I, w, r, p_C, p_A, \xi_A^*) \quad (1.39)$$

$$\ell^* = \ell^*(I, w, r, p_C, p_A, \xi_A^*) \quad (1.40)$$

$$\lambda^* = \lambda^*(I, w, r, p_C, p_A, \xi_A^*) \tag{1.41}$$

1.5.1 Comparative Statics

First, consider a change in the time devoted to leisure in equilibrium to a change in the agent's initial income. This expression is given by:

$$\frac{\partial \ell^*}{\partial I} = E(p_C U_{CA} - DU_{CC}) + U_{\ell C}(DU_{CA} - p_C U_{AA}) + G(U_{AA}U_{CC} - (U_{CA})^2) / |H| \tag{1.42}$$

where $B = U_{A\ell} - \lambda^* \xi_A^* r$, $D = \ell^* r \xi_A^* + p_A$, $E = U_{\ell A} - \lambda^* \xi_A^* r$,

and $G = w + r\xi^*$. H designates the four by four bordered Hessian matrix of the agent's utility maximization problem (which must be positive for an interior solution). The sign of equation (1.42) cannot be determined unambiguously. Again, the sign of the cross-partial terms cannot be determined without knowledge of the functional form of the utility function. In addition, the expression involves differences in positive terms.

Next, consider the effects of an increase in the market wage earned by the agent for devoting time to employment on equilibrium leisure:

$$\frac{\partial \ell^*}{\partial w} = -(T - \ell)\left(\frac{\partial \ell^*}{\partial I}\right) + \lambda^*[2Dp_C U_{CA} - D^2 U_{CC} - (p_C)^2 U_{AA}] / |H| \tag{1.43}$$

The sign of the above expression is ambiguous. The term $-(T - \ell))$ is necessarily negative (or zero if the agent devotes all their time to leisure), and thus the sign of the first term depends on whether leisure is a normal or inferior good. Note that the sign of the term in brackets depends only on the sign of the cross-partial term between consumption of the numeraire and alcohol (assuming diminishing marginal utility of consumption). As such, sufficient conditions for equation (1.43) to be positive are that leisure is an inferior good and that the marginal utility of the numeraire rises with increases in the consumption of alcohol.

Now consider an increase in the price of the numeraire commodity:

$$\frac{\partial \ell^*}{\partial p_C} = C^*\left(\frac{\partial \ell^*}{\partial I}\right) + \lambda^*[D^2U_{\ell C} + p_C GU_{AA} - DGU_{CA}] / |H| \tag{1.44}$$

The sign of the above expression is ambiguous given that it relies partially on the sign of the income effect. In addition, the first and third terms in the expression in brackets (the substitution effect) cannot be determined. Whether the agent consumes more or less leisure after an increase in the price of the numeraire depends on the sign of the difference between the income and substitution terms.

Next, consider the effects of a change in the main parameter of interest, namely an increase in the full price of alcohol:

$$\frac{\partial \ell^*}{\partial p_A} = A^*\left(\frac{\partial \ell^*}{\partial I}\right) + \lambda^*[DGU_{CC} - Dp_C U_{\ell C} - p_C GU_{CA} + E(p_C)^2] / |H| \tag{1.45}$$

Although the consumption of alcohol makes the agent more susceptible to victimization (recall the victim's budget constraint), even equation (1.45) is not *necessarily* negative! Again, the sign of the above expression depends partially on the income effect. Now consider the bracketed expression in the second term. Only the first and fourth terms of this expression can be signed unambiguously (again assuming diminishing marginal utility of consumption). The sign of the second and third terms of the expression can be either positive or negative depending on the sign of the cross marginal utilities.

Finally, consider the effects of a change in the marginal probability of victimization:

$$\frac{\partial \ell^*}{\partial \xi_A^*} = \lambda^* r \ell^* [DGU_{CC} - Dp_C U_{\ell C} - p_C GU_{CA} + E(p_C)^2] / |H| \qquad (1.46)$$

The sign of the above expression depends on the sign of the term in the squared brackets, but this is identical to the bracketed expression in equation (1.45). As such, the sign of equation (1.46) cannot be determined unambiguously.

1.6 Conclusion

This chapter developed a static theoretical model of alcohol consumption and the incidence of criminal activity in the tradition of Becker's (1968) rational offender framework. It was shown that changes in the full price of alcohol will lead to ambiguous changes in the time allocated by offenders to criminal income-generating activities, and to the time allocated by potential victims to activities that may expose them to violence. In addition, it was shown that the comparative statics effects with respect to the other model parameters could not be signed with certainty. The implication of this is that increasing the cost of consuming alcohol will not necessarily reduce crime even if alcohol consumption is associated with the propensity to commit violent acts.

While the model developed here motivates the necessity of empirical methods to ascertain the effectiveness of alcohol control policies in mitigating the incidence of crime, there are several issues that it does not address. For instance, it does not explicitly consider the effects that alcohol dependency may have on the agent's equilibrium time allocation decision between legitimate and illegitimate income-generating activities. Alcohol addiction may degrade the skills necessary to work effectively in legal enterprises and cause a substitution into illegitimate ones. Of course, addicts may also substitute illegal income-generating activities for legal ones for the purpose of financing their consumption decisions. As such, a dynamic framework might be more suited to examining the effects of alcohol consumption on the agents' time allocation decisions and earnings over their life-cycle. Second, the model does not explicitly consider the possibility of strategic interaction between the offender and the victim. In addition to the effects of alcohol consumption, an offender's (victim's) time allocation decision will be influenced by their conjecture of the behavioral responses of potential victims (offenders) to their own choice of actions. While such a model is likely to produce similar ambiguities, future research could employ game-theoretic models to more fully explore the implications of this interdependence on the effects of alcohol control policies.

CHAPTER 2

ALCOHOL AND THE INCIDENCE OF CRIME: AN OVERVIEW OF THE EMPIRICAL LITERATURE AND AN ALTERNATIVE APPROACH

2.1 Introduction[16]

Questions regarding the relationship between alcohol and criminal behavior must be answered empirically since theory cannot give us unambiguous predictions. There is a substantial empirical literature on this subject, of course, so perhaps the relevant questions have already been answered. This critical review suggests that they have not been and that an alternative empirical approach may be able to cast some light on the subject. Section 2.2 reviews the large empirical literature on the alcohol-crime relationship that has been produced by criminologists, sociologists, and other disciplines. Given the "economic approach" taken here, the relatively sparse academic research done by economists on the alcohol-crime relationship is discussed in section 2.3. Section 2.4 discusses some related empirical literatures that may be informative (e.g., on the determinants of violent crime other than alcohol, and on the determinants of alcohol-related traffic fatalities). Finally, section 2.5 comments on concerns with previous alcohol-and-crime studies and concludes.

2.2 Empirical Studies on the Relationship between Alcohol Consumption and Crime

The strongest circumstantial evidence for the positive link between alcohol consumption and the subsequent expression of violent criminal acts comes primarily from *observational* (as opposed to *experimental*) studies. These observational studies can be broadly classified into two categories based upon their chosen unit of observation: *individual-specific* and *aggregate-level*

[16]Parts of this chapter also appear in Zimmerman (2000).

studies (Pernanen 1981). Aggregate level studies are of particular interest for the purposes of this report in that they allow for the observation of the effects of alcohol control policies (or other exogenous shifts in the supply of alcoholic beverages) on the incidence of criminal and/or violent behavior.

2.2.1 Individual-Level Studies

The objective of individual-specific studies is to determine if and when alcohol was consumed by a perpetrator and/or victim of crime prior to its commission and, (in some cases) the degree to which alcohol can be inferred as a causal source of criminal behavior. Such studies typically generate their data through personal interviews with incarcerated individuals, analyzing blood/urine alcohol levels of arrestees or emergency room admissions, or by examining police arrest records (in the rare cases where alcohol involvement is recorded).

Using a sample of over three-hundred prisoners in North Carolina imprisoned for serious assaultive crimes, Mayfield (1976) estimated that thirty-six percent were problem drinkers. Wolfgang's (1975) seminal study found that alcohol was consumed by either the victim or offender in two-thirds of the nearly six hundred homicides examined in Philadelphia. According to Johnson, et al. (1978), seventy-two percent of rapes between 1966 and 1975 in Winnipeg involved alcohol consumption by offenders and/or victims. Lindquist's (1986) study of homicides in Sweden between 1970 and 1981 found that two-thirds of the perpetrators and nearly one-half of the victims were intoxicated at the time of the offense. Goodman, et al's. (1986) study of several thousand homicide victims in Los Angeles over a nine year period found that alcohol was consumed by the victim in forty-six percent of the cases. In addition, it was found that thirty percent of these homicide victims had blood alcohol levels in excess of the legal minimum for intoxication. Finally, surveys of incarcerated inmates conducted by the Bureau of Justice

Statistics (1999) have shown that over one-third of violent offenders had been drinking just prior to their commission of the crime and more than half were reported to have been drinking heavily.[17]

Further evidence of a link is provided by studies that have found a strong tendency for incarcerated criminals to have developed problems associated with the long-term (chronic) abuse of alcohol products (e.g. alcoholism). For instance, Banay (1942) found a statistically significant proportion of alcoholics among three thousand males imprisoned between 1938 and 1940 and found that those incarcerated for violent criminal offenses, such as assault or rape, where the most likely to have been consuming alcohol at the time of the offense.[18]

2.2.2 Aggregate-Level Studies

Aggregate level studies explore the alcohol-crime relationship by abstracting away from the individual unit of observation and focusing on the relationship between broad proxies of alcohol consumption (e.g., the number of retail liquor outlets in a city) and criminal activity (e.g. the total number of violent offenses) within a given time period or across different periods using

[17]Studies of incarcerated populations may be of little practical use from a policy perspective since they do not take into account the effects of alcohol consumption on the portion of the population that is *not* incarcerated. As such, one is left with no source of variation in the potential outcomes of alcohol consumption (all observations committed violence after consuming alcohol). Since individuals are, of course, not randomly assigned into incarceration, one must take into account the potentially serious selection bias. Finding that a high proportion of incarcerated individuals consumed alcohol before commission of their crimes does not necessarily mean that alcohol played a *causal* role. For instance, it may be the case that the incapacitative effects brought on by high levels of alcohol consumption may make individuals easier to catch if they "bungle" their crimes at a higher rate than non-drinking offenders. In addition, to the extent that society views deviant behavior as more acceptable if alcohol was consumed in the process, inmates may over-exaggerate the role alcohol played in the commission of their crime so as to avoid harsher sentences.

[18]The development of chronic drinking problems (i.e., alcoholism) may have special implications for the theoretical development of a relationship between alcohol consumption and violent crime. For instance, chronic alcohol abusers may involve themselves in certain income-generating crimes (such as robbery) in order to finance their addiction but consuming alcohol does not 'cause" the individual to engage in criminal activity. However, research by Collins and Schlenger (1988) has provided evidence that it is the acute rather than the chronic use of alcohol (e.g., alcoholism) that has greater explanatory power in predicting the probability that an individual will engage in violent crime.

three different statistical techniques: 1) time series analysis [e.g., Lenke (1990), Lester (1992), Ensor and Godfrey (1993)], 2) cross-section analysis [e.g., Dull and Giacopassi (1987), Lester (1993, 1995), Parker (1995), Parker and Rebhun (1995)] and 3) cross-section time series pooling techniques (Cook and Moore 1993a; Parker 1993; Parker and Rebhun 1995).

Many of the earliest aggregate studies on the alcohol-violent crime relationship employ time-series data. Such studies examine the simple covariance of crime rates and levels of per-capita alcohol consumption (or some proxy thereof) over some specified time interval for a particular unit of observation. Perhaps the most widely known study of this kind is that of Lenke (1975), who found a positive and statistically significant correlation between the two over the period 1960-1973 for various Scandinavian countries. Lester's (1992) study, which employs annual time series data over the years 1966 through 1985 and controls for the occurrence of divorce and the national rate of unemployment, finds a positive statistical relationship between per-capita alcohol consumption and the rate of homicide in Australia.

Makela (1980) observed a reduction in rates of interpersonal violence following a strike by workers in the state-monopolized liquor stores in Finland while Makela, et al. (1981) found an increase after a lifting of restrictions on beer sales in the same country in 1969. Olsson and Wikstrom (1982) found that following an experiment mandated by the Swedish government that closed state-owned retail liquor stores on Saturdays for several months, incidents of outdoor assault fell for all days of the week (most significantly by twenty-five percent on Saturdays). Haugue (1988) compared rates of interpersonal violence during the last five weeks out of a nine-week strike by workers in the Norwegian state-owned liquor monopoly (by which time all sales of liquor had ended) with the same time period in the preceding year. Haugue accounts for a possible downward trend in rates of violence over the period but still finds that reported incidents of interpersonal violence fell comparatively by fifteen percent during the period under observation.

Because the number of observations are limited in time series studies, the number of policy and control variables that can be considered is severely constrained. Cross-section studies may be more attractive from a policy perspective if a large number of observations can be gathered. Parker and Rebhun (1995) report on one study of homicide rates using data from 256 U.S. cities, for instance, and Parker (1995) uses a 153 observation sample consisting of the 50 U.S. states and the District of Columbia for a three year period. Parker's (1995) study is particularly ambitious and suggestive, as he attempted to examine the effects of alcohol consumption on five dependent variables representing different categories of homicides, within the context of the theories of violence discussed above. His findings are quite mixed, however, perhaps because of collinearity problems. His regressions include alcohol consumption as well as other variables that are determinants of consumption (state monopolization of the wholesale and retail sales of alcoholic beverages, and the number of on-site licensed outlets), for instance, thus creating potential problems for interpretation of all of these coefficients. To determine the effect of alcohol controls on violence, it may be necessary to estimate two equations (one for alcohol consumption as a function of controls and another for violence as a function of consumption) or a single reduced form equation (violence as a function of controls without consumption), as suggested in the discussion of the DUI literature below.

Cross-section studies can also suffer from limitations on the number of variables if the sample is small, and from measurement problems and missing variable biases which confound the reliability and interpretation of coefficients. Cross-section time-series pooling of data offers the opportunity to alleviate at least some of these problems by expanding sample size and controlling for fixed effects, and such techniques are beginning to be applied in the alcohol and violence literature. Cook and Moore (1993a) use 1979-1987 state data in a model similar in form (but not detail) to DUI models discussed below to examine the effect of beer excise taxes on murder, rape, assault, and robbery, in both two equation models with fixed-effects (regressions with violent crimes as a function of beer consumption, and an equation with beer consumption as a function of beer taxes), and reduced-form fixed-effects models. They find significant positive relationships between beer consumption and all measures of violent crime except murder, a significant and negative correlation between beer taxes and beer consumption, and in the reduced-form model, significant negative impacts of beer taxes on rape and robbery.

Cook and Moore's (1993a) findings are intriguing because they suggest that alcohol policy can influence the level of violent crime, but they are troubling because they suffer from significant deficiencies. Parker (1993) criticizes Cook and Moore (1993a), for instance, because they do not include variables to control for poverty, race, and other factors relevant in the deprivation or routine-activities theories of violence. Therefore, he pools data from the fifty states and the District of Columbia from 1976 through 1983 to look at the impact of changes in state drinking age laws on homicide rates (represented by a dummy variable for the year the drinking age was raised to 21), controlling for alcohol consumption, as well as socio-economic variables like infant mortality (a poverty index), racial composition, state population, and an index of inequality. Parker and Rebhun (1995) also apply the model to six different homicide rates (classified according to the age of the offender and whether there was a known relationship between the offender and victim). Beer consumption is generally significant, as in Cook and Moore (1993a) except for one of their dependent variables, offenders ages between 21 and 24 where the offender and victim knew each other, while the change in the drinking age is significant in this regression but not in the others [note that since drinking age is one determinant of consumption, using both variables in one regression can be problematic, so either two equations or a reduced form without consumption, as in Cook and Moore (1993a) is more appropriate]. The socio-economic variables listed above are also generally significant across models. These additions to Cook and Moore (1993a) demonstrate that both other policy variables (e.g., drinking age) and non-policy socio-economic controls should be considered in order to see if the results with taxes are robust (e.g., the significant coefficient may be biased due to missing variables), and to see if other alcohol control variables are also effective (or more effective).

The extent of this possible bias is indicated in a study of DUI by Mast, et al. (1999) (the empirical DUI literature is discussed below in more detail). They estimate three different fixed-effects beer quantity equations for states using 1984-1992 data. The first controlled for taxes as in Cook and Moore (1993a) and drinking age as in Parker and Rebhun (1995), along with various socio-economic determinants of beer consumption. The beer tax coefficient was large, negative, and highly significant (the drinking age variable was also significant). The second regression added four variables that have been found to be determinants of beer price and quantity in beer market studies, and the beer tax coefficient was reduced by half while the coefficient on drinking age actually increased by about 25 percent. The third added controls for attitudes toward drinking (e.g., portions of the population of various religions) and the beer tax coefficient was no longer significant as it was reduced to near zero, suggesting that beer taxes may be endogenous as some of the same attitudes that lead to reductions (or increases) in alcohol consumption lead to political support for high (or low) beer taxes (the drinking age variable remained significant and of roughly the same magnitude as in the second regression). Failure to control for such factors make the Cook and Moore (1993a) results suspect. The Cook and Moore (1993a), Parker (1993) and Parker and Rebhun (1995) studies can also be criticized for not including direct deterrence variables. Furthermore, Cook and Moore (1993a) did not control for non-tax determinants of consumption, some of which may be relevant in the context of the deprivation and routine activities theory of violence. Therefore, this study builds on the foundation laid by these studies in an effort to gain a clearer understanding of the alcohol-policy/alcohol-consumption/violent-crime relationships.

2.3 The Effects of Alcohol Control Policies on the Incidence of Criminal Acts: Evidence from the Economics Literature

The first published study to examine the alcohol-crime relationship conducted by economists is Cook and Moore (1993a), but it is not the only economic study of this subject. Chaloupka and Saffer (1992), as reviewed in Grossman and Markowitz (1999), also examine the direct effects of real beer excise taxes on the violent crimes examined in Cook and Moore (1993a) using a panel of state-level data over the years 1975 to 1990. However, the authors also include controls for crime deterrent variables and drug consumption. The authors find a relatively low elasticity, concluding that a doubling of the beer excise taxes would reduce rapes by 3.0 percent and robberies by 4.7 percent. In addition, the authors also consider the effects of higher excise taxes on the incidence of the property crimes of burglary, larceny, and motor vehicle theft. The authors' results predict that a doubling of state excise taxes will reduce both burglaries and larcenies by 1.3 percent.

Unlike Cook and Moore (1993a) and Chaloupka and Saffer (1992), more recent studies have employed the use of individual-level survey data to examine the effects of alcohol prices and/or control policies on specific acts of interpersonal violence. These studies include the incidence of child abuse (Markowitz and Grossman 1998a and b, 2000), spousal abuse

(Markowitz 1999), violence on college campuses (Markowitz and Grossman 1999), and physical fights and weapons carrying by teens (Markowitz 2000-2000a and b).

Markowitz and Grossman (1998a and b) examine the effects of several alcohol control policies on two dichotomous measures of violence derived from the Conflict Tactic Scale (CTS) as applied to data from the 1976 Physical Violence in American Families Survey. The first measure is an indicator of whether a parent committed an act that could cause serious injury to a child and is deemed the severe violence indicator. The second measure employed indicates whether a parent harmed a child in any manner besides slapping or spanking. This is termed the overall violence indicator. In addition, the authors employ a measure of the frequency of violence against children: the log of the number of times in the past year a respondent committed an act within the overall violence scale. With respect to the overall violence specification, the authors find a stable beer excise tax elasticity of approximately —0.12 percent. In this regard, however, it should be noted that in the empirical literature on DUI discussed below, studies using data from periods prior to 1988 have found significant beer tax impacts on traffic fatalities, but studies using data including periods since 1988 generally are not [e.g., see Mast, et al. (1999); Young and Likens (2000)]. Markowitz and Grossman (1998b) also find that residence in a dry county or in a state that prohibits liquor store windows displays is predicted to lower the probability of committing violence against a child. Grocery store sales of alcohol, per-capita retail outlets, and various advertising restrictions are predicted to have no effect on overall violence against children.

Markowitz and Grossman's (1998a and b) estimate tax elasticities with respect to severe violence are relatively larger in magnitude but less robust to model specification. In controlling for only the real level of beer excise taxes, the authors estimate a tax elasticity of approximately

—0.29. However, when other alcohol control policies are controlled for the estimated tax elasticity falls to —0.16.[19] Greater alcohol availability as measured by the number of retail outlets that sell alcohol per-capita is predicted to increase the incidence of severe violence against children. However, dry county residence, grocery store sales, and advertising restrictions are never found to be statistically significant determinants of violence against children either individually or jointly.

Finally, a one-percent increase in the beer excise tax is predicted by Markowitz and Grossman (1998a and b) to lower the number of times a parent commits an act of overall violence against a child in the past year by approximately 0.093 percent. None of the availability measures are found to be statistically significant. Of the advertising restriction variables, prohibition of billboards advertisements and window displays are statistically significant. However, the latter variable takes an unanticipated positive sign.

Markowitz and Grossman (2000) extend their previous analysis (Markowitz and Grossman 1998a and b) by extending their data set to include the 1985 version of the survey. This allows for the construction of a panel data set and the use of fixed effects specifications to control for the influences of unobserved state sentiments that determine both the incidence of child violence and the presence of alcohol regulations in addition to conducting a cross-sectional analysis for each year. Unlike their previous study, the authors only employ the use of the severe violence indicator as the dependent measure. In addition, the authors examine the incidence of violence committed by male and female parents separately. With respect to the 1976 sample of female parents, the authors estimate that a one-percent increase in the beer tax will decrease the

[19]Similar but even greater sensitivity of the beer tax coefficient to model specification is also apparent in the DUI study where it is examined (Mast, et al. 1999).

probability of severe violence by approximately 0.33 percent on average. As expected, more retail outlets per-capita and higher percentages of the state population that reside in dry counties are predicted to increase and decrease the probability of severe child abuse respectively. Neither grocery store sales nor advertising restrictions have an effect on the incidence of severe child abuse. For the 1985 female cross-section, the authors estimate a tax elasticity of —0.13. However, the 1985 female cross-section is found to be sensitive to the price of cocaine whereas the 1976 sample is not. In addition, the availability and advertising restriction controls are never found to be statistically significant in the 1985 female cross-section.

For male parents, the 1976 cross-section results indicate that the beer tax elasticity is sensitive to the inclusion of alcohol control measures. When only the beer tax measure is included, the authors find a negative and statistically significant tax elasticity. Controlling for other alcohol control policies and illicit drug consumption greatly reduces the magnitude of the estimated tax effect and the estimate actually turns statistically insignificant, much as in recent DUI studies (Mast, et al. 1999). For the 1985 male cross-section, the estimated coefficients on the beer tax measure are always found to be *positively* correlated with the probability of committing severe child abuse and statistically significant. In addition, none of the illicit drug, availability, or advertising controls are ever found to be statistically significant in either the 1976 or 1985 cross-sections.

The results of likelihood ratio tests indicated that pooling of the separate data sets was appropriate only for the female samples, and in particular only the female models which include the illicit drug consumption and alcohol availability measures. Without including state fixed-effects, the estimated beer tax elasticity in the pooled female sample takes a value of approximately —0.21 when also controlling for drug consumption and alcohol availability measures. Adding a vector of state dummies to the set of independent variables turns the estimated elasticity on the beer tax coefficient statistically insignificant. The authors do note that as a set the state dummies are not statistically significant. As such, they argue that the state dummies "do not capture any unobserved state sentiment towards drinking or violence; rather, they act as irrelevant included variables that are correlated with the beer tax" (Markowitz and Grossman 2000, p. 280). As such, the apparent multicollinearity between the beer excise tax and the state dummies drives the estimated tax elasticity to zero in the fixed-effects specification.

Using data from the 1985-1987 National Family Violence Survey, Markowitz (1999) seeks to determine the effectiveness of alcohol control policies on mitigating the incidence of spousal abuse. The author constructs two indicator dependent measures derived from the Conflict Tactic Scale. The first is an indicator of whether males were violent towards their wives (termed wife abuse). The second is a similar variable with respect to female abuse of their husbands (termed husband abuse). Either victim or offender responses are counted in the construction of the dependent variable. The alcohol beverage price employed by the author is a composite measure of prices for beer, liquor, and wine derived from the Inter-City Cost of Living Index of the American Chamber of Commerce Researchers Association (ACCRA). Availability of alcohol is proxied through the number of retail outlets per-capita and the proportion of the state population residing in dry counties.

Markowitz estimates several reduced-form violence equations for wife and husband abuse using linear probability models. For the 1985 cross section of wife abuse, the estimated coefficient on the composite price measure is always negative and statistically significant. This estimate is stable across model specification, but takes an implausibly high elasticity (ranging from 3.1 to 3.5). Neither of the availability measures is statistically significant. In the 1985 cross-section of husband abuse, the composite price measure is never statistically significant. Retail availability is found to be a statistically significant determinant of husband abuse, but takes an unanticipated negative sign.

Finally, the author uses all three years of the survey to take advantage of the panel properties of the data. First consider the estimates of wife abuse. Without the inclusion of individual dummies, the estimation results indicate that the composite alcohol price measure is negative and statistically significant only when the availability measures are excluded from the reduced-form specification. In addition, the estimates on the percent of the state population residing in dry counties and the number of retail outlets per-capita are statistically insignificant when included in the reduced-form specification. When removing individual fixed-effects, the composite price measure is always negative and statistically significant across model specifications. However, the availability measures are again never found to be statistically significant.

For the incidence of husband abuse, the estimation results show that the composite price of alcohol has no statistically significant effect in any specification. However, increases in a state's population residing in dry counties are predicted to decrease the incidence of husband abuse in two specifications. When removing individual fixed-effects, the price of alcohol becomes a negative and statistically significant determinant of husband abuse. Neither of the availability measures is statistically significant in any model specification.

Grossman and Markowitz (1999) examine the effects of alcohol beverage prices on the incidence of violence on American college campuses. Using data from the 1989-1991 Core Alcohol Drug Surveys of College Students, the authors construct the following indicators of violence: getting in trouble with the police, residence hall, or other college authorities, damaging property or pulling a fire alarm, getting into an argument or a fight, and taking advantage of another person sexually or having been taken advantage of sexually. The authors again construct reduced-form violence equations for each of these measures. In addition the authors estimate a structural crime equation where alcohol consumption is entered as a determinant into the violence equation. Since alcohol is an endogenous right-hand side variable, this equation is estimated via two-stage least squares.

The price of alcohol employed is the real price of beer in the state the college student attends school. These data are derived from the Inter-City Cost of Living Index published by ACCRA. The only other alcohol control measure included in the analysis is the per-capita number of retail outlets, although the authors do attempt to control for the consumption of marijuana and cocaine.

For each dependent measure the authors estimate three different specifications based upon inclusion of per-capita income, per-capita retail outlets, and controls for religious affiliation. The estimated coefficient on the beer price coefficient is negative and statistically significant in fourteen of the fifteen specifications. However, the authors find that the beer price coefficients fall substantially when state-specific controls for drinking sentiment (in particular the religious affiliation variables) are included in the reduced-form specification. The estimated coefficient on the number of retail outlets per-capita is always positive (except when trouble with the police, etc. is the dependent measure and religious affiliation is not controlled for) and statistically significant (except when trouble with the police, etc. is used as the dependent measure). Finally, the authors find that illicit drug consumption has no discernable effect on any of the violence measures.

With respect to the estimation of the structural violence equation, the authors employ the average number of drinks consumed by the student in a week. Whether using beer price or beer price along with marijuana decriminalization and per-capita outlets as instruments, the authors find that alcohol consumption having a positive and statistically significant impact across all models. However, the consistency of the ordinary least squares estimates is accepted in eight of the twelve specifications, and not surprisingly the authors find little difference between the ordinary least squares and two-stage least squares estimates of alcohol consumption for each of the dependent measures.

Markowitz (2000b) seeks to determine the role alcohol consumption plays in determining the incidence of physical fights and weapon carrying by teenagers. Using data from the 1991, 1993, and 1995 National School-Based Youth Risk Behavior Surveys, the authors construct dichotomous dependent measures of whether the high school student had been involved in a physical fight in the past year and whether he/she carried a gun in the past thirty days. In addition, a dichotomous measure of whether the respondent carried a weapon besides a gun (such as a knife) was also employed as a dependent variable.

Structural violence equations are estimated where the measures of alcohol consumption are the number of days in the past thirty days on which the respondent had at least one drink and the number of days in the past thirty days on which the respondent had five or more drinks of alcohol in a row (a measure of binge drinking). Since alcohol consumption and participation in violence or weapon carrying may be determined by latent risk preferences, the author also includes controls for seat belt usage, whether the respondent considered committing suicide in the past year, and the number of sports teams on which the respondent plays either inside or outside of school. The structural equations are estimated using both linear probability models (ordinary least squares) and a two-stage estimation procedure (since consumption is an endogenous variable). The real state-level excise tax on beer, an indicator of marijuana decriminalization, and cocaine price are used as instruments to predict alcohol consumption in the first stage regressions.

The ordinary least squares estimates show that binge behavior is a positive and statistically significant determinant of physical fighting and carrying a gun or other type of weapon. For physical fighting, the two-stage least squares estimate of alcohol consumption is ten times larger than its ordinary least squares counterpart. For the probability of carrying a gun the two-stage estimate of consumption is negative and statistically insignificant. For the probability of carrying other weapon types, the two-stage estimate of consumption becomes negative and statistically significant. The author argues that the unexpected results in the gun and other weapon carrying specifications may be due to unobservable regional effects and attempts to deal with them by adding dummy variables for the region of the country the student respondent resides. However, negative estimates of binge drinking are still found in the second-stage estimates of both gun and other weapon carrying. These results lead the author to conclude that there is simply no (positive) causal relationship between binge drinking and the tendency to carry weapons by teenagers. The ordinary and two-stage estimates of drinking are positively correlated with the probability of being in a physical fight. The ordinary least squares estimates also indicate that higher teenage levels of drinking are predicted to increase the probability of carrying a gun or some other weapon. However, the two-stage estimates for gun carrying are found to be negative and statistically insignificant, and the estimates for other weapons carrying to be negative and statistically significant. Again, the author attempts to control for the potential biasing effects of regional-specific factors by adding dummy variables for the students' region of residence. However, the two-stage estimates of both gun and other weapon carrying remain negative and statistically significant.

Markowitz (2000a) examines the incidence of robbery, assault, and sexual assault (females only) and their relationship to alcohol consumption based on data from the 1989 and 1992 International Victimization Surveys. This data set is made up of nearly fifty-thousand potential victim respondents from sixteen different countries. The respondents were asked whether they were a victim of robbery, assault, or sexual assault in the past year, or if any person(s) attempted to commit one of these offenses against them.

The author uses two separate measures for the price of alcoholic beverages. The first measure is the price of an ounce of "pure" alcohol expressed in real U.S. dollars. The second measure is the tax on one ounce of pure alcohol expressed in real U.S. dollars.[20] Other alcohol control variables included are the legal blood alcohol level indicative of impaired or drunk driving, a dichotomous indicator for television or radio advertising restrictions on alcohol, and measures of the minimum legal drinking age for purchase of beer or wine.

The probability of respondent victimization is determined through probit estimation of reduced-form violence equations. First consider the robbery specification. The estimation results indicate that the estimated coefficient on the constructed price measure is negative and statistically significant only in specifications that do not remove country fixed-effects. Removing country fixed-effects turns the estimated coefficient positive and statistically insignificant.[21]

[20] Since actual data on alcohol prices and taxes are not available for all countries, the author employs proxy measures derived from household expenditures on alcohol, total tax revenues, and consumption data.

[21] It should be noted that country dummies can only be used for those countries that are in both years of the survey. This reduces the sample size to eight included countries. In addition, only blood alcohol levels are included as an additional alcohol control measure.

Similar results are found for the tax variable. Without country dummies the estimated tax coefficient is negative and statistically significant. With country fixed-effects removed the estimated tax coefficient remains negative but turns statistically insignificant. The author argues that both results are due to the high degree of collinearity between the alcohol price proxies and the country dummy variables.

Higher legal blood alcohol levels and restrictions on advertising are estimated to raise the incidence of robbery, while advertising restrictions and higher legal minimum drinking ages are predicted to lower it. However, the latter results are found to be statistically significant only when the tax is used as the alcohol price proxy. When controlling for country fixed-effects, the estimated coefficients on the blood alcohol level turn statistically insignificant.

With respect to the incidence of assault, without controlling for country fixed-effects the estimated coefficients on the price and tax proxies are negative and statistically significant. Adding country dummies turns the estimated coefficients on the constructed price variable and tax measure statistically insignificant. In the full country sample using the constructed price measure, blood alcohol levels, advertising restrictions minimum legal drinking ages all take an unanticipated positive sign with the later two variables being statistically significant.

Markowitz (2000a) also examines the direct effect of the alcohol control polices on each of the dependent measures by estimating reduced-form violence equations. Negative and statistically significant estimates are again obtained for the beer tax variable in the physical fighting specification. A one percent increase in the level of the beer tax is predicted to lower the probability of physical fighting by approximately 0.02 percent. For carrying a gun, beer taxes are predicted to have no statistically discernable effect. Finally, beer taxes are found to be positively correlated with the probability of carrying other types of weapons. In addition, the inclusion of regional dummies does not change the signs or estimated effects of any of the reduced form models.

2.4 Related Empirical Literature that Can Inform an Alcohol-Violence Study

Two bodies of literature other than the alcohol-violence studies discussed above are particularly pertinent to this research. First, studies that investigate determinants of violent crime other than alcohol provide the basic model to which consideration of the impact of alcohol consumption will be added. Second, methodology employed in the research on alcohol control policies as means for reducing traffic fatalities will be adapted to the study of alcohol and violence. Therefore, we briefly discuss each before indicating how this research differs from the existing alcohol-violence literature.

2.4.1. Determinants of Violent Crime

As noted in the previous chapter, there are several theories about the nature of violence (e.g., deterrence theory, deprivation theory, routine activities theory). A large empirical literature also explores various determinants of violent crime, including many experimental programs conducted by police in local jurisdictions. The empirical contributions to the violent crime literature that are of direct relevance to this study, however, are those that use aggregate data for geographic units such as police beats, local jurisdictions, counties or states. They tend to focus on *deterrence* and/or *deprivation theories*, in part because these theories provide testable hypotheses that can be considered with such data. The models that will be developed in this proposed project will build on this literature, but the addition of alcohol control policy issues allows consideration of some hypotheses stemming from *routine activities theory* as well. While early studies of this type generally used single equation models [e.g., Ehrlich (1973); Sjoquist (1973)], the standard empirical model evolved to consist of a set of simultaneous equations with the crime rate(s), the probability(ies) of arrest, and a measure(s) of deterrence resources (e.g., police employment or budget) as dependent variables [for reviews, see Cameron (1988) and Benson, et al. (1994)]. Such models are predicated on the idea that the crime rate affects the resources available to the criminal justice system (i.e., voters' willingness to pay for criminal justice resources is a function of the level of crime as well as other factors), which in turn affects the crime rate via the deterrence effects of the probability of arrest and conviction, and severity of punishment. The reason for using multiple equation estimation techniques is to avoid simultaneity bias.[22] Furthermore, when consideration of alcohol consumption and policy is added, additional simultaneity issues become relevant: do factors which influence crime also influence alcohol consumption (e.g., as implied by the *common cause* hypothesis)?

Many studies of crime using aggregate data have analyzed the FBI Index I crimes rates in total, combining property and violent crimes in the analysis, but a few have estimated violent crime models. The generic cross-section model using state or local observations, is:

$$V_i = f(X_i, P_i, C_i, S_i),$$

where V_i is the violent crime rate in jurisdiction i. X_i is a vector containing variables that represent legal income opportunities (income, unemployment, poverty, education levels) and other socio-demographic factors (racial and age distributions, urbanization) that might affect the

[22] Some empirical and theoretical findings suggest that this is not appropriate for a model of violent crime for two reasons. First, empirical studies suggest that more property crime significantly increases the demand for police resources, but violent crime does not (Avio and Clark 1976; Sollars, et al. 1994; Benson, et al. 1992). Second, a common finding is that a higher probability of arrest has a deterrent value but that the marginal increments in police resources do not produce a higher probability of arrest (Cameron 1988; Benson, et al. 1994). This result is not surprising given that the property and violent crimes studied in these models account for only a portion of total police activity. Increases in police resources need not increase deterrence of any specific crime because those resources can be allocated to other activities (Benson, et al. 1994, 1998). Thus, two key links required for a simultaneous model of violent crime are weak or non-existent, and some studies that have tested for simultaneity reject it (Layson 1985, Trumbull 1989). However, in a recent study, Doyle, et al. (1999) test for endogeneity in a crime model and find it, so similar tests are employed below.

opportunity cost of punishment and that control for implications of *deprivation theory*. P_i is the probability of arrest or a set of variables that are expected to determine that probability, C_i is the probability of conviction given arrest, and S_i is the severity of punishment. This regression is often estimated in a simultaneous system with separate regressions explaining P_i (as a function of police resources and other factors, and perhaps another equation explaining police resources as a function of crime rates and other factors), as noted above. In general, empirical findings strongly suggest that higher legal earnings discourage illegal activities, and that this type of "positive deterrence" is stronger than the "negative deterrence" provided by the criminal justice system (Grogger 1991; Ehrlich 1996; Cornwell and Trumbull 1994; Doyle, et al. 1999). Nonetheless, while critics question whether the deterrence hypothesis applies to violent crimes [e.g., see Paternoster and Iovanni (1986)] many violent-crime studies indicate that offenders do respond to deterrence variables [recent studies with such results include Sloan, et al. (1994b), Horney and Marshall (1992), Sollars, et al. (1994), Lott and Mustard (1997), and Lott (1998)]. Critics also point to measurement problems that presumably bias such results, however.

Critics note that the reported deterrent effect on the offense rate, the dependent variable, of the probability of arrest, proxied by the arrest/offense ratio, could simply be a product of spurious correlations resulting from measurement error. In this light, Brier and Fienberg's (1980, p. 188) influential review concluded that there is "no reliable empirical support in the existing econometrics literature either for or against the deterrence hypothesis." This observation, reinforced by Cameron (1988) and others, was addressed by Levitt (1998a) who developed a model for determining the extent of measurement error. Tests of his model for seven major Uniform Crime Report crime categories conclude that measurement error biases are likely to be relevant in only one of them: auto theft. Thus, Levitt's (1998a) results suggest that a study focusing on the violent crime categories (homicide, sexual offenses, assault, robbery) will not suffer from such problems. In this context, a fixed-effects model, which is designed to deal with unobserved heterogeneity among observations in time-series cross-section pools of data, is suggested by Levitt's (1998a) findings, and has begun to appear in the crime literature (Cornwell and Trumbull 1994; Lott and Mustard 1997; Lott 1998; Levitt 1998a; and Benson, et al. 1998). After all, such unobserved heterogeneity is characteristic of jurisdictions since communities with identical socio-economic and demographic characteristics have very different crime rates (Glaeser, et al. 1996). And importantly, if crime reporting behavior in a jurisdiction is relatively constant over time, as suggested by Bound and Krueger (1991) in another context, the use of fixed effects models also attenuates another measurement error problem that has plagued this literature: the widely recognized inaccuracies in the number of offenses known to the police arising due to the reporting behavior of victims and police departments.

Empirical studies also consistently find that socio-economic conditions help explain crime rates. Measures of income levels, unemployment rates, age, and race are often important explanatory variables. Thus, support for either *deprivation theory* or the opportunity cost hypotheses posed by economists (empirically it is very difficult to distinguish between these two hypotheses since both predict similar signs for the various coefficients estimated for socio-economic data) is also produced. Indeed, one of the clearest implications of these studies is that a strong economy with low unemployment and good legitimate income earning opportunities reduces crime.

2.4. 2. Determinants of Drunk Driving Fatalities

Economists have produced a growing number of statistical studies that explore the relative effectiveness of policies intended to reduce traffic fatalities using aggregate data [there is also a substantial literature using individual data from surveys but that literature is not directly applicable to this study], generally by estimating the deterrence effects of laws and law enforcement activities aimed at drunk drivers, age restrictions on alcohol consumption, and alcohol taxation. Several studies emphasize findings first reported in Cook (1981a), that higher beer taxes significantly reduce vehicle mortality rates (e.g., Saffer and Grossman, 1987a, 1987b; Evans, et al. 1991; Chaloupka, et al. 1993; Mullahay and Sindelar 1994; Ruhm 1995a, 1995b). Indeed, the U. S. Department of Health and Human Services (1988, p. 18) draws upon this empirical literature and reports that "research evidence shows that an increase in the excise tax could have the largest long-term effect on alcohol-impaired driving of all policy and program options available," and similar conclusions are highlighted in academic reviews (Phelps 1988; Chaloupka 1993; Grossman, et al. 1993). Furthermore, until recently, this literature has generally suggested that laws and law enforcement efforts intended to directly deter driving under the influence (DUI) are not particularly effective in reducing traffic fatalities, at least relative to taxes. Raising the legal drinking age from 18 to 21 is generally shown to significantly reduce fatalities (all states have now done so), and virtually all studies also find that fatalities are reduced by some laws making arrests more likely or punishment more severe, but the relevant laws vary depending on variable selection, model specification, and the time period studied [see Benson, et al. (1999) for a review].

This statistical literature's implications for the relative efficacy of taxation and direct deterrence of DUI through law enforcement and sanctions is surprising, however. For one thing, the criminology literature suggests that direct deterrence can be very effective against DUI. In his extensive review, Sherman (1997, p. 18) noted that "The evidence on drunk driving, in contrast [to the literature on illicit drugs], is one of the great success stories of world policing... [T]he sheer numbers of consistent results from quasi-experimental evaluations of proactive drunk driving arrest crackdowns [e.g., Homel (1990); Hurst and Wright (1980); Ross (1973, 1975, 1977, 1981, 1992); Ross, et al. (1982)] suggest a clear cause and effect. The ability of the police to control drunk driving appears to be a direct and linear function of the amount of effort they put into it." Thus, the statistical studies done by economists are contradicted by criminologists' experimental studies. For another thing, taxes on alcohol would appear to be a very "blunt" instrument for controlling DUI. Indeed, recent studies of alcohol markets by economists appear to contradict the economic studies of DUI as they indicate that such taxes have only a relatively small impact on the money price of alcohol and that money price in turn has only a relatively small impact on consumption decisions [e.g., Sass and Saurman (1993), Young and Likens (2000)]. The law of demand certainly holds, but other factors such as transactions costs due to market structure characteristics and regulations, and general attitudes towards alcohol (only some of which are considered in some DUI studies) are relatively important determinants of quantity consumed. There also is growing empirical evidence from studies using survey data indicating that the price elasticity of demand for alcohol may be lowest among heavy drinkers (Kenkel, 1996; Chaloupka and Wechsler 1996; Sloan, et al. 1994b). If the college males, 21 to 24 year olds, and/or heavy-drinking males examined in such studies are particularly prone to drink and drive, findings that beer taxes are an effective way to combat DUI fatalities are, once again, surprising. After all, given the degree of product variety in alcohol markets, an individual can be quite responsive to price changes without actually reducing alcohol consumption. Tax increases could lead to considerable changes in consumption patterns across brands of beer as cheaper brands are purchased when taxes rise, or across alcohol types as liquor or wine are purchased as beer taxes rise, without actually reducing consumption of alcohol at all, at least for heavy drinkers. In light of these considerations, economists have continued to examine the DUI deterrence issue, and some of the most recent studies raise significant questions about the veracity of the effect of taxes (Young and Likens 2000; Mast, et al. 1999; Dee 1999), while others are beginning to support the experimental literature's conclusions by implying that direct law enforcement may actually be relatively effective may be relatively effective deterrents after all (Benson, et al. 1999, 2000). The proposed research will employ a methodology similar to these recent models of drunk driving, so let us consider the relevant modeling issues.

Drunk driving in jurisdiction i is expected to be a function of alcohol consumption in the jurisdiction (A_i), the expected criminal punishment for drunk driving as determined by the probability of being arrested and convicted (PRa_i and PRc_i), the expected severity of punishment (S_i), civil liability through tort action when an accident occurs (L_i) as suggested by Sloan, et al. (1994a, 1994b, 1995), and a vector, N_i, of variables measuring the likelihood of driving (drunk or not) for people who drink in the jurisdiction. Therefore, alcohol-related traffic deaths in the jurisdiction, F_i, are a function of these determinants of drunk driving, and a vector V_i containing measures of traffic, vehicle safety, and driver safety:

$$F_i = f(A_i, PRa_i, PRc_i, S_i, L_i, V_i, N_i).$$

Factors that determine A_i, such as taxes and drinking-age laws, clearly can have an impact on F_i, however, so most studies have not considered A_i directly. Instead, the common practice in the literature is to include measures of some of the determinants of A_i in the reduced form model of F_i. Alcohol consumption is determined by the interaction of supply and demand, as explained in more detail in the following chapter. The quantity demanded in the jurisdiction depends on the price, P_i, a vector of regulations that affect alcohol availability (R_{Di}) including the legal drinking age, income (M_i), and a vector of non-price (e.g., demographic) determinants of demand (X_i)

such as the age distribution of the population that influence attitudes toward alcohol consumption. Quantity supplied also depends on price, a vector of variables indicating the market characteristics that influence the level of competition (C_i) and regulations (R_{Si}) regarding entry and market practices, and costs of supplying alcohol. Assuming that production costs for any particular type of alcohol (e.g., beer) are roughly equal, differences in the costs of supplying alcohol across geographic markets should reflect transportation costs (T_i) and taxes (τ_i). In equilibrium, quantity supplied equals quantity demanded, and because the price is endogenous, the equilibrium quantity, A_i, can be estimated in reduced form, as in Sass and Saurman (1993) and Mast, et al. (1999). As noted above, however, the standard practice in the DUI literature has actually been to estimate the driver involvement equation in reduced form by implicitly substituting the quantity equation into the fatalities equation:

$$F_i = f(R_{Di}, M_i, X_i, C_i, R_{si}, T_i, \tau_i, PRa_i, PRc_i, S_i, L_i, V_i, N_i).$$

Since there are so many factors that might reasonably be hypothesized to affect traffic fatalities (laws intended to affect alcohol availability and prices, laws intended to affect driving and driving-under-the-influence, law enforcement efforts, traffic conditions, socioeconomic characteristics including attitudes toward drinking), researchers who estimate such a reduced-form model are forced to choose some limited specification. After all, attempting to control for all of the potential determinants of fatalities would require a model that would be both unmanageable and uninterpretable due to substantially collinearity between policy variables. The question inevitably becomes, which variables should be omitted, but because of this, the results of some studies may suffer from missing variable biases.

The restrictions imposed by using a reduced form model can be alleviated, at least to a degree, since fatality and equilibrium alcohol quantity equations can be estimated separately, as in Mast, et al. (1999).[23] A multiple equation model has its drawbacks due to the aggregation problems discussed in Cook (1981a). After all, a substantial portion of alcohol consumption probably is not related to drinking and driving. However, reduced form models also are problematic due to missing variable biases. Therefore, a relatively accurate understanding of DUI deterrence probably requires consideration of results from both kinds of models, recognized the relative shortcomings and benefits of each. Panel estimating techniques, including control for both time and state fixed-effects, also can alleviate at least some potential missing variable biases and they have been used in most of the recent DUI studies (Ruhm 1995a, 1995b; Sloan, et al. 1994a, 1994b; Evans, et al. 1991; Young and Likens 2000; Mast, et al. 1999; Benson, et al., 1999). Fixed-effects models bias coefficients toward zero, making it more difficult to find "meaningful results" in the form of significant relationships, as noted by Saffer and Grossman (1987a, p. 369), and Chaloupka, Saffer and Grossman (1993, p. 172). An inability to interpret coefficients due to such biases certainly can be a problem. However, Saffer and Grossman (1987b, p. 413) and Mast, et al. (1999) report that their tax coefficients are not reduced in absolute value by the inclusion of state dummies in their models [indeed, the coefficients increase in Mast, et al. (1999)]. While the coefficients for some of the other variables may be insignificant because of the bias that arises with the multicollinearity of fixed-effect models, if a Hausman (1978) test indicates that the model is preferred over an ordinary-least-squares model, and if some variables change sign when fixed effects are controlled for, a finding reported by Ruhm (1995a), then failure to control for them also means that some coefficients are biased and

---

[23]If alcohol consumption is not affected by driver involvement in alcohol-related fatalities, then the driver involvement and alcohol consumption equations can be estimated in a recursive model. It may be the case, however, that while alcohol consumption is not directly affected by drunk driving fatalities, both are functions of unmeasured sentiment toward alcohol. In Mast (1996), tests for endogeneity were conducted and it was not found to be a problem.

not "meaningful." That is, while the insignificant coefficients on some of the variables may be misleading in a fixed-effects model due to the biases they create, the significance and signs of other variables in an OLS model can also be misleading.[24]

Mast, et al. (1999) also found that alcohol taxes do not affect DUI fatalities in both two-equation recursive models and in some reduced form models using a panel of state data for the 1984-92 period. They suggest that the reason other studies have stressed the importance of tax effects is that they have used data from a period prior to 1988 whose results do not generalize to more recent data periods [a finding also supported by Young and Likens (2000) reduced-form models], and because they suffer from at least two sources of missing variable biases.[25] First, other than taxes and drinking age, characteristics of the alcohol markets have been largely ignored. Taxes are correlated with such factors so tax coefficients have been biased in the

---

[24]An alternative approach to dealing with the issue of significance of policy variables in a fixed-effects DUI model is suggested in Benson, et al. (1999). They note that it may be the overall package of policy instruments that are important, rather than single policy variables. This point was motivated by Chaloupka and Wechsler (1996) who used an index developed by Mothers Against Drunk Driving (MADD) to reflect the restrictiveness of each state's drunk-driving laws targeting youths and young adults as their deterrence variable, and concluded that strong state policies "significantly reduce all measures of drinking in both specifications for the underage and older college student samples." Similarly, Evans, et al. (1991: 279) reported "no conclusive evidence that any specific form of punitive legislation is having a measurable effect" on traffic fatalities, but they found that states with laws allowing both sobriety checkpoints and preliminary-breath-test laws had 24 (22) percent fewer single vehicle occupant night-time fatalities (single vehicle occupant fatalities). To consider this hypothesis, a number of group effects were tested. The impact in each of their DUI fatality regressions of removing all alcohol control and deterrence variables was highly significant. Consideration of more narrowly-focused subsets of deterrence variables revealed that removal of those factors influencing policing (the probability of being stopped and/or arrested) had a significant impact, and among these variables, those grouped as influencing the probability of being stopped (open-container laws, anti-consumption laws, and police per capita) appeared to be the most important. Indeed, once an alleged DUI offender has been stopped it appears that subsequent actions may have relatively little deterrent impact, as the group of variables controlling for severity of punishment did not contribute significant explanatory power.

[25]There also has been a general lack of systematic control for enforcement effort in the literature, in part because a direct measure of the probability of arrest and punishment does not exist. Mast, et al. (1999) controlled for various potential determinants of the probability of arrest as proxies for enforcement effort, however, and found that this may not be a significant missing-variable-bias problem. On the other hand, Benson, et al. (1999) found that as a group, the determinants of the probability of arrest for DUI are important deterrents, so even exclusion of such variables does not bias the tax coefficient in a statistical sense, it can "bias" the policy conclusions by making direct deterrence appear relatively ineffective.

negative direction. Second, some (but not all) DUI studies fail to control for factors that influence attitudes toward alcohol consumption, such as religious sentiments, which also apparently impact the likelihood of high alcohol taxes being established.

Because of the inherent structural similarities between the study of alcohol's impact on violence and on traffic fatalities, this study initially adopts a similar approach to this DUI model, substituting violent crime equations with additional controls for alcohol consumption, for DUI fatality equations.[26] It goes well beyond our DUI research, however. For instance, since beer is the drink of choice for young people who are disproportionately represented among alcohol-related traffic fatalities, beer consumption is an important factor in alcohol-related vehicle fatalities, while liquor consumption is not (Mast, et al. 1999). The age distribution of violent crime arrestees may be older, so we also consider liquor and wine consumption as potentially more important determinants of violent crime than beer consumption. Furthermore, we explore the possibility that drunk-driving policies themselves may influence alcohol consumption as well driving behavior, an issue not directly considered in the DUI literature to date. Tests for endogeneity are also performed, as suggested by the empirical crime literature discussed above, and as a result, simultaneous estimation techniques are employed. Finally, in a search for ways to alleviate simultaneity bias, a metropolitan level data set is examined in addition to the state level data typically employed in the DUI literature.

2.5 Conclusions: Commentary on Previous Alcohol-Violence Studies

One critical factor which is almost always cited to explain trends in crime rates or the incidence of criminal behavior are deterrence factors. However, except for the analysis of

[26]This does not imply that DUI and violent crimes are similarly motivated, of course, as the violent crime equation is quite different from the fatality equation. Indeed, similar modeling procedures also have been employed to study other adverse consequences of alcohol abuse (e.g., binge drinking, cirrhosis).

Chaloupka and Saffer (1992), none of the previous alcohol-violence studies explicitly control for the effects of deterrence. While it is arguable that the effect of deterrent measures may be diminished in that alcohol impairs an individual's capacity to correctly calculate the (expected) costs associated with their actions (e.g. intoxication may give individuals a feeling of invulnerability or make them more short-sighted), law enforcement measures need not be completely ineffective in constraining the behavior of drinking offenders. Indeed, the overall explanatory power of the models estimated in Markowitz and Grossman (1998a and b, 1999) and Markowitz (1999, 2000b) tend to be relatively low, perhaps due to the failure to account for the effects of deterrence factors.

A more fundamental issue is the surprising result that beer excise taxes appear to be a viable policy instrument for lowering the incidence in crime in several studies. These taxes make up only a small portion of the retail price of beer and as such would not be expected to substantially reduce beer consumption even if dramatically increased. This notion is exacerbated by the fact that the price elasticity of alcohol for the demographic group that drinks the most and commits the most crime (namely younger males) appears to be relatively inelastic (Sloan, et al.1994a; Chaloupka and Wechsler 1996; Kenkel 1996). A possible explanation for the apparent efficacy of beer taxes might be the failure of previous studies to consider other non-tax determinants of alcohol beverage prices. As such, the excise tax coefficient in previous studies may also be explaining the variation in these other omitted factors and therefore biased upwards. In addition, consumers can readily switch between more expensive alcohol brands and cheaper ones given the high degree of product differentiation in alcoholic beverages types. Tax increases might therefore lead to changes in consumption patterns within beverage types, but not to decreases in the overall level of consumption. Finally, previous work on the relationship between alcohol consumption and the incidence of Index I crimes [e.g., Chaloupka and Saffer (1992); Cook and Moore (1993b)] has only considered the effects of beer consumption/taxes. However, the age distribution of violent crime offenders tends to be right-shifted relative to property crime offenders (Blumenstein 1985) and older males tend to substitute other alcoholic beverage types (e.g. hard liquor and wine) for beer at a greater rate than younger males. As such, examining the effects of liquor and wine consumption and *their* respective policy controls (e.g., excise taxes) on the incidence of Index I crimes might lead to additional or contradictory policy implications. In addition, laws aimed at lowering the rate of drunk-driving increase the cost of engaging in drinking behavior outside of the home. If most drinking-crime incidents also occur within this scope of activities (e.g., because potential victims are relatively more vulnerable or because potentially violent criminals have ready outlet for their violent tendencies), then DUI laws may also serve as a potential policy tool for reducing the incidence of property and violent crime in addition to drunk driving. None of the empirical studies reviewed above have examined the effects of such DUI laws in an empirical model of criminal participation.

CHAPTER 3

THE EFFECTS OF ALCOHOL CONTROL POLICIES ON ALCOHOL CONSUMPTION: EVIDENCE FROM STATE LEVEL PANEL DATA

3.1 Introduction

The studies reviewed in the second chapter suggest that the consumption of alcohol and the incidence of criminal activity tend to be positively correlated. Despite the theoretical ambiguity regarding the nature of the alcohol-crime relationship, the common perception remains that the consumption of alcohol alters individual behavior (e.g., by making individuals more outgoing or aggressive) and directly contributes to the incidence of many types of crime (e.g., barroom assaults, date rape, spousal homicide, etc.). Recognizing the alcohol-crime link and negative public sentiment towards excessive drinking, some policy makers and academics [e.g., Cook and Moore (1993a), Boyum and Kleiman (1995)] have advocated the use of laws aimed at curbing the consumption of alcohol (most notably excise taxes and minimum legal drinking ages) as tools to fight crime.

Identifying and understanding the factors that influence alcohol consumption becomes of central concern for determining the efficacy of such policies and their implications for social welfare. For instance, different alcohol control policies may not have symmetric effects on consumption. If minimum legal drinking ages appear to reduce consumption whereas excise taxes do not, then implementing the latter is clearly inefficient since they do not generate the intended benefit and result in only a loss of consumer surplus. In addition, since states self-select the number and types of alcohol control policies they implement (as well as numerous other factors such as their duration and level of enforcement) such policies may prove ineffective when other determinants of consumption (such as socioeconomic/demographic characteristics) are controlled for. The purpose of this chapter is to empirically determine whether the level or existence of several alcohol control policies are negatively correlated with various measures of alcohol consumption while controlling for other demand and supply (and therefore price and quantity) determinants. If so, this provides at least preliminary insight into which policies may be effective in mitigating the incidence of criminal activity.

Four models of per-capita alcohol consumption are specified and estimated. Specifically, separate consumption equations are estimated for beer, distilled spirits (liquor) and wine. In addition, a total alcohol consumption measure is estimated as well. Particular attention is paid to the effects of the most widely advocated alcohol control policies: excise taxes and minimum legal drinking ages. The potential effects of anti-DUI laws on alcohol consumption are also considered.

The chapter proceeds as follows. Section 3.2 presents a simple market model of alcohol consumption (as suggested by the discussion of the DUI literature in Chapter 2) and discusses the empirical methodology to be employed. Section 3.3 discusses the data and constructed variables used to make the market model operational. Section 3.4 presents the estimation results and conclusions are offered in Section 3.5.

3.2. A Market Model of Equilibrium Alcohol Consumption[27]

A simple theoretical model of the market for a particular alcoholic beverage (e.g., beer) is presented in this section. First consider the demand side of the alcoholic beverage market.

[27]This section and the next are drawn, with minor modifications, from Zimmerman (2000) where a similar analysis but with a different data period (1985-92) is presented. It is informative to compare the results here with those in Zimmerman (2000) in order to note the surprising lack of robustness for many of the policy variables. Indeed, a surprising number of changes in sign and significance occur by simply adding two addition years to the data set. Thus, considerable caution should be exercised in interpreting these results. Reasons are suggested below, and an alternative data set is examined in Chapter 7 in hopes of alleviating at least some of the issues raised in this and the next two chapters where results from the analysis of state level data are examined.

Similar to previous studies (Hogarty and Elzinga 1972; Ornstein and Hanssens 1985, 1987; Coate and Grossman 1988; Nelson 1990; Sass and Saurman 1993), the demand determinants of alcohol consumption are classified into three categories: economic, demographic, and regulatory. To keep the model as simple as possible we shall initially consider only the price of alcohol (P) and income (M) as the relevant economic determinants. Any laws that affect the availability of alcohol (e.g. minimum legal drinking ages) are regulatory determinants (R_D). Demographic determinants such as the age distribution of drinkers are denoted by X. As such, the demand function for alcohol is given by

$$D = D(P, M, R_D, X) \qquad (3.1)$$

The supply determinants of alcohol are classified as economic, degree of market competition, and cost. Again, for simplicity we shall consider the price of alcohol and the level of exogenously determined excise taxes (τ) as the economic determinants of supply. Market competition determinants (C) are any structural market characteristics that govern such factors as entry and strategic business practices. Government-imposed regulations on the producers of alcoholic beverages (R_C), such as those prohibiting brewers from entering into exclusive territorial contracts with distributors, may also influence equilibrium market outcomes. Let S denote the aggregate quantity of alcohol supplied to the market. Assuming that production costs across firms are roughly equal for a particular alcohol type, differences in the costs of supplying alcohol across states should reflect transportation costs (T) and taxes:

$$S = S(P, C, R_C, \tau, T) \qquad (3.2)$$

We assume that the regulatory/competition measures described above do not provide opportunity for monopoly profits. Thus the alcohol market can be characterized as being perfectly competitive in this model. As such, the equilibrium quantity of a particular kind of alcohol consumed (e.g., beer) is determined by the interaction of market supply and demand, or where

$$D(P, M, R_D, X) = S(P, C, R_C, \tau, T) \qquad (3.3)$$

Let $A*$ denote alcohol quantity that satisfies the above equality. Because price is endogenous (i.e., determined simultaneously with quantity), the equilibrium quantity can be estimated in reduced form (Sass and Saurman 1993; Mast, et al. 1999) as a function of the exogenous variables only:

$$A^* = f(M, C, R_D, R_C, T, \tau, X) \qquad (3.4)$$

Laws that limit availability should reduce equilibrium alcohol consumption as should laws that reduce the intensity of competition. Thus $A^*_{RD} < 0$, $A^*_C > 0$ where subscripts denote partial derivatives. All else equal, equilibrium consumption should be negatively related to taxes $(A^*_\tau < 0)$ and (assuming alcohol is a normal good) consumption should be positively related to income $(A^*_M > 0)$. Various non-price determinants of demand have different effects on consumption, so hypotheses regarding these relationships are discussed below when the empirical model is specified.

3.3 Empirical Methodology and Data

Determining the effects of alcohol control policies on alcohol consumption requires the specification and estimation of equation (3.4). Alcohol control policies are, of course, not randomly assigned across states. Rather, individual states self-select their legal provisions regarding the consumption and distribution of alcoholic beverages. Failure to account for sources of observation—specific heterogeneity may bias the estimated coefficients in the structural model if these factors influence both the level of the included explanatory measures and the level of the dependent variable. To alleviate the problems of missing variables biasing the estimated coefficients in the structural model we control for all latent sources of state-specific heterogeneity that are time-invariant and may thus be considered "fixed-effects". These observation specific fixed-effects are removed by estimating all consumption equations with the inclusion of state indicator (dummy) variables. In addition, to remove the biasing effects of time-variant non-observation specific factors all models are estimated with the inclusion of year dummy variables.

The data consists of a panel of contiguous state-level observations over the years 1985 to 1994. This results in 480 potential observations. All continuous independent variables are converted to logarithms. As such, their estimated coefficients may be interpreted as elasticities. Monetary variables are adjusted for inflation as well as interstate cost-of-living (COL) differentials. The data used to construct the dependent and independent variables are discussed below. Detailed descriptions of data sources and their descriptive statistics can be found in Appendix A.

3.3.1 Dependent Variables

In the empirical specification of equation (2.4) the dependent variables are the per-capita levels of consumption of beer (BEER), distilled spirits (LIQUOR), and wine (WINE). In addition, a specification of total alcohol (TOTALC) consumption is estimated as well.

Data on actual consumption of alcohol by beverage category does not exist. As such, following previous studies (Ornstein and Hanssens 1985; Nelson 1990; Sass and Saurman 1993) we employ state level shipments of alcohol (in gallons) as a proxy for consumption. Each of the alcohol shipment quantities is divided by the state population ages eighteen and over (rather than the minimum legal drinking age) to account for underage consumption.[28]

3.3.2 Independent Variables

All models contain five variables to control for the socioeconomic/demographic determinants of drinking. M1844 is the proportion of the state population ages 18 and over which is male and between the ages of 18 and 44. This variable is included to capture the segment of the population most likely to consume alcoholic beverages. This variable is expected to be positively correlated with consumption. NONMET and POPDEN control for the proportion of the state population which resides in non-metropolitan areas and the state population density, respectively. These variables do not have unambiguous predicted effects on consumption a priori and are included to control for the effects of state heterogeneity on consumption. PERBLK is the percentage of the population that is black and is included to capture effects of race-specific sentiment towards drinking. Finally, GINI is a Gini coefficient calculated to capture the effects (if any) of income disparity on consumption patterns. This measure does not have an unambiguous expected sign either.

Three variables are used to control for the economic determinants of alcohol consumption: INCOME, TOURISM, and UR. INCOME is defined as real per-capita disposable income. If alcohol is a normal good, this variable should be positively correlated with consumption. TOURISM is defined as the sum of state hotel, motel, and tourist court receipts as

[28]Ideally, the shipment measures would include alcohol produced illegally and cross-border shipments. However, these factors are likely to small given that alcohol can be legally purchased in every state and, as of the late 1980s, all states had imposed a minimum legal drinking age of twenty-one. As noted by Ornstein and Hanssens (1985), using shipments data to proxy distilled spirits (and to some extent wine) consumption might lead to measurement error since not all sales in a given year are necessarily consumed. This error is likely to be small as well and accounted for in the error term.

a percentage of retail sales. It is hypothesized that this variable will be positively correlated with beer consumption, and in particular, consumption in bars and restaurants. UR is a measure of the state unemployment rate. The effects of unemployment on consumption cannot be determined a priori. Individuals may tend to increase consumption during periods of unemployment to relieve stress associated with being unemployed or simply because the "cost" of leisure time (one aspect of which might be the consumption of alcohol) is reduced. On the other hand, unemployment may cause consumption to fall if leisure time is reduced (e.g., time is instead devoted to seeking employment or acquiring job skills), if disposable income is lowered (which may also be explained by INCOME), or if individuals substitute into other mood-altering substances (e.g., illicit drugs).

The effects of religious sentiment on the consumption of alcohol is also considered by including the proportion of the state population that is identified as Catholic (CATH), Mormon (MRM), Southern Baptist (SOBAP), or belonging to other Protestant denominations (PROT). Mormons and Southern Baptists prohibit the consumption of alcohol and other Protestant beliefs differ across denominations. Catholics do not prohibit the use of alcohol. Ornstein and Hanssens' (1985) results indicate that all groups are positively associated with beer consumption and negatively correlated with wine and liquor consumption. These results may indicate that individuals who belong to these religious groups view the consumption of alcoholic beverages with higher ethanol as more morally reprehensible than those with low ethanol content. As such, no predictions are made regarding the signs of these religious affiliation variables.

Following Sass and Saurman (1993), the beer consumption equations include several measures intended to control for the effects of price determinants. MANDATE represents the portion of the year in which state laws mandating exclusive territories for beer distributors were in effect. The expected sign on this variable is ambiguous a priori. It is possible that mandated exclusive territories reduce competition, raise retail prices, and lower alcohol consumption. On the other hand, Telser (1960) argued that manufacturer imposition of vertical restraints (such as resale price maintenance or exclusive territories) may induce advertising and promotional efforts that increase consumption. CASHLAW is a dummy variable taking a value of one if a state has a law in a given year that requires that retailers pay immediately for beer purchased from wholesalers. Such laws will tend to raise the transactions costs for beer retailers and are thus expected to raise the retail price of beer and reduce consumption. FORCE is an indicator of states that have a law requiring beverage containers of certain sizes to be returnable and carry deposits. These laws are also expected to raise beer prices by increasing the transactions costs associated with selling beer (such as the costs of collecting deposits and keeping detailed records) and buying beer (such as the costs buyers must incur to retain and return bottles or cans). Finally, MINDIST represents the distance from the nearest major beer brewery to the most populous city in each state.[29] This variable is a proxy for transportation costs and is expected to be negatively correlated with beer consumption as it should also raise the retail price of beer.

Alcohol control policies (regulatory factors) that are hypothesized to influence consumption patterns can be classified into four general categories: availability, taxes, advertising, and driving-under-the-influence (DUI) deterrents. Availability factors consist of three variables. The first is the fraction of the state population residing in dry counties (DRYPER). Since the full cost of acquiring alcoholic beverages for individuals in dry counties is relatively higher (e.g., must travel greater distances to obtain alcohol) compared to those residing

[29] These breweries are Anheuser-Busch, Miller, Stroh, Heileman, Pabst, and Coors.

in non-dry counties, this variable is expected to be negatively correlated with consumption. The second is LEGAL, defined as the percentage of the 18-20 year old population that can legally drink beer with alcohol content of 3.2 percent and above. The coefficient of this variable is expected to be positive. The number of per capital (population over 18) retail drinking establishments, DRINK, is the third measure of availability.

Advertising regulations are captured by the inclusion of two dichotomous variables: NOSIGN and NOPRINT. NOSIGN is an indicator that a state prohibits price advertising of alcohol on billboards or other publicly displayed signs. NOPRINT is an indicator of state prohibition of price advertising in newspapers and magazines. While these variables are specific to beer advertising, they are included in the non-beer specifications to control for underlying state-specific sentiments towards alcohol regulation. Each of these variables is expected to be negatively correlated with consumption as their presence should increase consumer search costs.

The variables BEERTAX, LIQTAX, and WINETAX serve as the tax measures for the beer, distilled spirits, and wine alcohol categories respectively. BEERTAX is defined as the real (i.e., adjusted for inflation) federal plus state excise tax on a six-pack of beer, adjusted for geographic differences in cost-of-living (COL). LIQTAX and WINETAX are defined as real COL-adjusted state plus federal gallonage excise taxes of the respective alcohol groups. The tax level that was in effect for the majority of the calendar was used for years in which the tax level changed. Excise tax measures are expected to be negatively correlated with consumption.

Following Wilkinson (1987), a set of regulatory measures aimed at mitigating the incidence of driving under the influence (DUI) is also included in all model specifications. The variables PBT, NOPLEA, DRAM, and ILDUM are dichotomous indicators of specific anti-DUI laws. PBT indicates the existence of a state law in a given year that allows police officers to administer preliminary breath tests on drivers stopped for being suspected of driving under the influence. DRAM represents states that allow drinking establishments that served alcohol to a DUI offender to be sued by the victims of the drunk-driver. Thus, this is a "civil-law" source of potential deterrence for selling to people who may drink a drive. Several potential criminal-law deterrents are also included. NOPLEA represents states that require individuals who are apprehended for a DUI violation to be tried for a DUI offense. The variable ILDUM indicates whether or not a state has a law making it a crime to drive with a blood alcohol content at or above some predetermined level. Under such laws, state prosecutors do not have to show that the driver of a motor vehicle was actually impaired to get a DUI conviction. It is expected that each of these measures will be negatively correlated with alcohol consumption since their existence makes consuming alcohol a relatively riskier and thus costlier activity. Furthermore, these laws should have particularly important impacts on drinking in bars and restaurants or at private parties rather than at home.

The variable JAIL is defined as the minimum number of days an individual must serve in jail after apprehension for a DUI offense. FINE is defined as the real COL-adjusted minimum fine for an individual's first DUI conviction. Finally, SUS measures the minimum number of days that a DUI offender's license is suspended for their first DUI conviction. Each of these variables is expected to be negatively correlated with consumption.

3.4 Estimation Results of Per-Capita Alcohol Consumption Models

This section presents the estimation results of the beer, distilled spirits, wine, and total alcohol consumption specifications. Excise taxes on alcoholic beverages exhibit little variation over time. For instance, using similar data Dee (1999) reports that approximately sixty-four percent of the sample variation in beer excise taxes can be explained by the presence of state indicators alone. When year indicators are also added, approximately ninety-four percent of the variation is explained. For this reason, two sets of estimation results are presented for each alcohol specification: one with only state fixed-effects removed and one with both state and year fixed-effects removed. The estimation results on the coefficients of the state and year dummy are not reported. All specifications are statistically significant at conventional levels.

3.4.1 Per-Capita Beer Consumption

Table 3.1 presents the estimation results of per-capita beer consumption. Not surprisingly, the proportion of the population that is male between the ages of 18 and 44 is positively correlated with per-capita beer consumption in the first model, but it turns negative in the second. It is statistically significant in both specifications. Obviously, this variable is quite sensitive to specification [also see Zimmerman (2000) in this regard]. NONMET is negative in both specifications but never statistically significant. POPDEN is positive but statistically insignificant with only state fixed-effects removed but turns negative and statistically significant when period effects are removed as well. PERBLK is negative but insignificant in both specifications.

GINI changes signs as controls for period effects are added, but is never significant. Real per-capita disposable income is positive and statistically significant across both specifications, while UR is negative and statistically significant in both models. TOURPCT takes an unanticipated negative sign in both specifications and it is statistically significant.

With respect to religious affiliation, in the one-way fixed-effects model MRM and SOBAP are found to be negatively correlated with per-capita beer consumption, and both are statistically significant. These results would appear reasonable since these churches prohibit the consumption of alcohol. The estimated coefficient on CATH is positive and statistically significant. This too seems reasonable since the Catholic Church does not explicitly forbid the consumption of alcohol. However, when period fixed-effects are removed, all religious affiliation coefficients turn positive with MRM statistically significant at conventional levels and CATH marginally (at the 10 percent level of confidence) significant (the other protestant coefficient is positive in both specifications but insignificant). These results are actually consistent with the findings of Ornstein and Hanssens (1985) and Mast, et al. (1999), however, and may indicate that Southern Baptists and Mormons substitute the consumption of beer for the consumption of alcoholic beverage with higher ethanol content (such as liquor and wine).[30]

The coefficient on real COL-adjusted beer excise taxes (BEERTAX) is positive in the first specification, although insignificant. It turns negative when year fixed-effects are removed, however, and it is marginally significant (at the 10 percent level). The sensitivity of this coefficient to specification and to the inclusion or exclusion of other variables is consistent with findings reported in Mast, et al. (1999).

With respect to the beer market variables, MANDATE, MINDIST, and FORCE are never statistically significant. CASHLAW takes the anticipated negative sign and is statistically significant in both specifications. The variables LEGAL, DRINK, and DRYPER were included as availability determinants of alcohol consumption. LEGAL is not statistically significant, but this may reflect the fact that for several years now drinking age has been 21 everywhere so the only variation in this variable comes in the early years of the sample period. The variable DRINK is positive in both specifications and statistically significant. This supports the hypothesis that more retail drinking establishments per-capita will lower the full cost of obtaining/consuming alcohol and tend to increase per-capita consumption. The percentage of the state population

[30]It should be noted that the data used to construct the church membership data are only collected every decade and are interpolated/extrapolated for all other years. As such, there is little intra-state variation in these data and the constructed variables may be measuring linear trends within each state.

residing in dry counties is positive in both specifications and significant when year fixed-effects are removed. This may reflect the endogeneity of this variable (e.g., in states where beer drinking tends to be high, voters in some counties attempt to control the resulting problems by going dry).

The civil law DUI deterrence variable, DRAM, is statistically significant in both specifications, and positively correlated with per-capita beer consumption. This may be surprising, as dram shop laws have been found to reduce traffic fatalities in studies of DUI [e.g., see Benson, et al. (1999) for a review and supporting evidence]. However, these laws may be endogenous, or alternatively, they may lead to a substitution of drinking at home for drinking out, and since the monetary cost of home consumption is relatively low, total consumption could rise while DUI behavior declines. Among the criminal law variables, PBT, ILDUM, and NOPLEA take the hypothesized negative sign, but they are not significant. The variables JAIL and FINE are not significant either. As noted by Wilkinson (1987) and Benson, et al. (1999), there are several possible explanations for these findings. It may be that the deterrence variables are endogenous with respect to consumption and thus suffer from simultaneity bias. Wilkinson attempts to break the potential simultaneity by estimating a simultaneous equations system but still finds similar results. Another problem is that the variables representing many of these laws (as well as dram shop laws) are highly correlated, possibly making the estimates of their coefficients unreliable. Alternatively, individuals may perceive the likelihood of arrest for DUI to be so low that they do not substantially lower their drinking [see Benson, et al. (2000) for a discussion of this issue]. Thus, while these results do not suggest that criminal DUI laws affect beer consumption very much, it does not follow that they cannot have an impact. The results reflect the policing practices of the time period under consideration, when the probability of DUI punishment may have been very low.[31]

3.4.2 Per-Capita Liquor Consumption

Table 3.2 presents the estimation results of the liquor specification. Since several states regulate the distribution of liquor by mandating state-owned monopoly retail outlets, excise tax data could only be obtained on a subset of observations. Specifically, the resulting sample consisted of 300 observations.

The coefficient for the variable M1844 is surprisingly negative in both specifications and takes an estimated elasticity of approximately one in absolute value. NONMET and PERBLK are negative but statistically insignificant in both models. Population density is negative and highly significant in the first specification and remains significant when year fixed-effects are removed. State income inequality as measured by GINI is also negative and marginally significant in the first specification but looses significance when year fixed-effects are controlled.

Affiliation with the Southern Baptist or other Protestant churches is predicted to decrease per-capita liquor consumption in both models. MRM is negative and statistically significant in the one-way model but turns positive and statistically insignificant when year fixed-effects are controlled for. CATH is negatively correlated with per-capita liquor consumption in both models but is never statistically significant.

The control for the legal drinking age, LEGAL, is found to carry an unanticipated negative sign in both specifications but is never statistically significant. DRYPER has a positive sign that is marginally significant in the first specification and becomes statistically significant at

[31]For instance, Wilkinson (1987) estimates the expected penalty for drunk driving to be approximately $1.05.

conventional levels when year fixed-effects are controlled for. On its own, this might not be surprising, since the cost of drinking rises for those who want to drink in dry counties, but it does not rise by the same amount for beer and alcohol. The cost of transporting alcohol is less than the cost of transporting beer, so those who do drink may tend to substitute alcohol for beer, thereby increasing alcohol consumption. However, the positive sign for the dry county variable in the beer equation does not support this substitution effect. Therefore, the possibility that dry county designation is endogenous remains [see discussion of the beer regressions regarding this result]. As predicted, the per-capita number of drinking establishments is found to be positively correlated with per-capita liquor consumption but is never statistically significant.

The advertising restriction variable NOSIGN takes the anticipated negative sign across both specifications but is never statistically significant. NOPRINT is found to be positively correlated with per-capita liquor consumption but is never statistically significant.

Of the anti-DUI variables, PBT, JAIL, and ILDUM are found to be negatively correlated with per-capita liquor consumption, but only ILDUM is statistically significant in both specifications (PBT is marginally significant in the one-way model). FINE and SUS are both positive and are statistically significant in both models, while DRAM is also positive and marginally significant in the one-way specification but not in the two-way [endogeneity issues, multicollinearity, and/or the substitution effect discussed for the beer results may explain these findings].

Finally, the real COL-adjusted liquor excise taxes (LIQTAX) is found to be negatively correlated with per-capita liquor consumption in the two specifications. LIQTAX is also found to be marginally significant in the first regression and significant at conventional levels in the second. In the two-way model, a ten percent increase in liquor excise taxes is predicted to decrease per-capita liquor consumption by approximately four percent. These results indicate that liquor taxes may serve as a viable policy instrument for reducing the incident of criminal activity [note that the liquor tax impact appears to be relatively robust compared to the impact of beer taxes on beer consumption].

3.4.3 Per-Capita Wine Consumption

Table 3.3 presents the estimation results of the wine consumption equation. Some observations were lost since tax data could not be obtained for three states in the sample. Omission of these states resulted in 450 sample observations.

The variable M1844 takes the anticipated positive sign in the one-way specification and is statistically significant. In fact, the estimated coefficient is larger than one in absolute value. Upon removing year fixed-effects, the coefficient of this variable turns negative and statistically insignificant. The proportion of the state population that resides in non-metropolitan areas has a marginally significant positive effect on the level of per-capita wine consumption when both state and period effects are controlled. Population density is found to be negatively correlated with consumption and statistically significant in the one-way model. However, the estimated coefficient on this variable turns positive and significant in the two-way specification. GINI is found to be negatively correlated and statistically significant in both specifications. This indicates that states with greater income disparity tend to be associated with lower per-capita levels of wine consumption.

Once again, the variable DRYPER takes a positive sign in both specifications and turns statistically significant in the two-way model [see discussion in Section 3.4.2 in this regard]. LEGAL takes the expected positive sign in both models but is never statistically significant. The number of licensed drinking establishments is positively correlated with per-capita wine consumption as expected in the one-way specification but turns statistically insignificant when period fixed-effects are removed.

Of the religious affiliation variables, MRM is statistically significant at conventional levels in the one-way model and takes a negative estimated coefficient, but it turns positive and marginally significant in the two-way model. SOBAP is also positive and becomes significant in the second specification. The other Protestant variable is similarly positive and significant in the two-way specification. CATH, on the other hand, is negative and insignificant in both models.

Real per-capita disposable income is positive and statistically significant in the two-way model, indicating that wine is a normal good (although it is negative and insignificant in the one-way model). State unemployment rates are statistically insignificant (with a positive sign) in both models while TOURPCT takes an unanticipated negative sign in both specifications and is always statistically significant.

With respect to the criminal DUI deterrence variables, NOPLEA is consistently negative and statistically significant. SUS and ILDUM are also negative but they are never significant. FINE and JAIL are positive and significant in both models, however (PBT is also positive but never significant). The civil law variable (DRAM) is negative for wine, significant in the first specification, and marginally significant in the second. Finally, WINETAX is negatively correlated with per-capita wine consumption in both specifications, but it is statistically insignificant in the two-way model.

3.4.4 Total Per-Capita Alcohol Consumption

Table 3.4 presents the estimation results where the sum of beer, liquor, and wine consumption per-capita is used as the dependent measure. Due to missing observations on the liquor and wine excise tax data the sample included 290 observations. The relevant excise tax measure in this specification, TOTTAX, was computed in a similar to fashion to the total alcohol price measure employed in Markowitz (1999). First, annual state excise tax rates for each alcohol category were converted into pure ethanol tax rates based on the percentage of alcohol in each beverage. These "pure" tax rates were then weighted by the relevant consumption share and adjusted for inflation and interstate cost-of-living differentials. Summation over the three adjusted tax quantities formed the composite tax rate.

The variable M1844 is insignificant in the one-way specification but it takes an unanticipated negative sign in the two-way model. NONMET is positive but never found to be statistically significant. POPDEN is negative and statistically significant in the first specification but is predicted to increase per-capita consumption in the second, where it is also significant. State income inequality (GINI) is negative in the one-way model but it switches signs when controls for period effects are added. It is only marginally in first specification, however.

LEGAL is not significantly correlated with total per-capita alcohol consumption. The per-capita number of drinking establishments is predicted to increase per-capita consumption, and is statistically significant in the second model. The percentage of the state population residing in dry counties takes a positive sign and it is statistically significant in the two-way specification.

With respect to the beer market variables, FORCE, CASHLAW, and MINDIST take the anticipated negative estimated values in the one-way model but only CASHLAW is statistically significant. When controlling for year fixed-effects, the estimated coefficients on FORCE and MINDIST turn positive but remain statistically insignificant. CASHLAW remains negative and statistically significant. The control for mandated exclusive territories on beer distributors, MANDATE, is found to be positively correlated with total per-capita alcohol consumption and is statistically significant in the two-way specification.

Of the religious affiliation variables, only MRM and CATH are statistically significant in the one-way specification with the latter taking a positive estimated elasticity. When controlling for year fixed-effects all religious affiliation variables take positive estimated elasticities and MRM, CATH, and SOBAP are statistically significant at conventional levels.

Real COL-adjusted per-capita disposable income is positively correlated with per-capita consumption and statistically significant in both specifications. State unemployment rates are predicted to significantly lower per-capita consumption rates in both cases. As in all other specifications, TOURPCT is negatively correlated with consumption. In this case it also is statistically significant.

DRAM is positive and statistically significant in both specifications. NOPLEA and FINE are the only criminal DUI deterrence variables with significant negative coefficients in the one-way model, and ILDUM is significantly positive. All three lose significance when controls for period effects are added, while SUS becomes significant and positive. The composite alcohol tax measure, TOTTAX, is not statistically significant and takes a positive estimated elasticity in the one-way model, although it turns negative in the two-way specification.

3.5 Conclusion

The results of this chapter indicate that if alcohol consumption has a causal effect on crime then several alcohol control policies may be effective policy tools for reducing the incidence of crime given that they appear to be negatively correlated with alcohol consumption. Limiting the number of licensed drinking establishments may reduce the per-capita consumption of beer, for instance, and cash laws may as well. Beer taxes may have a marginal effect too, although this finding (and most others) must be treated with caution as it is not robust across specifications (or time periods). On the other hand, DUI laws do not appear to reduce beer drinking (but they may influence consumption of other types of alcohol, as noted below). Higher distilled spirits excise taxes are found to be negatively correlated with liquor consumption and may therefore be effective policy instruments, in contrast to results with respect to the efficacy of beer and wine excise taxes which both appear to be somewhat more tenuous. Illegal per-se laws directed at driving under the influence also may reduce liquor consumption but other DUI controls are not robust across liquor consumption specifications. On-the-other-hand, dram shop laws appear to reduce wine consumption and limits on the number of drinking establishments also may. Thus, different policy instruments appear to be better suited for limiting consumption for different types of alcohol. There is no single "silver bullet" that can be expected to reduce consumption of all alcohol types across the board. One thing is apparent: the widely advocated proscription of using excise taxes, and particularly beer excise taxes, as a means of mitigating the myriad adverse outcomes associated with alcohol consumption may be somewhat premature.

It must be stressed that several of the DUI laws are found to be positively correlated with per-capita consumption rates. A possible explanation for these findings (and others) might be endogeneity bias. That is, consumption rates may induce states to adopt laws governing the price and/or availability of alcohol (taxes and laws are mandated by legislative action that cannot be changed except with substantial time lags, however, so this may not be the explanation). If endogeneity is a factor using data from local jurisdictions rather than states may alleviate the problem, so an alternative data set is also examined in a later chapter of this report. Another problem is collinearity, however. Many of these laws have been established in the same states, so the zero-one dummies are highly correlated. Thus, caution must be taken in interpreting the results.

CHAPTER 4

THE EFFECTS OF ALCOHOL CONSUMPTION ON CRIME: EVIDENCE FROM STATE-LEVEL PANEL DATA

4.1 Introduction[32]

Several recent studies have shown that beer excise taxes and minimum legal drinking ages may serve as effective policy tools for lowering the incidence of some types of criminal activity (Chaloupka and Saffer 1992; Cook and Moore 1993a; Parker 1993; Parker and Rebhun 1995; Markowitz and Grossman 1998a, 1998b, 1999, 2000; and Markowitz 2000a, 2000b). The review in Chapter 2 suggests some of the theoretical and empirical shortcomings that these studies may suffer from. The previous chapter considered an expanded set of alcohol regulations and illustrated that several (e.g., the number of drinking establishments, perhaps some DUI penalties, taxes on liquor and possibly beer) may provide means for reducing alcohol consumption. Under the assumption that higher rates of alcohol consumption increase the rate of criminality, might these regulations also be effective crime-fighting policy tools? Not necessarily. The theoretical results presented in chapter two suggested that an increase in the full price of alcohol will tend to have an ambiguous effect on the equilibrium crime rate (even under the assumption of downward-sloping Marshallian demand curves for alcohol). As such, it is not certain that alcohol control policies will be effective in lowering rates of crime (even if they do lower alcohol consumption). Determining whether alcohol control laws lower the incidence of crime is therefore an empirical exercise.

---

[32]The first four sections of this chapter and parts of other sections are minor modifications of material in Zimmerman (2000).

The empirical methodology and data employed in this chapter addresses several of the shortcomings encountered in previous studies. This study first examines the effect of alcohol on crime by employing a consumption proxy, namely per-capita shipments of alcohol by state. Many previous studies, in contrast, have relied upon the use of alcohol *excise taxes* as a proxy for price in a reduced-form model and generally conclude that higher taxes will lead to fewer crimes (Cook and Moore 1993a; Markowitz and Grossman 1998a, 1998b, 1999, 2000; and Markowitz 2000b). However, the apparent effectiveness of beer excise taxes as a policy tool for mitigating the incidence of crime is somewhat surprising given two well-established empirical facts. First, beer taxes comprise only a small portion of the retail price of beer (Dee 1999). Indeed, recent studies of beer markets have found that beer excise taxes have only a relatively small impact on the money price of alcohol and that money price in turn has only a relatively small impact on consumption (Sass and Saurman 1993, Young and Likens 2000). Thus even relatively large percentage changes in tax levels would not be expected to raise retail beer prices (which consumption decisions are actually based on) appreciatively. Second, although consistently negative, direct estimates of the price elasticity of demand for alcohol are typically low [see the review of Lueng and Phelps (1993)] and especially so among the segment of the population that commits the most crime *and* does the heaviest drinking, i.e. young males (Sloan, et al. 1994a; Chaloupka and Wechsler 1996; Kenkel 1996).

A possible explanation for the apparent efficacy of excise taxes in previous empirical work is their failure to account for all the relevant determinants of both crime and alcohol consumption. As such, a large predicted effect of excise taxes on the dependent crime measure may be almost entirely spurious. The vast empirical literature on crime suggests that deterrence measures and labor market conditions (and/or variables capturing aspects of deprivation theory) are often important determinants of criminal activity. However, these types of controls have been largely ignored in previous empirical studies of alcohol and crime [although some studies, e.g., Parker (1993), have considered some of them]. With respect to alcohol consumption, non-tax determinants of alcohol price (and thus the quantity of alcohol consumed) are also typically left out of the empirical specification (except for drinking age which has often been considered). As shown in Chapter 3, some market structure characteristics and regulations will affect the cost of supplying alcohol and thus influence the retail price of alcoholic beverages. As such, all relevant determinants of equilibrium (alcohol) quantity should be included in the empirical model. Since shipments data will implicitly reflect both market demand *and* supply considerations (whereas excise tax data do not) they allow for a better determination of the *potential* effects of alcohol control policies on crime rates.

Another shortcoming of most previous studies is their reliance on reduced form crime specifications that potentially ignore important simultaneous relationships. This concern is of particular relevance to this study given that both alcohol consumption and criminal justice variables are employed in the empirical specification. Estimation results reported below on a series of endogeneity tests conducted on the basic crime equations make clear the need to develop a simultaneous-equations framework that (at least partially) controls for the numerous interdependencies between the alcohol consumption and crime-related variables.

Finally, previous work has not examined the effects of other alcohol beverage types. To date only beer or composite "consumption" measures (e.g., excise taxes as a proxy for alcohol price being substituted for consumption in a reduced-form specification) have been considered. This study also analyses the effects of liquor and wine consumption within individual, composite, and simultaneous consumption specifications. In addition, laws aimed at lowering the rate of drunk-driving increase the cost of engaging in drinking behavior outside of the home and apparently lower drunk driving fatalities (Benson, et al. 1999; Mast, et al. 1999). If many drinking related crimes occur outside the home, then DUI laws may also reduce the incidence of property and violent crime. This is the only study to date that has examined the effects of such DUI laws in an empirical model of criminal participation.

The remainder of the chapter proceeds as follows. The next section explains the methodology employed in conducting the empirical analysis. Section 4.3 discusses issues of endogeneity bias in estimating the basic structural crime equations where consumption and deterrence variables are treated as exogenous determinants. Section 4.4 summarizes the data and construction of the variables employed. Section 4.5 presents estimation results of crime supply functions where alcohol consumption and criminal justice variables are assumed to enter as an exogenous determinant. In section 4.6 endogeneity tests are conducted on all models to determine if parameter estimates suffer from simultaneity bias. It is shown that simultaneity bias is present in all crime models except murder. Based upon these tests, Chapter 5 presents ordinary least squares (OLS) semi-reduced-form estimation results for murder and two-stage least squares (2SLS) estimates for all other crime categories, examines the validity of the results presented in the simultaneous equations framework by presenting several tests of the instruments employed, presents the estimated impacts of the various alcohol control policies on the individual crime categories, and compares those results to the implications drawn from fully specified-reduced form equations that are typically employed it the related literature.

4.2 An Empirical Model of the Relationship Between Per-Capita Alcohol Consumption and Crime

Developing an empirical model of criminal participation requires the identification and specification of those factors that are most likely to be both empirically important and testable given the nature of the available data (Levitt and Lochner 1999). In this study we develop the following taxonomy to characterize the determinants of criminal behavior: deterrence factors, opportunity cost factors, demographic factors, and consumption factors. Each of these four categories is discussed below.

4.2.1 Deterrence Factors

Deterrence factors are those that constrain an individual's decision to engage in criminal behavior. These represent the expected cost/punishment a criminal will incur by devoting time to illegal activities. Under usual (and possibly restrictive) assumptions of risk aversion, a higher expected punishment reduces the number of crime committed.[33]

Estimating the magnitude of the effect of deterrent factors has been the major thrust of the research conducted in the economics of crime since Becker's (1968) theoretical model was first developed. This has also been a major research issue in criminology and other disciplines, of course, but given the economic approach adopted in the theoretical model presented in Chapter 1, we shall emphasize the economic analysis here as well. Recognizing the potential simultaneity

---

[33]Deterrence factors can be divided into "public" and "private" categories. Public deterrence refers to the set of laws that govern the administration of the criminal justice system and the punishment of offenders (e.g., capital punishment laws, expenditures on police and prisons, mandatory sentencing provisions, etc.). Private deterrence refers to the expenditures of individual citizens on goods and/or services that protect against criminal victimization (e.g., guns, alarm systems, gated communities, guard dogs, etc.) or any behavior that limits a potential victims exposure to crime. In addition, the level of aggregation to which deterrence factors are applied can be classified into either *general* or *specific* deterrence. General deterrence refers to the effect of community law enforcement efforts (such as the number of police officers on patrol) on individual behavior. Specific deterrence measures the effect of a given individual's previous punishment on his current behavior.

between crime rates and deterrence factors, early researchers often employed simultaneous equations methods using cross-sectional or time-series data [see Taylor (1978) for a review of these early studies], and Chapter 3 for a brief discussion. More recent studies (Cornwell and Trumbull 1994; Lott and Mustard 1997; Lott 1998; Levitt 1996; 1997; Benson, et al. 1998; Doyle, et al. 1999) have employed the use of pooled cross-sectional time-series data (allowing for estimation of fixed-effects modes to control for the effects of unobservable observation-specific heterogeneity) and instrumental variable estimators (to break the simultaneity between crime rates and the endogenous deterrence factors). The recent work of Steven Levitt (1996, 1998a and b) has been particularly influential in this regard. Levitt (1996) uses the timing of state prison-overcrowding legislation as an instrumental variable to predict state growth rates in prison populations and finds an elasticity of crime with respect to prisoner populations of approximately —0.10 for violent crimes and —0.30 for property crimes. Levitt (1998b) uses the timing of mayoral and gubernatorial elections as instrumental variables for the number of sworn police officers in a city. Using city-level U.C.R. data, Levitt estimates the elasticity of violent crime with respect to sworn police officers to be approximately unitary elastic (-1.0) and the elasticity for property crime to be relatively inelastic (approximately —0.3) when purged of their endogeneity with crime.

4.2.2 Opportunity Cost (or Deprivation Theory) Factors

Opportunity cost factors reflect the level of foregone income and/or consumption (i.e., the consumption that would be given up if caught and incarcerated for committing a crime) resulting from participation in criminal activities. Of course, higher values of these foregone alternatives will typically lead to a reduction in criminal participation.

Two of the most frequently used measures of the opportunity cost of criminal activity are the level of disposable income and the rate of unemployment (a proxy for the probability of gaining legitimate work), both of which may also be indicators of deprivation. However, the use of these variables suffers from two serious shortcomings. First, it is possible that variables such as per-capita income capture the *return* to criminal activity as well as the opportunity cost of (or deprivation of) potential criminals. In other words, higher wealth levels may actually capture the *expected benefit* of crime rather than the expected opportunity cost of crime. These possibilities suggest that the sign of such variables is uncertain. For instance, several studies have found a *negative* relation between rates of unemployment and crime (Trumbull 1989; Witte 1980; Doyle, et al. 1999). Second, there is some doubt as to what extent such measures can be construed as an exogenous source of variation in the crime equation. For instance, it may be the case that crime rates determine the level of local unemployment as firms may be less likely to locate in high-crime areas. Alternatively, high levels of per-capita income or wages may reflect compensating differentials realized by laborers for working in high-crime areas (Gould, et al. 1999). As such, the estimated coefficients on these factors may suffer from the same kind of simultaneity bias associated with deterrence factors.

4.2.3 Other Socioeconomic/Demographic (or Deprivation) Factors

Socioeconomic/demographic factors include such variables as age, race, ethnicity, and sex. Most studies have found that the strongest predictors of criminal involvement are being young and male, although the age distribution of violent crime arrests suggest that this is less true for violent crimes (Blumenstein 1985). Younger (male) individuals may be more likely to engage in crime due to such factors as risk-preference, myopia, peer-effects, and low human-capital accumulation. In fact, age may be the best single predictor of criminal involvement (Freeman

1996). Individuals belonging to minority groups may also have a greater tendency to commit crime due to labor market discrimination or because of inferior educational attainment (e.g., having attended lower "quality" schools relative to those in the majority group), as suggested by deprivation theory. Note that many of these factors will inherently share a strong interaction with the opportunity cost factors discussed previously.

4.2.4 Consumption Factors

Consumption factors are goods whose use affects an individual's criminal participation through psychological/physiological influence (e.g., by altering the brain's chemistry and thereby making an individual act more aggressive) or by lowering the expected cost of committing crimes (e.g., social norms might dictate that individuals are not fully responsible for their behavior while intoxicated). Examples include mood-altering substances such as alcohol, marijuana, cocaine, and other legal or illicit drugs. Criminal involvement for the purpose of financing the consumption of such commodities (e.g., because of addiction) and/or "employment" in an illegal market for their distribution (e.g., engaging in the drug trade, bootlegging alcohol, smuggling cigarettes across state borders, etc.) are not included as they are better classified under opportunity cost factors.

4.3 Issues of Endogeneity

Given the above determinants of crime, the first step in the empirical methodology will be the specification and estimation of supply of crime functions where alcohol consumption is directly entered as an explanatory variable:

$$CR = f(DET, OC, DEM, ALC) \qquad (4.1)$$

where the dependent variable (CR) is the crime rate. Based upon the above classification of explanatory variables, DET is a vector of deterrence factors, OC a vector of variables that determine the opportunity cost of criminal involvement, DEM a vector of socioeconomic/demographic controls, and ALC a measure of alcohol consumption.

The estimated coefficients of a crime supply function such as that specified in equation (4.1) may be biased and/or inconsistent in the presence of two types of bias: *omitted variable bias* and *simultaneity bias*. The nature and implications of each of these biases, and the steps used to overcome them, are discussed below.

4.3.1 Omitted Variable Bias

Omitted variables bias concerns the failure to adequately control for latent observation-specific factors which determine the level of *both* the dependent variable of interest (CR), and at least some of the other included explanatory measures (DET, OC, DEM, and ALC). Failure to account for this inherent heterogeneity across observations would lead to inconsistent estimates of the coefficients on the explanatory variables for the model specified in (4.1). This empirical bias is analogous to the theoretical "common cause" hypothesis discussed in Chapter Two and could give rise to a spurious correlation between alcohol consumption and crime.

The procedure used to account for omitted variable bias in the crime equation is the same as that used in the alcohol consumption equations presented in Chapter Three. To control for the unobserved observation-specific effects that are time-invariant, which may be considered fixed-effects, a vector of state identifiers (dichotomous indicators for each state) are included in the crime model represented in (4.1). The estimated coefficients may nevertheless be inconsistent by failing to account for the effects of time-varying factors that affect the level of the dependent variable across all observations symmetrically (e.g., national crime-prevention campaigns). For this reason (4.1) will also include a vector of year indicators.[34]

4.3.2 Simultaneity Bias

The inclusion of group and period identifiers eliminates the problem of omitted variable bias due to unobserved observation-specific heterogeneity, but it does not correct for the potential bias that may result from reverse causation between the dependent and explanatory measures. This simultaneity bias causes the error term of the empirical specification of (4.1) to be non-orthogonal to the endogenous explanatory measure. This in turn leads to inconsistent parameter estimates via ordinary least squares estimation.

As mentioned previously, deterrence measures may be partially determined by the level of crime and thus be endogenous to the crime equation. Simultaneity bias may also be introduced into the crime model by controlling for alcohol consumption. For instance, according to the *conditional model* of the alcohol-crime relationship, individuals may consume alcohol to facilitate (lower the cost of) engaging in criminal activity. Alternatively, individuals who reside in areas of high criminal activity may be more prone to the consumption of alcohol if drinking helps to bring down the high stress-levels associated with residing in such an area. In either case, the direction of causality is reversed and the estimated coefficient on alcohol consumption may be inconsistent.

To determine whether the basic crime model (with the inclusion of controls for fixed effects) is plagued by simultaneity bias, we follow Doyle, et al. (1999) and conduct Durbin-Wu-

---

[34]As noted earlier, fixed-effects models have recently gained widespread use in the economics of crime literature. Cornwell and Trumbull (1994) was the first study within the economics of crime literature to employ a fixed-effects model. Numerous other studies employing data at various levels of aggregation (Lott and Mustard 1997; Lott 1998; Levitt 1996, 1998a, 1998b; Benson, et al. 1998; Doyle, et al. 1999) have recently followed.

Hausman (hereafter DWH) endogeneity tests. Model specification tests of this sort rely on the identification of instrumental variables which determine the level of the potentially endogenous explanatory variable(s) but which are not correlated with the error term of the crime equation. If results of the DWH tests indicate that the deterrence and alcohol consumption controls are not exogenous to crime, a simultaneous equations framework should be employed for those specifications. Estimations via two-stage least squares (2SLS) are presented in Chapter 5 in an effort to derive consistent parameter estimates.

4.4 Data

The data set employed here consists of a panel of pooled cross-sectional time-series at the state level of aggregation. The data set includes the years 1985-1994. The dependent and independent variables and their limitations are discussed below. Specific data sources and descriptive statistics of all variables employed in the chapter can be found in Appendix A.

4.4.1 Dependent Variables

The dependent measures used in the empirical analysis are state-level violent Index Crimes collected by the F.B.I. in the Uniform Crime Reports (UCR). Reported violent crimes consist of the offenses of murder, rape, robbery, and aggravated assault. Each individual crime category as well as an aggregate violent crime category will be used as dependent measures. The five dependent variables are per-capita crime rates of the form:

$$CR_{ji,t} = \frac{TOTCRIME_{j,i,t}}{TOTPOP_{i,t}}$$

where TOTCRIME denotes the total number of crimes of type j in the ith state in t^{th} year and TOTPOP denotes the total state population.

The influential review of Brier and Fienberg (1980) criticized the use of UCR data in estimating supply of crime functions. UCR data consists only of offenses which are reported to (and actually recorded by) authorities within a given state or local law enforcement agency. These data are then *voluntarily* reported by these agencies to the FBI. Of course, the UCR data will tend to not correspond to the *actual* number of offenses committed. A large portion of crimes committed in a given jurisdiction are not reported to law enforcement agencies although in the case of murder the number will be close to the "actual" rate. In addition, it has been shown that crime reporting behavior at both the victim and agency levels can vary dramatically across jurisdictions [see Skogan (1976)]. Thus, any statistical analysis using UCR data must account for the potential biasing effects of reporting behavior.

Another shortcoming of UCR data is that they do not provide an accurate assessment of crimes committed *per offender*. For instance, in the process of attempting to break into a car, an offender might assault the vehicle owner who walks in on him/her unexpectedly. Upon investigation, the police presumably record only the most serious offense (the assault) committed. Crimes with multiple victims may also be recorded as a single offense. For example, if an offender robs a group of tourists the event may get recorded as a "single" robbery. Alternatively, one recorded criminal event may actually involve multiple offenders (e.g., a gang of youths assaulting an individual).

Finally, some reported crimes might be "non-deterrable" offenses. For instance, Glaser (1977) argues that murders committed without premeditation (e.g., those committed during an interpersonal disputes or "crimes of passion") should not be included in the estimation of a murder supply function. While legal standards differentiate between premeditated (first-degree) and non-premeditated (second-degree) murders, UCR murder statistics do not make any such distinction. Likewise, some non-premeditated offenses occur without any relationship between the offender and the victim whatsoever. Examples of such murders include serial or mass killings committed by mentally disturbed individuals. From a statistical perspective, such offenses appear as purely random events (Rubin, et al. 1999). It may be the case that non-premeditated violent crimes do not fit within the confines of the traditional deterrence model, because the individual would have chosen to commit the crime no matter how high the level of deterrence factors. Inclusion of such offenses in the empirical model, which cannot be avoided, may bias coefficients on deterrence measures, and it is expected that such incidents will tend to be more common for violent crimes rather than property crimes since the later are typically undertaken for monetary gain.

The use of fixed-effects models to estimate the crime equation may also help attenuate the problems with using UCR data, at least to a degree, if incomplete reporting behavior of victims of crime and law enforcement agencies if reporting tendencies are relatively constant over time (Bound and Krueger 1991).[35]

[35]Another alternative to using aggregate-level UCR data might be individual-level victimization surveys as employed in Markowitz and Grossman (1998a, 1998b, 2000), Grossman and Markowitz (1999), and Markowitz (2000a, 2000b). However, the use of aggregate-level data has several distinct advantages over individual-level data. Recent theoretical work in the economics of crime has emphasized that one individual's decision to participate in criminal activity may be determined by another's choice (Glaeser, et al. 1996, Marceau 1997, Schrag and Scotchmer 1997, Tabarrok 1997). Individual-level crime data may not be able to capture the extent of these externality effects (although this might not be of particular importance to the incidence of intra-family crimes such as spousal and child abuse). A second advantage to aggregate data is that important socioeconomic/demographic characteristics that are likely to influence the level of crime can be better controlled for, such as the sex and age distribution across the population (Gould, et al. 1999). Third, and most important, individual victimization data are not geographical disaggregated, so it is impossible to evaluate the impact of deterrence and opportunity costs on crime.

4.4.2 Independent Variables

Deterrence variables. Four variables are used across all crime specifications to control for deterrent factors (DET). The first of these, ARRESTS, is the crime-specific arrest rate defined as

$$ARREST_{j,i,t} = \frac{TOTARREST_{j,i,t}}{TOTCRIME_{j,i,t}}$$

where TOTARREST is the total number of arrests made for crime j in state i during period t. Note that using this arrest measure may be problematic since TOTCRIME appears both the left and right-hand sides of the crime equation. This would tend to introduce a ratio bias on the estimated coefficient of ARREST. For instance, if one year the number of reported crimes (TOTCRIME) were to fall while the actual number of crimes remained constant, the arrest rate as defined above would be biased upward and incorrectly "explain" the lower reported crime rate. A technique for identifying the presence of this ratio bias in crime data has recently been developed by Levitt (1998a) who shows that state-level U.C.R. data is not susceptible to this problem.

The second deterrence variable, POLICE, is constructed as the number of persons employed in full-time equivalent police protection services divided by the number of persons ages 18 and over. The expected sign on POLICE is ambiguous *a priori*. If higher police presence deters criminal activity, then the expected sign is negative. If, on the other hand, individuals are more likely to report crimes the greater the number of police (e.g., because they believe larger police forces are more likely to solve crimes) then the expected sign is positive.

Following Levitt (1999) and others, we include the third deterrence measure, PRISON, defined as the number of persons incarcerated in state prisons divided by the number of persons ages eighteen and over.[36] *Ceteris paribus*, the expected sign of this variable is ambiguous. Higher prison populations might be negatively correlated with crime via deterrence or incapacitation effects. On the other hand, while in prison convicts might accumulate crime-enhancing skills (e.g., through interaction with older, more experienced criminals). This acquisition of criminal human capital, coupled with depreciated human capital, would decrease the expected return in legitimate income-generating activities relative to illegitimate ones and thus lead to more crime.

The final deterrence measure included in all crime regressions, NRA, is defined as the total number of persons in a state holding registered membership in the National Rifle Association divided by the number of persons eighteen years of age and older. This variable is included as a potential measure of private deterrence. The expected sign on this variable is ambiguous *a priori*. Assuming NRA membership reflects the level state gun-ownership, the *facilitation effect* holds that higher gun ownership levels will increase criminal's access to guns through theft, overpowering victims, or the black market (Cook 1981b; Kellermann, et al. 1995; McDowall, et al. 1995; Cook and Ludwig 1996; Cook and Leitzel 1996; Hemenway 1997; Ludwig 1998; Dezhbakhsh and Rubin 1999). Alternatively, the *deterrence effect* maintains that higher levels of gun ownership reduces the incidence of crime since a criminal is subject to greater uncertainty regarding the potential armed response of a victim (Kleck and Patterson 1993; Polsby 1994, 1995; Lott and Mustard 1997; Lott 1998).[37]

---

[36]Levitt (1996) notes that ideally one would like to control for jail populations as well. Jail population data are updated only every five years. In addition, Levitt notes that only one percent of state prisoners are held in jails due to prison overcrowding.

[37]The controversial studies of Lott and Mustard (1997) and Lott (1998) relied upon the use of dummy variables reflecting the adoption of laws with shall-issue provisions of concealed handgun ownership. We rely on the use of the *NRA* variable for two reasons. First, Dezhbakhsh and Rubin (1999) have shown that the Lott-Mustard use of state

Two other measures of deterrence are included in the murder specifications. The first is a proxy of the probability of being sentenced to death given that a criminal has been arrested for murder. The Bureau of Justice Statistics no longer publishes conviction data. An alternative to using conviction data is to use sentencing data. State sentencing data covers the stages of the legal process between the date of apprehension and when the execution is meted out. The probability of being sentenced to death for murder given arrest, $P_{c|a}$, is defined as

$$P_{c|a} = \frac{(\#\ persons\ sentenced\ to\ death)_{i,t}}{(\#\ arrests\ for\ murder)_{i,t-2}}$$

assuming a two period lag between the arrest date for murder and the subsequent date of sentence. We define the probability of execution given conviction, $P_{e|c}$, as

$$P_{e|c} = \frac{(\#\ persons\ executed\ for\ murder)_{i,t}}{(\#\ persons\ senteced\ to\ death)_{i,t-6}}$$

Bedau (1997) finds the average time between sentencing and execution is six years. Note that this time lag is incorporated into the above measure.[38]

Opportunity cost (deprivation theory) variables. Three variables are included to control for the opportunity cost of crime. The first is the state unemployment rate, UR. To the extent that this variable reflects the probability of gaining legal employment, its expected sign is

dummy variables might introduce model misspecification. Second, the dichotomous nature of several of the instrumental variables used in the 2SLS of the analysis conducted here did not allow for the additional inclusion of the gun dummies.

[38] Some states with death penalties had years in which no offenders were sentenced to death. Thus, some of the constructed execution probabilities were undefined since the denominator takes a value of zero. Following Rubin, et al. (1999), for any states where the execution probability was undefined the probability from the most recent year within the past four years was substituted. For Louisiana, the constructed execution probability took an extremely large value (3.0) in one year. Estimation results were extremely sensitive to this large outlier. As such, the average execution probability of this observation and the next closest (within four years) was used instead. This resulted in a value of 1.6, rather than 3.0. Of course, values greater than 1.0 cannot be actual probabilities, but this measure is consistent with Sah (1991) who argues that criminals use the most proximate information available in developing their subjective probabilities [see Rubin, et al. (1999, p. 13)].

positive. Another possibility is that individuals who are unemployed commit some crimes for the purposes of venting their frustration or anger over not gaining work. On the other hand, higher rates of unemployment may reduce the scope of an individual's routine activities (i.e., lower their probability of victimization). Or, higher unemployment may serve the role of a deterrent factor since it results in a greater number of persons staying at home and thus increases the probability of an offender-victim confrontation that discourages crimes such as burglary.

The second opportunity cost variable, INCOME, is defined as the level of per-capita disposable income. This measure is adjusted for inflation and interstate cost-of-living (COL) differentials.[39] Again, the expected sign on this measure is ambiguous. To the extent that this variable measures legitimate-income generating opportunities for potential criminals, its expected sign is negative. However, if instead this variable reflects the expected payoff to committing a crime, then the expected sign is positive. Of course, if private deterrence measures are normal goods (e.g., home security systems, gated housing communities, guard dogs, etc.), then this serves to diminish the magnitude of any positive effect from the latter explanation.

Freeman (1996) finds that most criminals are young, male, and have low levels of educational attainment. These persons are most likely to be responsive to changes in the level of wages in low-skilled sectors of the economy. The variable WAGELOW, serves as a proxy for the average hourly wage across three low-skilled employment sectors: wholesale trade, retail trade, and services. This wage measure might also serve as a better indicator of the agent's long-term

[39]Dumond, et al. (1999) argue that COL indexes overestimate differences in wages across metropolitan areas due to COL by about sixty-percent because of differences in demand across these areas. As such, per-capita income was adjusted by forty-percent of COL.

earnings potential than does unemployment, which tends to be relatively short-lived and highly cyclical (Gould, et al. 1999).[40]

Socioeconomic/demographic (and/or deprivation theory) variables. Five variables are employed to capture the socioeconomic/demographic factors hypothesized to influence the level of crime. The first, M1844, measures the proportion of the male state population that is male and between the ages of 18 and 44. This variable is expected to be positively correlated with most crimes. The variable GINI measures the extent of state income inequality. This variable is derived from the income distribution of families. The expected sign of this variable cannot be determined *a priori*. It is possible that the greater the extent of state income inequality, the higher is the sense of social "injustice" among those at the lower end of the distribution and the greater their tendency to commit crime. Doyle, et al. (1999) find that this measure takes both positive and negative values across different model specifications but is never statistically significant. However, the authors only consider the general cases of total property and violent crimes. The theoretical analysis of Chiu and Madden (1998) shows how under a particular set of assumptions income inequality might increase some types of property crime (e.g., burglary). As such, considering the effects of income inequality on separate violent crime categories also may be more appropriate.

The percentage of the state's population that is black, PERBLK, is included for two reasons. The first is to control for the lower quality of educational inputs traditionally attained by

---

40Various industry or individual-specific wage measures have been found to be negatively correlated with crime or recidivism rates [see Trumbull (1989); Witte (1980); Cornwell and Trumbull (1994); Grogger (1991); Doyle, et al. (1999), Gould, et al. (1999)]. Using county-level data, Gould, et al. (1999) attempt to purge the endogeneity of wages with respect to crime using instrumental variables estimation and generally identify negative correlations. Lochner (1999) argues that the use of wages as an opportunity cost measure for criminal involvement may be inappropriate since they do not accurately measure "potential" wages. That is, observed wages will tend to be less than potential wages since individuals pay for general human capital by initially accepting lower wages.

African-Americans and other minorities relative to whites. Inferior educational attainment might lower the ability of blacks to enter into legitimate high-skill jobs. In addition, if blacks are discriminated against in the labor market, then their probability of gaining legitimate employment is diminished. In either case, this variable is expected to be positively correlated with the crime rate.

NONMET measures the proportion of the state's population living in non-metropolitan areas as designated by the Census Bureau. The concentration of targets, relative degree of anonymity, and other characteristics of metropolitan areas probably tend to lower the expected cost of crime, all else equal, so relatively rural environments are expected to have less crime. Thus, the expected sign on the coefficient of this variable is negative.

Ayers and Donohue (1999) note that over the late 1980's and early 1990's (the time period of this analysis) crime rates rose dramatically in areas where markets for crack cocaine developed. The authors criticize Lott (1998) in not controlling for the development of crack markets and argue "since crack is an omitted variable that affects crime, its omission from the crime regression will bias the estimated effect of shall issue laws if states where crack was prevalent shunned shall issue laws" (Ayers and Donohue 1999, pp. 444-445). At the current time, no adequate price or quantity data exist for the crack trade over the period of this analysis. However, drug related activities tend to be relatively high within inner cities. As such, we include a measure of population density (POPDENS) to help control for the emergence of the crack trade over our sample. To this extent the expected sign of POPDENS is positive. However, the probability of offender recognition may be higher in more densely populated areas.[41]

[41] Of course, one might also argue that cities tend to promote social anonymity. It is recognized that population density in a state maybe a poor proxy for the population density of cities.

__Consumption variables__. The main independent variables of interest are the controls for alcohol consumption. Three measures of per-capita alcohol consumption are considered: beer (BEER), wine (WINE), and distilled spirits (LIQ). These are the same variables used as the dependent measures in the alcohol consumption equations of chapter three. In addition to analyzing the effects of each consumption measure on the various crime categories separately, a total per-capita consumption (TOTALC) and inclusive specification (all three categories controlled for in one equation) will be examined as well. As suggested earlier, higher alcohol consumption may affect individual myopia and risk preferences and lead to a greater tendency to commit crime. Another possibility is that higher consumption increases the probability of victimization since persons who have been drinking may appear to be "easier" targets. A third possibility is that individuals consume alcohol before committing crimes in order to use their consumption as an excuse for their aberrant behavior if apprehended. As such, the expected signs on the various consumption measures are positive.

Finally, following Rubin, et al. (1999) in the murder regressions the number of robberies per-capita (ROBB) and the number of burglaries per-capita (BURG) are also included as explanatory measures. Some murders may result out of the commission of such crimes and thus they should be included in estimating the murder supply equation. In addition, Forst, et al. (1978) and McKee and Sesnowitz (1977) as cited by Rubin, et al. (1999) find that the magnitude of the effect on deterrence variables particular for murder fall dramatically when other crimes are controlled for.

__4.5 Estimation Results of Index I Crime Models with Exogenous Alcohol Consumption__

This section presents the estimation results of the violent crime models where alcohol consumption is modeled as an exogenous determinant. Five specifications are estimated for each crime category and differ in the included measure of alcohol consumption. The first specification only controls for per-capita beer consumption, the second only for liquor, and the third only for wine. These will be referred to as the "individual" specifications. The fourth specification aggregates the three consumption measures to determine the effects of total alcohol consumption on crime. The fifth specification includes all three separate consumption proxies entered simultaneously. These models will be referred to the "total" and 'inclusive' specifications, respectively.

All dependent measures and independent variables except for the probability of conviction given arrest and the probability of execution given conviction are estimated after taking the logarithmic transformation. As such, the estimated coefficients on these variables may be interpreted as elasticities. All monetary variables are adjusted for inflation and interstate cost-of-living differentials. Finally, all models control for state and year fixed effects (coefficients on these dummy variables are not reported) and all are estimated via ordinary least squares (OLS).[42]

4.5.1. Per-capita Murders

Table 4.1 presents the two-way fixed effects (2WFE) results where the dependent variable is defined as the number of per-capita murders and the various measures of alcohol consumption and deterrence are assumed to enter the specification as exogenous determinants. The murder arrest rate, ARRMUR, is found to be negative and statistically significant in all five specifications. A ten percent increase in the arrest rate for murder is predicted to decrease per-capita murders by approximately one percent. Per-capita police employment and prison populations are never statistically significant at conventional levels. The probability of obtaining

[42] We have tested these models for auto correlation and heteroskedasticity and neither appears to pose a problem. Durbin-Watson statistics are close to 2.00, suggesting no first-order auto correlation. Using the method of White (1978) to correct for heteroskedasticity yield very little change in t-values. Therefore, corrections are not called for.

a death sentence conviction for murder conditional on arrest ($P_{c|a}$) takes the expected negative sign and is marginally significant (at the ten percent level) in three of the specifications. The probability of execution for murder given conviction ($P_{e|c}$) is found to also take a negative sign across all specifications as expected but is never statistically significant. State membership in the N.R.A. is found to be positively correlated with per-capita murders in all specifications but also is never significant. It must be noted that all of these deterrence variables are very sensitive to specification and/or data periods, however. For instance, Zimmerman (2000) employs data through 1992 rather than 1994, as used here, and finds significant positive coefficients for NRA and significant negative coefficients for $P_{c|a}$ in all equations. Furthermore, excluding the observations where the proxy for the arrest rate exceeds 1.0 makes the arrest rate coefficient and the coefficient on $P_{c|a}$ insignificant with the data employed here, while the coefficient on $P_{e|c}$ becomes significant. This lack of robustness suggests considerable caution in interpreting these findings.

Real COL-adjusted per-capita disposable income (INCOME) and state unemployment rates are both insignificant in all specifications. Low-skill wages (WAGELOW) are similarly always statistically insignificant but it has a positive estimated coefficient.

As expected, the proportion of the state population over the age of eighteen that is male and between the ages of eighteen and forty-four (M1844) is positively correlated with per-capita murders and it is always at least marginally significant (at the 10 percent level of confidence or higher). The percentage of the state population that is African-American (PERBLK) is found to be negatively correlated with per-capita murders but it is statistically significant in only two specifications. Greater state income disparity as measured by GINI is also found to carry a negative sign across all specifications and it is marginally significant in all specifications except the inclusive model. NONMET takes a negative estimated coefficient in all models but is never statistically significant. Finally, state population density is consistently positive but never found to have a statistically discernable effect on per-capita murders.

The variables ROBBERY and BURGLARY were included in the murder models to control for the tendency of these crimes to result in the death of the intended victim. As expected, each of these controls carries a positive estimated coefficient and both are statistically significant at conventional levels. Specifically, a ten-percent increase in the per-capita incidence of robbery is expected to increase per-capita murders by approximately three percent while a ten percent increase in burglaries produces about a two percent increase in murders per-capita.

The main explanatory variables of interest are the per-capita alcohol consumption controls. Interestingly, per-capita beer consumption (BEER) is found to be negatively correlated with per-capita murders, and it is marginally significant in the inclusive specification.[43] Per-capita liquor consumption (LIQ) is found to be positively correlated with per-capita murders, however, and it also is marginally significant in the inclusive specification. A ten percent increase in per-capita consumption of liquor is predicted to result in approximately four percent more murders per-capita. Per-capita wine consumption (WINE) also is positively correlated with

[43]In Zimmerman (2000), where data through 1992 are employed, the negative coefficient is significant, however. Cook and Moore (1993a) also report that the relationship between beer and murder differed from the relationship between beer and other violent crime categories, but Parker (1993), and Parker and Rebhun (1995) find a significant relationship between measures of homicide and beer consumption. Chaloupka and Saffer (1992) report a significant impact of beer taxes on murder. Clearly, the alcohol/murder relationship is sensitive to data and/or model specification. One can only speculate about the result found here, but it may be indicative of a missing variable problem. For instance, there is some evidence that beer is a substitute for some illicit drugs (DiNardo and Lemieux 1992, Chaloupka and Laixuthai 1997, Model 1993) so low beer consumption could correlate with relatively high levels of drug market activity. In addition, beer tends to be the alcoholic drink of choice among younger persons, but the age distribution of arrests for murder tends to be skewed in the direction of older offenders. Alternatively, beer could simply be a substitute for other alcohol beverage types, suggesting that controls for other types of alcohol should be considered in the murder specification. To this extent, note the discussion of that per-capita liquor consumption coefficient.

per-capita murders but it is not statistically significant. The variable TOTALC measures the sum of beer, liquor, and wine consumption per-capita. As with beer, this measure is found to be negatively correlated with per-capita murders but it is statistically insignificant. This is most likely due to the fact that beer shipments constitute the majority of total alcohol shipments to states.

These results suggest that if alcohol influences the likelihood of murders at all, it is liquor rather than beer or wine that matters. Thus, any policy tools that aim directly at beer [e.g., beer excise taxes, as advocated by some researchers] may have no impact on reducing murders (it may even increase murders if people substitute liquor for beer and there actually is a causal link between liquor and murder).[44]

4.5.2. Per-capita Rapes

Table 4.2 presents the 2WFE estimation results where the per-capita number of rapes is employed as the explanatory variable. ARREST is found to be negatively correlated with per-capita rapes as expected and is statistically significant across all specifications. A ten-percent increase in the rape arrest rate is expected to result in approximately 0.7 percent fewer rapes per-capita. Per-capita police employment is found to be positively correlated with per-capita rapes but it is statistically insignificant across all specifications. Per-capita prison populations are found to be negatively correlated with per-capita rapes across all specifications and are always statistically significant. A ten-percent increase in the level of this variable is expected to decrease per-capita rapes between 1.5 and 2.2 percent. The estimated coefficient of state N.R.A.

[44] For relatively infrequent crimes such as murder and rape, it is possible that variation in the crime rates in small states might affect the results. We tested this hypotheses by removing North Dakota, South Dakota, Vermont, Wyoming, and Delaware. In most regressions there is no difference in the results and when the results are sensitive to this change in specification our overall conclusions are unaffected. Indeed, they are probably strengthened since some apparently significant relationships between alcohol and violence disappear.

membership is always positive and statistically significant. This appears to contradict the deterrence hypothesis for private gun ownership proposed by Lott and Mustard (1997) and others, but it should be noted that this coefficient is sensitive to the data period employed [e.g., Zimmerman (2000) reports an insignificant relationship using data through 1992].

The variable INCOME is found to be positively correlated with per-capita rapes in all alcohol specifications but it is always statistically insignificant. Contrary to expectations, the variable M1844 is negatively correlated with per-capita rapes in all specifications and it is always at least marginally significant. These surprising results may point to the importance of alcohol in rape. While most studies find that the portion of the population that is young and male is an important explanatory variable, it may be that it is alcohol consumed by these individuals that is a key, since the relationship between liquor and rape appears to be quite strong, as discussed below. The percentage of the population that is African-American is never significantly correlated with per-capita rapes. Non-metropolitan population is positive in all alcohol specifications and significant in those specifications that do not include liquor as an explanatory variable. State population density if found to be negatively correlated with per-capita rapes in all specifications and is always statistically significant. This is consistent with the hypothesis that higher population density increases the likelihood of offender recognition and thus decreases the incidence of rape. A ten percent increase in this variable is expected to decrease per-capita rapes by approximately seven percent.

Wine consumption apparently has no significant relationship to rapes per-capita, but beer consumption might, and liquor consumption appears to be important explanatory variable. The beer coefficient is significant and positive in the beer equation but it is insignificant in the inclusive model. On the other hand, the liquor coefficient is positive and significant in both the liquor and the inclusive equations. Since beer consumption and liquor consumption are correlated to a degree the significant beer coefficient in the first column of Table 4.2 may arise because the liquor variable is missing. Once again, the efficacy of policies targeted at beer come into question. A ten percent increase in liquor consumption appears to lead to a five percent increase in rapes, however. The estimated elasticity on TOTALC also takes the expected positive sign and is statistically significant. A ten percent increase in total alcohol consumption is predicted to increase per-capita rapes by approximately seven percent.

4.5.3 Per-capita Robberies

Table 4.3 presents the results when per-capita robbery is the dependent variable. The robbery arrest rate is found to be negatively correlated with per-capita robberies in all specifications but is never statistically significant. These results probably reflect the endogeneity of this variable. Per-capita police employment is found to be positive in each specification and it is always statistically significant, suggesting that victims may report more robberies as police employment rises. PRISON is negatively correlated with per-capita robberies and is statistically significant in each specification, supporting the deterrence hypothesis. State N.R.A. membership is never statistically significant.

The variable INCOME is found to be negatively correlated with the dependent variable in all specifications and it is marginally significant in the last two specifications. State unemployment rates are clearly sensitive to the alcohol variables included. The signs are negative in the first three models but only statistically significant in the third (wine) specification, and then positive and insignificant in the last two models. Low-skill wages are positive, but lack of significance implies that they are predicted to have no discernable effect on the per-capita incidence of robbery.

As expected, M1844 is positive and statistically significant in all model specifications. NONMET is found to be negatively correlated with per-capita robberies in each specification but it is statistically insignificant. The estimated coefficients on state population density are never statistically significant either. GINI is negatively correlated with per-capita robberies in each specification and statistically significant. Finally, the estimated coefficients of PERBLK are negative and statistically significant in all specifications.

Alcohol consumption appears to be important in explaining robbery rates. The estimated elasticities on per-capita beer, liquor, and wine consumption are each positive and statistically significant in their respective specifications. A ten percent increase in the level of each is predicted to increase per-capita robberies by approximately nine, five, and two percent respectively. The total alcohol consumption coefficient carries a statistically significant positive coefficient and takes a value of greater than one. A ten percent increase in TOTALC is predicted to increase per-capita robberies by approximately eleven percent. Finally, in the inclusive specification all three consumption categories are positive, but only beer and liquor are statistically significant at conventional levels. A ten percent increase in per-capita beer or liquor is predicted to increase per-capita robberies by approximately seven and four percent respectively.

4.5.4 Per-capita Assaults

Table 4.4 presents the estimation results for per-capita aggravated assaults. Contrary to deterrence theory, ARREST and PRISON are insignificant in each specification, although ARREST is consistently negative. POLICE is positive in all equations and marginally significant in the liquor and wine specifications. States N.R.A. membership is consistently negative but never significant.

INCOME and WAGELOW are never statistically significant but the unemployment coefficients are always significantly negative. The variable M1844 is always positive and statistically significant, as expected. NONMET and POPDEN are always negative, NONMET is always statistically significant, and POPDEN is at least marginally significant in all specifications except the total model. PERBLK always takes the unanticipated negative sign and is always statistically significant. Finally, higher levels of state income inequality are predicted to have a positive effect on per-capita assaults, and the result is always at least marginally significant.

As with the other crime rates, wine consumption appears to be irrelevant as an explanatory variable. Other than this, however, the effects of alcohol consumption on per-capita assaults appear to be somewhat different than for other types of violent crime. BEER takes a positive sign and is statistically significant in both the beer and inclusive specifications. A ten-percent increase in BEER is predicted to increase per-capita assaults by approximately ten percent. On the other hand, liquor consumption is not statistically significant in either the liquor or the inclusive specifications. As would be expected given the previous results, TOTALC is positively correlated with per-capita assaults and statistically significant, apparently due to the large effect of beer. A ten percent increase in total per-capita alcohol consumption is predicted to increase the number of assaults per-capita by approximately ten percent.

4.5.5. Total Per-Capita Violent Crimes

Table 4.5 presents the results for the total violent crime models. Since assault is the largest component of this aggregate measure of violence, the results are quite consistent with those reported in Table 4.4. The total violent crime arrest rate, ARREST, is positive in each case but never statistically significant. These results may reflect the potential endogeneity of this variable with respect to the total violent crime rate. The coefficient on PRISON is consistent with deterrence theory, however. It is always negative and at least marginally significant in every regression except the inclusive specification. Police employment per-capita is positive in each specification and at least marginally significant in the beer, liquor and wine specifications. N.R.A. membership is negative, but only marginally significant in the liquor specification.

The estimated coefficient on per-capita disposable income and WAGELOW are statistically insignificant in all the models. Higher state unemployment rates are predicted to decrease the per-capita number of total violent crimes in each specification. Specifically, a ten percent increase in the rate of unemployment is predicted to decrease per-capita total violent crimes by approximately one percent.

As predicted, the estimated coefficients on the variable M1844 are positive in each specification and always statistically significant. In each case the estimated elasticity associated with this variable is near three. The variables NONMET and POPDEN are negatively correlated with per-capita total violent crimes in each specification and are always statistically significant. PERBLK is also always significantly negative. In the beer specification, this variable is negative and statistically significant. Finally, state income inequality as measured by GINI is consistently positive but always insignificant, as in Doyle, et al. (1999).

Positive and statistically significant estimated coefficients are found on the respective consumption measures in the beer and liquor models, but not in the wine specification. A ten percent increase in the level of per-capita beer (liquor) consumption is predicted to increase the per-capita number of total violent crimes by approximately nine (three) percent. In the inclusive specification, however, only BEER is statistically significant at conventional levels. TOTALC is also found to be a positive and statistically significant determinant of per-capita total violent crimes. A ten percent increase in TOTALC is predicted to increase per-capita total violent crimes by approximately 9.5 percent.

4.5.6. Summing Up

The results of the full sample 2WFE-OLS regression models indicate that the consumption of some types of alcoholic beverages may be an important determinant of participation in or victimization in some criminal activities. Perhaps more importantly, they suggest that whatever the alcohol crime relationship may be, it varies across crime types. Wine consumption never appears to matter for any crime category, for instance, and alcohol in general appears to have very little impact on murder (although liquor may be a factor). On the other hand, liquor consumption apparently is a significant determinant of rape while beer probably is not, beer consumption appears to be strongly related to assaults while liquor has no significant influence, and both beer and liquor apparently influence the level of robbery. It seems that either the people involved in or the circumstances that produce a significant number of the different types of violent crimes are predominantly consumers of different types of alcohol. Two lessons come from this analysis then. First, in considering the impact of alcohol on violence, aggregating violent crime categories will produce results that do not generalize across crime types. The crimes should be disaggregated as much as possible. Second, alcohol should also be disaggregated, and both liquor and beer consumption should be seen as possible policy targets depending on which types of criminal activities are being targeted. Aggregating alcohol or focusing on only one type of alcohol (e.g., beer) will not produce results that generalize either.

The regressions reported in this chapter support the hypothesis that alcohol is a factor that influence violent behavior, but they do not tell us much about the efficacy of various alcohol policy alternatives. It is tempting to combine these results with those from the previous chapter, of course, treating alcohol consumption and crime rate equations as a recursive model. This would imply, for example, that higher liquor taxes should reduce rapes and robberies. Indeed, if a ten percent increase in liquor taxes reduces liquor consumption by four percent, as suggested by the second column of Table 3.2, then rapes might be expected to decline by about two percent, given the coefficient on LIQ in Table 4.2. There are potential problems with such interpretations, however. For instance, some alcohol control policies may not appear to be significant determinants of the level of alcohol consumption but they may still have an impact on crime because they lead to substitutions between at-home and away-from-home drinking. For instance, if policies against drunk driving induce people to drink more at home and less where they must drive, then to the degree that alcohol consumption affects some violent crimes that tend to occur when victims and perpetrators are away from home (e.g., robbery), that crime type may decline even with no reduction in consumption of alcohol. On the other hand, crimes involving domestic violence (family assaults) may rise. Therefore, it may be appropriate to consider the impact of alcohol control variables in reduced form crime equations (note that if some variables appear to affect crime but not consumption support for the *conditional* and/or *interactive* hypotheses regarding the alcohol-crime relationship would be implied). In addition, while these models are corrected for omitted variable bias, to the degree that is possible, their parameter estimates may still be inconsistent due to simultaneity. If this is the case, then the equations estimated in this chapter and the previous one should be re-estimated with simultaneous estimation techniques. Durbin-Hausman-Wu (DWH) endogeneity tests are therefore estimated for all models to determine if the estimated coefficients in the structural crime equations potentially suffer from simultaneity bias.[45]

4.6 Endogeneity Tests

We consider two sets of independent variables as being potentially endogenous to crime rates. The first set consists of the deterrence factors. Specifically, we shall consider ARREST, POLICE, and PRISON as potentially endogenous in all specifications. In the murder specifications, $P_{c|a}$ and $P_{e|c}$ will also be considered potentially endogenous. We do not treat NRA as endogenous since membership in the National Rifle Association is more likely to be influenced by political ideology than by crime rates. The second set of variables consist of the alcohol consumption factors (BEER, LIQUOR, WINE, and TOTALC).[46]

The first step of the DWH test procedure is to identify instrumental variables for each of the potentially endogenous explanatory variables. An instrumental variable (IV) is any measure that is correlated with the variation of the endogenous explanatory variable but is uncorrelated with the variation in the dependent measure. Doyle, et al. (1999) used the total violent crime arrest rate as an instrumental variable for ARREST in their property crime models. For this to be a suitable IV they assume that criminals do not substitute between property and violent crimes [support for which is found in Levitt (1998a)] and that the total violent crime arrest rate is uncorrelated with the error term in the property crime models. Doyle, et al. (1999, p. 723) argue:

[45]For a detailed discussion of this testing procedure the reader is referred to Davidson and MacKinnon (1993).

[46]Again, it is arguable that the opportunity cost of crime factors are also endogenous to the crime equation, and that the DUI laws are endogenous to the alcohol consumption equations. However, controlling for the potential endogeneity of these factors is not pursued here due to the difficulty in identifying appropriate instrumental variables for these measures and to facilitate comparisons of the results found here to those of previous studies.

> It is unlikely that a large shock to property crime would elicit increased efforts to police the streets that would in turn increase the likelihood that a violent crime results in arrest. Instead, a large shock to property crime is typically met by changes in people's behavior that lower their chances of being victims of property crime, such as locking the doors to their cars and homes and installing auto-theft detection devices and the like. In fact, well under 20% of all property crimes result in arrest.

In this context, we shall use the total property crime arrest rate (TOTARR) as an IV estimator for ARREST in our violent crime models. This IV estimator may be more problematic since large increases in violent crimes might lead to more intensive patrolling of streets by the authorities. This in turn could change the probability of apprehension for committing property crimes and would disqualify TOTARR as a true IV estimator (Doyle, et al. 1999, p. 737). However, no other obvious candidates as an IV estimator presents itself.

Per-capita personal tax revenue (TAX) is used as an IV estimator for per-capita police employment. It is assumed that states whose residents have stronger preferences for law enforcement will express their preferences by voting for tax increases that fund greater expenditures on police (Cornwell and Trumbull 1994).

Following the strategy of Levitt (1999) we use a set of dummy variables indicating the status of state prison overcrowding litigation as IV estimators for PRISON. Levitt's study covered a longer time horizon than this one, and consequently not all his status categories could be employed. Specifically, the time period of Levitt's study allowed him to identify six prison-overcrowding litigation categories whereas this study only allows for the identification of four. As such, we use three "contemporaneous" IV estimators of state prison-overcrowding litigation:

PREDEC, FINAL, and FURTHER.[47] PREDEC is a dummy variable taking a value of one if a state had a court decision regarding the status of prison overcrowding under appeal and zero otherwise. FINAL is a dichotomous indicator of a court handing down a decision regarding prison overcrowding with no further appeal. FURTHER is an indicator of subsequent court intervention following an initial ruling.[48] In addition, a set of corresponding indicators regarding the status of litigation within two to three years following a status change is included: PREDEC23, FINAL23, and FURTHER23.[49] Assume that court decision was handed down and placed under appeal in 1985, a final decision entered in 1988, and further court action taken in 1989. In 1985 PREDEC ("preliminary decision, year of status change") would take a value of one. In 1986 and 1987, the litigation status would be "preliminary decision, two to three years following a status change" and PREDEC23 would take a value of one in each year (PREDEC would be zero). In 1988 the litigation status would be "none of the above" since the last status change was more than three years previous. In 1989 the litigation status would be "further action, year of status change" and FURTHER would take a value of one.

With respect to the deterrence-probability measures in the murder specification, per-capita prison admissions (PADM) serve as the IV estimator for the probability of conviction given arrest. Prison admissions are assumed to measure the strain of the incarceration system

---

[47]The omitted category is "released by court," which is an indicator of dismissal of a case or relinquishing of the court's oversight of prisons.

[48]It should be noted that Levitt (1999) focused on the effects of changes in litigation status on prison population *growth rates* and the subsequent effect of the predicted growth rates on changes in aggregate crime rates. These same IV estimators may be less applicable to the *level* specifications employed here.

[49]The prison overcrowding litigation status dummies were defined to be mutually exclusive. In addition, we implicitly assume that any changes in prison overcrowding litigation occurred (took effect) at the beginning of the year in which the change occurred. See Levitt's (1999) footnote 18.

(Rubin, et al. 1999). For the probability of execution given apprehension, we employ the percentage of the vote cast for the Republican candidate in the biannual elections of U.S. Representatives. Off-election years are linearly interpolated. This variable is expected to exert a positive influence on $P_{e|c}$.

The minimum distance from a state's central city to the nearest major brewery (MINDIST) is used as an IV estimator of per-capita beer consumption. For per-capita liquor, wine, and total alcohol consumption we use the per-capita number of licensed drinking establishments (DRINK). For this to be a suitable instrument, we must assume that the per-capita number of drinking establishments is not correlated with the error term of the crime equations. For the inclusive specifications, both MINDIST and DRINK are used as predictors of consumption.

The next step of the test procedure is to regress each of the potentially endogenous variables on the exogenous determinants (including the group and period dummies) and the instruments described above:

$$ENDOG_{q,i,t} = \gamma_0 + \sum_{r=1}^{R} \gamma_{r,i,t} X_{r,i,t} + \sum_{p=1}^{P} \gamma_{R+p} IV_{q,i,t} + \eta_{q,p,i,t} \qquad (4.2)$$

where $ENDOG_{q,i,,t}$ denotes the qth endogenous variable and $IV_{q,i,t}$ its corresponding instrumental variable estimator. $X_{r,i,t}$ denotes the r^{th} exogenous determinant and $\eta_{q,p,i,t}$ denotes the error term. The predicted values from estimation of equation (4.2) are then included in the crime regressions as explanatory variables:

$$CR_{j,i,t} = \Theta_o + \sum_{r=1}^{R} \Theta_{r,i,t} X_{r,i,t} + \sum_{q=1}^{Q} \Theta_{R+q} ENDOG_{q,i,t} + \sum_{\hat{q}=1}^{\hat{Q}} \Theta_{R+Q+\hat{q}} \hat{Y}_{\hat{q},i,t} + \mu_{r,q,\hat{q},i,t} \qquad (4.3)$$

where $\hat{Y}$ denotes the predicted value of the qth endogenous variable obtained from estimation of equation (4.2). Estimation of equation (4.3) then allows for the computation of a critical F-statistic and with its associated p-value. A high value of this F-statistic and a low p-value reject the null hypothesis of exogeneity of the deterrence and consumption factors and thus indicate that these variables are endogenous.

Table 4.6 presents the results of the DWH test for each violent crime specification. The critical p-values for the murder specifications are relatively high (failing to reject the null hypothesis of exogeneity) and larger than most other violent crime specifications. All other violent crime models reject the null of exogeneity for most specifications. Note that across all model specifications the DWH results of the inclusive or total alcohol consumption specifications generally give the highest critical p-values.

The results indicate that the deterrence and consumption factors are most likely exogenous to the per-capita murder rate and probably endogenous to all other violent and property crime categories. It is thus concluded that the estimated coefficients from the 2WFE per-capita murder models, which measure the direct impact of alcohol consumption on per-capita murders, do not suffer from simultaneity bias. For the other crime specifications we must deal with the issue of simultaneity bias, as the tests suggest rejection of the null hypothesis of exogeneity of the deterrence and consumption factors.

4.7 Conclusion

The results in this chapter suggest that the consumption of alcohol may be a significant determinant of many types of Index I violent crimes. Specifically, per-capita liquor consumption is found to be a positive and statistically significant determinant of per-capita rapes, robberies, and possibly murder, while per capita beer consumption is a significant determinant of assault and robbery. The inclusive specifications are probably the most relevant models as they consider all three alcohol consumption measures (per-capita beer, liquor, and wine) together. Therefore, the differences between the estimated signs of the coefficients in this specification and in the individual models are summarized in Table 4.7 (presented here rather than in the table section of the end of the report). The sign of the coefficient from OLS model is before the slash mark (/) and the coefficients from the 2SLS model follows. A plus sign indicates that the consumption measure was a marginally significantly (at the 10 percent level of confidence) positive determinant, a double plus implies significance at conventional levels (5 percent or higher), and a negative sign would suggest a significant negative determinant if there were any [note in this regard, however, that Zimmerman (2000) finds a negative relationship between beer and murder using data through 1992 rather than 1994]. A zero indicates that the estimated coefficient on the consumption measure was statistically indistinguishable from zero.

Table 4.7

Comparison of Coefficient Signs of Per-Capita Consumption Measures between Individual and Inclusive Specifications for Violent Crimes (1985-1994)

	Murder	Rape	Robbery	Assault	TVC
BEER	0/0	++/0	++/++	++/++	++/++
LIQ	0/+	++/++	++/++	0/0	++/0
WINE	0/0	0/0	++/0	0/0	0/0

These results emphasize the importance of controlling for the effects of at least beer and liquor consumption before drawing policy implications regarding alcohol regulations and crime based on findings with beer (or liquor) alone. The null hypothesis of the exogeneity of the deterrence and consumption variables could be rejected for some specifications of all crime models except murder, however, so careful consideration of the endogeneity of these factors must be made before any conclusions regarding the efficacy of alcohol control policies towards mitigating the incidence of crime can be drawn. This suggests that a simultaneous equations estimation strategy must be employed to purge the correlation between the endogenous variables and the error term of the crime equations.

CHAPTER 5

THE EFFECTS OF ALCOHOL CONTROL POLICIES ON THE INCIDENCE OF INDEX I VIOLENT CRIMES: EVIDENCE FROM STATE-LEVEL PANEL DATA

5.1 Introduction[50]

The 2WFE-OLS estimation results presented in the previous chapter provide strong evidence that alcohol plays a significant role in explaining state-level Index I crimes. Chapter three indicates that several alcohol control policies tend to affect alcohol consumption. However, as also explained in Chapter 4, for most types of Index I offenses alcohol consumption and deterrence measures appear to be endogenous factors. Given these three findings, work remains if we are to determine whether alcohol control polices can be used as a tool for reducing the incidence of crime. This chapter applies an econometric methodology that attempts to gain insight into this question.

With respect to per-capita murders, the DWH endogeneity test results reported in Chapter 4 allow for a semi-reduced-form model where the determinants of alcohol consumption are entered directly into the basic crime equation. These results are presented in section 5.2. However, similar semi reduced-form models probably are not appropriate for the other crime categories given the apparent endogeneity of the deterrence and consumption factors. This stands in contrast to the relatively sparse economics literature that has examined the alcohol-Index I crime relationship to date (Chaloupka and Saffer 1992, Cook and Moore 1993a). By not considering all possible sources of endogeneity (as well as not fully specifying all crime

[50]This section and parts of others draw from Zimmerman (2000) with minor modifications.

equations) the findings of these studies may be inaccurate. Section 5.3 models the determinants of crime, alcohol consumption, and deterrence within a simultaneous equation framework. Estimation via two-stage least squares (2SLS) is then employed in an effort to derive consistent estimates of all explanatory variables. Alcohol policy implications from these 2SLS models are discussed in Section 5.4 where fully-specified reduced form OLS regressions are also presented as points of comparison. Section 5.5 provides results from tests of the simultaneous estimation procedure itself, and Section 5.6 concludes.

The theoretical determinants of alcohol consumption and their empirical measurement were discussed in chapter three. The same demand and supply determinants are employed in the models presented below. All models control for state and year fixed effects. Monetary variables are adjusted for inflation and state cost-of-living differentials. Finally, all continuous dependent and explanatory measures are log-transformed.

5.2 The Estimated Effects of Alcohol Consumption on the Per-Capita Incidence of Murder: Evidence from A Semi-Reduced-Form Specification

To determine the effects of alcohol control policies on per-capita rates of murder we employ the following empirical methodology. Let the equilibrium aggregate per-capita murder rate be function of deterrence, opportunity cost, and demographic factors as well as equilibrium alcohol consumption:

$$MUR^{*} = f(DET, OC, DEM, A^{*}) \tag{5.1}$$

where MUR^{*} denotes the equilibrium per-capita murder rate. Recall from chapter two that the equilibrium quantity demanded of alcohol consumption can be estimated in reduced form as a function of income, competition factors, availability factors, transportation costs, taxes, and other exogenous demand and supply determinants:

$$A^* = f(M,C,R_D,R_C,T,\tau,X) \quad (5.2)$$

substituting equation (5.2) into equation (5.1) gives rise to the following semi-reduced-form specification:[51]

$$MUR^* = f(DET,OC,DEM,M,C,R_D,R_C,T,\tau,X) \quad (5.3)$$

Note that in equation (5.3) the determinants of alcohol consumption enter directly into the basic crime equation as explanatory measures.

Table 5.1 presents the estimation results of the reduced form murder specification given in equation (5.3). The columns are the semi-reduced-form specifications for the beer, liquor, wine, total, and inclusive alcohol categories respectively. Coefficient estimates on state and year dummies are not reported.

ARREST is significant and negatively correlated with per-capita murders in each specification. POLICE also carries a negative sign in each specification and is at least marginally significant in every regression except the liquor specification. Similarly, the probability of conviction for murder given apprehension is negatively related to the per-capita incidence of murder in each specification, although it is significant at conventional levels only in the beer specification (and marginally significant in the wine model). Increasing the probability of execution given conviction for murder also has at least a marginally significant negative effect on the rate of murder per-capita in all models except the in the inclusive specification where the sign is negative but the coefficient is insignificant. All of these results support the deterrence

[51]The terminology of "semi"-reduced-form is used since the factors which are theoretically endogenous to the murder rate (i.e., deterrence and consumption variables) are treated as exogenous in the specification (given the results of the DWH tests).

hypothesis. PRISON is always positive, however, and statistically significant, while N.R.A. membership per-capita is also positive and at least marginally significant in the first three specifications.

The variables M1844, POPDEN and PERBLK are never statistically significant. GINI has positive and significant coefficients in the liquor and inclusive models but is insignificant in the other specifications (and negative in two of them). NONMET is positive and statistically significant in all specifications except the total model.

INCOME and WAGELOW are never statistically significant determinants of per-capita murders, but the unemployment rate is a significant positive determinant in every specification. As expected, LROBBERY and LBURGLARY are always positive and statistically significant. The religious variables are always insignificant (with the exception of the negative SOBAP variable in the beer model), while TOURPCT is marginally significant and negative in the liquor and wine regressions.

BEERTAX is *positive* but statistically insignificant in the beer and inclusive specification and WINETAX is also insignificant with a negative sign in the wine model and a positive sign in the inclusive model. On the other hand, LIQTAX carries a negative sign and is statistically significant in both the liquor and inclusive specifications. The coefficient is surprisingly (and suspiciously) large in absolute value, suggesting that a ten percent increase in the liquor excise tax will reduce murders per-capita by seven to thirteen percent. TOTTAX is similarly negative and significant in the total alcohol model, implying that a ten percent total alcohol tax increase will generate a five percent reduction in per-capita murders.

The beer market variables MANDATE, CASHLAW, FORCE, and MINDIST appear in the beer, total, and inclusive specifications. These variables are included to control for non-tax determinants of the (full) retail price of beer.[52] FORCE is consistently and significantly negative, while MANDATE is also negative but significant in only the total and inclusive models. The other two variables are never significant. With respect to the availability controls, the variable DRYPER is always negative but never statistically significant. DRINK also is always negatively correlated with per-capita murders but only marginally significant in one (the total) specification. The proportion of the state population between the ages of eighteen and twenty that can legally consume beer (LEGAL) is similarly negative and marginally significant in the beer and wine specifications. The advertising restriction variables, NOPRINT and NOSIGN, are never statistically significant. Regarding the anti-DUI controls, the civil law variable, DRAM, is negatively correlated with per-capita murders and statistically significant in every specification. The criminal law variables are relatively unimportant, however. FINE is significantly negative in the wine specification, and SUS is marginally significant and negative in the inclusive model. Both of these variables, as well as PBT, NOPLEA, JAIL, and ILDUM are insignificant in all other cases.

Overall, these result support conclusions in Chapter 4. To the degree that alcohol influences the likelihood of murder, it appears that it is liquor that matters. Liquor taxes (but not beer taxes or wine taxes) appear to be a potential policy tool in murder prevention. This conclusion must be considered as tentative, however, as it is sensitive to the data period examined. Zimmerman (2000) found no significant tax impact in a similar analysis using 1985-92 data, for instance.[53] Furthermore, while FORCE and possibly MANDATE laws appear to

---

[52]See Appendix A for a description of these variables.

[53]In addition, as noted above, the liquor tax coefficient appears to be suspiciously large. A possible explanation for this surfaces in the discussion of the simultaneous equation specifications for the other violent crime categories. This explanation casts further doubts on the efficacy of liquor tax policy as a tool in fighting violent crime.

reduce murders, neither was significant in Zimmerman (2000). On the other hand, dram shop laws are consistently negative and significant in both Zimmerman (2000) and this analysis, suggesting that creating incentives for the owners of liquor dispensing establishments to monitor their customers and limit their drinking to avoid civil liability can also reduce the chances that those customers will commit a homicide and/or be a homicide victim. Since dram shop laws also appear to reduce drunk driving fatalities [e.g., see Benson, et al. (1999) and Mast, et al. (1999)], there appears to be support for states which have not adopted them to consider doing so.

5.3 The Estimated Effects of Alcohol Consumption on the Per-Capita Incidence of Non-Murder Index I Violent Crimes: Evidence from a Simultaneous Equations Specification

In this section the equilibrium rate of a particular violent Index I offense (for all violent offenses except murder) are determined by the simultaneous interaction of five factors: the supply of offenses, the probability of apprehension, the level of police employment, the rate of imprisonment, and the consumption of alcohol. All variable definitions are the same as discussed previously. Recall that the results of the DWH endogeneity tests indicated that the use of a semi-reduced-form specification (as was used in the case of murder) was not appropriate given the endogeneity of the deterrence and consumption variables. The basic crime equation is of the form presented previously in equation (4.1):

$$\begin{aligned} CR_{i,j,t} = {} & \beta_o + \beta_1 ARREST_{i,j,t} + \beta_2 POLICE_{i,t} + \beta_3 PRISON_{i,t} \\ & + \beta_4 NRA_{i,t} + \beta_5 M1844_{i,t} + \beta_6 UR_{i,t} + \beta_7 NONMET_{i,t} \\ & + \beta_8 INCOME_{i,t} + \beta_9 PERBLK_{i,t} + \beta_{10} GINI_{i,t} + \beta_{11} WAGELOW_{i,t} \\ & + \beta_{12} POPDEN_{i,t} + \beta_{13} ALC_{i,t} + \sum_{i=1}^{N} \beta_{13+i} STATE_{i,t} \\ & + \sum_{t=1}^{T} \beta_{13+N+t} YEAR_{i,t} + \eta_{i,t} \end{aligned} \qquad (5.4)$$

where STATE and YEAR denote state and year dummies respectively. The variables ARREST, POLICE, PRISON, and the relevant measure of alcohol consumption (ALC) will be treated as endogenous determinants. As such, separate reduced-form structural equations must be specified and estimated for these variables in the first stage regressions.

The probability of arrest (ARREST) for a particular crime is modeled as:

$$\begin{aligned} ARREST_{i,j,t} &= \alpha_0 + \alpha_1 POLICE_{i,t} + \alpha_3 CR_{i,j,t} + \alpha_4 ALC_{i,t} \\ &+ \alpha_5 TOTARR_{i,t} + \sum_{i=1}^{N} \alpha_{5+i} STATE_{i,t} \\ &+ \sum_{t=1}^{T} \alpha_{5+N+t} YEAR_{i,t} + \eta_{i,t} \end{aligned} \quad (5.5)$$

It is hypothesized that an increase in the overall size of the police force (POLICE) should increase the probability of solving all types of crime. It is expected that the higher the total number of crimes committed (CR), the lower the likelihood that an individual crime will be solved. Consumption of alcohol serves as an argument to the probability of arrest equation to control for the possibility that criminals who consume alcohol may be more likely to bungle crimes and/or their efforts to avoid apprehension. Thus, the hypothesized sign on ALC is positive. Following the DWH endogeneity tests, TOTARR is the total property crime arrest rate.

A state's demand function for police protection services is given by:

$$\begin{aligned} POLICE_{i,t} &= \pi_o + \pi_1 INCOME + \pi_3 CR_{i,j,t} + \pi_4 ALC_{i,t} \\ &+ \pi_5 TAX_{i,t} + \pi_6 POP\,45\,P_{i,t} + \sum_{i=1}^{N} \pi_{6+i} STATE_{i,t} \\ &+ \sum_{t=1}^{T} \pi_{6+N+t} YEAR_{i,t} + \eta_{i,t} \end{aligned} \quad (5.6)$$

It is hypothesized that the greater the level of total crime the greater the demand for police protection services. To the extent that police protection is a normal good POLICE should be positively related to INCOME. Given that alcohol is commonly *perceived* as contributing to various socially undesirable outcomes we allow it to also serve as an argument to the police demand function. All else equal, it is hypothesized that ALC will be positively correlated with POLICE. As mentioned previously, states with stronger preferences for police services will vote for higher taxes to finance their provision. Assuming that these states would have larger police protection forces for reasons not relating to crime, the variable TAX serves as an instrument to the police demand equation (Cornwell and Trumbull 1994). The variable POP45P is defined as the number of individuals ages forty-five and over in a given state divided by the states' total population. Older persons may be more susceptible to victimization by criminal offenders and/or be more risk averse. In either case it is hypothesized that older individuals will have stronger preferences for police protection services (Benson, et al. 1992).

Per-capita prison populations are modeled as:

$$\begin{aligned} PRISON_{i,t} = {} & \xi_o + \xi_1 ARREST_{i,t} + \xi_2 POLICE_{i,t} + \xi_3 PREDEC \\ & + \xi_4 FINAL + \xi_5 FURTHER + \xi_6 PREDEC\,23 \\ & + \xi_7 FINAL\,23 + \xi_8 FURTHER\,23 \\ & + \sum_{i=1}^{N} \xi_{8+i} STATE_{i,t} + \sum_{t=1}^{T} \xi_{8+N+t} YEAR_{i,t} + \eta_{i,t} \end{aligned} \tag{5.7}$$

Of course, higher values of ARREST and POLICE are expected to be positively related to PRISON all else equal. PREDEC, FINAL, FURTHER, PREDEC23, FINAL23, and FURTHER23 are the status of prison overcrowding legislation indicators employed as instrumental variables following Levitt (1999). Each of these indicators is expected to be negatively correlated with per-capita prison populations.

Per-capita alcohol consumption (ALC) is modeled as suggested in chapter three but now is assumed to be a function of the crime rate, deterrence factors, and DUI laws. Thus:

$$
\begin{aligned}
ALC_{i,t} = {} & \psi_o + \psi_1 M1844 + \psi_2 UR + \psi_3 NONMET \\
& + \psi_4 INCOME + \psi_5 PERBLK + \psi_6 GINI + \psi_7 POPDEN \\
& + \psi_8 TAX + \psi_9 DRYPER + \psi_{10} DRINK + \psi_{11} MRM \\
& + \psi_{12} SOBAP + \psi_{13} CATH + \psi_{14} PROT + \psi_{15} PBT \\
& + \psi_{16} NOPLEA + \psi_{17} DRAM + \psi_{18} JAIL + \psi_{19} FINE \qquad (5.8) \\
& + \psi_{20} SUS + \psi_{21} ILDUM + \psi_{22} LEGAL + \psi_{23} CR_{i,j,t} \\
& + \psi_{24} ARREST_{i,j,t} + \psi_{25} POLICE_{i,t} \\
& + \sum_{i=1}^{N} \psi_{26+i} STATE_i + \sum_{t=1}^{T} \psi_{27+N+t} YEAR_t + \eta_{i,t}
\end{aligned}
$$

For the beer and total alcohol consumption equations, the beer market variables MANDATE, CASHLAW, FORCE, MINDIST, and DRYPER serve as additional instruments in equation (5.8). To the extent that individuals use alcohol as an excuse to commit crimes or offenders tend to target victims who have been drinking (e.g., because they make for easier targets) CR is expected to be positively related to ALC. It is hypothesized that the deterrence variables ARREST and POLICE will be negatively related to ALC. For instance, if individuals perceive alcohol as a contributing factor to their potential engagement in criminal activity (whether it is intentional or not) then deterrence factors may influence and individual's alcohol consumption decision independently of affecting their crime supply decision.

The method of two-stage least squares (2SLS) is used to estimate the simultaneous system expressed by equations (5.4)-(5.8). Equations (5.5)-(5.8) constitute the first-stage regressions. The system's set of exogenous determinants and instrumental variables serve as the explanatory variables in the estimation of each first-stage regression. The fitted values from these regressions (note that these are still denoted ARREST, POLICE, BEER, WINE, and TOTALC in the 2SLS models) are then used in place of the corresponding endogenous variables in the estimation of equation (5.4). The resulting estimates of the predicted values of the endogenous variables are purged of their correlation with the error term and are rendered consistent estimates of the effects of deterrence and consumption measures on the various per-capita crime rates. All reported second-stage t-ratios are corrected to account for using predicted rather than actual values of the endogenous variables in estimation of the second-stage crime regressions.[54]

Tables 5.2-5.5 present the 2SLS estimation results of the individual crime models. Due to missing liquor and wine excise tax data, the liquor, wine, total, and inclusive 2WFE-OLS specifications are also estimated for comparison between the OLS and 2SLS models, and Tables 5.7-5.11, discussed in Section 5.5, contain the fully-specified (i.e., specifications including all of the variables used in the first stage regressions for the 2SLS models) reduced-form OLS regressions for all of the violent crime categories (including murder) to provide further points of comparison between the simultaneous estimates and their OLS counterparts.

5.3.1. Per-Capita Rapes

Table 5.2 presents the estimates of the per-capita rape 2SLS and corresponding OLS specifications (and Table 5.7 shows the corresponding fully specified reduced-form models for rape). Note that while ARREST is significantly negative in beer and wine specifications of the OLS models, control for simultaneity makes the variable consistently insignificant. The same thing happens to PRISON which is actually significantly negative in all five OLS models but insignificant in the 2SLS models. On the other hand, POLICE is insignificant in the OLS models (except for a marginally significant negative sign in the total violent crime specification) but it becomes significant and positive in the beer and wine specifications of the 2SLS models. While

[54] Recall that we do not report the first stage regressions because each of the five second-stage rape regressions has at least four first stage regressions associated with it (the inclusive model has six first stage regressions), so reporting first stage regression results for the four crime categories would require tables with an additional 88 regressions. However, these results are available from the authors upon request.

NRA is at least marginally positive in two of the OLS models, it loses its significance in the simultaneous equation models.

The variable M1844 takes a surprisingly negative sign in all 2SLS specifications and it is always statistically significant (as it was in some of the OLS models). The 2SLS estimates of NONMET remain statistically significant in the beer and wine specifications, where it is positive, and remains statistically insignificant in all others. POPDEN retains its negative sign in all specifications as well as its statistical significance after controlling for simultaneity, while GINI and PERBLK remain statistically insignificant in all models except the 2SLS wine specification.

INCOME, which is significant in the liquor, total, and inclusive OLS specifications, where it is positive, is positive and statistically significant in all models after controlling for simultaneity. The unemployment rate, which is significant in only one OLS model, the wine specification where it is negative, turns positive and marginally significant in the 2SLS version of that model but remains insignificant in all other specifications. WAGELOW is never found to have a statistically discernable effect on per-capita rapes.

BEER loses its positive and significant coefficient in the 2SLS specifications. It is negative and statistically insignificant in the beer-only specification, while its sign remains positive in the inclusive model, but loses significance. Thus, any inferences drawn in Chapter 4 from OLS crime regressions regarding the potential beneficial impact of reducing beer consumption and therefore reducing rape must now be reconsidered. The 2SLS estimates of LIQ retain its positive sign in the individual specification, but loses significance, while it retains both the positive sign and significance in the inclusive model. Therefore, at least some support for the conclusions drawn in Chapter 4 regarding the impact of liquor on rape remain intact. Wine becomes significant in both the wine and inclusive models when controls for simultaneity are considered, but in both cases the signs are negative, implying that increases in wine consumption will reduce rapes. This may suggest that wine is consumed by individuals or in situations which are not likely to create opportunities for rape, while liquor is consumed by different types of individuals or in different types of situations that can lead to rape, either by making potential victims more vulnerable or by making potential offenders more aggressive. This finding with regard to wine occurs for other crimes as well, so it is discussed again below. TOTALC retains its positive coefficient but turns statistically insignificant after controlling for simultaneity.

5.3.2. Per-Capita Robberies

Table 5.3 presents the subsample OLS and 2SLS estimates for per-capita robbery. Both the OLS and 2SLS estimates of ARREST are statistically insignificant in all specifications. The 2SLS estimate of POLICE is positive and statistically significant in all 2SLS specifications (it also is positive and significant in the OLS beer and wine specifications but insignificant elsewhere). PRISON is found to be negatively and significantly correlated with per-capita robberies in all OLS models but controls for simultaneity make this variable insignificant in all the 2SLS estimates. NRA is marginally negative in the liquor and marginally positive in the total OLS models but significantly negative in the 2SLS wine specification and insignificant in all other OLS and 2SLS models.

Where the estimated coefficients on M1844 is positive and statistically significant in the OLS models (the beer and wine specifications) it becomes insignificant after controlling for simultaneity. In two cases where it is insignificant in the OLS models (liquor and total) it becomes significant and negative, while it remains insignificant in the inclusive model. NONMET and POPDEN are never statistically significant in either the OLS or 2SLS models. GINI is negative and at least marginally significant in all of the OLS specifications except the inclusive model but the variable only remains significant in the wine specification after controlling for simultaneity (it also becomes marginally significant in the inclusive model). The OLS and 2SLS estimates of PERBLK are always negative and statistically significant.

BEER retains its positive sign and statistical significance in the 2SLS beer specification, while wine and liquor turn insignificant in their individual 2SLS models. The inclusive specification tells a different story, however. BEER is insignificant in both the OLS and 2SLS specifications, LIQUOR is positive and significant in both the OLS and 2SLS models, and WINE is significant and negative in the 2SLS version after being insignificant in the OLS model. Finally, TOTALC is statistically insignificant in both OLS and 2SLS models.

5.3.3. Per-Capita Assaults

Table 5.4 presents the subsample OLS and 2SLS estimates for per-capita assault. ARREST is insignificant in every specification (although it is marginally positive in the 2SLS wine specification). While the OLS estimates for POLICE are all insignificant, the 2SLS estimates are positive and statistically significant in the liquor, wine, and total specifications. PRISON is also never significant in the OLS models but it is at least marginally significant and positive in all of the 2SLS specifications. The National Rifle Association membership per-capita variable is also insignificant in the OLS models but it is significantly negative in the beer and wine 2SLS models.

The variable M1844 remains positive and statistically significant in the beer and wine specifications after controlling for simultaneity. For the liquor, total and inclusive cases this variable is never statistically insignificant. NONMET is always negative and statistically significant in the OLS specifications, and remains significantly negative in the beer, wine, and inclusive (but only marginally) models after controlling for simultaneity. POPDEN is also at least marginally significant and negative in all of the OLS models except the total specification, but the only 2SLS where it retains significance is the wine model. GINI is marginally significant and positive in the OLS beer and wine models, and significantly positive in the 2SLS beer model, but it is insignificant in all other specifications. On the other hand, PERBLK is at least marginally significant and negative in all OLS specifications, and it retains at least marginal significance in the 2SLS beer, wine, and inclusive models.

INCOME is always statistically insignificant, except for the marginal positive sign in the 2SLS wine specification. In contrast, the unemployment rate is significantly negative in all OLS models and retains its negative sign after controlling for simultaneity, although it remains significant in only the wine specification. Finally, WAGELOW is never found to have a statistically discernable effect on per-capita assaults in either the OLS or 2SLS models.

BEER remains positive and statistically significant after controlling for simultaneity in both the beer and the inclusive specifications. Note that the estimated coefficients on BEER are probably unrealistically large, however, and it rises dramatically in the 2SLS models (from 1.02 to 2.92 in the beer specification and from .75 to 1.51 in the inclusive model). LIQ remains statistically insignificant after controlling for simultaneity, but WINE becomes significantly negative in both the wine and inclusive specifications. Finally, TOTALC remains positive and statistically significant in the 2SLS model.

5.3.4. Per-Capita Total Violent Crimes

Table 5.5 presents the subsample OLS and 2SLS estimates for per-capita total violent crime (TVC). ARREST retains its surprising positive sign in all specifications although it is statistically significant in only one case (the 2SLS wine results). The 2SLS estimates of POLICE are all positive and at least marginally significant, after being at least marginally significant and negative in the OLS total and inclusive models, insignificant but negative in the OLS liquor specification, and at least marginally significant and positive in the OLS beer and wine models. PRISON is at least marginally significant and negative in all of the OLS models, but it is positive in all of the 2SLS models and significant in every specification except the wine model. 2SLS estimates of NRA are found to be statistically significant and negative in the beer and wine models, while the coefficient is insignificant in all other 2SLS models and all OLS models except wine where it is marginally negative.

M1844 retains its positive and significant sign in the beer and wine specifications after controlling for simultaneity, but it is insignificant in all other OLS and 2SLS models. NONMET is always negative, and it is at least marginally significant in both beer models, the OLS liquor model, both wine specifications, and the OLS inclusive model. POPDEN is significantly negative in each of the OLS models but it retains statistical significance only in the wine specification after controlling for simultaneity. Similarly, PERBLK is always significantly negative in the OLS models, and retains the significant negative sign in the beer, wine and inclusive 2SLS specifications. In all specifications estimates of GINI are never found to be statistically significant.

The OLS estimates of INCOME are never found to be statistically significant, but these coefficients are at least marginally significant and positive in all of the 2SLS estimations except the inclusive model. The OLS and 2SLS estimates of UR are generally negative but this variable is statistically significant in every OLS model and insignificant in every 2SLS model. WAGELOW is never statistically significant in any of the OLS or 2SLS models.

The BEER coefficient is statistically significant and positive in both OLS and 2SLS specifications of the beer and inclusive models. Again the coefficients seem to be unreasonably large and the 2SLS estimates are larger than the OLS estimates. LIQ is also positive and significant in both the OLS liquor and inclusive models and retains the positive sign in both cases when simultaneity bias is controlled for, but it loses significance in the liquor model while remaining significant in the inclusive model (where the coefficient also seems unreasonably large). As with other crime categories, the wine coefficient turns from insignificant in the OLS wine and inclusive models to significant and negative in the 2SLS models. TOTALC is positive but turns statistically insignificant after controlling for simultaneity.

5.3.5. Summing Up

Table 5.6 (presented here rather than in the Table section at the end of the report) shows the signs of the alcohol coefficients in the OLS and 2SLS models for the individual alcohol specifications and the inclusive specifications. A zero in the table means that the coefficient is negative, a single plus sign implies a marginally significant (at the ten percent level of confidence) positive coefficient, a double plus implies a positive coefficient that is significant at conventional levels (five percent or less), and a double minus sign implies that the coefficient is significantly negative at conventional levels. The symbol before the slash (/) represents the OLS result and the symbol after the slash represents the 2SLS result. This Table emphasizes several important implications. First, an obvious inference is that wine consumption is not a cause violent of crime. Indeed, the robustness of the significant negative sign in 2SLS specifications strongly reinforces the conclusions suggested above in the discussion of the findings regarding rape. In particular, wine apparently is consumed by individuals or in situations which are not likely to create opportunities for violent crimes of any kind, while liquor and/or beer are consumed by different types of individuals or in different types of situations that can lead to various kinds of violent crime, either by making potential victims more vulnerable or by making potential offenders more aggressive. Thus, it appears that policies that limit the consumption of beer and liquor, and/or alter the circumstances under which these types of alcohol are consumed may reduce some violent crimes.

Table 5.6

Comparison of Coefficient Signs of Per-Capita Consumption Measures between OLS and 2SLS Specifications for Violent Crimes for Individual Alcohol and Inclusive Models (1985-1994)

	Rape	Robbery	Assault	TVC
LBEER				
Beer Model	++/0	++/++	++/++	++/++
Inclusive Model	++/0	0/0	++/++	++/++
LLIQ				
Liquor Model	++/0	++/0	0/0	++/0
Inclusive Model	++/++	++/++	0/0	+/++
LWINE				
Wine Model	0/--	+/0	0/--	0/--
Inclusive Model	0/--	0/--	0/--	0/--

Now, the question is, which type of alcohol plays a role in different types of violent crime. Table 5.6 clearly reveals that for the most part the findings are not robust across specifications. The only results that are robust are those for assault. It appears that beer consumption significantly influences the level of assault while alcohol consumption does not.

Thus, policies directed at controlling beer may influence assault. Beer also appears important for total violent crime, of course, but that is probably because assaults are the largest component of this aggregate variable. Beer's impact is not robust in the rape or robbery models, however. In the case of robbery, adding alcohol and wine to the model (i.e., moving from a beer only to an inclusive model) makes beer insignificant in both OLS and 2SLS models. Therefore, it appears that the significant beer coefficient in the beer only robbery models reflects a missing variable bias. In the case of rape, moving from OLS to 2SLS specifications makes the beer coefficient insignificant in both the beer and inclusive specifications. Thus, the significant beer coefficient in the OLS rape specifications reflects simultaneity bias. In sum, it appears that the only likely violent crime target for beer policies is assault.

We have already concluded that liquor control policies may have desirable impacts on the level of murder. We can also conclude that such policies will probably not have any impact on assaults. For rape and robbery the conclusions are not as obvious. In each case, moving from the OLS to the 2SLS specifications in the liquor only model makes the liquor coefficient insignificant, but in the inclusive model liquor retains its significance in both the OLS and 2SLS models. The lack of significance for the liquor variable in the liquor-only simultaneous-equation model apparently reflects a missing variable problem that is not detected in the OLS specifications. By accounting for the determinants of beer and wine in first-stage equations, and the resulting estimated variables in the second stage model, the significant impact of liquor is revealed. Thus, we conclude that in the most fully specified model, liquor appears to be an important determinant of rape and robbery, so liquor control policies might be expected to influence the level of these crimes as well as murders.

5.4 The Estimated Effects of Alcohol Control Policies on Non-Murder Index I Crimes

There are at least two ways to consider the impact of particular policy variables. The typical approach in the literature is use of a reduced form specification. Therefore, we present such models next. This approach has the advantage of possibly uncovering the impacts that policies could have by changing drinking practices and/or locations without affecting aggregate consumption (e.g., if drunk driving laws lead to more consumption at home without reducing consumption per se, but this in turn reduces some types of crimes, perhaps like robbery, and/or increases others like domestic violence, the impact would be masked in the 2SLS policy estimates discussed below). However, the tests reported in Chapter 4 indicate that the coefficients in these models suffer from simultaneity bias, so the following subsection derives policy implications from the 2SLS estimation procedure and compares them to the reduced-form results (multicolinearity and perhaps other problems discussed below also arise in all of the estimated policy impacts).

5.4.1. Fully Specified Reduced-Form Models

Tables 5.7 through 5.10 present the fully specified reduced form models for the four violent crime classifications that are modeled with simultaneous equations above (murder regressions are not presented since they would be almost identical to those reported earlier). For each set of regressions, we briefly note the non-alcohol-policy implications of these results before turning to the alcohol-policy inferences. The alcohol policy variables include LEGAL, DRYPER, DRINK, NOSIGN, NOPRINT, DRAM, PBT, NOPLEA, JAIL, FINE, SUS, ILDUM, and the respective excise taxes for every crime specification. In addition, the estimated effects of the beer market variables MANDATE, CASHLAW, FORCE, and MINDIST are considered for the beer, total, and inclusive specifications. Finally, state tourism is also a factor that could be influenced by policy makers. As such, the estimated effects of TOURPCT are also examined for every crime specification. The policy inferences will then be compared to the implications that can be drawn from the 2SLS models (i.e., the predictions from the first stage regressions as they affect the second stage results reported above) at the end of the next subsection.

Rapes Per-Capita. Table 5.7 shows the reduced-form rape regressions. Among the variables that do not reflect alcohol control policies, POP45P is consistently significant and positive (this indicates that there is a problem with the 2SLS estimates, as explained below), NONMET is significantly positive in the beer and wine models and marginally positive in the inclusive specification, WAGELOW is significantly negative in the liquor and wine specifications and marginally negative in the total alcohol model, POPDEN is significantly negative in the liquor regression, TAX is negative and significant in the total alcohol estimates and marginally negative in the inclusive model but marginally positive in the beer model, and FINAL23 tends to be at least marginally positive. Among the religious variables, MRM is positive and significant in every regression, SOBAP is significantly positive in the beer and wine specifications, PROT is negative and significant in every regression, and CATH is significantly negative in the beer, wine and inclusive models. All other non-alcohol-policy coefficients are insignificant.

The alcohol control variables LEGAL, DRINK, and MANDATE are never significant, and the same is true for PBT and JAIL among the drunk-driving controls, while FORCE is only marginally positive in one specification (the inclusive model) and DRAM is similarly only marginally positive in the liquor model. MINDIST is always significantly negative, however, suggesting that the higher the cost of transporting beer into a state (and therefore the higher the price) the lower is the level of rape. CASHLAW also is negative in the three models where it appears, and it is significant in one specification (total), marginally significant in another (inclusive), but insignificant in the third (beer). The beer tax variable is also consistently negative where it appears, suggesting that policies directed at raising the price of beer can reduce rapes. This is consistent with the OLS results presented above, of course, but not with the 2SLS results. Indeed, the liquor tax variable is positive, and significantly so in the inclusive model. This probably reflects one of two factors: simultaneity bias, as implied above and further considered below, and/or multicolinearity between the tax variables (and particularly the wine and beer taxes with a correlation coefficient of .65). Tourism has a negative sign in all models and it is significant in the beer and wine specifications, suggesting that states attracting relatively large numbers of tourists have fewer rapes per-capita. This could be endogenous, however, if tourists tend to choose destinations that have relative low violent crime rates. DRYPER is positive in each specification and significant in the beer, wine, and inclusive models, surprisingly implying that states with larger portions of their populations in dry counties have more rapes per-capita. The advertising variables, NOSIGN and NOPRINT do not provide any strong implications, as they tend to change signs, and each is only significant in one specification (NOPRINT is negative in the wine model and NOSIGN is marginally positive in the beer specification).

The DUI variables also reveal little in the way of potential policy actions. While PBT is always negative it is never significant, as noted above, and NOPLEA is always positive (significantly so in the beer and inclusive models), as are SUS (significantly so in the beer and wine models and marginally significantly in the inclusive model) and ILDUM (which is also significant in the liquor model). Yet, FINE is like PBT in that it is also always negative, and it is significant in the beer model (and marginally significant in the liquor and wine models). Thus, the implications are somewhat conflicting. If all significant signs were negative it would imply that DUI controls reduce rapes, and if all signs were positive it would suggest that discouraging drunk driving leads to changes in the location and/or circumstances of alcohol consumption that increase the chances of rapes (e.g., more females have dates to their homes or go to the homes of their dates, perhaps creating greater opportunities for "date rapes"). The largest number of significant signs are positive, perhaps supporting the later hypothesis, but the results for FINE suggest otherwise. There actually appears to be another explanation, however. These DUI law variables tend to be correlated, and the small positive effects are consistent with a multicolinearity problem which is affecting the precision of the estimates.

Robberies Per-Capita. Among the non-alcohol control variables in Table 5.8, representing the reduced-form robbery regressions, MRM is consistently at least marginally significant and positive, SOBAP is always significant and negative, CATH is significantly negative in the liquor, total and inclusive models, and PROT is significantly positive in the beer and wine models but marginally negative in the liquor model. POP45P is, once again, significantly positive in all specifications except the beer model. Several variables appear to be significant in one or two specifications, including NRA (positive in the liquor and total models and marginally so in the inclusive model), M1844 (positive in the wine model), PERBLK (negative in the beer specification and marginally negative in the wine model), GINI (negative in the beer model), and WAGELOW (positive in the total and inclusive models). TAX (negative in the liquor regression), and some of the prison litigations variables used as IV estimators in the first stage prison regressions in the preceding section are significant. All other control variable coefficients are insignificant.

LEGAL is surprisingly negative, and significantly so in the liquor, total, and inclusive models, suggesting that a larger portion of the population consisting of legal young drinkers (18 to 21) means fewer robberies. This may imply that young drinkers are relatively unattractive robbery victims, of course, as they may be less likely to be carrying much cash and they may also be relatively more likely to resist (e.g., because they are in relatively good physical condition or because they are relatively myopic and less risk averse). None of the alcohol tax variables are ever significant, so alcohol tax policy does not appear to be effective in limiting robberies. MANDATE and MINDIST are also always insignificant, reinforcing the implication that influencing the price of beer will not impact robbery. In contrast, however, CASHLAW is negative, and it is significant in two of the three models where it appears (total and inclusive), and FORCE is at least marginally positive in all three regressions. If forced deposit laws raise the price of taking liquor out of establishments to drink it at home or on the road, relative to the price of drinking at liquor establishments, these results may be consistent with the results for DRINK, however. DRINK is always positive and it is significant in the total and inclusive models and marginally significant in the beer and liquor models. This suggests that limits on the availability of drinking establishments reduce robberies, perhaps because fewer vulnerable victims tend to be available as people substitute drinking at home for drinking out. NOSIGN is significantly negative in every specification, suggesting that restrictions on advertising may reduce consumption, or at least reduce consumption in some situations in which robberies are likely to occur, thereby reducing robbery rates. NOPRINT is almost always positive (except for the insignificant negative sign in the inclusive model), however, although it is almost never significant (it is significantly positive in the liquor model). The sign on DRYPER switches from specification to specification and it is never significant at customary levels (it is marginally positive in the beer model, however). Tourism is, once again, significantly and negatively related to the crime rate, reinforcing the possibility suggested above that it is endogenous (tourists are attracted to low crime states).

Once again, the DUI control variables provide a mixed message. DRAM is positive in every regression, and it is significant in the liquor, total and inclusive models (and marginally significant in the wine specification), suggesting that making liquor establishment owners liable for negative consequences produced by their drunk patrons leads to more robberies. Perhaps heavy drinkers are forced to leave establishments more frequently, creating more targets of opportunity for robbers. Among the criminal law DUI variables, ILDUM is always negative and significant while PBT is always positive and significant. FINE is also positive in every regression and it is significant in three (liquor, total, and inclusive) and marginally significant in one (wine). Similarly, SUS and NOPLEA are always positive but significant only one case each (the beer model for SUS and the liquor model for NOPLEA) and marginally significant in another (the wine specification for SUS and the inclusive model for NOPLEA). Finally, JAIL is never significant although it is always positive. As suggested above, multicolinearity appears to be a problem with these variables.

Per-Capita Assaults. Several of the control variables in Table 5.9, where the reduced-from assault regressions are presented, appear to be important predictors of assault. M1844 is significantly positive in all five specifications, for instance, as might be anticipated, and POP45P and ATPC are also consistently significant and positive (suggesting, once again, that there may be problems with the simultaneous equation estimates, as detailed below). The unemployment rate is always negative and significant. Other variables are less consistent, but important in some specifications. NONMET is significantly negative in the beer and wine specifications, as is POPDEN in the beer model. INCOME is marginally positive in the beer and wine models, and

TAX is significantly positive in the beer model. For the religion controls, CATH has a significant positive sign in the beer and wine models, SOBAP is significantly positive in the liquor and inclusive models but significantly negative in the beer model, and the other two religion variables are generally not significant, nor are all of the other non-alcohol policy coefficients not yet mentioned.

For the most part, the alcohol control variables appear to have little impact, but there are important exceptions. LEGAL is consistently positive and significant in all of the assault regressions, suggesting that raising the drinking age and therefore reducing the portion of the population between 18 and 21 that can legally drink can significantly reduce assaults. Of course, all states have now done so, but these results do suggest that mixing alcohol with young people tends to produce assaults. That is, the alcohol impact on assault tends to be situation specific to younger people rather than a general phenomena across all ages and situations. This perception is reinforced by the tax coefficients. Taxes are not a policy tool that are targeted at a specific group of drinkers, like minimum drinking age laws are. They affect every drinker whether they tend to cause problems when they drink or not. Yet, the only significant alcohol tax coefficient is for beer in the beer specification, and it is *positive.* That is, higher beer taxes supposedly lead to more assaults. However, the coefficient turns negative and insignificant in the inclusive model. Thus, this broad based policy has no impact on assaults, while the more narrowly focused or targeted policy appears to. The same conclusion applies to all of the other broad-based policies, as MANDATE, CASHLAW, FORCE, MINDIST, DRINK, NOSIGN, and NOPRINT are never statistically significant at the customary .05 level (nor is DRYPER). Tourism is, once again negative and it is significant in every specification except the beer model, reinforcing the suggestion made above that tourists are attracted to low-violent-crime-rate destinations.

As with rape and robbery, the DUI variables provide very mixed implications. In this case, PBT is always negative, and it is significant in every specification except one (wine). FINE is also significantly negative in the wine regression and marginally negative in the beer model. On the other hand, NOPLEA is always positive and significant and SUS is marginally positive in the liquor, total, and inclusive models. JAIL and ILDUM are never significant, but the civil law variable, DRAM, is once again, positive, and significant in the liquor and total specifications (and marginally significant in the beer and inclusive models).

Per-Capita Total Violent Crimes. Table 5.10 contains the reduced-form total violent crime regressions. As with assault, M1844, POP45P, and ATPC are always significantly positive and unemployment is always significantly negative in the total violent crime models. NONMET is also negative, and significantly so in the beer and wine specifications, INCOME is marginally positive in the beer and wine models, and TAX is always insignificant and is (marginally) positive in the beer regression. PERBLK is negative but only marginally significant in the beer model. MRM is significantly positive in every regression while SOBAP is negative in all models but only significantly so in the beer model. CATH changes signs across regressions and is significantly negative in one case (the inclusive model), marginally so in another (the total model), and marginally positive in a third (the wine model). PROT is similarly inconsistent in sign and is only marginally significant in the wine model, and positive.

Again, as with assault, LEGAL is positive, but in this case it is only significant in the wine specification and marginally significant in the beer model. All tax coefficients are insignificant except the beer tax coefficient in the inclusive model. In this case, however, the coefficient goes from positive in the beer specification to significantly negative in the inclusive model. CASHLAW is also significantly negative in the inclusive model as well as the total model, but insignificant in the beer model. FORCE is significant and positive in the total and inclusive models. Thus, the results do not completely map the findings for assault, as some coefficients appear to be more consistent with the robbery regressions. For instance, DRINK is always positive and significantly so in the liquor, total, and inclusive models. MANDATE, MINDIST, DRYPER, NOSIGN, and NOPRINT are never significant, however. Tourism again is significant and negative in all specifications.

Representing the civil law opportunities for sanctioning DUI, DRAM is once again positive, and significantly so in the liquor, total, and inclusive specifications. Among the criminal law variables, NOPLEA is always significantly positive, SUS is at least marginally positive in the liquor, total, and inclusive models, FINE is marginally positive in the inclusive regression but has a negative sign in three of the other specifications, PBT and ILDUM are generally negative but never significant, and JAIL is insignificant with positive signs.

Summing Up. While considering these reduced-form regression results on a crime by crime basis may create the impression that there is very little in the way of consistency and therefore of information in the results, comparison of the results across crimes is potentially revealing. Therefore, Table 5.11 (presented in the text rather than at the end of the report with the statistical tables) summarizes the signs of several of the alcohol policy variables across the crime categories. The tourism variable is not included because it is always negative and frequently significant, perhaps imply that it is endogenous, as suggested above, and MANDATE is not included because it was never significant. The DUI variables are also not included in this Table, but they are discussed below. As in previous tables, 0 implies insignificant, + means marginally positive, - means marginally negative, ++ indicates significantly positive, and -- implies significantly negative.

One thing that stands out when the results are considered together is the fact that whatever the relationship is between alcohol and violent crime, it is not the same relationship across types of violent crime. For Assault, for instance, the only alcohol policy that appears to matter is drinking age, but this policy variable has no impact on rape and it has the opposite sign for robbery. But this can make sense because of the differences in the crimes and their circumstances. Assault may well be a young person's problem, by-in-large, as young people are relatively myopic and relatively less risk averse, so they are relatively more likely to get into violent confrontations, particularly when they drink. On the other hand, these same characteristics make young drinkers relatively unattractive as robbery victims, since they may be more likely to resist (in addition, they may carry relatively little of value, making them less attractive as victims). Or consider the implications of DRINK. A relatively high number of alcohol outlets makes drinking cheaper (due to competition, lower travel costs, etc.). Therefore, drinkers will go out to obtain alcohol relatively more frequently, and if drinkers are relatively attractive targets for robbery then robberies should rise, as they appear to. However, the same does not occur with other kinds of violent crime. Regulations against advertising on signs also appear reduce robberies, perhaps by reducing the inclination to "stop someplace for a drink."

Table 5.11

Comparison of Coefficient Signs of Selected Alcohol Policy Variables between Reduced-Form Specifications for Violent Crime Categories (1985-1994)

Crime/ Model	Taxes Beer	Taxes Liquor	Taxes Wine	Taxes Total	Legal	Drink	Mindist	Cashlaw	Force	Dryper	Nosign	Noprint
RAPE												
Beer	--				0	0	--	0	0	++	+	0
Liq.		0			0	0				0	0	0
Wine			0		0	0				++	0	--
Tot.				0	0	0	--	--	0	0	0	0
Inc.	--	++	0		0	0	--	--	+	++	0	0
ROBBERY												
Beer	0				0	+	0	0	+	+	--	0
Liq.		0			0	+				0	--	++
Wine			0		--	0				0	--	0
Tot.				0	--	++	0	--	++	0	--	0
Inc.	0	0	0		--	++	0	--	++	0	--	0
ASSAULT												
Beer	++				++	0	0	0	0	0	0	0
Liq.		0			++	0				0	0	0
Wine			0		++	0				0	0	0
Tot.				0	++	0	0	0	0	0	0	0
Inc.	0	0	0		++	0	0	0	0	0	0	0
T.V.C.												
Beer	0				+	0	0	0	0	0	0	0
Liq.		0			0	++				0	0	0
Wine			0		++	0				0	0	0
Tot.				0	0	0	0	--	++	0	0	0
Inc.	--	0	0		0	++	0	--	++	0	0	0

Another thing that stands out is the apparent unimportance of alcohol taxes as a means of reducing violent crime. The one exception appears to be beer taxes as a mechanism for reducing rapes. However, as noted in the previous section, the relationship between beer and rapes tends to disappear when simultaneity biases are controlled for. And of course, those biases arise in these reduced-form regressions. Additional reasons for questioning the validity of this result also become apparent when the simultaneity interactions are examined below. Therefore, this and all of the other results reported here must be considered with extreme caution. Perhaps if they are robust when derived from a simultaneous equation system (see the next subsection), then they can be taken more seriously. Simultaneity bias does not appear to be the only problem however. Multicolinearity probably makes the coefficient estimates for the DUI laws imprecise, and it may also be affecting the tax coefficients in the inclusive models. With regard to the DUI variables, which are not summarized in Table 5.11, the coefficients also display some fairly striking patterns across the different crime categories. PBT is always significantly positive for robbery, for instance, and almost always significantly negative for assault, while it is not related to rape. ILDUM is significantly negative for all robbery specifications but generally unrelated to any of the other crimes. FINE is generally negative for rape and perhaps assault, but generally positive for robbery. DRAM is generally positive for both robbery and assault, and so on. Clearly, some kind of relationships probably exist between DUI controls and violent crime, but all of the coefficients tend to be very small even as many are significant and positive (a sign of multicolinearity). Intuitively, these variables might be expected to reduce consumption, but perhaps their actual impacts are to shift the location and/or circumstances of consumption, and in this context, they appear to have different impacts on the different crime categories, just as drinking age laws, regulations on drinking establishments, and perhaps taxes do. Again, however, all of these results are probably plagued by simultaneity bias, so let us turn to estimates of the policy impacts in the 2SLS models.

5.5 Alcohol Policy Estimates from Simultaneous Equation Models

Given the estimation results from the simultaneous equations system developed in the previous section and the policy implications of the reduced-form specifications, the next step in the empirical analysis is a determination of the predicted effects of each alcohol control policy on the respective crime rates that can be drawn from the 2SLS models. These effects are ascertained by (i) multiplying the estimated coefficients for ARREST, POLICE, PRISON, BEER, WINE, LIQ, and/or TOTALC (depending on the specification) from a second-stage regression by the policy coefficients obtained in each of the corresponding first-stage regressions and then (ii) summing over each multiplicative term.[55] Recall that as noted in section 5.3, the entire set of exogenous determinants and instrumental variables serve as explanatory variables in each first stage equation, so there are policy coefficients in each of these equations. This summing-up procedure is done for each specification of every crime category estimated within the simultaneous equations system. The estimated impacts of policy variables for each crime (except murder, of course) are provided in Tables 5.12-5.15. Estimates of the effects of LEGAL, DRYPER, DRINK, NOSIGN, NOPRINT, PBT, NOPLEA, DRAM, JAIL, FINE, SUS, ILDUM, MANDATE, CASHLAW, FORCE, MINDIST, and TOURPCT are presented.[56]

Per-Capita Rapes. The predicted effects of alcohol control policies on the incidence of per-capita rapes are presented in Table 5.12. First, note that in the liquor and total specifications all policies are predicted to have no affect because the corresponding second stage coefficients estimates are all statistically indistinguishable from zero. These results correspond with the reduced form estimates reasonably well, however [see Table 5.16 presented in the text below, where the signs and significance of reduced-form and 2SLS policy impacts are compared].

Alcohol policy effects on the incidence of rape appear in the BEER, WINE and inclusive models. In this context, beer taxes appear to reduce rapes, just as in the reduced-form model, but this is actually a surprising result given the second stage results reported in Table 5.2 since beer

[55]Estimated coefficients that were statistically insignificant in either the second stage or the first stage regressions were given weights of zero.

[56]Note that the estimates reported for the dichotomous policy variables are not elasticities *per se*. The sign of the respective policy variables are of primary concern for this discussion.

is not significant. Furthermore, LEGAL, which measures the portion of the population that is under 21 that can legally drink is negative in each case, suggesting that raising the drinking age (lowering LEGAL) will increase rapes, in contrast to the reduced form predictions. This also is a very surprising result, given the implicit and occasionally explicit assumption in most of the literature that alcohol policies work exclusively through their impact on the price and therefore the quantity demanded of alcohol (this assumption is necessary to justify drawing policy inferences from reduced form models). Therefore, considering the way that the impacts of beer taxes and legal arises is instructive. This requires a discussion of the various first stage regressions associated with each of the second stage regressions reported in Table 5.2.[57]

Since beer is insignificant in the second stage equations reported in Table 5.2, the *apparent* policy impacts actually feed through other second stage coefficients. In particular, the coefficient on POLICE is +1.2695 and significant in the beer model, and in the first stage regression explaining POLICE, the BEERTAX coefficient is a -0.0657 and significant.[58] That is, the first stage regression implies that an increase in beer taxes correlates negatively with police employment, and since police employment is positively correlated with rape, beer taxes are negatively correlated with rape. Multiplying the two coefficients together produces the -0.0834 coefficient reported in Table 5.12. This suggests that the reduced-form equation results suffer from simultaneity bias (e.g., the very large coefficient on beer taxes in the first column of table 5.7) *and* from misinterpretation, if the presumption is that the negative correlation reflects the

---

57 Recall that we do not report the first stage regressions because each of the five second-stage rape regressions has at least four first stage regressions associated with it (the inclusive model has six first stage regressions), so reporting first stage regression results for the four crime categories would require tables with an additional 88 regressions. However, these results are available from the authors upon request.

58 Note that for convenience we will be rounding to four decimal places, so the actual values reported in Tables 5.12-5.15 which are calculated without such rounding may differ from those that can be calculated with the coefficients reported in this section.

impact of beer taxes on beer consumption. Now, how should this result be interpreted? Why would beer taxes reduce police resources? It is difficult to come up with any *causal* argument. Therefore, it seems reasonable to assume that the relationship is spurious.[59] That is, beer taxes correlate negatively with something else that is positively correlated with police resources. Perhaps voters who tend to support expenditures on policing also oppose taxation on beer. And of course, the most reasonable interpretation of the positive coefficient on POLICE is not that more police resources increase rapes, but instead, that victims are more likely to *report* rapes when they perceive that there are more police to respond to their report. Thus, not only is the relationship between beer taxes and police resources likely to be spurious, but if it is not spurious the resulting impact on reported rapes per-capita is likely to be reduced reporting by victims rather than reduced numbers of actual rapes per-capita. This clearly reveals the problems with accepting reduced form estimates when simultaneity bias exists, and furthermore, it reveals the problems with drawing policy inferences even if the simultaneous equation estimates *appear* to support the reduced form estimates (in sign at least, if not in magnitude).

Beer taxes have a negative impact in the inclusive model too, of course. In this case the impact feeds through the first stage liquor and wine equations. The coefficient on liquor in the second stage (+0.5301) reported in Table 5.2 is multiplied times the beer tax coefficient in the first-stage liquor regression (-0.2208) and that is added to the second stage wine coefficient (-0.7590) multiplied times the first stage beer tax coefficient in the wine regression (zero since

---

[59]Note that this type of relationship characterizes, at least to a degree, all of the policy estimates from the simultaneous equation models, as explained below, and therefore, the alcohol-policy conclusions drawn from the reduced-form murder regressions discussed above should also be considered in this light. Even though the test performed in Chapter 4 reject endogeneity for murder, that does not mean that the correlation between some alcohol control variable in the reduced form model only works through alcohol consumption. The suspiciously large liquor tax coefficient may reflect a correlation with one of the deterrence variables, for instance (this is the case for some of the 2SLS violent crime models discussed here, after all, as will be explained below), and therefore, at least part of the correlation may be spurious.

the coefficient is insignificant), producing the negative policy impact. So beer taxes are negatively correlated with liquor consumption, which in turn is positively correlated with reported rapes per-capita, producing a negative beer tax effect on rapes. If the assumption is that increasing beer taxes and therefore beer prices will reduce liquor consumption, the apparent implication is that beer and liquor are complements rather than substitutes. This would be a very surprising result. However, an alternative explanation appears to be likely in light of the fact that the estimated first stage coefficient on liquor taxes in the liquor equation is significantly positive (thus producing the positive policy impact for liquor taxes reported in Table 5.12. As suggested in the discussion of the reduced-form models, while the inclusive model probably is superior to the others in one sense (it alleviates the missing variable bias that arises when the various alcohol types are considered alone), it appears to introduce multicolinearity problems due to correlation between some of the alcohol control variables, including, for instance, a high correlation between liquor and wine taxes (furthermore, as explained below, these results apparently still suffer from simultaneity bias). Therefore, these tax coefficients are suspect.

Now consider the negative coefficient on LEGAL. Its impact is also working through the POLICE coefficient in the second stage regression in the beer-only model, since the BEER coefficient is not significant. The coefficient on LEGAL in the first-stage police regression is -0.0525, and multiplying that times 1.2695 produces the policy estimate in Table 5.12. So what does this imply? States with relatively high low drinking ages (and therefore high values for LEGAL) have relatively fewer police. Again, it is difficult to imagine a direct causal explanation for this negative correlation. The relationship is probably spurious, reflecting population characteristics that are not captured by the variables used in the regressions (e.g., voters who tend to support relatively permissive drinking age laws are also resistant to spending for police services). LEGAL is also negative in the wine model where it, once again, is dominated by its impact on the POLICE coefficient in the second stage regression. In this case, there are two affects at work. The POLICE coefficient is 1.4715, and the corresponding first-stage LEGAL coefficient is -0.0482, but this implied effect is increased by the significant WINE coefficient (-0.4215) and its corresponding marginally significant first-stage LEGAL coefficient (+0.0558). The same spurious relationship involving police employment is reinforced by the fact that wine consumption rises as LEGAL rises (drinking age falls) and wine consumption is negatively related to rapes per-capita. As suggested above, the wine coefficient in the second stage model clearly does not imply that more wine consumption reduces rapes. Rather, it suggests that the circumstances in which wine is consumed are not conducive to such crimes. Finally, consider the inclusive model where LIQUOR and WINE are the potential sources of policy effects. In this case, however, the coefficient on LEGAL in the first-stage liquor equation is not significant, so its only impact is through its +0.0842 coefficient in the first-stage wine regression (multiplied the second-stage wine coefficient of -0.7590). Drawing any strong policy inferences from these relationships is, therefore, highly questionable.

Increases in tourism are also associated with lower numbers of rapes per-capita in the beer and wine models (as was the case in the reduced form models). However, this impact also feeds through the same first-stage regressions as LEGAL does. In the beer-only model, for instance, tourism is significant and negative in the first stage police regression, in the wine model it is positively and significantly related to wine consumption, and in the inclusive model, tourism is insignificant in both the first stage liquor and wine regressions. DRYPER also appears significant and negative in the wine and inclusive models because it has a significant and positive coefficient in the first stage wine regression in both models. It is also positively related to liquor consumption but the resulting positive effect (+0.5302 from the second stage times +0.1206 from the first-stage liquor equation) is more than offset by the negative impact through wine (-0.7590 times +0.2582).

Finally, let us consider the various beer market variables that appear to be important policy tools in the beer and the inclusive models. Again, they do not have an impact through beer consumption since the second stage beer coefficient is insignificant. They also must work through the POLICE coefficient in the beer model and the WINE and/or LIQUOR coefficients in the inclusive model. In the beer model, FORCE has a marginally significant +0.0495 coefficient and MINDIST has a significant +0.0001 coefficient. These correlations cannot be seen as causal, so once again drawing policy inferences would be inappropriate. In the inclusive model, the liquor regression has a significantly positive coefficient on MANDATE and on FORCE, while CASHLAW is significant and negative. Perhaps MANDATE and FORCE cause a substitution of liquor for beer, and since liquor has a positive effect on rape in the inclusive model, they could lead to more rapes. This might seem reasonable, but FORCE is also significant and positive (MANDATE has a positive sign too, but it is insignificant) in the first stage beer regression, which undermines this substitution affect hypothesis. Furthermore, CASHLAW should have a similar effect unless it correlates with other efforts by the state to raise the price of liquor too. The affects from the liquor equation dominate for MANDATE and FORCE since neither coefficient is significant in the wine regression, but CASHLAW is also significant and negative in the wine regression, and this effect is larger than the impact that feeds through the second-stage LIQUOR coefficient. Thus, the aggregate policy impact appears to be in the same direction for all three variables, but they actually arise through different channels. Therefore, while these impacts tend to contradict the findings in the reduced form models (except for FORCE), they really do not provide clear policy guidelines in either the reduced-form or the simultaneous-equation cases.

The various controls for drunk driving policy are irrelevant in the beer-only model, but they appear to have impacts in the wine and inclusive models. As with the reduced-form models, however, the results are mixed and apparently contradictory. DRAM is positive in both cases, but this is because it is negatively (and marginally significantly) related to wine consumption (i.e., it discourages wine consumption) which is in turn negatively related to rapes (DRAM is insignificant in the first stage liquor regression). Similarly, in the wine-only model NOPLEA and SUS are also negatively related to wine consumption while JAIL, FINE, and ILDUM are positively related to wine consumption. In the inclusive model, NOPLEA is once again significant and negative in the wine regression, JAIL is positive and significant, and the other criminal DUI variables are insignificant, but in the liquor regression, NOPLEA is positive and significant (reinforcing the impact from the wine regression), as is FINE.

The bottom line seems pretty clear. While the second stage regressions for rape support the hypothesis that there may be a relationship between liquor and rapes, and while both reduced-form regressions and aggregate policy impacts derived from simultaneous equation models suggest that some alcohol policy options may be effective at reducing rape, careful consideration of the source of these potential impacts in the simultaneous equation model casts considerable doubt on all of these conclusions. No clear alcohol-policy implications can be taken from these simultaneous-equation models of the determinants of per-capita rapes. Indeed, the correlation between rape and various alcohol policy variables appears to be largely spurious, and the correlation between alcohol consumption measures and rape may be spurious too.

Per-Capita Robberies. The predicted effects of alcohol control policies on the incidence of per-capita robberies are presented in table 5.13. In this case, all of the models have predicted impacts because at least one of the deterrence or alcohol coefficients is significant in each of the second stage regressions. However, note that in the liquor-only, wine-only, and total-alcohol models reported in Table 5.3, the only significant second stage variable of relevance is POLICE. Therefore, all of the alcohol policy effects arise through the first stage police equation rather than consumption equations. For reasons suggested above, any policy inferences drawn from such relationships are probably inappropriate, as the correlation is likely to be spurious. Thus, we shall not go into detail regarding the *apparent* (but inappropriately so) policy inferences from these three models.

POLICE is also significant in the beer-only and the inclusive models reported in Table 5.3, so the policy impacts in Table 5.13 for these models are partially impacted by this relationship. However, consumption coefficients are also significant in these two models (BEER in the beer-only model, and both LIQ and WINE in the inclusive model where BEER is insignificant). The relative contribution of the POLICE and the consumption impacts of the policies for the beer model are considered first. The second stage POLICE coefficient (+2.0594) is multiplied by first stage coefficients on beer taxes (-0.0810), LEGAL (0.0 since the variable is insignificant), DRINK (+0.0291), MINIDIST (0.0), FORCE (0.0), the tourism variable (-0.0976), and NOSIGN (+0.0201). These results are added to those derived by multiplying the BEER coefficient (+1.6911) times each of the first-stage coefficients in the consumption equation: beer taxes (-0.6569), LEGAL (-0.0525), DRINK (0.0), MINIDIST (+0.0001), FORCE (+0.0495, a marginally significant coefficient), TOURPCT (-0.1438), and NOSIGN (0.0). Note that the DRINK and NOSIGN relationships arise exclusively because of their correlations with police employment. While more drinking establishments and the existence of regulations against advertising on signs are both correlated with more police, it is difficult to see any causal argument that would justify the use of these policy options to control robberies. The tourism variable is negative in both of the relevant first-stage models, and produces results consistent with the reduced-form model, probably with the same explanation being relevant (i.e., tourism is endogenous as tourists are attracted to states with low violent crime rates). On the other hand, LEGAL, MINIDIST, and FORCE are working exclusively through beer consumption, and the beer tax impact through consumption is quite large in absolute terms (beer taxes are negatively correlated with police employment too, implying that the tax impact reported in Table 5.13 is probably at least partly spurious, but note the relative magnitude of the two relationships: the impact through consumption is apparently almost seven times the impact through the police equation), so perhaps these policy options can be supported.

The relationship with LEGAL implies that lowering the drinking age (i.e., raising the portion of the legal drinking population that is below the age of 21) will reduce robberies, a result also implied in the reduced-form model. While this negative correlation may be correct (e.g., young drinkers may be less attractive targets for robbery than older drinkers because they may be more likely to resist and/or because the valuables they carry are less attractive), we would be very hesitant to extrapolate the relationship beyond these data to infer that it would hold as more and more states lower the drinking age. Furthermore, lowering the drinking age would have serious consequences in other areas (e.g., increased DUI fatalities). A forced deposit law apparently increases robberies too. Perhaps it encourages a substitution of drinking in bars rather than buying beer to take home, by raising the full-price of at-home beer consumption (either the money price is higher if bottles are not returned, or transactions costs are higher if bottles are saved and returned to lower the money price) thereby creating more targets for robbers. Alternatively, raising the full price of beer may lead to a substitution of liquor for beer, and as suggested by the inclusive model, it is really liquor that matters here. This explanation might also be consistent with the MINIDIST results, which presumably imply higher beer transport costs and therefore higher beer prices will increase robberies. It tends to be contradicted by the beer tax results, however, since higher beer taxes reduce beer consumption in the simultaneous equation model, and apparently reduce robberies. In this regard, however, recall the discussion of Tables 5.3 and 5.11, where it was noted that the beer-only specification apparently suffers from missing-variable bias, as beer consumption loses significance in the inclusive model while liquor consumption becomes significant (and as explained below, beer taxes are negatively related to liquor consumption in the inclusive model, further undermining the substitution hypothesis, although multicolinearity is probably a problem in that model). Thus, the policy inferences drawn from the beer-only model suffer from the same missing variable bias and must be considered with extreme caution.

In this light, let us turn to the inclusive model where three of the second-stage coefficients of relevance are significant: POLICE (+1.0447), LIQ (+0.8983), and WINE (-1.0440). The significant coefficients from the first-stage police regression are for liquor taxes (-0.4498), FORCE (-0.0614), and MINIDIST (+0.0002). The first-stage liquor consumption regression has several significant policy variables: beer taxes (-0.2208), liquor taxes (+1.0049), MANDATE (+0.1017), CASHLAW (-0.0395), FORCE (+0.1647), and DRYPER (+0.1206). Finally, the first-stage wine consumption equation includes significant coefficients for LEGAL (+0.0842), liquor taxes (+0.510), CASHLAW (-0.0509), DRYPER (+0.2582), and NOSIGN (+0.0575). Note that MINIDIST is significant only in the first-stage police regression, suggesting a spurious policy implication. Similarly, NOSIGN and LEGAL's estimated policy impacts arise because their positive correlations with wine consumption which in turn is negatively correlated with robberies, but for reasons suggested above, such relationships do not provide clear policy implications. DRYPER's net negative effect also reflects its positive correlation with wine consumption and the fact that the resulting negative impact is greater than the offsetting positive correlation that this variable has with liquor consumption. CASHLAW's apparent positive sign is similarly dominated by its negative relationship with wine consumption which is larger than the offsetting negative relationship it has with liquor consumption.

Four non-DUI policy variables remain for discussion, MANDATE, FORCE, and the beer and tax variable, all of which work exclusively through the first-stage liquor consumption equation, and the liquor tax variable which is significant in all three of the relevant first-stage regressions. MANDATE and FORCE are beer market policy variables which appear to positively affect liquor consumption in the inclusive model. Perhaps these policies raise the price of beer relative to the price of liquor, leading to a substitution of liquor for beer, as suggested above. However, the apparent negative impact of beer taxes on liquor consumption appears to undermine this substitution effect hypothesis. As suggested in the context of the discussion of rape policies, however, the tax variables appear to suffer from multicolinearity bias (or as explained below, they may still suffer from simultaneity bias). In this regard, note that the liquor tax is actually positively and significantly related to liquor consumption, a result that is inconsistent with economic theory and most empirical studies. The reason for the negative sign on liquor taxes in Table 5.13 is that the apparent positive impact through liquor consumption is offset by the fact that liquor taxes are negatively correlated with police employment and positively correlated with wine consumption. Thus, it appears that, as with the rape estimates, these policy implications should be viewed with extreme caution.

Finally, let us briefly consider the DUI policy variables. Only DRAM is significant in the beer model, and that is because it is significant in the first-stage police equation (states with dram shop laws tend to have more police). It is insignificant in the beer consumption regression. DRAM also appears positive in the inclusive model, but for a different reason. It is marginally significant in the police regression, but its sign is negative. However, it is also marginally significant and negative in the wine consumption equation, which produces a larger positive effect than the negative impact working through the police regression (DRAM is insignificant in the liquor consumption regression). The criminal law DUI variables once again produce mixed implications, with three negative impacts (PBT, JAIL, and ILDUM), and two positive impacts (NOPLEA, FINE). These relationships reflect the significant negative correlation between police employment and the PBT and FINE variables, positive (negative) correlations between liquor consumption and the NOPLEA and FINE (ILDUM) variables, and negative (positive) correlations between wine consumption and NOPLEA (JAIL). While some potential policy impact might be implied by such results, it is not clear what it is, particularly given the collinearity between the variables.

The implications drawn above for rape are, by in large, relevant for robbery too. In particular, even though the second stage regressions for robbery support the hypothesis that there may be a relationship between alcohol and robbery, and even though both reduced-form regressions and aggregate policy impacts derived from simultaneous-equation models imply that some alcohol policy options may be effective at reducing robberies, careful consideration of the source of these potential impacts in the simultaneous-equation model casts considerable doubt on all of these conclusions. No clear alcohol-policy implications can be taken from these simultaneous-equation models of the determinants of per-capita robberies, since the correlation between robberies and various alcohol policy variables appears to be largely spurious.

Per-Capita Assaults. The predicted effects of alcohol control policies on the incidence of per-capita assaults are presented in Table 5.14. The implications here are similar to those for robbery and rape in one respect: they should be considered with caution because the implied policy impacts actually work through a complex web of interactions. Indeed, the relationships are even more complex for assault than they are for rape and robbery, because each of the second-stage regressions reported in Table 5.4 contain at least two significant coefficients of relevance. The only significant coefficients in the liquor specification are for POLICE and PRISON, however, so the apparent policy impacts in Table 5.14 for that model are largely spurious. Similarly, for the wine model, the significant coefficients are for POLICE, PRISON (marginally), ARREST (marginally), and WINE which is negative. For reasons noted earlier, these results are not likely to provide realistic policy implications. Therefore, we are left with the beer, total, and inclusive models to consider in more detail. We shall consider the non-DUI variables for each in turn, and then briefly consider the DUI variables for all of the models.

In the beer-only model, MANDATE, FORCE, and DRYPER are only significant in the first-stage prison regression. They have no impact in the beer consumption equation. Therefore, the apparent policy implications for these variables are probably spurious. On the other hand, DRINK and NOSIGN are significant in the beer consumption regression but not in the prison equation. They imply that more liquor outlets and anti-sign-advertising laws both tend to increase assaults. The first result makes sense, of course, given the implication that beer consumption causes assaults. If the beer market is highly competitive so prices are low and beer is widely accessible, more beer will be consumed. The NOSIGN results are not intuitive, however, although they are common in the beer-only models (see Table 5.16, and note that the implication tends to be consistently reversed in the inclusive models, suggesting that this could be a missing variable problem). The other two variables are significant in both the beer consumption and prison regressions, so the impacts in Table 5.14 are aggregations of two effects. The second stage coefficient for beer (+2.9231) and prisons (+0.8437) are multiplied times -0.0809 and +0.1844 respectively to produce the beer tax policy estimate, and by -0.0976 and +0.2727 respectively to generate the TOURPCT estimate. Since the two impacts are offsetting in each case, the net result reflects the dominance of the beer consumption relationships. The negative sign on tourism is consistent with several others already discussed, and probably reflects the endogeneity of this variable. However, raising beer taxes appears to be a way to reduce assaults. This implication must be considered with caution, however, as it is reversed in the inclusive model (see the discussion below).

Four of the policy variables in the total alcohol model effects arise exclusively through the deterrence regressions (police and/or prison): MINIDIST, DRYPER, MANDATE, and FORCE. Since these variables do not significantly effect total alcohol consumption the apparent policy implications are probably spurious. NOSIGN gets its positive sign because it is positively related to total alcohol consumption, as it was with beer consumption in the beer-only model, perhaps for the same reason. DRINK and TOURPCT are both significant in the total alcohol consumption and prison first-stage regressions, while total alcohol taxes is significant in all three relevant regressions. Therefore, to obtain the effects of DRINK reported in Table 5.14, the second-stage regression coefficients on PRISON (+0.5850) and total alcohol consumption (+0.9812) must be multiplied by -0.0698 and +0.0376 respectively. The effects are offsetting so the larger consumption effect produces the positive sign. Increasing alcohol sales outlets appears to increase assaults, possibly either because alcohol is less costly and more easily obtained and alcohol causes assaults, or because more people are in situations which are conducive to assaults. Tourism's coefficients in the first-stage prison regression is +0.3979, and it is -0.1190 in the total alcohol consumption equation, so while tourism's impact on assault would be negative like several other cases already discussed if its impact only worked through the consumption regression, the positive correlation with prison dominates in this case.

The alcohol tax impact in Table 5.14 suggests that increasing alcohol taxes will reduce consumption and therefore reduce assaults, but this is not the case. The alcohol tax coefficients in the police and prison regressions are negative (-0.1416 and -0.6039 respectively), but the coefficient in the alcohol consumption regression is *positive* (+0.1413, multiplied times the +0.9812 total alcohol coefficient in the second stage regression). Thus, the negative relationship reported in 5.14 reflects the dominant effect of the two deterrence regressions. Clearly, any policy inferences drawn from this finding would be questionable.

Finally, let us turn to the inclusive model. In this case, the variables working exclusively through the prison regression are MANDATE, FORCE, MINIDIST and beer taxes. In other words, even though both beer and wine are significant in the second stage regressions, the estimated impacts of beer and wine taxes are insignificant in both first-stage consumption equations, as are the three beer market variables listed above. Thus, these *apparent* policy relationships are probably purely spurious. CASHLAW, DRYPER, and NOSIGN produce significant effects in the prison and wine-consumption regressions, so for reasons discussed above, these policy estimates also probably will have no relevant implications. LEGAL appears to have a negative effect, but it is because it is positively correlated with wine consumption. It is insignificant in the beer consumption equation. Tourism and liquor taxes are significant in all three regressions, while DRINK and wine taxes are significant in the prison and beer regressions. That is, as with the beer-taxes/beer-consumption relationship, wine taxes are not significant determinants of wine consumption.

The second-stage coefficients of relevance for determining the cumulative impact of these variables are +0.4045 (PRISON), +1.5118 (BEER), and -0.8107 (WINE). DRINK has a coefficient of -0.641 in the prison regression and +0.0424 in the beer consumption regression, producing the net positive impact of this variable on assaults, as in the beer and total models. Thus, limitations on alcohol outlets consistently appears to be a potential mechanism for reducing assaults, subject to the caveats raised below regarding remaining simultaneity bias in these models. TOURPCT has coefficients of +0.4542, -0.1229, and -0.1831 in the prison, beer-consumption, and wine-consumption regressions respectively, producing the net positive impact reported in Table 5.14.

Alcohol tax effects are somewhat surprising. The wine tax variable is positively correlated with prison population, with a coefficient of +0.0907, and negatively correlated with beer consumption (-0.0320) so the net effect is negative, but as noted above, it does not work through wine consumption itself. Perhaps beer is a substitute for wine, so raising wine taxes leads to greater beer consumption (although it should also reduce wine consumption if this is the case), thereby increasing assaults, but drawing such an inference is questionable for reasons discussed earlier (e.g., multicolinearity, remaining simultaneity bias). The liquor tax coefficients in the three first-stage regressions are -0.8072 (prison regression), +0.3752 (beer equation), and +0.2533 (wine regression), so the estimated negative effect of liquor taxes reflects the combined negative impacts through the prison and wine consumption models which offset the positive impact through the beer consumption model. It is not clear what should be made of this result either. Perhaps liquor is a substitute for both beer and wine, so higher taxes on liquor increase consumption of both (although it should reduce alcohol consumption if this is the case). Then, if beer consumption is causally related to assaults, assaults would rise, but this is offset here because of the negative correlation between wine consumption and assault (and all other violent crimes), along with the apparent spurious relationship between liquor taxes and prison population. The conclusions reached above for rape and robbery appear to apply again, perhaps with one exception. It appears that limits on the number of alcohol outlets could reduce assaults. However, even this relatively robust policy relationship is suspect in light of the multicolinearity (and remaining simultaneity bias discussed below) that plagues these data. Briefly note that once again, the implications drawn from the DUI policy variables are inconsistent and conflicting. In each case involving significant consumption variables, DRAM is positive. This occurs because of a positive impact on beer consumption in the beer and inclusive models, a positive impact on total alcohol consumption in the total model. Again, each model includes both positive and negative criminal-law DUI effects, so the net effect of DUI controls clearly cannot be determined.

Per-Capita Total Violent Crimes. The predicted effects of alcohol control policies on the incidence of total violent crimes (TVC) per-capita are presented in table 5.15. The policy predictions in the liquor and total alcohol models arise entirely through the police and/or prison regressions, however, suggesting that the relationships are spurious. Similarly, while wine consumption is significant in the wine model, it is negative, which has little to say for alcohol policy, as explained above. Therefore, we shall focus on the beer and the inclusive models where potentially relevant consumption impacts arise. Since the implications of the DUI control variables are, once again, mixed and contradictory, we shall not discuss them, focusing instead on the more direct alcohol control policies.

In the beer model LEGAL, FORCE, MINIDIST, MANDATE, and DRYPER have predicted impacts because they are significant in the police and/or prison first-stage regressions. They are not significant in the consumption equation, suggesting that these are probably spurious correlations. DRINK and NOSIGN are both positive in the beer consumption regression, but insignificant in the deterrence regressions. Not surprisingly, since assaults are the largest component of total violent crimes, this is consistent with the assault results and the same interpretations apply. Tourism is negative in the police and beer regressions and positive in the prison equation, producing the negative net impact that has been discussed previously. This leaves beer taxes, which are negatively related to police employment, positively related to prison population, and negatively related to beer consumption, with a net negative effect. Therefore, as with the beer-only model (but not the inclusive model) for assault, the implication may appear to be that higher beer taxes have a negative impact on total violent crimes, although the same important caveats remain.

Five of the six relevant coefficients are significant in the second stage of the inclusive model, suggesting that the policy relationships are very complex in this case. Indeed, the only coefficients that are significant in just one or both deterrence regressions (police and prison population) are MINIDIST (positive in both) and TOURPCT (positive in the prison regression; note that it has a negative sign in all of the consumption equations but they are all insignificant). LEGAL is positive in the first-stage wine regression but insignificant in all other equations, producing the negative effect reported in Table 5.15. DRYPER is also positive in the wine regression, and this apparent policy impact is reinforced by a significant negative coefficient in the police equation. Therefore, these two variables probably do not provide useful policy options for reasons suggested above.

In order to see the sources of the other policy estimates, note that the coefficients in the second stage model are +0.5931 (POLICE), +0.2321 (PRISON), +0.8356 (BEER), +0.47263 (LIQ), and -0.9218 (WINE). With these estimates in mind, let us consider the beer market variables (other than taxes, discussed below). Only CASHLAW actually has a significant effect in the beer consumption regression where its coefficient is -0.0159. This effect is complemented by the -0.4938 coefficient on CASHLAW in the liquor consumption regression, and offset by a -0.0573 coefficient in the wine regression and a +0.0527 coefficient in the prison equation. The net effect is the positive estimate reported in Table 5.15, but obviously not because of its impact on beer consumption. Clearly, the relationship with prison population is probably spurious but the impacts on liquor and wine are not obvious either, and probably spurious too. The other beer market variables are insignificant in the beer consumption equation but significant elsewhere. MANDATE and FORCE are both positive (+0.1945 and +0.1334 respectively) in the prison population regression and the liquor regression (+0.1104 and +0.0979 respectively, perhaps because of a substitution effect, but in that case it should be negative in the beer regression). NOSIGN is positive in both the beer and wine regressions (+0.0183 and +0.0537) but the resulting negative effect through the wine regression is greater than the positive effect through the beer equation, creating the apparently incorrect impression that in aggregate this regulation can reduce alcohol consumption and therefore violent crime. DRINK is also positive in the beer regression (+0.4153), suggesting that increasing the number of alcohol outlets increases beer consumption, but the estimate in Table 5.15 is relatively small because DRINK has a significant negative correlation (-0.0623) with prison population. Thus, limitations on outlets might reduce consumption and violent crime.

Wine taxes do not have a significant effect on wine consumption. The negative policy estimate for this variable arises because wine taxes are negatively correlated with beer consumption (-0.0263) and this impact is larger than the positive effect arising in the prison regression (+0.644). Liquor taxes are even more surprising, since they are positively related to liquor consumption (+0.3941) and beer consumption (+0.4333), but these effects are offset to a degree by negative coefficients in the police (-0.2492) and prison population (-1.2540) regressions. Beer taxes are the only alcohol taxes that have the anticipated relationship in their own consumption model (a -0.1184 coefficient), but the effect is reinforced in the estimate reported in Table 5.15 by a negative impact in the liquor consumption regression (-0.3129) and offset by a positive relationship to prison population (+0.5016). Thus, it might be concluded that beer taxes can be effective in limiting total violent crime, although the conclusion must be considered with caution in light of the potential multicollinearity problems noted above, and the remaining simultaneity biases that are discussed below. Before considering this simultaneity issue, however, let us sum up the implications that can be drawn from the two procedures used to derive policy effects.

Summing Up. Table 5.16 summarizes the signs from the simultaneous-equation estimates and compares them to the reduced-form estimates provided above. In this case, however, while 0 implies no significant impact, + indicates at least a marginally significant positive impact and - implies at least a marginally significant negative relationship. The symbol before the slash (/) represents the reduced form results and the symbol after the slash represents the simultaneous equation implications. When the estimates for all impacts are zero for the simultaneous equation results, it means that the second stage coefficients for the deterrence and alcohol variables are all insignificant [these models are also indicated with superscript a]. Furthermore, in several cases there are important reasons for questioning the relevance of the simultaneous-equation estimates. For instance, many of the *apparent* alcohol policy impacts actually arise exclusively through the first-stage deterrence regressions since all second-stage consumption coefficients are insignificant. The resulting policy relationships are, therefore, largely if not entirely spurious. This is the case for some models, and for some policy implications derived in other models, so the individual coefficients (and where relevant, the models) are marked with a superscript b. In addition, since the WINE coefficient in the second stage regressions are generally significant and *negative* in most of the models where it appears, policy inferences that are dominated by their influence on wine consumption do not provide relevant implications. Therefore, all policy variables whose signs reflect the dominance of the impact derived through either the first-stage wine regressions or the first-stage deterrence regressions are indicated by a superscript c. Another potentially misleading result occurs when beer market policy variables appear significant but the effect arises through non-beer market first-stage results (either because the variables have no impact in the beer consumption model, or because the beer consumption coefficient is insignificant in the second stage regression). These results are indicated with a superscript d. Finally, wine taxes appear to be important in some cases, even though the impact does not arise through the wine consumption regression, arising instead through some other channel (e.g., the liquor, beer, and/or deterrence regressions). These results are designated with a superscript e.

Table 5.16

Comparison of Coefficient Signs of Selected Alcohol Policy Variables between Reduced-Form Specifications and Simultaneous Equation Models for Violent Crime Categories (1985-1994)

Crime/ Model	Taxes: Beer	Taxes: Liquor	Taxes: Wine	Taxes: Total	Legal	Drink	Mindist	Cashlaw	Force	Dryper	Nosign	Noprint	Mandate	Tourpct
RAPE														
Beer[b]	-/-[b]				0/-[b]	0/0	-/+[b]	0/0	0/+[b]	+/0	+/0	0/0	0/0	-/-[b]
Liq.[a]		0/0			0/0	0/0				0/0	0/0	0/0		0/0
Wine[c]			-/+[c]		0/-[c]	0/0				+/-[c]	0/0	-/0		-/-[b]
Tot.[a]				0/0	0/0	0/0	-/0	-/0	0/0	0/0	0/0	0/0	0/0	0/0
Inc.	-/-[d]	+/+	0/0		0/-[c]	0/0	-/0	-/+[c]	+/+[d]	+/-[c]	0/-	0/0	0/+[d]	
	0/0													
ROBBERY														
Beer	0/-				0/-	+/+[b]	0/+	0/0	+/+	+/0	-/+[b]	0/0	0/+	-/-
Liq.[b]		0/-[b]			0/0	+/0				0/-b	-/0	+/0		-/-[b]
Wine[b]			0/0		0/-[b]	0/0				0/0	-/0	0/0		-/-[b]
Tot.[b]				0/-[b]	-/-[b]	+/0	0/+[b]	-/+[b]	+/+[b]	0/-[b]	-/-[b]	0/0	0/+[b]	-/0
Inc.	0/-[d]	0/-[c]	0/0		-/-[c]	+/0	0/+[b]	-/+[c]	+/+[b]	0/-[c]	-/-[c]	0/0	0/+[d]	-/0
ASSAULT														
Beer	+/-				+/0	0/+	0/0	0/0	0/+[b]	0/+[b]	0/+	0/0	0/+[b]	
	0/-													
Liq.[b]		0/-[b]			+/0	0/0				0/+[b]	0/0	0/0		-/+[b]
Winec			0/+[c]		+/-[c]	0/0				0/-[c]	0/+[c]	0/+[c]		-/+c
Tot.				0/-[c]	+/0	0/+	0/+[b]	0/-	0/+[b]	0/+[b]	0/+	0/+	0/+[b]	-/+c
Inc.	/+[b]	0/-[c]	0/-[e]		+/-c	0/+	0/+[b]	0/+[c]	0/+[b]	0/-[c]	0/-c	0/0	0/+[b]	-/+c
T.V.C.														
Beer	0/-				+/-[b]	0/+	0/+[b]	0/0	0/+[b]	0/+[b]	0/+	0/0	0/+[b]	-/-
Liq.[b]		0/-[b]			0/0	+/0				0/+[b]	0/0	0/0		-/0
Wine[c]			0/+[c]		+/-[c]	0/0				0/-[c]	0/+[c]	0/+[c]		-/-[c]
Tot.[b]				0/-[b]	0/0	0/-[b]	0/+[b]	-/0	+/+[b]	0/+[b]	0/0	0/0	0/-	-/+[b]
Inc.	/-	0/+	0/-[e]		0/-[c]	+/+	0/+[b]	-/+[c]	+/+[d]	0/-[c]	0/-[c]	0/0	0/+[d]	-/+[b]

Note: The symbol before a slash (/) represents the reduced form results and the symbol after the slash represents the simultaneous equation results for each policy variable. The symbols used are:

0 = no significant relationship;

+ = at least a marginally significant positive relationship;

- = at least a marginally significant negative relationship;

The superscripts indicate the following:

a: the estimates for all impacts are zero for the simultaneous equation results because the second stage coefficients for the deterrence and alcohol variables are all insignificant;

b: the *apparant* alcohol policy impacts actually arise exclusively through the first-stage deterrence regressions since all second-stage alcohol consumption coefficients are insignificant;

c: all policy variables' signs reflect the dominance of the impact derived through either the first-stage wine regressions (the WINE coefficient in the second stage regressions are generally significant and *negative* in most of the models where it appears) or the first-stage deterrence regressions.

d: beer market policy variables appear significant but the effect arises through non-beer market first-stage results.

e: wine tax impacts that do not arise through the wine consumption regression.

Given the qualifications listed above, one might ask why not simply consider the policy impacts that feed exclusively through the first-stage liquor and beer regressions and ignore the rest. One reason for stressing *all* sources of correlation is that this is precisely what is done in a reduced form equation. Therefore, revealing the very significant amount of spurious correlation that apparently exists between various alcohol policy variables and violent crime rates is one way of stressing one of the shortcomings of reduced-estimation procedures as a source of information about potential policy impacts when at least some of the underlying relationships actually involve simultaneous interactions.

We could simply stress that several signs change as we move from the reduced form to the simultaneous equations model to make this point, of course. Indeed, some of these changes lead to very different inferences. For instance, LEGAL was significantly and positively related to assaults (and negatively related to robbery), suggesting that large numbers of young drinkers was an important determinant of assault rates (but that they were unattractive targets for robbery). However, when corrected for simultaneity bias, this sign turns insignificant or negative, making it consistent with the implications for robbery where the sign remains negative. Several other coefficients also change sign or significance (either becoming significant or losing significance implied in the reduced-form results. For instance, the tourist variable was almost always significantly negative in the OLS regressions but it is frequently positive in the simultaneous equation models (although it still is negative in several models). One of the most dramatic changes in significance is with DRINK, which was only significant as a determinant of robbery in the OLS models, suggesting that people who drink out may be attractive as robbery victims. But in the simultaneous equation model, DRINK is positively related to assault in several models while losing significance in most of the robbery models. Similarly, MANDATE was never significant in the reduced-form models, but it is consistently significant and generally positive in the simultaneous equation models. Some things have not changed, of course. In particular, the criminal DUI variables continue to give mixed signals, with both positive and negative signs in every specification (although not necessarily for the same variables that produced the signs in the reduced form models).

Indeed, there are some consistencies across the simultaneous equation results too, perhaps suggesting robustness and therefore relatively strong conclusions. This brings us back to the qualifications listed above (and indicated by the superscripts in Table 5.16). These points suggest that even where implications from the reduced-form specifications *appear* to be robust when simultaneity bias is treated, they are still problematic. Much of the apparent correlation that is assumed to be causal in a reduced-form model is actually spurious. Thus, the aggregate policy impacts implied by the values reported in Tables 5.12 through 5.15 cannot be taken seriously. Indeed, perhaps the only implications that should be considered are those which do not have a superscript in Table 5.16. However, even some of these coefficients are suspect. For instance, the simultaneous equations estimates should be considered in light of the discussion of Table 5.6. Recall, for example, that the beer equations results for robbery were not robust when liquor and wine were added. That is, they suffered from missing variable bias. Therefore, the beer-only model's policy estimates for robbery discussed above should also be considered with skepticism, as they reflect the same missing variable problems. If we focus only on the exclusive models, however, we are still left with many puzzles. Liquor taxes appear to be positively related to rapes and total violent crimes, for instance. This simply does not make sense. One problem is probably multicollinearity in the alcohol tax variables, as noted above. Liquor and wine taxes are highly correlated, and since wine (and therefore wine taxes) tends to be negatively (positively) related to crimes, the correlated liquor tax variable tends to be too. These variables may also be correlated with other alcohol control, and/or DUI control variables. Another problem may be that simultaneity bias remains a problem, as explained below (an inference that can be drawn from some of the coefficients in the reduced-form models for variables employed as IV estimators). Tests for this possibility are available, so we consider them next.

5.5.1 Tests of the Simultaneous Equations Estimation Procedure

The 2SLS results presented for non-murder Index I crimes in the previous two sections relied upon the use of instrumental variables within a two-stage least squares (2SLS) estimation procedure to derive consistent parameter estimates of the deterrence and consumption variables after it was shown that these factors may suffer from simultaneity bias. The extent to which those estimates are *actually* consistent depends largely, *but not entirely*, on the degree to which the variation in the instruments is truly exogenous to the variation in the dependent variables.

Using instruments that are only weakly correlated with their endogenous counterpart will tend to inflate the standard errors on the coefficient estimates in the second-stage regression. However, Bound, et al. (1995) draw attention to two other estimation problems associated with the use of weak instrumental variables. First, even if the correlation between the instruments and the error term in the original equation [equation (4.1) in this case] is small (i.e., arbitrarily close to zero), a low correlation between the instrument and the endogenous variable may lead to large inconsistency in the second stage estimates. Of course, some correlation between the instrumental variables and the error term in the original equation is likely to be present given the extremely complex interactive nature of the alcohol-crime relationship. Second, low correlation between the instrument and endogenous variable will bias the 2SLS estimates in the direction of the OLS estimates in finite samples. The magnitude of this finite sample bias will approach the (potential) simultaneity bias present in the OLS estimates as the regression R^2 between the instrumental variables and endogenous measures approaches zero.

This section discusses the results of three tests conducted on each of the crime models to determine whether the 2SLS estimates suffer from the above estimation problems. One is a test of over-identifying restrictions in the spirit of Basmann (1960). The other two tests are developed and advocated by Bound, et al. (1995) to determine whether finite-sample bias is present in the second stage estimates. The first test to be considered is an F-statistic which refers to a test of the joint significance of the excluded instruments in each of the first-stage regressions. Values of this test statistic that approach one (the lower bound) indicate that the coefficient estimates from the second-stage crime regressions may suffer from finite sample bias. In addition, the F-test provides some insight into whether inclusion of state and year dummies into the first stage regressions biases the estimated policy effects downwards. Recall that the estimated coefficient on the excise tax in the beer consumption regression presented in Chapter 2 appeared to be highly sensitive to the inclusion of year dummies (the estimated elasticity was driven towards zero). Of course, the instruments in the first-stage regressions must have sufficient predictive power for the second-stage estimates to be valid. If controlling for fixed effects removes "too much" of the variation in the instrumental variables, the estimation results in Section 5.4 using the simultaneous equations may not be accurate representations of the actual policy effects. The second test is a partial R^2 statistic. This is simply the coefficient of variation from a regression of each of the first-stage dependent measures on its independent variables that do not also appear in the basic crime equation. Higher values of this statistic indicate that the instruments are good predictors of the endogenous measure and consequently that the second-stage estimates are less likely to suffer from finite-sample. The third test statistic is discussed below after briefly explaining the results from these first two.

We do not present the actual results here because they can easily be summarized.[60] The F-statistics are always statistically significant at the one-percent level. As such, the second stage estimates of the crime models are unlikely to be suffering from finite sample bias. In addition, the F-statistics indicate that the inclusion of state and year dummies in the first stage regressions is not likely to lower the predictive power of the excluded instruments to such an extent that the second stage estimates are rendered invalid. The partial R^2 statistics indicate that the instruments are good predictors of the endogenous variables.

Now we turn to the third test. Since the number of instruments employed in each specification exceeds the number of endogenous variables, insight into the exogeneity of the instruments can be garnered though tests of over-identifying restrictions on the excluded set of instruments. The test statistic is calculated by multiplying the number of observations times the coefficient of variation obtained from a regression of the residuals of the second-stage crime equations on all the exogenous and instrumental variables. The critical value of this test statistic is distributed as a χ^2 random variable. The degrees of freedom of this test statistic are equal to the number of over-identifying restrictions in the particular specification. A high value of the χ^2 test statistic (low p-value) rejects the null hypothesis of exogeneity of the over-identifying instruments. Again the actual statistics need not be presented, but in this case, because the results

[60] Actual test statistics are available from the authors upon request.

are uniformly disappointing. The computed p-values of these tests for all the crime categories and their specifications are small and thus the set of excluded instruments cannot be considered as exogenous to the crime equation. While disappointing, this is not surprising after examining the reduced-form results where POP45P was frequently significant as were some of the other IV estimators (e.g., TAX, ATPC). These instruments were employed because there were precedents for using each in the crime literature, therefore providing the potential for comparability, and frankly, because other potentially superior instrumental variables could not be found. However, the results here indicate the second stage estimates of the crime equations are not entirely purged of their simultaneity with the dependent measures and must still be considered biased estimates. Despite our efforts to deal with the endogeneity issue, we have not been successful with these state level data. Therefore, we shall turn to an alternative data set in hopes of dealing with the problem more effectively.

5.6 Conclusion

Several recent studies have demonstrated that minimum legal drinking age laws and/or beer excise taxes may be effective policy tools in mitigating the incidence of crime. The apparent efficacy of alcohol control policies has been found with the use of both individual (Markowitz and Grossman 1998a, 1998b, 2000; Grossman and Markowtiz 1999; Markowitz 1999, 2000a, 2000b) and aggregate level data (Chaloupka and Saffer 1992, Cook and Moore 1993a). Using a more fully specified model than those found in previous studies of alcohol and Index I crimes [e.g., Chaloupka and Saffer (1992), Cook and Moore (1993a)] and considering the potential endogeneity of several key variables (including alcohol consumption), the results of this Chapter initially *appear* support some of the findings of these previous studies, while also suggesting that some other alcohol policy variables could be important too. However, while a murder OLS regression does not appear to suffer from simultaneity bias, implying that reduced-form estimates for murder are relatively reliable, all other violent crime categories should be considered in a simultaneous equation framework, something that has not been done in the literature. Doing so apparently changes a number of inferences, suggesting that failure to control for simultaneity bias can produce misleading implications. Furthermore, consideration of the source of correlations between alcohol policy variables and violent crime rates in simultaneous equation systems reveals that much of the apparent correlation is probably spurious. These results also suggest that the same problem could plague the reduced-form murder regression. Regrettably, we also have not been able to fully purge the result of such biases from this state level data, however, so even the simultaneous-equation policy estimates that may not be spurious must be considered as tentative. Specifically, the above results must be interpreted with the caveat that the second stage estimates are most likely biased due to the weakness of the instruments employed in the first-stage regressions. The extremely complex interactive relationship between alcohol and crime that has received great attention outside of the economics profession. Given the numerous theoretical pathways through which various types of alcohol consumption might be correlated with crime and vice-versa, it is not surprising that the identification of instruments that determine consumption but not crime rate would prove problematic. One possible remedy for this situation might be the use of crime and consumption data at lower levels of geographical aggregation (e.g., the city or county level) where state policies can be considered truly exogenous.[61] Therefore, in an effort to avoid at least some of the simultaneity problems we turn to metropolitan level data

[61]Many of the estimated coefficients of the state indicators in the residual regressions (available from the authors upon request) are statistically significant.

on alcohol and crime rates, in hopes of avoiding at least some of the simultaneity that produces state level policies.[62]

[62]Another factor may be that the use of the prison-overcrowding legislation dummies to predict the *level* of prison populations is appropriate. Using the same prison population and litigation status data over the years 1971-1993, Levitt (1996) concludes that the prison-overcrowding instruments are exogenous to *growth rates* in crime. As such, estimation of the model in terms of differences may change the outcome of the test of over-identifying restrictions. Indeed, the potential sensitivity of over-identification tests in aggregate crimes models is demonstrated in Doyle et al. (1999). For their total property crime model, Doyle, et al.'s over-identification tests fails to reject the null of exogeneity of the excluded instruments when estimation is conducted in either levels or first-differences. However, for violent crimes the null is rejected in levels but *not* in first-differences.

CHAPTER 6

THE EFFECTS OF ALCOHOL CONSUMPTION AND ALCOHOL CONTROL POLICIES ON VIOLENT CRIME: EVIDENCE FROM METRO LEVEL PANEL DATA

6.1 Introduction

In hopes of alleviating the simultaneity problems that plague state level data, we obtained data on metropolitan level alcohol shipments. These data are matched with metropolitan crime rate data in an effort to re-estimate the relationships explored in Chapters 3, 4, and 5 where state level data were used. Alcohol shipments data are reported for 50 metropolitan areas. Regrettably, beer shipments have only been compiled for five years (1994-1998). Liquor and wine data are available for longer time periods (1991-1998), and all the data are employed in the liquor and wine specifications, but the limits on the beer sample also limit the sample size for the total-alcohol and the inclusive specifications. Furthermore, missing observations for several variables reduced the sample size even more. Thus, for instance, while 130 observations are available for the beer-consumption model, only 85 observations can be used for total-alcohol consumption model, and only about 65 are complete in the total alcohol and inclusive crime rate models. These limits on sample size severely limit our ability to control for potential determinants of both alcohol consumption and crime. Thus, as shall be seen, the results appear to be highly suspect due to missing variable biases (and probably even more severe multicollinearity than in the state-level data). They are, nevertheless, reported below because the results do support important general implications derived from the state-level analysis. Section 6.2 reports estimates of alcohol consumption regressions corresponding to those in Chapter 3, while Section 6.3 presents basic crime models with alcohol consumption as an explanatory variable, similar to those in Chapter 4. Limits in sample size and multicollinearity problems prevented endogeneity testing such as those employed in Chapter 4, as explained in Section 6.4, so we take a conservative approach and report both reduced-form and simultaneous-equation estimates for all crime categories (including murder where endogeneity was rejected using the state level data) in Sections 6.5 and 6.6 respectively (although data limitations mean that many of the models simply will not estimate). The implications from these reduced-form and simultaneous-equations are compared to each other and to the state level results in the conclusion in Section 6.7. Note that in all of the discussion we focus only on signs and significance rather than magnitudes, in part because the estimates are not likely to be reliable due to shortcomings in the data. This focus also facilitates comparisons across the various specifications and between the state and metro level findings.

6.2 Determinants of Alcohol Consumption

The purpose of this section is to empirically determine whether the level or existence of several alcohol control policies are correlated with various measures of alcohol consumption in metropolitan areas while controlling for other demand and supply (and therefore price and quantity) determinants. If the results are consistent with those reported in Chapter 3 where state-level data are used, those findings will be reinforced. Furthermore, whatever the findings, they may provide at least some insight into which policies might be effective in mitigating the incidence of criminal activity if alcohol consumption and violent crime appear to be related.

As in Chapter 3, four models of per-capita alcohol consumption are specified and estimated: beer, distilled spirits (liquor), wine, and total alcohol consumption. Determining the effects of alcohol control policies on alcohol consumption requires the specification and estimation of equation (3.4). As explained in Chapter 3, alcohol control policies are not randomly assigned across states. Individual states self-select their legal provisions regarding the consumption and distribution of alcoholic beverages. To account for sources of observation–specific heterogeneity that may bias the estimated coefficients in the structural model and to alleviate the problems of missing variables biasing the estimated coefficients in the structural model, we controlled for observation specific fixed-effects in Chapter 3 by estimating all consumption equations with the inclusion of state indicator (dummy) variables. We are unable to estimate fixed-effects models with the metropolitan data, however, because of the limited sample sizes. In an effort to at least partially control for such fixed effects, we include dummies reflecting the region in which the metropolitan area lies, using four broad regional categories (west, midwest, south, and northeast, with northeast being the reference group). Limits on sample size also force us to consider dropping other variables employed in the state-level analysis. Therefore, we chose not to include the DUI control variables, in part because they are highly collinear, making interpretation of their coefficients problematic anyway. All models are estimated with the inclusion of year dummy variables, however, in order to control for factors that may be fixed across observations but changing over time (e.g., national economic conditions). In this regard, the small sample size for the total-alcohol model does not allow us to simultaneously control for both regional and time fixed-effects, so separate regressions controlling for regional and then time effects are presented.

The data consists of panels of metropolitan-level and state-level observations over the years 1991 to 1998 for wine and liquor, and 1994 to 1998 for beer and total alcohol. All continuous independent variables are converted to logarithms, so the estimated coefficients may be interpreted as elasticities.[63] Monetary variables are adjusted for inflation as well as interstate

[63]The coefficients on variables expressed as percentages actually cannot be interpreted as elasticities, of course.

cost-of-living (COL) differentials. Detailed descriptions of data sources and their descriptive statistics can be found in Appendix B.

In the following empirical specifications of equation (3.4) the dependent variables are the per-capita levels of consumption of beer (BEER), distilled spirits (LIQUOR), and wine (WINE). In addition, a specification of total alcohol (TOTALC) consumption is estimated as well. Data on actual consumption of alcohol by beverage category does not exist, of course, so we employ metropolitan level shipments of each alcohol type (in gallons) as a proxy for consumption. Each of the alcohol shipment quantities is divided by metropolitan area population ages eighteen and over (rather than 21, the minimum legal drinking age) to account for underage consumption.

There are other variables in addition to the DUI-control variables and observation-specific fixed effects that were included in Chapter 3's regressions but are not included here (regional and time fixed effects are included in an effort to at least partially alleviate the missing variable biases, as noted above). First, all states had set their legal drinking ages at 21 by the beginning of our data periods, so there is no variation in LEGAL (all observations would be zero) and we cannot include it. Second, DRYPER is available only at the state level, not the metro level, so it is not included. There are also several variables which are used in place of the state-level variables that were employed in Chapter 3, either because the same variables are not available at the metropolitan level, or because an alternative variable appears to be better at capturing the hypothesized relationships that we are attempting to control for. The proportion of the population that is in poverty (POV90) in 1990 (from the 1990 Census) is used as a measure of income inequality, replacing GINI from the state level regressions. Similarly, NONMET is replaced by the percentage of the metropolitan population that resides in urban areas in 1990 (URB90). The portion of households that are single-women-headed in 1990 (FHH90) replaces the per-capita black population since the black population measure does not appear to capture the demographic effects of interest (note the FHH90 variable is generally significant in the following regressions). Another variable for which annual state level observations are available is also replaced with 1990 Census reported for metro areas - the portion of the population that is male between 18 and 44 (M184490). Finally, while real per-capita *disposable* income was used in the state level regressions, data on disposable income are not available at the metro levels so this variable is replaced with real per-capita income (INCOME).

Several other variables are only available at the state level, including the religion variables (CATH, MRM, SOBAP, and PROT), the unemployment rate (UR), and TOURPCT. They are, nonetheless, included. State policy variables are also included (CASHLAW, MANDATE, MINIDIST, FORCE, NOSIGN, and NOPRINT). DRINK is similarly based on the number of alcohol outlets in the state, but we divide outlets by MSA population ages 18 and over. The alcohol tax variables are the real (i.e., adjusted for inflation) federal plus state plus local excise tax on a gallon of beer, liquor, wine or total alcohol, adjusted for geographic differences in cost-of-living (COL). Note that the beer tax variable is therefore different from the one employed in the state level regressions where the taxes on a six-pack of beer was employed. The tax level that was in effect for the majority of the calendar is used for years in which the tax level changed.

For each of the consumption models we present three regressions. The first columns of Tables 6.1 through 6.4 report estimates from specifications which do not include controls for any fixed-effects in the first columns. The estimates reported in the second columns are for models which include dummies to control for year fixed-effects, and the results in the third columns are for specifications which control for both year and regional effects. In this case we report all of the coefficients including those for year and region dummy variables.

6.2.1 Per-Capita Beer Consumption

Table 6.1 presents the estimation results from metro-level per-capita beer consumption models. Note that out of the five years of data, all variables are available for only 150 observations.

As with the state level models reported in Chapter 3, TOURPCT takes a negative sign in all specifications and it is statistically significant. The proportion of the population that is male between the ages of 18 and 44 is positively and significantly correlated with per-capita beer consumption in each model, as anticipated. The same holds for FHH90. Several socio-economic (opportunity cost or deprivation) control variables are clearly sensitive to specification, however. Real per-capita income is negative and statistically significant in the first two specifications, for instance, but positive and significant when regional dummies are added. Similarly, the POV90 variable is significantly negative in the first regression, insignificant but negative in the second, and positive but still insignificant in the third. The URB90 coefficient is negative in each specification, but it is marginally significant in the first, significant at customary levels in the second, and insignificant in the third. On the other hand, the unemployment rate has an insignificant negative coefficient in the first specification but it is significantly negative in the second and third.

With respect to religious affiliation, PROT and CATH are found to be negatively correlated with per-capita beer consumption in every specification, and they are always statistically significant. The estimated coefficient on SOBAP is positive but statistically insignificant in the first two specifications but it turns negative and significant when regional effects are controlled for. On the other hand, MRM is negative and significant in the first and second models, but positive and significant when regional dummies are added.

Among the beer market variables, FORCE is always negative and significant, as is CASHLAW (although it is only marginally significant in the second specification). Thus, these policies appear to reduce beer consumption. On the other hand, MANDATE is positive and significant in the first two specifications, although it loses significance when regional dummies are included. MINIDIST is negative in the first two models (but only marginally significant in the second), but it turns positive and significant in the third. The variable DRINK is positive in all specifications and statistically significant, suggesting that limits on liquor outlets can reduce beer consumption. Finally, limitations on print advertising appears to reduce beer consumption as this coefficient is consistently negative and significant, while the NOSIGN coefficient changes from significantly negative in the first two models to significantly positive in the third. It is difficult to put too much faith on these estimated policy effects, however, because of the potential for missing variable due to the lack of observation-specific fixed effects. This potential problem is revealed by examining the coefficients on the beer tax variable.

The coefficient on real COL-adjusted beer excise taxes (BEERTAX) is significantly *positive* in every specification. This is a surprising result, of course, as taxes should be negatively related to consumption (although as explained in Chapters 2 and 3, the elasticity should not be large in absolute value). We believe that these positive signs are evidence of our inability to control for observation-specific fixed effects. For instance, in Mast, et al. (1999), a positive and significant tax effect was found in a model using 1984-1992 state-level data without state fixed effects, but the sign turned negative (although insignificant) when the state fixed effects were controlled for. The limited sample size prevents us from adding such controls here, as noted above, and while our hope was that regional dummies would suffice, they clearly do not. Thus, this coefficient appears to be unreliable due to missing variable bias, and the same clearly could be true of other coefficients.[64]

The expectation that missing variable bias is a problem is reinforced by adding the time and region controls. Doing so clearly influences the results, altering several coefficients as noted above, so it would not be surprising to find that better controls for fixed effects (e.g., metro dummies) would alter other coefficients (e.g., at least the tax variable). Note that the coefficients on several of these dummy variables are also significant, suggesting that per-capita beer consumption is greater in 1998 than in the earlier years, and that more beer is consumed per-capita in the northeast than in the south, west, and midwest.

6.2.2 Per-Capita Liquor Consumption

Table 6.2 presents the estimation results of the metro-level liquor specifications. Since several states regulate the distribution of liquor by mandating state-owned monopoly retail outlets, excise tax data could only be obtained on a subset of observations. Therefore, even though three more years of data are available for liquor consumption than for beer consumption, the liquor data is also relatively limited in sample size (we have 160 observations as opposed to the 130 used in the beer models).

Once again, TOURPCT takes a negative sign in all specifications and it is statistically significant. The proportion of the population that is male between the ages of 18 and 44 appears to be positively and significantly correlated with per-capita liquor consumption in the first two specifications, as expected, but when regional controls are added this coefficient turns negative

---

[64] Another possible explanation is that beer taxes are endogenous. Perhaps politicians take advantage of relatively high levels of beer consumption by imposing relatively high taxes on beer in order to generate larger tax revenues.

and insignificant. The same holds for FHH90 which is positive and significant in the first model, positive and marginally significant in the second, and insignificant but negative in the third. Real per-capita income and POV90 are both negative and statistically significant in all three specifications, while the URB90 coefficients is significantly positive in each specification. The unemployment rate has an insignificant negative coefficient in the first specification but it is significantly negative in the second and third.

The religious affiliation coefficients are quite consistent across the three specifications. CATH is always insignificant (although it does change sign), MRM is always positive and statistically significant, and SOBAP is always significantly negative. The only religious variable that appears to be significantly impacted by the fixed effects controls is PROT, as it is significantly negative in the first two specifications, but it becomes positive although insignificant in the third.

The variable DRINK is negative in all specifications and statistically significant, suggesting that limits on liquor outlets increase liquor consumption in contrast to their impact on beer. Perhaps such limits lead to a substitution of liquor for beer as they raise the cost of beer consumption relatively more than the cost of liquor consumption (although missing variable bias may also explain the sign of this coefficient). Limitations on print advertising appears to reduce liquor consumption as it did with beer, since this coefficient is consistently negative and it is significant in the first and third specifications. On the other hand, the NOSIGN coefficient is significantly positive in each regression. Again, it is difficult to put too much faith on these estimated policy effects, because of the potential for missing variable due to the lack of observation-specific fixed effects. And once again, this potential problem is revealed by examining the coefficients on the tax variable. The coefficient on real COL-adjusted liquor excise taxes is significantly *positive* in every specification, just as beer taxes are in the beer regressions. As with beer, we expect that these positive signs are evidence of our inability to control for observation-specific fixed effects. Thus, this coefficient appears to be unreliable due to missing variable bias, and the same may be true of other coefficients.[65]

6.2.3 Per-Capita Wine Consumption

Table 6.3 presents the estimation results of the wine consumption equations, where 174 observations from 1991 through 1998 are employed. Once again, TOURPCT is always significantly negative. M184490, POV90, URB90, and FHH90 are never significantly correlated with per-capita wine consumption. INCOME is negative and statistically significant in the first specification, but insignificant in the second and third, while UR has an insignificant negative coefficient in the first specification but it is significantly negative in the second and third. The religious affiliation coefficients are all negative, and the CATH and PROT coefficients are always significant while the SOBAP coefficient is always insignificant. MRM is significant in the first and second specifications but insignificant in the third.

DRINK is significantly positive in all specifications, suggesting that limits on liquor outlets reduces wine consumption as it does beer (but not liquor). Limitations on print advertising appears to reduce wine consumption as it did with beer and liquor, since this coefficient is consistently negative and significant. On the other hand, the NOSIGN coefficient

65 Another possible explanation is that liquor taxes are endogenous, as suggested in the preceding footnote. The expectation that missing variable bias is a problem is once again reinforced by adding the time and region controls since these additions alter several coefficients as noted above, so again, it would not be surprising to find that better controls for fixed effects (e.g., metro dummies) would alter other coefficients (e.g., at least the tax variable). Note that the coefficients on several of these variables are also significant, although the implications from the time dummies differ from those in the beer regressions. While per-capita beer consumption is greater in 1998 than in the earlier years, per-capita liquor consumption is less in 1998 than in all previous years. More liquor is consumed in the northeast than in the midwest, but the coefficients on the south and west dummies are not significant (although they are also negative).

is positive in each regression, and it is marginally significant in the first and significant at conventional levels in the third. Again, these results must be considered with caution because of the potential for missing variable bias due to the lack of observation-specific fixed effects, as the coefficient on real COL-adjusted wine excise taxes is significantly *positive* in every specification, just as beer taxes are in the beer regressions and liquor taxes are in the liquor equations.[66]

6.2.4 Total Per-Capita Alcohol Consumption

Table 6.4 presents the estimation results where the sum of beer, liquor, and wine consumption per-capita is used as the dependent measure. Due to missing observations on the liquor and wine excise tax data and the limited number of years of beer data, the sample includes only 85 observations. With this limited sample size, collinearity between the time and regional dummies is so high that the model will not estimate when both sets of fixed effect controls are included. Therefore, the third column in Table 6.4 differs from the third columns in the previous three tables. It contains results from a regression estimated with just the regional dummies rather than both regional and time dummies.

Once again, TOURPCT takes a negative sign in all specifications but it is not statistically significant in third column. The proportion of the population that is male between the ages of 18 and 44 is negatively and significantly correlated with per-capita total alcohol consumption in each model, as is FHH90. Real per-capita income and URB90 are insignificant in all specifications, while POV90 is significantly positive in all three models. The unemployment rate

[66] Wine taxes also may be endogenous, as suggested in the two preceding footnotes. The expectation that missing variable bias is a problem is once again reinforced by adding the time and region controls, however, as some coefficients change with these additions. Note that the coefficients on several of these variables are also significant. Per-capita wine consumption is greater in 1998 than in all previous years (similar to beer but the opposite of wine). Furthermore, more wine is also consumed in the northeast than in the midwest, but the coefficients on the south and west dummies are not significant (although they are also negative), as with liquor consumption (the northeast is also the region in which metropolitan areas with the greatest beer consumption per capita are located).

is insignificant in the first and third specifications but it is significantly positive in the second model where time dummies are included. PROT and SOBAP are always negative, but SOBAP is significant in only the first specification while PROT is significant in the first and second models. CATH and MRM are never significant.

Among the beer market variables, MANDATE and MINIDIST are both positive and at least marginally significant in every model, and FORCE is negative and significant in the first model but insignificant in the other two. DRINK is positive in all specifications but statistically significant only when no fixed effects dummies are included. Finally, limitations on print advertising appears to have no significant impact on total alcohol consumption, while the NOSIGN coefficient is significantly negative in the first two models and insignificantly negative in the third. In contrast to the individual alcohol models, the tax coefficients in these regressions are all negative, as anticipated. However, they are never significant. The potential for missing variable bias due to the lack of observation-specific fixed effects also makes all of these results suspect, although in this case (and again in contrast to the individual alcohol consumption models) the coefficients on the dummy variables are never significant.

6.2.5 Summing Up

As with the results from Chapter 3, the estimates in this section suggest that if alcohol consumption has a causal effect on crime then several alcohol control policies may be effective policy tools for reducing the incidence of crime given that they appear to be negatively correlated with alcohol consumption. Limiting the number of licensed liquor outlets and limiting print advertising may both reduce the per-capita consumption of beer, for instance, and both forced deposit and cash laws may as well. Limitations on print advertising also appear to reduce consumption of wine and distilled spirits (but, surprisingly, not total alcohol, which makes the results suspicious), although limiting liquor outlets apparently increase consumption of distilled spirits and total alcohol while reducing wine and beer consumption. The most widely advocated prescription of using excise taxes as a means of mitigating the negative consequences of alcohol consumption is not supported, however. Indeed, excise taxes are consistently positive and significant in all of the individual alcohol consumption models, and insignificant although negative in the total consumption model. This probably reveals a severe weakness with the data, however, as limited sample sizes prevent controls for missing variable bias in the form of observation-specific fixed effects. This problem means that all policy prescriptions must be treated with considerable caution. Nonetheless, we shall proceed with the next step in the analysis, an examination of the effects of alcohol consumption on violent crime rates.

6.3 Alcohol and Violent Crime: Empirical Estimates With Metropolitan Data

Chapter 4 explains that developing an empirical model of criminal participation requires the identification and specification of those factors that are most likely to be both empirically important and testable given the nature of the available data. In this study we attempt to control for deterrence factors, opportunity cost factors, demographic factors (note that several of the opportunity cost and demographic factors also can be interpreted from a deprivation theory perspective), and alcohol consumption factors, each of which is discussed in detail in Chapter 4.

The dependent variables employed here are metropolitan violent crime rates calculated by dividing the number of reported violent crimes of a particular type (murder, rape, robbery, assault, and the total of the individual categories) in an MSA by the MSA population that is 18 years of age or above. In an effort to break the simultaneity biases that were present in the state level analysis, we employ state level deterrence measures as independent variables. The implicit behavioral assumption is that potential criminals gain general impressions about the probability and severity of punishment from state-wide data (e.g., news reports about executions, arrests, and so on) and base their expectations on these data. Since the metropolitan area only contributes a part of the state-level totals, we then assume that the metro level crime rates do not simultaneously determine state level spending on criminal justice resources, so tests of endogeneity are not required. As explained below, it turns out that endogeneity tests cannot be performed on these small samples anyway, and limited sample sizes also prevent the estimation of simultaneous equation systems with more than two equations, so we are essentially forced to make this assumption, and therefore, we use state-level rather than local-level deterrence measures in an effort to make the assumption at least somewhat more reasonable.

The actual deterrence measures employed are a subset of those used in Chapters 4 and 5: the arrest rate calculated with state-level arrests and reported crimes for each crime category, police employment per capita,[67] and for the murder specifications, the probability of conviction with a sentence of execution given an arrest for murder, along with the probability of execution given an execution conviction. Being sensitive to the issue of sample size limitations, particularly when we attempt to run the reduced-form and simultaneous models discussed below, we chose not to include NRA membership and prison population in these metro-level specifications. NRA membership did not appear to be consistently important in the state-level analysis, while inspection of the data suggested that the prison population variable is even more likely to be endogenous than the other state level variables (we also did not have this variable for the most recent year of the metro sample when the regressions were run).

---

[67] This variable is actually a modified version of the one used in the state-level regressions, as it is calculated by dividing the number of persons employed in full-time equivalent police protection services in a state by the MSA population ages 18 and older.

Opportunity cost and demographic (or deprivation theory) measures include some metropolitan-level variables and some state-level variables. Metro level variables are M184490, INCOME, URB90, and FHH90, each of which is defined in Section 6.2 and used in the consumption models (the reasons for using these data rather than variables employed in the state-level analysis are also the same as those given in 6.2). State-level variables are the unemployment rate (UR) and WAGELOW, each of which is used in the state-level analysis in Chapters 4 and 5 and defined in Chapter 4 (or see Appendix B where all of the variables employed here are defined, and where sources and summary statistics are presented).

The alcohol consumption measures are those employed in Section 6.2 as dependent variables. In addition, in an effort to alleviate missing variable biases, we include the same year and region dummies that are employed in the consumption regressions (again, sample size limitations mean that observation specific fixed effects cannot be controlled for).

Tables 6.5 through 6.10 present estimates from specifications for each of the crime categories that correspond to those presented in Chapter 4. Each table reports five specifications which differ by the alcohol consumption measure employed. Specifications include beer only, liquor only, wine only, total alcohol, and an inclusive model with beer, liquor and wine entered as independent explanatory variables.[68] It must be stressed, however, that the alcohol consumption coefficients in the inclusive model suffer from multicollinearity problems, because the metro-level consumption measures are highly correlated. The correlation coefficient between the log of beer and the log of liquor is .902, for instance, and the beer-wine and liquor-wine

[68] As with Chapter 4, we only report specifications with fixed-effect controls included (in this case, for years and regions). Inclusion of these controls clearly affects several coefficients in the models, suggesting that missing variable biases exist without such controls. Of course, this also suggests that the inability to control for observation-specific fixed effects because of limited sample sizes means that such biases may remain, and these results also must be considered with caution.

coefficients are .742 and .860 respectively. Therefore, the reported consumption coefficients in the inclusive models are not likely to be reliable due to multicollinearity, and furthermore, the coefficients in the beer, liquor, and wine specifications probably suffer from missing variable bias (as explained below).

6.3.1 Murder

Table 6.5 reports the results for the metro-level murder specifications. Note that with one exception, the deterrence variables are always insignificant. The only exception is POLICE, which is negative and significant in the beer, total, and inclusive models. The opportunity-cost/demographic/deprivation-theory variables appear to be relatively significant, however. POV90 is at least marginally significant and positive in every specification except the inclusive model; FHH90 is always positive and significant at traditional levels; INCOME is positive and significant in the liquor, wine and total specifications; the unemployment rate is significantly positive in the beer, liquor and total models; the wine, total and inclusive specifications all have significantly negative WAGELOW coefficients; M184490 is positive and significant in the beer and wine models; and URB90 is positive and significant in the wine specification. Also note that the time and region dummy-variable coefficients are consistently positive and generally significant (implying that murder rates are lower in 1998 that in previous years, and lower in the metro areas of the northeast than in the other regions), except for the liquor specification where these coefficients are generally insignificant except for the 1991 and 1992 coefficients which are significantly negative.

Now turn to the alcohol consumption coefficients. Note that each of the specifications for individual alcohol types and the total alcohol specification imply that each type of alcohol consumption significantly raises murders. This contrasts with the findings in the state level model. However, given the high correlation between the individual alcohol consumption measures reported above, these results do not necessarily imply that each type of alcohol impacts murder rates. Instead, it may be that one type does and the other coefficients are significant due to missing variable biases. While the inclusive specification might reveal which type dominates (note that it appears that both beer and wine increase murders in that specification while liquor reduces murders, in sharp contrast to the state-level models), the high degree of collinearity between the variables also makes the coefficients in this specification unreliable. Thus, the most that can be said is that some type of alcohol may influence murder rates, but these results cannot tell us which one it is. Furthermore, the inability to control for observation-specific fixed effects means that even this conclusion must be considered with considerable caution.

6.3.2 Rape

Table 6.6 reports the five rape rate specifications. POLICE is negative and at least marginally significant in all five models while the probability of arrest is never significant. POV90 is significant and positive in every specification except the inclusive model, as with the murder regressions, but FHH90 is always negative and significant at traditional levels in the rape models, in sharp contrast to the murder results. INCOME is positive and significant in the liquor and total specifications. The unemployment rate is clearly sensitive to specification, as it significantly negative in the beer specification, significantly positive in the total alcohol model, and insignificant in the other regressions. The WAGELOW coefficients are always positive and at least marginally significant, however, while M184490 is always negative and significant. URB90 is negative and significant in the total and inclusive specifications but insignificant in the other models. Almost all of the coefficients on the time dummy variables are insignificant (implying that rape rates are not changing very much over time in these metro areas), except for the wine specification where the 1991 and 1992 coefficients are significantly positive. On the other hand, the regional dummies are always positively related to rape rates (implying that reported rapes are lower in the northeast metropolitan areas than in the rest of the metro areas), although they are insignificant in the inclusive model (and the coefficient on the west dummies is also insignificant in the total-alcohol model).

The alcohol coefficients in the metro-level rape models tell a somewhat different story than they do in the state-level results presented in Chapter 4. In this case, beer appears to be positively related to rapes in the beer-only model, and total alcohol consumption is significant in the total specification, but liquor and wine are both insignificant in their respective models, and none of the alcohol consumption measures are significant in the inclusive model (recall that these variables are highly correlated, however, making the coefficients in the inclusive model suspect). Furthermore, while beer may influence rape rates, the inability to control for observation-specific fixed effects means that this conclusion also must be considered with considerable caution.

6.3.3 Robbery

The five metro-level robbery specifications are reported in Table 6.7. In this case, the only significant deterrence variable is POLICE and it is only significant in one model - the inclusive specification where it is negative. POV90 is significant and positive in only one specification, the wine model, but FHH90 is always positive and significant at traditional levels in these robbery regressions, in sharp contrast to the rape results (but similar to the murder findings). INCOME is positive and at least marginally significant in the beer, liquor and wine specifications, while the unemployment rate is significantly positive in every regression except the inclusive model. The WAGELOW coefficients are negative and significant in the wine, total, and inclusive models, while M184490 is marginally significant and positive in the beer and liquor specifications and URB90 is negative and at least marginally significant in the beer, liquor and wine models. Almost all of the coefficients on the time dummy variables are positive although significance levels vary from specification to specification, with the 1994 coefficient always significant and all of the year dummies being significant in the total and inclusive models (implying that robbery rates are lower in 1998 than in previous years for these metro areas). The regional dummies vary considerably across specifications. None are significant in the beer model, midwest is significant and negative in the liquor and wine models, and the west and south coefficients are both at least marginally significant and positive in the total and inclusive specifications.

Beer is not significantly related to robberies in the beer-only model, and total alcohol consumption is insignificant in the total specification, but liquor and wine are both significant in their respective models. Note that these are precisely the opposite conclusions to those implied for rape, once again reinforcing implications drawn from the state-level data that whatever the alcohol-violence relationship may be, it appears to vary across types of violent crimes. Furthermore, while none of the alcohol consumption measures are significant in the inclusive model for rape, they are all significant in the inclusive model for robbery, with beer and wine having positive signs and liquor having a negative sign (recall that these variables are highly correlated, however, making the coefficients in the inclusive model highly suspect, and the inability to control for observation-specific fixed effects means that all these conclusions must be viewed as being very tentative).

6.3.4 Aggravated Assault

Results from aggravated assault specifications are reported in Table 6.8. The deterrence implications are quite different in this case than they have been for the other three violent crime categories. The probability of arrest is at least marginally significant and negative in every specification except the beer-only model, while POLICE is consistently positive, although it is only significant in the liquor and wine equations. POV90 is sensitive to specification in this case as it is significant and positive in the beer, liquor and wine regressions, but then significantly negative in the total specification, and insignificant in the inclusive model. FHH90 is always positive and significant at traditional levels in the assault models, again in sharp contrast to the rape results but with similar implications to the murder and robbery findings. INCOME is positive and significant in the beer, liquor, wine and inclusive specifications, and the unemployment rate is at least marginally significant and positive in every regression. The WAGELOW coefficients are also positive and significant in every model except the beer regression, and M184490 is significantly positive in the liquor, total and inclusive specifications. URB90 is negative and significant in all of the models. Almost all of the coefficients on the time dummy variables are negative in these models, although once again, significance levels vary from specification to specification (in general, it appears that assault rates are higher in 1998 than in previous years for these metro areas, although perhaps not substantially higher than in 94, 95, 96, and 97). The regional dummies also are almost always negative (although significance levels vary considerably across specifications), suggesting that metro areas in the northeast may have more reported assaults than those in other regions.

Beer, liquor, wine, and total alcohol are all significant and positive in their individual specifications, suggesting a relationship between alcohol consumption and assault. However, as in the case of murder, the high correlation between the individual alcohol consumption measures reported above suggests that these results do not necessarily imply that each type of alcohol impacts assault rates. Rather, one type may be important but the other coefficients are significant due to missing variable biases. The inclusive specification could tell us which type dominates, but as emphasized above, the high degree of collinearity between the variables means that the coefficients in this specification are unreliable (note that it appears that wine increases assaults in the inclusive specification while liquor and beer are both insignificant, a result that cannot be taken seriously). Thus, the most that can be said is that some type of alcohol may influence assault rates, but these results cannot tell us which one it is. And once again, the inability to control for observation-specific fixed effects means that the broad implication that alcohol of some type matters must be considered with caution.

6.3.5 Total Violent Crime

The total-violent-crime regressions are reported in Table 6.9. Not surprisingly, given that assaults make up the largest portion of total violent crimes, these results are very similar to those reported in Table 6.8. For instance, as with assaults, the deterrence implications are quite different in this case than they have been for the other three violent crime categories, once again stressing a conclusion reached with the state level data: specifically, general policy inferences probably should not be drawn from studies that aggregate crime categories or consider only one type of crime (just as they should not be drawn from studies that aggregate alcohol or consider just one type of alcohol). The probability of arrest is at least marginally significant and negative in the beer, liquor and total specifications, while POLICE is only significant in the liquor and inclusive equations but with conflicting signs. Also as in the assault model, POV90 is sensitive to specification: it is significant and positive in the wine regression, and is otherwise insignificantly with conflicting signs. FHH90 is always positive and significant at traditional levels just like it was in the assault models. INCOME also is positive and significant in the beer, liquor, wine and inclusive specifications, and the unemployment rate is at least marginally significant and positive in every regression, as they were in the assault models. Furthermore, consistent with the assault results, WAGELOW is positive and significant only in the liquor model, M184490 is significantly positive in the beer, liquor, total and inclusive specifications, and URB90 is negative and significant in all of the models. Again, almost all of the coefficients on the time dummy variables are positive in these models, although once again, significance levels vary from specification to specification, while the regional dummies are also generally negative with significance levels varying considerably across specifications.

Beer, liquor, wine, and total alcohol are all significant and positive in their individual specifications, suggesting the same relationship between alcohol consumption and total violent crime that appeared for assault. Once again, of course, the correlation between the individual alcohol consumption measures implies that one type actually may be important and that the other coefficients are significant due to missing variable biases. The high degree of collinearity between the variables means that the coefficients in the inclusive specification are also unreliable (it appears that beer and wine increase violent crimes in total in the inclusive model and that liquor has a significant negative effect, a result that cannot be taken seriously). Finally, we again stress that the inability to control for observation-specific fixed effects means that any implication that alcohol of some type matters must be considered with caution.

6.3.6 Summing Up

In terms of specific implications, very little can be drawn from the results presented in this section because of two data problems. First, the limited sample sizes means that we are unable to control for observation-specific fixed effects so at least some of the coefficients probably suffer from missing-variable biases. Second, collinearity between the three measures of alcohol consumption make coefficient estimates in the inclusive models unreliable and it also means that the implications of the individual alcohol models (beer, liquor and wine) may be unreliable due to missing variable bias. Despite these very serious shortcomings, however, at least two important *general implications* drawn from the state-level analysis reported in Chapters 3 through 5 are supported: (1) policy inferences from a statistical study of violent crime that either aggregates the individual violent crime categories or focuses on only one category cannot be generalized because deterrence, socio-economic/demographic/deprivation, and alcohol consumption relationships all vary dramatically across violent-crime categories; and (2) policy inferences drawn from statistical studies of alcohol and violence that either aggregate alcohol types or focus on one type of alcohol cannot be generalized because whatever the alcohol-violence relationships might be, they apparently vary considerably across crime categories with different types of alcohol having different influences. The metro-level analysis has proven to be disappointing with regard to specific policy implications for more reasons than the two cited at the beginning of this paragraph, as explained next.

6.4 Endogeneity Tests

The limitations of the data became much more restrictive in conducting the endogeneity tests. Indeed, these tests can not be performed because of the high degree of collinearity between the predicted value of the consumption variables and the actual value in the "unrestricted" model.

As a result, either the models would not estimate at all or, within the regressions that would estimate each variable is statistically insignificant while the models have very high R^2 and significant F tests (an indication of multicollinearity). Therefore, there are no endogeneity test results worth reporting. This does not mean that endogeneity is not a problem, of course. While we employ state-level deterrence variables and *assume* that they are exogenous, both the alcohol consumption and the violent crime rate variables are metro level observations which could influence each other. Therefore, we take a relatively conservative approach and run both 2SLS and reduced form specifications for all of the crime rates (including murder, where state-level tests suggest that there are not simultaneity bias) in order to compare the two estimation procedures. The 2SLS estimates only treat the alcohol consumption variables as endogenous, however, since treating the deterrence variables as endogenous too is generally not possible due to the limits on sample sizes, forcing us to make the assumption noted above (indeed, as reported below, several of the models treating just the alcohol variables as endogenous can not be estimated), and motivating our use of state level rather than local deterrence variables in hopes of making the assumption reasonable. We report the reduced-form models next, followed by the 2SLS results in Section 6.6, and the concluding Section compares the two sets of implications.

6.5 Reduced-Form Violent Crime Estimates Controlling for Alcohol Policies

Given the limitations in the metro-level data and the resulting inability to carry out endogeneity tests, we follow the typical approach in the literature and develop reduced form specifications. As explained in Chapter 5, this approach has the advantage of possibly uncovering the impacts that policies could have by changing drinking practices and/or locations without affecting aggregate consumption, but the expectation that simultaneity bias exists means that any specific policy implications implied by these estimates must be considered with caution.

Indeed, if the implications do not compare favorably with those from the 2SLS estimation procedures reported below, they clearly should not be considered as reliable. Nonetheless, some implications may prove to be robust, so these results are worth considering and comparing to those derived below (and to those derived with the state level data).

Tables 6.10 through 6.15 present the reduced-form models for the four specific violent crime classifications and total violent crime. For each set of regressions, we briefly note the implications from the non-alcohol-policy variables before turning to the alcohol-policy inferences. The alcohol policy variables include DRINK, NOSIGN, NOPRINT, and the respective excise taxes for every crime specification. In addition, the estimated effects of the beer market variables MANDATE, CASHLAW, FORCE, and MINIDIST are considered for the beer, total, and inclusive specifications. Finally, as noted in Chapter 5, state tourism is also a factor that could be influenced by policy makers, so the estimated effects of TOURPCT are also examined for every crime specification.

6.5.1 Murder Per-Capita

Table 6.10 shows metro-level reduced-form murder regressions. First note that due to the small sample size and/or collinearity between variables (e.g., the tax variables) the total and inclusive models would not estimate. This proves to be a problem for all of the reduced form and the simultaneous equation models.

The beer, liquor and wine models would estimate, but for a substantial number of variables, the results are surprising inconsistent across these specifications. For instance, among the variables that do not reflect either crime or alcohol control policies, POV90 is significant and positive in the beer model but insignificant in the liquor and wine specifications, M184490 is significantly negative in the wine regression but insignificant in the other two, URB90 is negative and significant in the liquor and wine models but insignificant in the beer equation, INCOME is significantly positive in the beer model and insignificant in the other two, and FHH90 is positive and significant in the beer specification and insignificant in the liquor and wine models. The unemployment rate and WAGELOW are insignificant in all three models, as are all of the religion variables. The only deterrence variable that is even marginally significant is the probability of conviction with a death sentence, and this only occurs in the beer regression where the coefficient is, surprisingly, positive. All of the time and region dummies are insignificant in the beer and liquor regressions except for the WEST in the liquor model. All of the time dummies are at least marginally positive in the wine specification where the region dummies are insignificant.

None of the alcohol control policies are significant in the beer regression except the CASHLAW variable which is marginally negative. NOPRINT has a significantly negative sign in the liquor specification, but none of the other policy variables are significant. This same variable is also significantly negative in the wine specification, as is the tourism variable. In addition, wine taxes are unexpectedly significant with a negative sign. This result is difficult to accept as realistic, but it is the only indication that alcohol taxes may influence murder rates.

It is surprising to see such differences across specifications, not only in terms of policy implications but also for the other variables. The beer-only specification is quite different from the other two, as it involves several more variables and a shorter time period, so the differences between this model and the other two are not too surprising. However, the major difference between the wine and liquor specifications are the tax variables which tend to be highly correlated (there are also some differences in observations due to missing data on taxes and/or state-monopoly policies; for instance, there are ten more observations in the wine specification than the liquor model), and yet the results are quite different. This reinforces the conclusion drawn above that whatever the alcohol-violence relationship may be, it varies across alcohol types.

6.5.2 Rapes Per-Capita.

The reduced-form rape regressions are presented in Table 6.11. As was the case for murder, the total and exclusive models would not estimate. Also as in the murder estimates, for a substantial number of variables, the results are inconsistent across the beer, liquor and wine specifications. POV90 is significant and positive in the beer and wine models, M184490 is significantly negative in the wine regression but insignificant in the other two, URB90 is negative and marginally significant in the wine model but insignificant in the beer and liquor equations, INCOME is significant and positive in the beer model and insignificant in the other two, FHH90 is significantly negative in the wine regression but insignificant in the beer and liquor equations, and WAGELOW is significant and positive in the liquor regression and marginally positive in the wine model. Only the unemployment rate is insignificant across all three specifications among all of the socio-economic and demographic variables. Some of the religion variables are also significant, but only in the beer regression where MRM is significantly positive and SOBAP is significantly negative. The two crime deterrence variables are never significant, and the same is true of the time dummies. The West dummy is negative and significant in the beer regression, however, while the south dummy has a significantly positive coefficient. None of the region dummies are significant in the other two regressions.

Several alcohol control policies are significant in the beer regression. The CASHLAW is positive and significant in this case (the opposite result to murder), as is MINIDIST. MANDATE is significantly negative, and so is BEERTAX. None of the alcohol control variables are significant in the liquor specification, however, and only NOPRINT is significant (and negative) in the wine model. Thus, it appears that some beer control policies may impact rape rates, while policies directed at other types of alcohol will not (unless simultaneity or missing variable biases explain the results).

6.5.3 Robberies Per-Capita

Table 6.12 reports the estimates from the metro-level reduced-form robbery regressions. In this case, the total model did not estimate and inclusive models actually did estimate, but the results are essentially meaningless. None of the individual coefficients are significant even though the adjusted R^2 is reasonably high and the F-test suggest that the overall regressions are significant. This is an indication of severe multicollinearity, of course. Therefore, our discussion must focus on the beer, liquor and wine specifications.

Once again, for a substantial number of variables, the results are inconsistent across the specifications. POV90 is significant and positive in the beer model but insignificant in the liquor and wine equations, like it was in the murder models, M184490 is significantly negative in the wine regression but insignificant in the other two just as it was in Table 6.10, and the same is true for URB90 which is negative and significant in the liquor and wine models but insignificant in the beer equation. INCOME is marginally significant and positive in the beer model, significantly positive in the liquor model, and insignificant in the wine model. FHH90 is significantly positive in the beer specification, marginally positive in the liquor model, and insignificant in the wine model, the unemployment rate is significantly positive in the beer model, and WAGELOW is insignificant in all three models. All of the religion variables are insignificant in the first two regressions, but MRM and CATH have a significantly positive coefficient in the wine regression where PROT is also significant but negative. None of the region or time dummies are significant at conventional levels in the first two regressions (although the 1994 dummy is marginally significant in the beer regression and the west dummy is marginally positive in the liquor model), but all of the time dummies are significantly positive in the wine specification.

None of the alcohol policies are significant in the beer regression. NOPRINT has a marginally significant negative sign in the liquor specification, but none of the other policy variables are significant. Finally, wine taxes are surprisingly significant with a negative sign in the wine regression. These findings are quite similar to those for murder, and quite different from those that were produced for rape. Thus, we once again conclude that aggregating violent crimes to study the alcohol-violence relationship (or simply the issue of violent-crime deterrence) or aggregating alcohol types (or considering only one type of alcohol) can produce misleading policy implications, since relationships can vary significantly across crime types.

6.5.4 Aggravated Assaults Per-Capita

Aggravated assault reduced-form models are presented in Table 6.13. Once again, the total alcohol specification did not estimate, although in this case, the inclusive model did. However, the inclusive model results are, again, meaningless, as only one of the individual coefficients is significant even though the adjusted R^2 is quite high and the F-tests suggest that the overall regression is significant, indicating severe multicollinearity.

The results for a substantial number of variables are once again inconsistent across the beer, liquor and wine models. POV90 is significant and positive in the beer model, for example, but significant and negative in the liquor and wine equations. M184490 is marginally negative in the wine regression but insignificant in the other models. INCOME has a significant and negative coefficient in the wine model, while FHH90 is significantly positive in the liquor and wine models, and insignificant in the beer model. The unemployment rate is marginally significant and negative in the wine specification but insignificant in the other two models, and WAGELOW is insignificant in all three models. The coefficient on URB90 is at least marginally negative in all three regressions, however. All of the religion variables are significant in one regression, but none are significant in two or more equations. CATH is significantly negative in the beer model, MRM has a significantly positive coefficient in the liquor regression, and both SOBAP and PROT are significant in the wine equation but with opposite signs (SOBAP is positive and PROT is negative). For the regional dummies, South is positive and significant in the beer specification, all three coefficients are negative and significant in the liquor model, and none of them are significant in the wine equation. The time dummy variables all have insignificant coefficients in the beer and liquor models, but once again, they all have significantly positive coefficients in the wine regression. Finally, both the probability of arrest and the police variables have significantly negative coefficients in the wine model, but they are insignificant in the beer and liquor specifications.

In the beer regression, both the tourism and the NOSIGN variables have marginally significant and negative coefficients, and beer taxes are significantly negative. NOPRINT is significantly negative in the liquor specification, and liquor taxes have a surprising positive and significant sign. WINETAX has a positive sign in the wine regression but is not significant. Finally, the tourism variable is marginally negative, DRINK is significantly positive, and NOSIGN is significantly negative in the wine specification. Once again, it is clear that if there is an alcohol-violence relationship, then the various alcohol types have different impacts on particular types of violent crime.

6.5.5 Total Violent Crimes Per-Capita

The aggregate violent crime rate results for the metro-level estimates are reported in Table 6.14. Again, the results for a substantial number of variables are inconsistent across the beer, liquor and wine models. POV90 is significant and positive in the beer model, but insignificant in the other regressions, while URB90 is significantly negative in the liquor and wine specifications. Both INCOME and UR have significant and positive coefficients in the beer model, and FHH90 is significantly positive in the other two equations. M184490 and WAGELOW are never significant in this case. The only religion coefficient that is not insignificant is MRM, and it is only significant in the wine regression, where it is positive. Once again, all of the year dummies have significant and positive coefficients in the wine specification, but only 1994 is marginally significant in the beer model, and none of the time dummies are significant in the liquor model. Several regional dummies have at least marginally significant coefficients. South is positive and marginally significant in the beer specification, the West and Midwest coefficients are negative and significant in the liquor model and marginally negative in the wine equation. The police variable is significantly positive in the liquor model, but insignificant in the other specifications and the arrest variable is never significant.

The tourism and cash law variables have marginally significant and negative coefficients in the beer specification, while other policy variables are insignificant. The two advertising variables have opposite effects in the liquor model. NOPRINT is significantly negative and NOSIGN is significantly positive. Liquor taxes also have a surprising positive and marginally significant sign. The advertising variables appear to have opposite impacts in the wine regression too, but in this case NOSIGN is significantly negative while NOPRINT is marginally positive. Finally, the tourism variable is marginally negative in the wine specification. Note that these results do not appear to correspond with those of any of the individual violent crime regressions, so once again, it is clear that if there is an alcohol-violence relationship, then aggregation of the violent crime categories will not provide valid insights regarding the relationships for the various individual crime types.

6.5.6 Summing Up

Table 6.15 (presented in the text rather than at the end of the report with the statistical tables) summarizes the alcohol policy variables across the crime categories. In the table, 0 implies that the coefficient is insignificant, + means marginally positive, - means marginally negative, ++ indicates significantly positive, and -- implies significantly negative.

Two things that stands out when the results are considered together have already been stressed. First, whatever the relationship is between alcohol and violent crime, it is not the same relationship across types of violent crime. Second, what ever the relationship is, it is not the same across alcohol types for individual crimes. In fact, there really are no obvious consistent relationships. Because of multicollinearity, missing variable biases (because we do not have enough data to control for observation-specific fixed effects, and we were forced to leave out other variables as well) and possibly because of simultaneity bias, however, little weight can be put on any of these specific policy results. To get some idea of the impact of potential simultaneity bias we now turn to 2SLS models.

Table 6.15

Comparison of Coefficient Signs of Alcohol Policy Variables between Reduced-Form Specifications for Metro-Level Violent Crime Categories

Crime/Policy Variables											
Model	Taxes		Minidist	Cashlaw	Force	Mandate	Drink	Nosign	Noprint	Tourpct	
	Beer	Liquor	Wine								
MURDER											
Beer	0			0	-	0	0	0	0	0	0
Liq.		0						0	0	--	0
Wine			--				0	0	--	--	
RAPE											
Beer	--			++	++	0	--	0	0	0	0
Liq.		0						0	0	0	0
Wine			0				0	0	--	0	
ROBBERY											
Beer	0			0	0	0	0	0	0	0	0
Liq.		0						0	0	-	0
Wine			--				0	0	0	0	
ASSAULT											
Beer	--			0	0	0	0	0	-	0	0
Liq.		++						0	0	--	0
Wine			--				++	++	0	0	
T.V.C.											
Beer	0			0	-	0	0	0	0	0	-
Liq.		+						0	++	--	0
Wine			0					0	--	+	-

The symbols imply the following relationships:

- 0 = insignificant;
- \+ = marginally positive;
- \- = marginally negative;
- ++ = significantly positive;
- -- = significantly negative.

6.6 Simultaneous Equation Estimates Using Metro Data

The method of two-stage least squares (2SLS) is used to estimate a simultaneous system with relevant alcohol consumption regressions estimated in the first stage. The system's set of exogenous determinants and instrumental variables serve as the explanatory variables in the estimation of each first-stage regression. The fitted values from these regressions (note that these are still denoted as BEER, LIQ, WINE, and TOTALC in the 2SLS models) are then used in place of the corresponding endogenous variables in the estimation of crime rate equations. All reported second-stage t-ratios are corrected to account for using predicted rather than actual values of the endogenous variables in estimation of the second-stage crime regressions.

Tables 6.16 through 6.21 present the second-stage 2SLS estimation results of the individual crime models. OLS specifications for the same set of observations are also estimated (note that these can differ from earlier OLS specifications due to differences in samples due to missing data for the first stage alcohol consumption regressions) and presented for comparison between the OLS and 2SLS models (and to compare to the reduced-form models already reported in Tables 6.10 through 6.14, discussed in Section 6.5).

6.6.1 Per-Capita Murders

Table 6.16 presents the estimates of the per-capita murder second-stage 2SLS and corresponding OLS specifications (and Table 6.10 shows the corresponding reduced form models for murder). Note that as with the reduced-form regressions reported in Section 6.5, the total and inclusive 2SLS models would not estimate due to data limitations. Another similarity between the reduced-form and OLS results is that there are fairly substantial differences in implications for the non-alcohol relationships across the beer, liquor and wine specifications. Finally, note that except for the alcohol consumption coefficients, the OLS and the 2SLS results reported in Table 6.16 are quite consistent.

POV90, M184490, and FHH90 are all at least marginally significant and positive in both the OLS and 2SLS models in the beer, liquor and wine specifications. URB90 is positive and significant in both the OLS and 2SLS wine models but insignificant in all of the beer and liquor equations, while INCOME is significantly positive in the OLS and 2SLS liquor and wine equations but insignificant in the beer models, and the unemployment rate is positive and significant in the OLS and 2SLS versions of the beer model but insignificant in the liquor and wine equations. WAGELOW is insignificant in all beer and liquor equations but significantly negative in both of the wine equations. Among the deterrence variables, POLICE is significantly negative and the probability of conviction with a death sentence is marginally positive in both beer equations but they are insignificant in all of the liquor and wine models, the probability of being executed given a capital conviction is marginally positive in the OLS version of the wine model but insignificant in all other equations, and the arrest variable is always insignificant. All of the time and region dummies are at least marginally significant and positive in both of the beer equations, and the same is true except for the 1997 dummy coefficient in the wine models. In the liquor regressions the dummies are quite different, however, as only the Midwest dummy has a significant positive coefficient and then only in the 2SLS estimation, while both the 1991 and 1992 coefficients are significantly negative in the OLS regression.

Now consider the relationships between the various alcohol types and murder rates. The story is a simple one to tell. In each case, the OLS estimate suggests a significant and positive relationship but the 2SLS estimate indicates that there are no significant relationships. It appears that the OLS estimates (and therefore the reduced-form estimates reported in Table 6.10) do indeed suffer from simultaneity bias. Given the insignificant coefficients for each of the alcohol types in their respective regressions, the policy implications are straightforward. No matter what the first stage regressions imply about alcohol control policy impacts on beer, liquor and wine consumption, the impact of these policies on murder rates is expected to be zero.

6.6.2 Per-Capita Rapes

The estimates of the per-capita rape 2SLS and corresponding OLS specifications appear in Table 6.17 (see Table 6.11 for the corresponding reduced-form models). As with the murder models, the total and inclusive 2SLS models would not estimate, but in addition, the liquor model also would not estimate. Therefore, the only models from which inferences can be drawn are the beer and wine specifications.

For these two models, POV90 is at least marginally significant and positive while FHH90 and M184490 are significantly negative in all equations. URB90 and WAGELOW are both positive and significant in both the OLS and 2SLS wine models but insignificant in the two beer equations. INCOME and UR are insignificant in each of the four equations. POLICE and the probability of arrest are both significantly negative in all beer and wine equations. All of the region dummies are significant and positive in all four regressions. The 1991, 1992 and 1993 coefficients are at least marginally significant and positive in the OLS and 2SLS wine equations while the 1994 and 1995 coefficients are significantly positive in the OLS and 2SLS beer regressions.

Beer is insignificant in both the OLS and 2SLS specifications, so none of the beer control variables should have any impact on rape. On the other hand, wine is negative and marginally significant in the OLS regression and it remains negative and significant in the 2SLS regression. As with the state level estimates, then, increases in wine consumption appear to reduce rapes.

This probably suggests that wine is consumed by individuals or in situations which are not likely to create opportunities for rape, of course, rather than that wine consumption actually reduces rapes.

6.6.3 Per-Capita Robberies

Table 6.18 presents the subsample OLS and 2SLS estimates for per-capita robbery (the reduced-form robbery models are in Table 6.12). Once again, the total and inclusive 2SLS models can not estimate, but the beer, liquor and wine models do.

POV90 is insignificant in both the OLS and 2SLS beer estimates, but it is significantly positive in all of the liquor and wine models. FHH90 is always significant and positive, as is M184490, except in the wine models. URB90 is significantly negative in the beer and liquor regressions but it is insignificant in both wine models. On the other hand, WAGELOW is negative and significant in both the OLS and 2SLS wine models but insignificant in the beer and liquor equations. INCOME is insignificant in the 2SLS regression for wine, but significantly positive in all other models, and UR is significantly positive in both beer and wine models but insignificant in both liquor models. POLICE has a positive and significant coefficient in both the OLS and 2SLS liquor specifications, but it is insignificant in all of the beer and wine models, and the probability of arrest is insignificant in every model except the 2SLS wine regression where it is marginally negative. The region dummies are insignificant in both of the beer regressions but both the West and South region's coefficients are significantly negative in the OLS and 2SLS liquor models, and MIDWEST is negatively significant in both wine models while the WEST dummy has a significantly positive coefficient in the 2SLS wine specification. The 1994 (1995) coefficients are positive and significant (marginally significant) in the two beer equations. In contrast, the 1991, 1992 and 1993 coefficients are negative and significant in the OLS liquor specification, although only 1991 and 1992 remain marginally significant in the 2SLS model (the 1995 OLS coefficient is also marginally significant but the 2SLS coefficient is insignificant). All of the time dummies except 1997 are positive and significant in the 2SLS wine equation, while the 1994 and 1995 coefficients are significantly positive in the OLS model where the 1993 and 1996 coefficients are marginally positive.

BEER is insignificant in the OLS model but it is marginally positive in the 2SLS model. Therefore, it appears that beer may have an impact on robbery, and that beer control variables may also have an impact. An examination of the first-stage regression for beer finds a positive and significant coefficient on DRINK (+0.8618); a negative and marginally significant coefficient on CASHLAW (-0.6217), and positive and marginally significant coefficient on FORCE (+0.6325). All other beer policy coefficients, including the beer tax coefficient (which is positive), are insignificant. Thus, the inference is that limiting the number of liquor outlets and introducing cash laws could reduce robberies, while forced deposits tend to increase robberies (assuming that the combination of two marginally significant coefficients is appropriate). In contrast to the beer results, both liquor and wine appear to be significantly positive in the OLS models but both turn insignificant in the 2SLS models.

6.6.4. Per-Capita Aggravated Assaults

The 2SLS and accompanying OLS regressions for aggravated assault are reported in Table 6.19 (for comparison, reduced-form assault regressions appear in Table 6.13). Again, the total and inclusive 2SLS models would not estimate, so only the individual alcohol models can be compared.

FHH90 and INCOME are always significant and positive (except income in the 2SLS liquor model) and URB90 is always significantly negative. POV90 is insignificant in the OLS and 2SLS beer and liquor estimates, but it is significantly positive in both of the wine models, but M184490 is significantly positive in all of the beer and liquor models while it is insignificant in the OLS wine regression and only marginally significant in the 2SLS wine model. WAGELOW is positive and significant in both the OLS and 2SLS liquor and wine models but insignificant in the beer equations, and UR is significantly positive in both beer and wine models but insignificant in both liquor models. POLICE has a positive and significant coefficient in both the OLS and 2SLS wine specifications, and it is marginally positive in the 2SLS beer regression, but it is insignificant in the other three models. On the other hand, the probability of arrest is significantly negative in every model. The region dummies are all significantly negative in both wine models, and the same is true of all of them in the liquor models except the SOUTH dummy in the 2SLS model (which is insignificant). In the beer models, however, only the MIDWEST coefficients are significant (they are negative). All of the time dummies have significantly negative coefficients in the wine models except the 1997 coefficients. However, only the 1994 coefficients are positive and marginally significant in the two beer equations, and all of the time dummies are insignificant in the liquor models (the 1992 coefficient in the OLS model is marginally negative).

BEER is insignificant in both the OLS and 2SLS assault models. However, LIQ and WINE are both positive and significant in their OLS and 2SLS specifications. Thus, liquor and wine control policies might be expected to reduce assaults. Consider the liquor results first. The first-stage regression has a significantly negative TOURPCT coefficient (-1.8909), and a significantly positive NOSIGN coefficient (+1.5044). The coefficients on DRINK, NOPRINT, and LIQTAX are all insignificant. The implied negative relationship between tourism and assault may reflect endogeneity as tourists tend to be attracted to destinations with low violent crime rates, but the positive relationship between sign advertising restrictions and assaults does not seem reasonable. It may reflect a missing variable bias due to the inability to control for observation-specific fixed effects. Turning to the first-stage wine consumption regression, we find significant positive coefficients on DRINK (+0.6808) and WINETAX (+1.7329), and significantly negative coefficients on TOURPCT (-1.1515) and NOPRINT (-0.9141). Thus, restrictions on alcohol outlets could reduce assaults, as could restrictions on print advertising. The tourism relationship again appears to be endogenous, and the wine tax impact does not seem reasonable, again probably reflecting missing variable bias, as explained in Section 6.2. While it may be that some alcohol control policies have an impact on assault, then, specific conclusions from these metro level estimates must clearly be considered with extreme caution.

6.6.5 Total Violent Crimes Per-Capita

Table 6.20 presents the subsample OLS and 2SLS estimates for per-capita total violent crime (TVC). As with the rape models, the liquor, total and inclusive 2SLS models would not estimate, so the only simultaneous equation models from which inferences can be drawn are the beer and wine specifications. In these two models, FHH90, UR, and INCOME are significantly positive and URB90 is significantly negative in all equations. POV90 is significant and positive in both the OLS and 2SLS wine models but insignificant in both beer models, while M184490 is significantly positive in the beer equations but insignificant in the two wine regressions. WAGELOW is always insignificant, as is POLICE. The probability of arrest is significantly negative in the two beer equations and marginally significant in the 2SLS wine models. The SOUTH and MIDWEST region dummies are significant and negative in two wine regressions but they are all insignificant in the beer models. The 1991 and 1992 coefficients are also significant and negative in both wine models while the 1994 coefficients are significantly positive in the OLS and 2SLS beer regressions.

Beer is marginally significant and positive in the OLS beer model but it is insignificant in the 2SLS specification. On the other hand, wine is positive and significant in both the OLS and 2SLS regressions. This suggests that policies directed at controlling wine consumption could reduce total violent crime, a result that is both very surprising and counter to the implications drawn from the state-level simultaneous equation estimates. Considering the first stage wine consumption regression, the implication once again is that TOURPCT is negatively related to violent crime, since its coefficient is significant and negative (-1.4651), as is NOSIGN (a significant -3.7771), while NOPRINT and WINETAX are both positively related to violent crime with significant coefficients of +2.9712 and +2.0831 respectively. As suggested earlier, tourism is probably endogenous as one factor influencing tourists' destination choices is violent crime rates. Limits on sign advertising could reduce violent crime, but the implication that limits on print advertising will increase such crimes suggests that the coefficients probably suffer from missing variable bias due to the inability to control for observation-specific fixed effects. This expectation appears to be reinforced by the very large and positive wine tax coefficient. Thus, any specific inferences about alcohol policy are once again unlikely to be valid.

6.6.6. Summing Up

Table 6.21 (presented here rather than in the Table section at the end of the report where the statistical tables are found) shows the signs of the alcohol coefficients in the OLS and 2SLS models for the individual alcohol specifications. A zero in the table means that the coefficient is insignificant, a single plus sign implies a marginally significant (at the ten percent level of confidence) positive coefficient, a double plus implies a positive coefficient that is significant at conventional levels (five percent or less), a single minus sign implies a marginally significant and negative coefficient, and a double minus sign implies that the coefficient is significantly negative at conventional levels. In addition, a ? indicates that the relationship is unknown because the relevant regression would not estimate. The symbol before the slash (/) represents the OLS result and the symbol after the slash represents the 2SLS result. This table might also be compared to Table 5.6 where the results from the state-level simultaneous equations can be found.

Table 6.21

Comparison of Coefficient Signs of Per-Capita Consumption Measures between OLS and 2SLS Specifications for Violent Crimes in Metropolitan-Level Individual Alcohol Models

	Murder	Rape	Robbery	Assault	TVC
Beer	++/0	0/0	0/+	0/0	+/0
Liquor	++/0	0/?	++/0	++/++	++/?
Wine	++/0	--/-	++/0	++/++	++/++

Symbols before a slash (/) represent OLS results and those after a slash represent 2SLS results. The symbols used and their meanings are:

0 = insignificant;
\+ = marginally positive;
\- = marginally negative;
++ = significantly positive;
-- = significantly negative.
? = unknown relationship (because the relevant regression would not estimate).

The results in Table 6.21 appear to imply that of all types of alcohol, wine consumption is probably the most important determinant of violent crime, a result that is strikingly contradictory to the implications in Table 5.6. Liquor also appears to be an important determinant of assault, while beer is not, another result that appears to contradict Table 5.6. Beer may have an impact on robbery according to the metro-level data but it does not in the state-level while liquor does.

Which results are to be believed? Probably none of them, at least in terms of specific policy implications, but the state-level results are probably somewhat less problematic than the metro-level data. While we were unable to purge the state-level relationships of simultaneity bias, we did at least address some of the simultaneity concerns, and we were able to control for a number of sources of potential missing variable biases, including those associated with observation-specific fixed effects, *and* we were able to estimate inclusive models. For the metro-level data, we were never able to estimate an inclusive model in a simultaneous equation framework, and we could not control for observation-specific fixed effects, so these results clearly suffer from missing variable biases. We also could not perform tests to determine whether simultaneity bias was a serious problem (although the changes in the various alcohol coefficients suggests that it often was) or if we successfully dealt with it.

6.7 Conclusions

The metropolitan level data set proved to be quite disappointing. Beer consumption data is available for 1994 through 1998, and supposedly for 50 metro areas, suggesting that sufficient observations could be employed to perform similar analysis to that done with state-level data. However, it turns out that many of the observations are missing, so the data sample is severely restricted, particularly when combined with crime and socio-economic data which also have some missing observations. As a consequence, we are unable to control for observation specific fixed effects, implying that all of the estimates probably suffer from missing variable bias. Furthermore, some of the variables are highly correlated (e.g., the alcohol consumption and tax variables) so some of the estimates suffer from severe multicollinearity. The small sample sizes combine with multicollinearity to prevent estimation of several models, including, for all practical purposes, all of the reduced-form and simultaneous-equation estimates of the total alcohol and inclusive models. The inability to estimate the inclusive models is particularly troubling since the inferences drawn from the individual alcohol models all could suffer from an additional source of missing variable bias (the measures of other types of alcohol). As a result, no specific inferences regarding alcohol policy variables' impacts on violent crimes that arise in the results reported in this chapter can be considered reliable. In spite of these very serious shortcomings in the data, however, four important *general implications* drawn from the state-level analysis reported in Chapters 3 through 5 are supported: (1) policy implications from a statistical study of violent crime that either aggregates all individual violent crime categories (i.e., focuses on TVC) or considers only one category cannot be generalized because deterrence, socio-economic/demographic/deprivation, and alcohol consumption relationships all vary dramatically across violent-crime categories; (2) policy implications drawn from statistical studies of the alcohol/violence relationship that either aggregate alcohol types or focus on one type of alcohol (e.g., beer) cannot be generalized because whatever the alcohol-violence relationships might be, they apparently vary considerably with different types of alcohol having different influences across crime categories; (3) drawing policy conclusions regarding the alcohol/violence relationship from reduced-form models appears to be inappropriate since they appear to suffer from simultaneity bias; and (4) if there is an alcohol/violence relationship, it probably is not that alcohol consumption itself causes violent crimes, but rather, the circumstances in which alcohol is consumed or the type of individuals who consume the various types of alcohol either make victims more vulnerable or potential offenders more likely to be aggressive. The first three of these are rather negative inferences, of course, as compared to the stronger policy-specific implications that one hopes for in this type of study, but they are clearly important from a policy perspective as they suggest extreme caution in attempting to draw policy-specific implications from studies that do not attempt to control for the very complex web of factors that may influence violent crime and the potential alcohol-violence relationship.

In this regard, recall the dramatic differences across the beer, liquor and wine specifications for the various violent crimes, not only in terms of policy implications but also for the other variables. While the beer-only specifications are quite different from the other two, as they involve several more variables and a shorter time period (perhaps suggesting that difference between these models and the other two are not too surprising), the major difference between the wine and liquor specifications are simply the type of alcohol, or in the reduced-form and first-stage 2SLS results, the tax variables which tend to be highly correlated (there are also some differences in observations due to missing data on taxes and/or state-monopoly policies), and yet the results are quite different. This reinforces the conclusion drawn above that what ever the alcohol-violence relationship may be, it varies across alcohol types. Thus, as suggested in earlier chapters, it may not be that it is alcohol consumption itself which is important, but instead, it is the circumstances in which alcohol is consumed or the type of individuals who consume the various types of alcohol. It also reinforces the general conclusion that aggregating alcohol or considering policy toward only one type of alcohol in an alcohol-violence study can be very misleading, as the findings are not likely to generalize.

Even though we do not believe that the specific policy inferences from the metro-level estimates are reliable because of data limitations, let us compare the simultaneous equation estimates to the implications from the reduced form equations in order to reinforce the point that using reduced-form models when simultaneity exists can produce misleading implications. Table 6.22 (presented in the text) provides such a comparison. In this case, however, while 0 implies no significant impact, + indicates at least a marginally significant positive impact and -

implies at least a marginally significant negative relationship. A ? indicates that the relevant regression would not estimate so no inferences could be drawn. The symbol before the slash (/) represents the reduced form results and the symbol after the slash represents the simultaneous equation implications. When the estimates for all impacts are zero for the simultaneous equation results, it means that the second stage coefficients for the deterrence and alcohol variables are all insignificant [these models are also indicated with superscript a; superscript b indicates that the simultaneous equation model would not estimate].

Table 6.22

Comparison of Coefficient Signs of Alcohol Policy Variables between Reduced-Form and 2SLS Specifications for Metro-Level Violent Crime Categories

Crime/Policy Variables Model	Taxes Beer	Taxes Liquor	Taxes Wine	Minidist	Cashlaw	Force	Mandate	Drink	Nosign	Noprint	Tourpct
MURDER											
Beer[a]	0/0			0/0	-/0	0/0	0/0	0/0	0/0	0/0	0/0
Liq.[a]		0/0						0/0	0/0	-/0	0/0
Wine[a]			-/0					0/0	0/0	-/0	-/0
RAPE											
Beer[a]	-/0			+/0	+/0	0/0	-/0	0/0	0/0	0/0	0/0
Liq.[b]		0/?						0/?	0/?	0/?	0/?
Wine[c]		0/-					0/0	0/+	-/-	0/+	
ROBBERY											
Beer	0/0			0/0	0/-	0/+	0/0	0/0	0/0	0/0	0/0
Liq.[a]		0/0						0/0	0/0	-/0	0/0
Wine[a]			-/0				0/0	0/0	0/0	0/0	
ASSAULT											
Beer[a]	-/0			0/0	0/0	0/0	0/0	0/0	-/0	0/0	0/0
Liq.		+/0						0/0	0/+	-/0	0/0
Wine			-/+				+/+	+/0	0/-	0/-	
T.V.C.											
Beer[a]	0/0			0/0	-/0	0/0	0/0	0/0	0/0	0/0	-/0
Liq.[b]		+/?						0/?	+/?	-/?	0/?
Wine			0/+					0/0	-/-	+/+	-/-

Symbols before a slash (/) represent reduced-form results and those after a slash represent 2SLS results. The symbols used and their meanings are:

0 = insignificant;
\+ = at least marginally positive;
\- = at least marginally negative;
? = unknown relationship (because the relevant regression would not estimate).

Superscripts indicate the following:

a: the second stage coefficients for the deterrence and alcohol variables are all insignificant;
b: the simultaneous equation model would not estimate;
c: the policy implications arise because of a significant and *negative* wine coefficient in the second stage model.

These results might also be compared to those in Table 5.16, although it should be recalled that there were a large number of qualifications regarding the interpretation of the findings reported in that table. One of those qualifications applies here as well. When the policy implications arise because of a significant and *negative* wine coefficient in the second-stage regression, then the expectation that those policy impacts will occur is clearly doubtful. This applies in one case, indicated with a superscript c in Table 6.22.

Even though these results are probably not reliable, note that the significant policy inferences from reduced-form regressions are often negated when simultaneity bias is controlled for. Furthermore, insignificant coefficients in the reduced-form models occasionally turn significant when a 2SLS estimation procedure is employed. Thus, once again, the standard practice of using reduced-form estimation procedures in alcohol-violence studies means that the resulting policy implications must be considered with considerable caution.

Another obvious implication has to be that there is little support for the use of any type of alcohol taxation in an effort to reduce violent crimes. The only negative relationship between taxes and any violent crime is in the wine model for rape and it arises because wine consumption is negatively related to rape while wine taxes are positively related to wine consumption. Indeed, there is little support for focusing on any specific policy with expectations that it will reduce all types of violent crime. The implications suggested earlier that violent crimes should not be aggregated in policy studies is also clearly supported. Note that the policy inferences from the total violent crime estimates do not correspond with any of the individual crime categories. In fact, they are generally contradictory to the implications that appear to arise in the individual crime models.

Finally, let us briefly compare the relatively limited policy implications from simultaneous equations suggested by the metro data (Table 6.22) to those derived from the state data (Table 5.16). There are some consistencies. For instance, the wine specification for rape in both the state and metro analysis produces policy implications exclusively through a negative and significant sign on wine consumption in the second-stage crime equation. However, the actual policy relationships are quite different (the only consistent finding is the insignificant sign on DRINK). Similarly, the beer model for rape and the liquor and wine models for robbery are consistent in the state and metro level analyses since there are no policy inferences arising through consumption relationships. The beer models for robbery is also consistent in the sense that both the state and metro level second-stage results had at least marginally significant beer coefficients, although the policy inferences drawn from these two data sets are generally contradictory (the only consistencies are the positive sign on FORCE and the insignificant NOPRINT relationships). Other consistencies appear to arise, but they actually occur for different reasons. For instance, in the wine specifications for both assault and total violent crime, wine taxes appear to have a positive impact on the crime rate, but in the state-level analysis this occurs because wine consumption is negative in the second-stage regression and taxes are negative in the first stage wine regression. In the metro case wine consumption is positive in the second stage regression and taxes are positive in the first stage (the comparable relationships for NOPRINT and TOURPCT in the wine model for total violent crime arise for similar reasons).

The state-level and metro-level analyses are probably striking for their differences than their similarities, however. We expect that at least some of these differences would disappear if we had sufficient metro level data to control for observation-specific fixed effects. Indeed, if simultaneity bias can be more easily controlled with metro than with state data, given sufficient numbers of observations, as we expect, then differences may still exist but the metro results would be more reliable than the state findings. Metropolitan data may ultimately prove to be more useful that state level data in analyzing the alcohol/violence relationship then, but we will have to wait at least another two years and perhaps more, so that sufficient observations can be collected to control for observation-specific fixed effects and to perform endogeneity tests.[69]

[69]If the high degree of collinearity between metro level measures of alcohol shipments persists, however, problems may still persist, as inclusive models will suffer from multicollinearity and models for specific types of alcohol will suffer from missing variable bias. Specifications other than log-log may alleviate this problem, even though they would be departures from the standard specifications in the literature. It is probably appropriate to consider different specifications [e.g., as in Young and Likens (2000) in the drunk driving literature], however, including change-form models [e.g., as in Benson, et al. (1998) in the crime literature].

CHAPTER 7

CONCLUSION

The 1990s have witnessed falling crime rates and increasingly stringent efforts to control the use and abuse of alcohol. Previous research has provided considerable evidence that particular adverse consequences of alcohol use, such as drunk driving fatalities, have been effected by at least some of these policies. Given the apparent link between alcohol consumption and crime (whatever its dominant etiology), it is not surprising that some policy advocates see alcohol regulations as a potentially useful crime control option. This sentiment is clearly expressed in Boyum and Kleiman (1995, pg. 315), who state: "There is one drug for which higher prices would clearly be crime-decreasing: alcohol". Considerable empirical work explores the alcohol-violence relationship. A general theme arising from some of the most recent academic literature using data at both the individual level (Markowitz and Grossman 1998a, 1998b, 2000; Grossman and Markowitz 1999; Markowitz 2000a, 2000b) and aggregate level (Chaloupka and Saffer 1992; Cook and Moore 1993a). is that higher alcohol (beer) prices will results in a lower incidence of various criminal outcomes. Higher legal drinking ages also appear to matter in some studies, but since all states have now raised drinking age to 21, this implication has little relevance for further changes. Support for restrictions on alcohol availability, such as limiting the number of retail outlets, is somewhat more tenuous.

Given the theoretical ambiguity regarding an alcohol-to-violence causal relationship explained in Chapter 1, and the limitations of the previous studies on the alcohol-crime relationship as discussed in Chapter 2, the purpose of this study was to determine whether the results of previous studies are robust to a more fully specified empirical specification that takes into account potentially important sources of missing variable bias and endogeneity.

The first step in achieving this objective is an initial investigation into the impact of alcohol control policies on rates of per-capita alcohol consumption. This analysis is performed first using a panel of state-level data, and the results are reported in Chapter 3, where state-level per-capita consumption equations are estimated for beer, liquor, wine, and total alcohol consumption. Missing variable problems are alleviated by controlling for fixed effects using state and time dummies, and by adding variables that have not previously been considered. For instance, unlike previous studies, laws aimed at mitigating the incidence of drunk-driving are also included in each specification as they may increase the cost of consuming alcohol outside of the home. Results imply that higher excise taxes on liquor and wine, as well as several anti-DUI laws are negatively correlated with consumption and thus potentially viable policy tools, assuming some causal relationship between alcohol and crime. Less support was found for important beer excise tax effects since they are highly sensitive to model specification.

Chapter 4 discusses efforts to determine the effects of alcohol consumption on the rates of Index I crimes using a panel of state-level data. Again, fixed-effects are controlled for. Furthermore, unlike most previous alcohol-crime studies [Chaloupka and Saffer (1992) being an exception] variables related to criminal deterrence and the opportunity cost of (or deprivation impacts on) crime are included in the structural crime equations. In addition, rather than employing excise taxes as a proxy for alcohol price (and thus implicitly alcohol consumption) actual consumption proxies (alcohol shipments by state) are used. Some consumption measures are positively correlated with per-capita crime rates. In particular, liquor consumption appears to be most important for some crimes (rape, perhaps murder) while beer consumption is most important for others (assault) and both liquor and beer may influence robbery. Thus, given the negative correlation between several alcohol control policies and the apparent positive correlation between some types of alcohol consumption and each crime, it seems reasonable to infer that alcohol control policies can be used to lower the incidence of crime. However, unlike previous research this study also considers the potential endogeneity of the deterrence and consumption factors and the dependent crime measures. It is shown that the null hypothesis of exogeneity of the deterrence and alcohol consumption variables can be rejected for all crime models except per-capita murder. As such, the coefficient estimates obtained on these variables in the OLS regressions reported in Chapter 4 can not be considered consistent.

Given the empirical results of the previous two chapters, Chapter 5 develops simultaneous equations systems with a panel of state-level data to measure (i) the effects of alcohol consumption on crime after controlling for the endogeneity of deterrence and consumption factors and (ii) to determine the effects of the various alcohol control policies on crime. These models also continue to control for missing variables with fixed-effect dummies and with the numerous variables other alcohol-crime studies have not considered. The second stage regressions tend to confirm the hypothesis that liquor consumption influences rape and robbery while beer consumption affects assault. Wine consumption is almost always significantly and negatively related to violent crime, however, indicating that it is not alcohol itself that matters as much as it is the circumstances of or people involved in alcohol consumption.

Given the apparent importance of liquor or beer in simultaneous equation models, estimated alcohol control policy effects are then obtained by multiplying the second-stage coefficients on the endogenous deterrence and consumption variables by each of the estimated policy coefficients in the first-stage regressions. The results are also compared to those generated by reduced form models. Superficially they appear to support the general conclusion that alcohol policies can influence violent crime rates, although precise policy prescriptions appear to differ from those derived in reduced-form models. While some policy impacts appear robust across both types of models, others are substantially different when simultaneity bias is taken into account. However, an examination of the first stage regression sources of the significant policy impacts reveals that many of them are actually arising through correlations with deterrence variables rather than consumption, or with wine consumption rather than liquor or beer consumption. Thus, the apparent policy impacts must be discounted as being largely spurious. These findings cast significant doubts on any reduced-form model implications about alcohol-policy effects on violence. Some of the policy implications do arise through consumption, however, so perhaps they are still relevant. Unfortunately, tests of over-identifying restrictions on the set of excluded instruments employed in each specification indicated that the instrumental variables employed cannot be considered exogenous to the crime equation. As such, the estimated deterrence, consumption, and policy effects arising from the simultaneous equations estimation procedure reported in Chapter 5 must be considered potentially biased estimates.

In an effort to alleviate the simultaneity bias problem an effort was made to repeat the analysis described above using a different data set. It was hoped that by employing crime and consumption data at lower levels of geographical aggregation (i.e., metropolitan areas), state mandated crime and alcohol control policies could be considered as "truly exogenous" to the structural model. Regrettably, this effort is marred by data limitations. While the data set for alcohol consumption supposedly reports data for 50 metropolitan areas, missing observations in these data or in matching data on other important variables effectively reduced the sample to 30 metro areas. Furthermore, while data on both liquor and wine shipments are available for several years, the beer data have not been reported for very long. Because of these problems, our sample size (and therefore our degrees of freedom) are severely limited, and as a result, we are unable to deal with the missing-variable biases that are apparent in the state-level study. We do not have enough degrees of freedom to include city (or even state) fixed-effect dummies, and we are forced to drop other variables as well. Thus, while the results are relatively more likely to be purged of simultaneity bias, they still suffer from missing variable problems, and as a consequence, they also must be considered as tentative. These results are reported in Chapters 6.

Clearly, the results of this analysis show that much more should be done before implications from existing studies of alcohol and violent crime are used to motivate policy changes. At this juncture there is no credible evidence that alcohol control polices, including taxation, have any impact on violent crime. Future research on this issue should place emphasis on model specification and the identification of adequate instrumental variables. The latter should prove to be especially challenging for future researchers given the extremely complex interactive nature of the alcohol-violence relationship. Indeed, this problem is further confounded since future research should take into account the endogeneity of the opportunity cost of (or deprivation influences on) crime variables as well as the deterrence and consumption controls.[70]

In spite of the need for additional research with better data, four important ***general implications*** can be drawn from this project: (1) policy implications from a statistical study of violent crime that either aggregates all individual violent crime categories (i.e., focuses on TVC) or considers only one category cannot be generalized because deterrence, socio-economic/

[70]Multicollinearity between state laws has also plagued the interpretation of estimated alcohol policy effects in previous studies and, almost assuredly, in this one as well. As such, estimation of policy effects through quasi-experimental or dynamic reduced-form models (i.e., vector autoregression) might prove advantageous.

demographic/deprivation, and alcohol consumption relationships all vary dramatically across violent-crime categories; (2) policy implications drawn from statistical studies of the alcohol/violence relationship that either aggregate alcohol types or focus on one type of alcohol (e.g., beer) cannot be generalized because whatever the alcohol-violence relationships might be, they apparently vary considerably with different types of alcohol having different influences across crime categories; (3) drawing policy conclusions regarding the alcohol/violence relationship from reduced-form models appears to be inappropriate since they appear to suffer from simultaneity bias; and (4) if there is an alcohol/violence relationship, it probably is a complicated relationship involving the circumstances in which alcohol is consumed and/or the type of individuals who consume the various types of alcohol that make victims more vulnerable or potential offenders more likely to be aggressive.

The first three of these are rather negative inferences, of course, as compared to the stronger policy-specific implications that one hopes for in this type of study, but they are clearly important from a policy perspective as they suggest extreme caution in attempting to draw policy-specific implications from studies that do not attempt to control for the complex web of factors that may influence the potential alcohol-violence relationship. Finally we acknowledge that many alcohol control policies may be justified to further worthy objectives that are unrelated to the suppression of violent crime.

REFERENCES

Appelbaum, E. and E. Erez. "The Effects of Sentencing Information on Crime Rates: A Theoretical Model," *Journal of Criminal Justice* 12 (1984): 503-508.

Avio, K. L. and Clark, C. S. Property Crime in Canada, An Econometric Study, Toronto: University of Toronto Press, 1976.

Ayers, Ian, and John J. Donohue III. "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy." *American Law and Economics Review* 1 (Fall 1999): 436-470.

Banay, R. S. "Alcoholism and Crime," Quarterly Journal of Studies on Alcohol 2 (1942): 686-715.

Basmann, R.L. "On Finite Sample Distributions of Generalized Classical Linear Identifiably Test Statistics," Journal of the American Statistical Association 55 (December 1960): 650-659.

Becker, Gary S. "Crime and Punishment: An Economic Approach," *Journal of Political Economy* 76 (March/April 1968): 169-217.

Bedau, Hugo A. "The Case Against the Death Penalty," Washington: American Civil Liberties Union, 1997.

Benson, Bruce L.; Kim, Iljoong; Rasmussen, David W.; and Zuehlke, Thomas W. "Is Property Crime Caused by Drug Use or Drug Enforcement Policy?" *Applied Economics* 24 (1992): 679-692.

__________; __________; and __________. "Estimating Deterrence Effects: A Public Choice Prospective on the Economics of Crime Literature," *Southern Economic Journal* 61 (1994): 161-168.

__________; __________; and __________. "Deterrence and Public Policy: Tradeoffs in the Allocation of Police Resources," *International Review of Law and Economics* 18 (1998): 77-100.

__________; Mast, Brent D.; and Rasmussen, David W. "Deterring Drunk Driving Fatalities: An Economics of Crime Perspective," *International Review of Law and Economics* 19 (June 1999): 205-225.

__________; __________; and __________. "Can Police Deter Drunk Driving," *Applied Economics* 32 (2000): 357-366.

__________; and Mast, Brent D. "Privately Produce General Deterrence," Presented at a conference on "Guns, Crime and Safety" organized by John Lott, Jr. and sponsored by the Yale Law School and the American Enterprise Institute, Washington, December 10-11, 1999 (revised March 2000).

Block, M.K. and J.M. Heineke. "The Allocation of Effort Under Uncertainty", *Journal of Political Economy* 81 (1973): 376-386.

__________; and R. C. Lind. "Crime and Punishment Reconsidered", *Journal of Legal Studies,* IV (1975a), 241-47.

__________; and __________. "An Economic Analysis of Crimes Punishable by Imprisonment," *Journal of Legal Studies,* IV (1975b), 479-92.

Bound, John and Krueger, Alan B. "The Extent of Measurement Error on Longitudinal Earnings Data: Do Two Wrongs Make a Right?", *Journal of Labor Economics* 9 No. 1 (1991): 1-24.

Bound, John; David A. Jaeger; and Regina M. Baker. "Problems with Instrumental Variables Estimation When the Correlation Between the Instruments and the Endogenous Explanatory Variables is Weak," *Journal of the American Statistical Association* 90 (June 1995): 443-450.

Boyum, David and Kleiman, Mark A. R. "Alcohol and Other Drugs," in James Q. Wilson and Joan Petersilia, editors, *Crime.* San Francisco: ICS Press (1995): 295-326.

Brier, Stephen S. and Fienberg, Stephen E. "Recent Econometric Modelling of Crime and Punishment: Support for the Deterrence Hypothesis?" *Evaluation Review* 4 (1980): 147-191.

Bradley, Martin B.; Norman M. Green, Jr.; Dale E. Jones; Mac Lynn; and Lou McNeil. *Churches and Church Membership in the United States 1990* (Atlanta: Glenmary Research Center, 1990).

Brown, William W. and Morgan O. Reynolds. "Crime and Punishment: Risk Implications", *Journal of Economic Theory* 6 (1973): 508-514.

Bureau of Justice Statistics, *Substance Abuse and Treatment, State and Federal Prisoners, 1997,* Special Report NCJ-172871 (Washington, DC: U.S. Department of Justice, January 1999).

Cameron, Samuel. "The Economics of Crime Deterrence: A Survey of Theory and Evidence," *Kyklos* 41 (1988): 301-323.

Chaloupka, Frank J. "Effects of Price on Alcohol-Related Problems," *Alcohol Health & Research World* 17 (1993): 46-53.

__________, and Laixuthai, Adit. "Do Youth Substitute Alcohol and Marijuana: Some Econometric Evidence." *Eastern Economic Journal* 23 (Summer 1997): 253-275.

__________ and Saffer, Henry. "Alcohol, Illegal Drugs, Public Policy and Crime," Mimeo 1992.

__________; __________; and Grossman, Michael. "Alcohol Control Policies and Motor-Vehicle Fatalities." *The Journal of Legal Studies* 22 (January 1993): 161-86.

__________, and Wechsler, Henry. "Binge Drinking in College: The Impact of Price, Availability, and Alcohol Control Policies," *Contemporary Economic Policy* 14 (October 1996): 112-124.

Cheeson, Harrel; Paul Harrison; and William J. Kassler. "Sex Under the Influence: The Effect of Alcohol Policy on Sexually Transmitted Disease Rates in the United States." *Journal of Law and Economics* (April 2000): 215-238.

Chiu, W. Henry and Paul Madden. "Burglary and Income Inequality." *Journal of Public Economics* 69 (1998): 123-141.

Coate, Douglas, and Michael Grossman. "Effects of Alcoholic Beverage Prices and Legal Drinking Ages on Youth Alcohol Use," *Journal of Law and Economics* 31 (1988): 145-171.

Collins. J.J. and Schlenger, W.E. "Acute and Chronic Effects of Alcohol-Use on Violence," *Journal of Studies on Alcohol* 49 6 (1988): 516-521.

Cook, Philip J. "The Effect of Liquor Taxes on Drinking, Cirrhosis, and Auto Accidents," in *Alcohol and Public Policy: Beyond the Shadow of Prohibition*, edited by Mark H. Moore and Daniel R. Gerstein (1981a).

__________. "The Technology of Personal Violence," in *Crime and Justice: A Review of Research*, edited by Tonry M. (Chicago: University of Chicago Press , 1981b).

__________, and Moore, Michael J. "Economic Perspectives on Reducing Alcohol-Related Violence," in *Alcohol and Interpersonal Violence: Fostering Multidisciplinary Perspectives*, edited by S. E. Martin (Washington, DC: National Institute on Alcohol Abuse and Alcoholism, 1993a).

__________ and __________. "Violence Reduction Through Restrictions on Alcohol Availability," *Alcohol Health Research World* 17 (1993b): 151-156.

__________, and James A. Leitzel. "Perversity, Futility, Jeopardy: An Economic Analysis of the Attack on Gun Cóntrol." *Law and Contemporary Problems* 59 (Winter 1996): 1-28.

__________, and Jen Ludwig. "Guns in America: Results of a Comprehensive National Survey Firearms Ownership and Uses." (report prepared for the National Institute of Justice, 1996).

Cordilia, A. "Alcohol and Property Crime: Explaining the Causal Nexus." *Journal of Studies on Alcohol* 46 (March, 1985): 161-171.

Cornwell, Christopher and Trumbull, William N. "Estimating the Economic Model of Crime With Panel Data," *Review of Economics and Statistics* 76 (May 1994): 360-366.

Culbertson, W. Patton, and David Bradford. "The Price of Beer: Some Evidence from Interstate Comparisons," *International Journal of Industrial Organization* 9 (1991): 275-289.

Davidson, Russell, and Mackinnon, James. *Estimation and Inference in Econometrics* (New York: Oxford University Press, 1993).

Davis, Michael L. "Time and Punishment: An Intertemporal Model of Crime", *Journal of Political Economy* 96 (April 1988): 383-390.

Dee, T. S. "State Alcohol Policies, Teen Drinking and Traffic Fatalities," *Journal of Public Economics* 72 (1999): 289-315.

Dezhbakhsh, Hashem; Paul H. Rubin. "The Effect of Concealed Handgun Laws on Crime: Beyond the Dummy Variables," working paper, Department of Economics, Emory University (January 1999).

DiNardo, John, and Thomas Lemieux. "Alcohol, Marijuana, and American Youth: The Unintended Consequences of Government Regulation." Irvine Economic Papers No. 90-92-17, March 1992.

Doyle, Joanne M.; Ahmed, Ehsan; and Horn, Robert N. "The Effects of Labor Markets and Income Inequality on Crime: Evidence from Panel Data," *Southern Economic Journal* 65 (1999): 717-738.

Dull, R. T. and Giacopassi, D. J. "The Impact of Local Alcohol Ordinances on Official Crime Rates: The Case of Tennessee," *Justice Quarterly* 4 (1987): 311-323.

Dumond, Michael J; Barry T. Hirsch; and David A. MacPherson. "Wage Differentials Across Labor Markets and Workers: Does Cost of Living Matter?," *Economic Inquiry* 37 (October 1999): 577-598.

Ehrlich, Isaac. "Participation in Illegitimate Activities: a Theoretical and Empirical Investigation," *Journal of Political Economy* 81 (1973): 521-565.

__________. "Crime and Punishment, and the Market for Offenses," *Journal of Economic Perspectives* 10 (Winter 1996): 43-68.

Ensor, T. and Godfrey, C. "Modelling the Interaction Between Alcohol, Crime and the Criminal Justice System," *Addiction* 88 (1993): 477-487.

Evans, William N., Neville, Doreen; and Graham, John D. "General Deterrence of Drunk Driving: Evaluation of Recent American Policies." *Risk Analysis* 11 (1991): 279-289.

Fagan, Jeffrey. "Intoxication and Aggression." In *Drugs and Crime*, edited by Michael Tonry and J.Q. Wilson (Chicago: The University of Chicago Press, 1990).

__________. "Interactions Among Drugs, Alcohol, and Violence." *Health Affairs* 12 (no.4, 1993a): 65-79.

__________. "Set and Setting Revisited: Influences of Alcohol and Illicit Drugs on the Social Context of Violent Events." In *Alcohol and Interpersonal Violence: Fostering Multidisciplinary Perspectives*, Susan E. Martin, ed., National Institute on Alcohol Abuse and Alcoholism Research Monograph 24, NIH Publication No. 93-3496 (1993b).

Forst, B.; V. Filatov; and L.R. Klein. "The Deterrent Effect of Capital Punishment: An Assessment of the Estimates" in A. Blumenstein, D. Nagin, and J. Cohen, eds., *Deterrence and Incapacitation:Estimating the Effects of Criminal Sanctions on Crime Rates* (Washington, D.C. : National Academy of Sciences, 1978).

Freeman, Richard B. "Why Do So Many Young American Men Commit Crimes and What Might We Do About It?" *Journal of Economic Perspectives* 10 (Winter 1996): 25-42.

Gelles, Richard J. and Claire P. Cornell, *Intimate Violence in Families*, Sage Publications, Newbury Park (1990).

Glaeser, Edward L., Bruce Sacredote, and Jose A. Scheinkman, "Crime and Social Interactions." *Quarterly Journal of Economics* 111 (1996): 507-548.

Glaser, D., "The Realities of Homicide versus the Assumptions of Economists in Assessing Capital Punishment." *Journal of Behavioral Economics* 6: (1977) 243-268.

Goodman, R. A.; Mercy, J. A.; Loya, F.; Rosenberg, M. L.; Smith, J. C.; Allen, M. H.; Varga, L.; and Kotts, R. "Alcohol Use and Interpersonal Violence-Alcohol Detection in Homicide Victims," *American Journal of Public Health* 76 (1986): 144-149.

Gould, Eric D., Bruce A. Weinberg; and David B. Mustard. "Crime Rates and Local Labor Market Opportunities in the United States: 1979-1995", unpublished manuscript (1999).

Grogger, J. "Certainty vs. Severity of Punishment," *Economic Inquiry* 29 (April 1991): 297-309.

Grossman, Michael, and Markowitz, Sara. "Alcohol Regulation and Violence on College Campuses," National Bureau of Economic Research Working Paper no. 7129 (May 1999).

__________; Sindelar, Judy L.; Mullahay, John; and Anderson, Richard. "Policy Watch: Alcohol and Cigarette Taxes," *Journal of Economic Perspectives* 7 (1993): 211-222.

Hakim, Simon, et al. "Interjurisdictional Spillover of Crime and Police Expenditures," *Land Economics* 55 (1979): 200-212.

Harlow, Caroline Wolf. "Drugs and Jail Inmates, 1989," in *Bureau of Justice Statistics: Special Report*. Washington, D. C.: U.S. Department of Justice, Bureau of Justice Statistics, August 1991.

Haugue, R. "The Effects of Changes in Availability of Alcoholic Beverages," In Social Control of the Drinking Driver, Laurence, M.D., Snortum, J.R., and Zimring, F.E. eds. (Chicago, University of Chicago Press: 169-187, (1988).

Hausman, Jerry. "Specification Tests in Econometrics," *Econometrica* 46 (1978): 1251-1271.

Homel, Ross. "Random Breath Testing and Random Stopping Programs in Australia," in *Drinking and Driving: Advances in Research and Prevention*, edited by R. J. Wilson and R. Mann (New York: Guilford Press, 1990).

Hemenway, David. "Survey Research on Self-Defense Gun Use: An Explanation of Extreme Overestimates." *Journal of Criminal Law and Criminology* 87 (August 1997): 1430-1445.

Horney, Julie and Ineke, Marshall, "Risk Perceptions Among Serious Offenders: The Role of Crime and Punishment." *Criminology* 30 (1992): 575-593.

Hogarty, Thomas F., and Kenneth G. Elzinga. "The Demand for Beer," *Review of Economics and Statistics* 54 (1972): 195-198.

Hurst, P. and Wright, P. "Deterrence at Last: The Ministry of Transport's Alcohol Blitzes," paper presented to the Eight International Conference on Alcohol, Drugs and Traffic Safety, Stockholm, 1980.

Johnson, S. D.; Gibson, L.; and Linden, R. "Alcohol and Rape in Winnipeg, 1966-1975," *Journal of Studies of Alcohol* 39 (1978): 1887-1894.

Kellermann, Arthur L.; Lori Wetsohal; Lauri Fischer; and Beverly Harvard. "Weapon Involvement in Home Invasion Crime." *The Journal of the American Medical Association* 273 (1995): 1759-1762.

Kenkel, Donald S. "New Estimates of the Optimal Tax on Alcohol," *Economic Inquiry* 34 (April 1996): 296-319.

Kleck, Gary, and E. Britt Patterson. "The Impact of Gun Control and Gun Ownership Levels on Violence Rates." *Journal of Quantitative Criminology* 9 (1993): 249-287.

Layson, Stephen. "Homicide and Deterrence: A Reexamination of the U. S. Time Series Evidence." *Southern Economic Journal* 52 (1985): 68-89.

Lenke, L. *Alcohol and Criminal Violence: Time Series Analysis in a Comparative Perspective* (Stockholm: Almqvist & Wiksell, 1975).

Lester, D. "Alcohol Consumption and Rates of Personal Violence in Australia," *Drug and Alcohol Dependency* 31 (1992): 15-17.

__________. "Restricting the Availability of Alcohol and Rates of Personal Violence (Suicide and Homicide)," *Drug and Alcohol Dependency* 31 (1993): 215-217.

__________. "Alcohol Availability, Alcoholism, and Suicide and Homicide," *American Journal of Drug and Alcohol Abuse* 21 (1975): 147-150.

Leung, S. and C.E. Phelps. "My Kingdom for a drink...? A review of estimates of the price sensitivity of demand for alcoholic beverages", in M.E. Hilton and G. Bloss eds., Economics and the Prevention of Alcohol-Related Problems. NIAAA Research Monograph No. 25. NIH Pub. No. 93-3513. (National Institutes of Health, Bethesda, MD) 1-31.

Levitt, Steven D. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Legislation" *Quarterly Journal of Economics* (1996): 319-351.

__________. "Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?" *Economic Inquiry* 36 (1998a): 270-290.

__________. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime" *American Economic Review* 87 (no. 3, 1998b): 353-372.

Light, Roy. "Alcohol as a Cause of Crime," *Legal Executive Journal* (January 1995): 28-29.

Lindquist, P. "Criminal Homicide in Northern Sweden, 1970-1981: Alcohol Intoxication, Alcohol Abuse and Mental Disease," *International Journal of Law and Psychiatry* 8(1986): 19-37.

Lipsey, Mark W.; Wilson, David B., Cohen, Mark A.; and Derson, James H. "Is There a Causal Relationship Between Alcohol Use and Violence" A Synthesis of Evidence," *Recent Developments in Alcoholism, Volume 13: Alcoholism and Violence*, edited by Marc Galanter (New York: Plenum Press, 1997).

Long, Sharon K.; Ann D. Witte; and Partice Karr. "Family Violence: A Microeconomic Approach." *Social Science Research* 12 (1983): 363-392.

Lochner, Lance. "Education, Work, and Crime: Theory and Evidence", unpublished manuscript (1999).

Lott, John R. Jr. *More Guns, Less Crime: Understanding Crime and Gun-Control Laws* (Chicago: University of Chicago Press, 1998).

__________, and Mustard, David B. "Crime, Deterrence, and Right-to-Carry Concealed Handguns," *Journal of Legal Studies* 26 (January 1997): 1-68.

Ludwig, Jen. "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data." *International Review of Law and Economics* 18 (1998): 239-254.

Makela, K. "Differential Effects of Restricting the Supply of Alcohol: Studies of a strike in Finnish Liquor Stores." *Journal of Drug Issues* 10 (1980): 131-144.

Makela et. al. "A Comparative Study of Alcohol Control." In Alcohol, Society, and the State Vol. 1 Toronto, Canada: Addiction Research Foundation (1981).

Markowitz, Sara. "The Price of Alcohol, Wife Abuse, and Husband Abuse," National Bureau of Economic Research Working Paper no. 6916 (January 1999).

__________. "Criminal Violence and Alcohol Beverage Control: Evidence from an International Survey," National Bureau of Economic Research Working Paper no. 7481 (January 2000a).

__________. "The Role of Alcohol and Drug Consumption in Determining Physical Fights and Weapon Carrying by Teenagers," National Bureau of Economic Research Working Paper no. 7500 (January 2000b).

Markowitz, Sara, and Grossman, Michael. "Alcohol Regulation and Violence Towards Children," National Bureau of Economic Research Working Paper no. 6359 (January 1998a).

__________. , and __________. "Alcohol Regulation and Domestic Violence Towards Children," *Contemporary Economic Policy* 16 (July 1998b): 309-320.

__________. , and __________. "The Effects of Beer Taxes on Physical Child Abuse," *Journal of Health Economics* 19 (2000): 271-282.

Mast, Brent D. *Deterring Alcohol Related Traffic Fatalities: The Role of Alcohol Consumption, Enforcement, and Punishment*, Florida State University Ph.D. Dissertation, Department of Economics, May 1996.

__________; Benson, Bruce L.; and Rasmussen, David W. "Beer Taxation and Alcohol-Related Vehicle Fatalities," *Southern Economic Journal* 66 (October 1999): 214-249.

Mayfield, D. "Alcoholism, Alcohol Intoxication, and Assaultive Behavior," *Diseases of the Nervous System* 37 (1976): 228-291.

McDowall, David; Collin Lofin; and Brian Wierseman. "Easing Concealed Firearms Laws: Effects on Homicide in Three States." *Journal of Criminal Law and Criminology* 86 (Fall 1995): 193-206.

McKee, David L., and Michael Sesnowitz. "Capital Punishment: The Canadian Experience," *Journal of Behavioral Economics* 6 (Summer-Winter 1977): 145-152.

Mocan, H. Naci; Stephen C. Billups; and Jody Overland. "A Dynamic Model of Differential Human Capital and Criminal Activity," National Bureau of Economic Research Working Paper no. 7584 (March 2000).

Model, Kayrn E. "The Effect of Marijuana Decriminalization on Hospital Emergency Drug Episodes: 1975-78," *Journal of the American Statistical Association* (1993): 737-747.

Mullahay, John, and Sindelar, Jody L. "Do Drinkers Know When to Say When? An Empirical Analysis of Drunk Driving." *Economic Inquiry* 32 (July 1994): 383-94.

Myers, Samuel L. "Estimating the Economic Model of Crime: Employment Versus Punishment Effects.", *Quarterly Journal of Economics* 98 (February 1983): 157-166.

National Research Council. *Understanding and Preventing Violence*, Albert J. Reiss and Jeffery A. Roth, eds., National Academy Press, Washington, D.C. (1993).

Nelson, Jon P. "State Monopolies and Alcoholic Beverage Consumption.", *Journal of Regulatory Economics* (no. 2 1990): 83-98.

O'Donoghue, Ted, and Rabin, Matthew. "Doing it Now or Later." *American Economic Review* 89 (March 1999): 103-124.

Olsson, O. and P.O.H. Wikstrom. "Effects of Experimental Saturday Closing of Liquor Retail Stores in Sweden." *Contemporary Drug Problems* 11(1982): 325-353.

Ornstein, Stanley I., and Dominique M. Hanssens. "Alcohol Control Laws and the Consumption of Distilled Spirits and Beer." *Journal of Consumer Research* 12 (1985): 200-213.

Ornstein, Stanley I., and Dominique M. Hanssens. "Resale Price Maintenance: Output Increasing or Restricting? The Case of Distilled Spirits in the Unites States," *Journal of Industrial Economics* 34 (1987): 1-18.

Parker, Robert N. "Rational Choice and Pooled Cross-Section Time Series: Theoretical and Methodological Pathways to New Understanding of the Alcohol/Violence Relationship," in *Alcohol and Interpersonal Violence: Fostering Multidisciplinary Perspectives*, edited by S. E. Martin (Washington, DC: National Institute on Alcohol Abuse and Alcoholism, 1993).

__________. "Bringing "Booze" Back in: The Relationship Between Alcohol and Homicide," *Journal of Research in Crime and Delinquency* 32 (1995): 3-38.

__________ and Rebhun, Linda-Anne. *Alcohol and Homicide: A Deadly Combination of Two American Traditions* (Albany, NY: State University of New York Press, 1995).

Paternoster, Raymond and Iovanni, LeeAnn. "The Deterrent Effect of Perceived Severity: A Reexamination." *Social Forces* 64 (1986): 751-777.

Pernanen, K. "Alcohol and Crimes of Violence," in *The Biology of Alcoholism, Vol. 4: Social Aspects of Alcoholism*, edited by B. Kissen and H. Begleiter (New York: Plenum Press, 1976).

__________. "Theoretical Aspects of the Relationship Between Alcohol and Crime." In *Drinking and Crime: Perspectives on the Relationships between Alcohol Consumption and Criminal Behavior*. James J. Collins Jr., ed. The Guilford Press, New York, NY (1981).

__________. *Alcohol in Human Violence* (New York: Guilford Press, 1991).

Phelps, Charles E. "Death and Taxes: An Opportunity for Substitution", *Journal of Health Economics* 7 (1988): 1-24.

Polsby, Daniel D. "Firearm Costs, Firearm Benefits and the Limits of Knowledge." *Journal of Criminal Law and Criminology* 86 (Fall 1995): 207-220.

Polsby, Daniel D. "The False Promise of Gun Control." *Atlantic Monthly* (March 1994): 57-70.

Rasmussen, David W.; Benson, Bruce L.; Kim, Illjong; and Zuehlke, Thomas W. "An Economic Analysis of Drug Crime and the Criminal Justice System," This Research was funded by the Florida Legislature under a contract administered by Economics and Demographic Research Division, *Joint Legislative Management Committee.* (December 1990).

Rasmussen, David W.; Benson, Bruce L.; and Sollars, David L. "Spatial Competition in Illicit Drug Markets: The Consequences of Increased Drug Enforcement," *Review of Regional Studies* 23 (Winter 1993): 219-236.

Reiss, Albert J. and Jeffery E. Roth, eds. *Understanding and Preventing Violence* (Washington, D.C.: National Academy Press, 1993)

Ross, H. Lawrence. "Law, Science and Accidents: The British Road Safety Act of 1967," *Journal of Legal Studies* 2 (1973): 1-78.

__________. "The Scandinavian Myth: The Effectiveness of Drinking-and-Driving Legislation in Sweden and Norway," *Journal of legal Studies* 4 (1975): 285-310.

__________. "Deterrence Regained: The Chesire Constabulary's 'Breathalyser Blitz'," *Journal of Legal Studies* 6 (1977): 241-249.

__________. *Deterring the Drunk Driver: Legal Policy and Social Control* (Lexington, MA: Lexington Books, 1981).

__________. *Confronting Drunk Driving: Social Policy for Saving Lives* (New Haven, CT: Yale University Press, 1992).

__________; McCleary, R.; and Epperlein, T. "Deterrence of Drinking and Driving in France: An Evaluation of the Law of July 12, 1978," *Law and Society Review* 16 (1982).

Rubin, Paul H.; Hashem Dezbakhsh; and Joanna Mehlhop. "Does Capital Punishment Have a Deterrent Effect? Evidence from Post Moratorium Panel Data", unpublished manuscript (1999).

Ruhm, Christopher J. "Alcohol Policies and Highway Vehicle Fatalities." National Bureau of Economic Research Working Paper Series no. 5195 (July 1995a).

__________. "Economic Conditions and Alcohol Problems." *Journal of Health Economics* 14 (1995b): 583-603.

Saffer, Henry and Grossman, Michael. "Beer Taxes, the Legal Drinking Age and Youth Motor Vehicle Fatalities." *Journal of Legal Studies* 16 (June 1987a): 351-374.

__________, and __________. "Drinking Age Laws and Highway Mortality Rates: Cause and Effect." *Economic Inquiry* 25 (1987b): 403-417.

Sah, Raaj K. "Social Osmosis and Patterns of Crime.", *Journal of Political Economy* 99 (December, 1991):1272-1295.

Sass, Tim R., and Saurman, David S. "Mandated Exclusive Territories and Economic Efficiency: An Empirical Analysis of the Malt-Beverage Industry." *Journal of Law and Economics* 36 (April 1993): 153-177.

Schmidt, P. and Witte, A.D. *An Economic Analysis of Crime and Justice: Theory, Methods, and Applications*, New York: Academic Press, (1988).

Schwab, R.M. and Zampelli, E.M. "An Economic Analysis of Crime and Justice: Theory, Methods, and Applications," New York: Academic Press, 1988 .

Schrag, Joel, and Suzanne Scotchmer. "The Self Reinforcing Nature of Crime." *International Review of Law and Economics* 17 (1997): 325-335.

Sherman, Lawrence W. "Policing for Crime Prevention," in *Preventing Crime: What Works, What Doesn't, What's Promising*, A Report To the United States Congress Prepared for the National Institute of Justice, edited by L. W. Sherman, D. Gottfredson, D. MacKenzie, J. Eck, P. Reuter, and S. Bushway (College Park, MD: University of Maryland Department of Criminology and Criminal Justice, 1997).

Sjoquist, David. "Property Crime and Economic Behavior: Some Empirical Results," *American Economic Review* 83 (1973): 439-446.

Skogan, W. "Citizen Reporting of Crime: Some National Panel Data," *Criminology* 13 (1976): 535-549.

Sloan, Frank A.; Reilly, Bridget A.; and Schenzler, Christopher M. "The Effects of Prices, Civil and Criminal Sanctions, and Law Enforcement on Alcohol-Related Mortality." *Journal of Studies on Alcohol* 55 (July 1994a): 454-65.

__________; __________; and __________. "Tort Liability versus Other Approaches for Deterring Careless Driving." *International Review of Law and Economics* 14 (March 1994b): 53-71.

__________; __________; and __________. "Effects of Tort Liability and Insurance on heavy Drinking and Drinking and Driving," *Journal of Law and Economics* 38 (April 1995): 49-77.

Sollars, David L.; Benson, Bruce L.; And Rasmussen, David W. "Drug Enforcement and Deterrence of Property Crime Among Local Jurisdictions," *Public Finance Quarterly* 22 (1994): 22-45.

Tabarrok, Alexander. "A Simple Model of Crime Waves, Riots, and Revolutions." *Atlantic Economic Journal* (September 1997): 274-288.

Tauchen, Helen V.; Ann Dryden Witte; and Sharon K. Long. "Domestic Violence: A Nonrandom Affair." *International Economic Review* (May, 1991): 491-511.

Taylor, John. "Econometric Models of Criminal Behavior: A Review." In *Economic Models of Criminal Behavior,* edited by J.M. Heineke, 35-82. Amsterdam: North Holland (1978).

Telser, Lester G. "Why Should Manufacturers Want Fair Trade?" *Journal of Law and Economics* 3 (1960): 86-105.

Trumbull, William N. "Estimations of the Economic Model of Crime Using Aggregate and Individual Data." *Southern Economic Journal* (1989): 423-439.

U.S. Department of Health and Human Services. *Surgeon General's Workshop on Drunk Driving: Proceedings* (Washington D. C.: Public Health Service, 1998).

White, Halbert. "A Heteroskedasticity Consistent Covariance Matrix and a Direct Test for Heteroscedasticity." *Econometrica* 48 (1978): 817-38.

Wilkinson, James T. "Reducing Drunken Driving: Which Policies Are Most Effective?" *Southern Economic Journal* 54, no. 2 (1978): 322-34.

Witte, A. D. "Estimating the Economic Model of Crime with Individual Data." *Quarterly Journal of Economics* 94 (1980): 57-84.

Wolfgang, M. E. *Patters in Criminal Homicide* (Montclair, NJ: Patterson Smith, 1975).

Young, Douglas A. and Likens, Thomas W. "Alcoholic Beverage Taxes and Auto Fatality Rates," *International Review of Law and Economics*, 20 (2000): 107-126.

Zimmerman, Paul R. "The Theoretical and Empirical Effects of Alcohol Control Policies on Crime." Unpublished Ph.D. dissertation, Florida State University, 2000.

APPENDIX A

STATE DATA :
DEFINITIONS, DESCRIPTIVE STATISTICS, AND SOURCES

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
MURDER	Per-capita murders (annual number of murders divided by total state pop.)	6.80E-05	3.61E-05	1,2
RAPE	Per-capita rapes (annual number of rapes divided by total state pop.)	0.00036411	0.000144335	1,2
ASSAULT	Per-capita aggravated assaults (annual number of aggravated assaults divided by total state pop.)	0.00304144	0.00158704	1,2
ROBBERY	Per-capita robberies (annual number of robberies divided by total state pop.)	0.00149785	0.00116642	1,2
TVC	Per-capita total violent crimes (annual number of total violent crimes divided by total state pop.)	0.00497142	0.00264808	1,2
BURGLARY	Per-capita burglaries (annual number of burglaries divided by total state pop.)	0.0114053	0.00384476	1,2

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
LARCENY	Per-capita larcenies (annual number of larcenies divided by total state pop.)	0.029649	0.00719545	1,2
MVT	Per-capita motor vehicle thefts (annual number of motor vehicle thefts divided by total state pop.)	0.00459145	0.00368194	1,2
TPC	Per-capita total property crimes (annual number of total property crimes divided by total state pop.)	0.0456458	0.0116952	1,2
ARRMUR	Murder arrest rate (annual number of murder arrests divided by annual number of murders)	0.766154	0.311196	1
ARRRAPE	Rape arrest rate (annual number of rape arrests divided by annual number of rapes)	0.34447	0.190726	1
ARRASS	Aggravated assault arrest rate (annual number of assault arrests divided by annual number of assaults)	0.392564	0.197511	1
ARRTVC	Total violent crime arrest rate (annual number of total violent crime arrests divided by annual number of total violent crimes)	0.350392	0.147082	1

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
ARRBURG	Burglary arrest rate (annual number of burglary arrests divided by annual number of burglaries)	0.117962	0.0421591	1
ARRLAR	Larceny arrest rate (annual number of larceny arrests divided by annual number of larcenies)	0.174066	0.0585192	1
ARRMVT	Motor vehicle theft arrest rate (annual number of motor vehicle theft arrests divided by annual number of motor vehicle thefts)	0.124206	0.0824548	1
ARRTPC	Total property crime arrest rate (annual number of total property crime arrests divided by annual number of total property crimes)	0.153362	0.0497588	1
POLICE	Per-capita police officers (number of individuals employed in full-time equivalent police protection services divided by state pop. age 18 and over)	0.00349837	0.000620506	2,3
PRISON	Per-capita prison population (number of persons in state prisons divided by state pop. age 18 and over)	0.00301915	0.0013444	2,17

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
$P_{c\|a}$	Probability of death sentence given arrest for murder (# persons sentenced to death in year t divided by # persons arrested for murder in year t-2)	0.0214297	0.0356858	1,17
$P_{e\|c}$	Probability of execution given execution conviction (# persons executed in year t divided by # persons sentenced to execution for murder in year t-6)	0.0574175	0.222617	1,17
NRA	Percent of state population in N.R.A.(# of persons with membership in the National Rifle Association divided by state pop. age 18 and over)	0.0180761	0.0072141	2,18
UR	Annual state unemployment rate (in %)	6.17161	1.84119	10
INCOME	Per-capita income (CPI deflated disposable income per-capita and adjusted for interstate cost-of-living differentials)	16175	1882.97	10
WAGELOW	Average low-skill wage (sum of total wages in wholesale trade, retail trade, and services divided by sum of persons employed in wholesale trade, retail trade, and services).	20.4217	2.12774	12
M1844	Males ages 18-44 (percent of state pop. ages 18 and over that is male and between the ages of 18 and 44)	0.288989	0.0150324	2
NONMET	Non-metropolitan population (percent of state population residing in non-metropolitan areas)	35.0422	21.6585	10

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
POPDEN	Population density (total state population divided by total state area in sq. miles)	159.498	217.81	2,10
CATH	Catholic (percentage of state population that is Catholic)	0.190338	0.134371	13
MRM	Mormon (percentage of state population that is Mormon)	0.0310491	0.105692	13
SOBAP	Southern Baptist (percentage of state population that is Southern Baptist)	0.0769909	0.104726	13
PROT	Protestant (percentage of state population that is other Protestant)	0.23639	0.100065	13
BEERTAX	Beer excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per six-pack of beer)	0.407711	0.163263	6,7
LIQTAX	Liquor excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of liquor)	19.3018	2.49914	8
WINETAX	Wine excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of wine)	1.16387	0.696235	9
TOTTAX	Total alcohol excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of total alcohol)	0.125452	0.048318	6,7,8,9
BEER	Per-capita beer consumption (state beer shipments divided by state pop. age 18 and over)	0.032268	0.005437	2,6,7
LIQ	Per-capita liquor consumption (state distilled spirits shipments divided by state pop. age 18 and over)	0.002141	0.00078	2,6,8

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
WINE	Per-capita wine consumption (state wine shipments divided by state pop. age 18 and over)	0.002598	0.001264	2,6,9
LEGAL	Percentage of state pop. between the ages of 18 and 20 that can legally drink beer with alcohol content of 3.2 percent.	.05784926	.1796901	16
DRYPER	Percent of state pop. residing in counties dry for beer.	.0275487	.1796901	16
DRINK	Per-capita drinking establishments (age 18 and over).	.3044E-03	.225E-03	12
NOSIGN	Beer price advertising restrictions indicator on signs and billboards (1 for states with restrictions, 0 otherwise)	.3619791	.481199	16
NOPRINT	Beer price advertising restrictions in print media indicator (1 for states with restrictions, 0 otherwise)	.1822916	.386588	16
FINE	Real COL-adjusted minimum fine for first DUI conviction (in dollars).	91.80029	150.289	14
ILDUM	Illegal per se indicator (1 for states with illegal per se DUI statute, 0 otherwise).	.8880208	.315752	14
PBT	Preliminary breath test indicator (1 for states that allow breath tests for drivers stopped under suspicion of DUI, 0 otherwise).	.4947916	.500625	14
SUS	Minimum license suspension for first DUI conviction (in days).	36.10156	63.3589	14
JAIL	Minimum jail sentence for first DUI conviction (in days).	.4010416	.726664	14

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
MANDATE	Portion of year that state mandate of exclusive territories on beer distributors in effect	.4883473	.498905	16
CASHLAW	Indicator variable for state law requiring retailers to pay immediately for beer purchased from wholesalers (1 for states with law, 0 otherwise).	.3125000	.464117	16
FORCE	Indicator variable for states with forced deposit law (1 for states with law, 0 otherwise).	.2013802	.400835	16
MINDIST	Distance along major highways from most populous city in a state to the nearest major brewery (among those six breweries with the largest market share in 1982: Anheuser-Busch, Miller, Stroh, Heilman, Pabst, and Coors).	151.3229	146.773	16
TOURPCT	Ratio of hotel, motel, and tourist court receipts to retail sales, multiplied by 100 (data for 1983-86 estimated by linear interpolation from 1982 and 1987 actual data, data for 1988-1991 interpolated from 1987 and 1992 actual data).	4.460855	10.4127	12

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
PERBLK	Percentage of the state pop. that is black.	0.083492	0.09032	2
GINI	State gini coefficient (derived from distribution of houslehold income by state).	0.415223	0.02550	2
NOPLEA	No DUI plea-bargaining indicator(1 for states that do not allow plea-bargaining on DUI charges, 0 otherwise).	0.16875	0.37492	14
DRAM	Dram shop liability indicator (1 for states with drop shop liability provisions, 0 otherwise)	0.664583	0.47262	14
TOTALC	Total per-capita alcohol consumption (sum of state shipments for beer, distilled spirits, and wine divided by the state pop. ages 18 and over).	0.036663	0.00656	2,6,7,8, 9
ARRROBB	Robbery arrest rate (annual number of robbery arrests divided by annual number of robberies)	0.252084	0.10922	1
POP45P	Percentage of total state pop. that is ages 45 years and older.	0.316573	0.02895	2
ARRTPC	Total property crime arrest rate (sum of annual number of arrests for burglary, larceny, and motor vehicle theft divided by the sum of annuaul burglaries, larcenies, and motor vehicle thefts).	0.153204	0.05212	1
TAX	Per-capita personal tax revenue (annual state tax revenue divided by the state pop. ages 18 and over, adjusted for inflation and state COL)	0.001705	0.00031	2,10

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
PREDEC	Preliminary decision indicator (1 for states where court has handed down a preliminary decision regarding prison overcrowding legislation in that year, 0 otherwise)	0.002083	0.04564	Levitt (1996)
FINAL	Final decision indicator (1 for states where court has made a final decision regarding prison overcrowding legislation in that year, 0 otherwise)	0.008333	0.09100	Levitt (1996)
FURTHER	Further decision indicator (1 for states where court order has mandated further action regarding prison overcrowding legislation in that year following an initial ruling, 0 otherwise)	0.010417	0.10163	Levitt (1996)
PREDEC23	Preliminary decision status indicator (1 for two to three years following preliminary decision if no other court action taken regarding prison-overcrowding legislation, 0 otherwise; see text)	0.004167	0.06448	Levitt (1996)
FINAL23	Final decision status indicator (1 for two to three years following final decision if no other court action taken regarding prison-overcrowding legislation, 0 otherwise; see text)	0.0125	0.11121	Levitt (1996)

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
FUR23	Further decision status indicator (1 for two to three years following court decision of further action with regard to prison-overcrowding legislation if no other court action taken, 0 otherwise; see text)	0.020833	0.14297	Levitt (1996)

Sources:

1. F.B.I. Uniform Crime Reports (various years)
2. U.S. Census Bureau (www.census.gov/population/www/estimates/statepop.html)
3. *Public Employment*, U.S. Census Bureau (various years)
4. Death Penalty Information Center (www.essential.org/dpic/firstpage.html)
5. National Archive of Criminal Justice Data (www.icpsr.umich.edu/NACJD/home.html)
6. *Brewers Almanac* (various years)
7. *Adams/Jobson' s Beer Handbook* (various years)
8. *Adams/Jobson' s Liquour Handbook* (various years)
9. *Adams/Jobson' s Wine Handbook* (various years)
10. *Statistical Abstract of the United States* (various years)
11. *Current Population Survey*, U.S. Bureau of the Census
12. *County Business Patterns,* U.S. Bureau of the Census
13. *Churches and Church Membership in the United States* (various years)
14. *Digest of State Alcohol-Highway Safety Related Legislation* (various years)
15. *Census of Service Industries*, U.S. Department of Commerce (various years)
16. *Modern Age Brewery Bluebook* (various years)
17. *Sourcebook of Criminal Justice Statistics*, U.S. Dept. of Justice (various years)
18. Lott and Mustard (1997)

APPENDIX B

METRO-LEVEL DATA: DEFINITIONS, DESCRIPTIVE STATISTICS, AND SOURCES (1991-1998)

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
MURDER	Per-capita murders (annual number of MSA murders divided by total MSA pop.)	0.000109	6.49E-05	1
RAPE	Per-capita rapes (annual number of MSA rapes divided by total MSA pop.)	0.00044	0.000152	1
ASSAULT	Per-capita aggravated assaults (annual number of MSA aggravated assaults divided by total MSA pop.)	0.00475	0.001979	1
ROBBERY	Per-capita robberies (annual number of MSA robberies divided by total MSA pop.)	0.003458	0.001947	1
TVC	Per-capita total MSA violent crimes (annual number of total violent crimes divided by total MSA pop.)	8.69E-03	3.68E-03	1
ARRMUR	Murder arrest rate (annual number of state murder arrests divided by annual state murders)	0.835618	0.351162	1
ARRRAPE	Rape arrest rate (annual number of state rape arrests divided by annual state rapes)	0.305876	0.098211	1
ARRASS	Aggravated assault arrest rate (annual number of state assault arrests divided by annual state assaults)	0.432193	0.194891	1
ARRTVC	Total violent crime arrest rate (annual number of total state violent crime arrests divided by annual state total violent crimes)	0.362279	0.136637	1
POLICE	Per-capita police officers (number of individuals employed in state full-time equivalent police protection services divided by 1990 MSA pop. age 18 and over)	0.018131	0.016054	1,3
FHH90	Proportion of 1990 MSA households headed by single female	0.120978	0.016974	2
POV90	Percentage of 1990 MSA pop. in poverty	11.4667	2.60175	2

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
$P_{c\|a}$	Probability of murder conviction given arrest for murder (# persons sentenced to death in year t in state j divided by # persons arrested for murder in year t-2 in state j)	0.017495	0.018703	1,17
$P_{e\|c}$	Probability of execution given execution conviction (# persons executed in year t in state j divided by # persons sentenced to execution for murder in year t-6 in state j)	0.134338	64.765	1,17
UR	Annual state unemployment rate (in %)	5.76	1.82318	10
INCOME	Per-capita income (1990 MSA CPI deflated income per-capita adjusted for interstate cost-of-living differentials)	14123.7	1345.07	10
WAGELOW	Average low-skill wage (sum of total state-level wages in wholesale trade, retail trade, and services divided by sum of state-level persons employed in wholesale trade, retail trade, and services, 1994 data used for 1995-1998).	20.5541	1.2737	12
M1844	Males ages 18-44 (percent of 1990 MSA pop. ages 18 and over that is male and between the ages of 18 and 44)	0.339669	0.045904	2
URB90	Percent urban (percent of 1990 MSA pop. residing in urban areas)	9.54333	5.96513	10
CATH	Catholic (percentage of state population that is Catholic, data for 1991-1998 extrapolated from 1980 and 1990 data).	0.224192	0.110289	13
MRM	Mormon (percentage of state population that is Mormon, data for 1991-1998 extrapolated from 1980 and 1990 data)	0.009391	0.008915	13
SOBAP	Southern Baptist (percentage of state population that is Southern Baptist, data for 1991-1998 extrapolated from 1980 and 1990 data)	0.060598	0.079619	13
PROT	Protestant (percentage of state population that is other Protestant, data for 1991-1998 extrapolated from 1980 and 1990 data)	0.211601	0.066767	13
BEERTAX	Beer excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of beer)	0.620354	0.154788	6,7
LIQTAX	Liquor excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of liquor)	13.5869	2.32524	8
WINETAX	Wine excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of wine)	1.33557	0.559793	9

VARIABLE	DEFINITION	MEAN	STD. DEV.	SOURCES
TOTTAX	Total alcohol excise tax (CPI deflated and state cost-of-living adjusted state plus federal excise tax rate per gallon of total alcohol)	0.290327	0.078773	6,7,8,9
BEER	Per-capita beer consumption (MSA beer shipments divided by MSA pop. age 18 and over)	0.015202	0.014236	2,6,7
LIQ	Per-capita liquor consumption (MSA distilled spirits shipments divided by MSA pop. age 18 and over)	0.000804	0.000979	2,6,8
WINE	Per-capita wine consumption (MSA wine shipments divided by MSA pop. age 18 and over)	0.001423	0.002326	2,6,8
DRINK	Per-capita drinking establishments (state drinking establishments divided by MSA age 18 and over).	0.001213	0.001184	12
NOSIGN	Beer price advertising restrictions indicator on signs and billboards (1 for states with restrictions, 0 otherwise)	0.266667	0.443141	16
NOPRINT	Beer price advertising restrictions in print media indicator (1 for states with restrictions, 0 otherwise)	0.166667	0.373457	16
MANDATE	Portion of year that state mandate of exclusive territories on beer distributors in effect	0.5	0.501045	16
CASHLAW	Indicator variable for state law requiring retailers to pay immediately for beer purchased from wholesalers (1 for states with law, 0 otherwise).	0.2	0.400836	16
FORCE	Indicator variable for states with forced deposit law (1 for states with law, 0 otherwise).	0.333333	0.47239	16
MINDIST	Distance along major highways from most populous city in a state to the nearest major brewery (among those six breweries with the largest market share in 1982: Anheuser-Busch, Miller, Stroh, Heilman, Pabst, and Coors).	52.4333	94.316	16
TOURPCT	Ratio of hotel, motel, and tourist court receipts to retail sales, multiplied by 100 (data for 1991 interpolated from 1987 and 1992 actual data, actual 1992 data used for 1993-1998).	3.12652	1.32213	12

Sources:

1. F.B.I. Uniform Crime Reports (various years)
2. 1990 U.S. Census
3. *Public Employment*, U.S. Census Bureau (various years)
4. Death Penalty Information Center (www.essential.org/dpic/firstpage.html)
5. National Archive of Criminal Justice Data (www.icpsr.umich.edu/NACJD/home.html)
6. *Brewers Almanac* (various years)
7. *Adams/Jobson' s Beer Handbook* (various years)
8. *Adams/Jobson' s Liquor Handbook* (various years)
9. *Adams/Jobson' s Wine Handbook* (various years)
10. *Statistical Abstract of the United States* (various years)
11. *Current Population Survey*, U.S. Bureau of the Census
12. *County Business Patterns,* U.S. Bureau of the Census
13. *Churches and Church Membership in the United States* (various years)
14. *Digest of State Alcohol-Highway Safety Related Legislation* (various years)
15. *Census of Service Industries*, U.S. Department of Commerce (various years)
16. *Modern Age Brewery Bluebook* (various years), various states statutes
17. *Sourcebook of Criminal Justice Statistics*, U.S. Dept. of Justice (various years)

APPENDIX C

CRIME DEFINITIONS OF THE UNIFORM CRIME REPORTS

Murder and Nonegligent Murder

The willful killing of one human being by another. Deaths caused by negligence, attempts to kill, assaults to kill, suicides, accidental deaths, and justifiable homicide are excluded. Justifiable homicides are limited to the killing of a felon. Traffic fatalities are excluded.

Forcible Rape

The carnal knowledge of a female forcibly and against her will. Included are rapes by force and attempts or assaults to rape. Statutory offenses (no force used-victim under age of consent) are excluded.

Aggravated Assault

An unlawful attack by one person on another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm. Simple assaults are excluded.

Burglary

The unlawful entry of a structure to commit a felony or a theft. Attempted forcible entry is included.

Motor Vehicle Theft

The theft or attempted theft of a motor vehicle.

Table 3.1
Per-Capita Beer Consumption Regression
(1985-1994)

	ONE-WAY F.E.	TWO-WAY F.E.
LM1844	0.5318*** (3.23)	-0.4132** (2.11)
LNONMET	-0.0022 (0.13)	-0.0082 (0.53)
LPOPDEN	-0.2816*** (4.58)	0.0428 (0.58)
LPERBLK	-0.0012 (0.72)	-0.0001 (0.07)
LUR	-0.0260*** (5.54)	-0.0592*** (8.35)
LINCOME	0.0861** (2.27)	0.1290*** (3.57)
LGINI	-0.0524 (1.14)	0.0553 (1.30)
LEGAL	-0.0007 (0.06)	0.0046 (0.41)
LBEERTAX	0.0177 (1.51)	-0.0394* (1.85)
MANDATE	-0.0069 (0.28)	0.0174 (0.79)
CASHLAW	-0.0117** (2.14)	-0.0135** (2.51)
FORCE	-0.0307 (1.21)	0.0200 (0.86)
MINDIST	0.0003E-02 (0.07)	0.0003E-01 (0.62)
DRYPER	0.0372 (1.16)	0.0583** (2.01)
LDRINK	0.0222** (2.51)	0.0352*** (3.91)
LTOURPCT	-0.0772** (2.06)	-0.0888*** (2.64)
NOSIGN	0.0132 (1.52)	0.0147* (1.94)
NOPRINT	-0.0097 (0.63)	-0.0117 (0.86)
LMRM	-0.1541*** (3.59)	0.1021** (2.05)
LSOBAP	-0.0786** (2.17)	0.0099 (0.29)
LCATH	0.1364** (2.45)	0.0822* (1.67)

Table 3.1 (continued)

	ONE-WAY F.E.	TWO-WAY F.E.
LPROT	0.0592 (1.18)	0.0658 (1.43)
PBT	-0.0104 (0.91)	-0.0002 (0.02)
NOPLEA	-0.0216 (1.52)	-0.0141 (1.11)
DRAM	0.0288*** (3.25)	0.0250*** (3.19)
JAIL	-0.0037 (0.16)	0.0158 (0.77)
FINE	0.0001E-02 (0.05)	0.0007E-02 (0.26)
SUS	0.0004E-01 (0.66)	0.0006E-01 (1.12)
ILDUM	-0.0137 (0.88)	-0.0165 (1.19)
CONSTANT		-4.0954*** (7.84)
STATE F.E.	Yes	Yes
PERIOD F.E.	No	Yes
N	480	480
Adj. R^2	.9677	.9753
F-stat.	190.00	220.63

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 3.2
Per-Capita Liquor Consumption Regression
(1985-1994)

	ONE-WAY F.E.	TWO-WAY F.E.
LM1844	-0.8905** (2.23)	-1.1821*** (2.99)
LNONMET	-0.0220 (0.65)	-0.0200 (0.67)
LPOPDEN	-1.3831*** (9.65)	-0.3768** (2.13)
LPERBLK	-0.0036 (0.89)	-0.0059 (1.64)
LUR	-0.0142 (1.45)	-0.0762*** (4.94)
LINCOME	0.0013 (0.02)	0.1304* (1.69)
LGINI	-0.2032* (1.90)	-0.0478 (0.51)
LEGAL	-0.0218 (0.71)	-0.0255 (0.95)
LLIQTAX	-0.1467* (1.74)	-0.4138*** (3.49)
DRYPER	0.1110* (1.65)	0.1699*** (2.85)
LDRINK	0.0098 (0.48)	0.0121 (0.59)
LTOURPCT	-0.1792** (2.30)	-0.1726** (2.51)
NOSIGN	-0.0110 (0.69)	-0.0193 (1.43)
NOPRINT	0.0398 (1.27)	0.0416 (1.58)
LMRM	-0.5866*** (5.60)	0.1165 (0.89)
LSOBAP	-0.3269*** (3.47)	-0.1890** (2.34)
LCATH	-0.1630 (1.20)	-0.1290 (1.12)
LPROT	-0.4742*** (4.06)	-0.3130*** (3.03)
PBT	-0.0391* (1.93)	-0.0219 (1.28)
NOPLEA	0.0244 (0.88)	0.0520** (2.18)
DRAM	0.0547** (2.36)	0.0226 (1.13)

Table 3.2 (continued)

	ONE-WAY F.E.	TWO-WAY F.E.
JAIL	-0.0481 (1.24)	-0.0406 (1.23)
FINE	0.0002*** (3.03)	0.0002*** (2.93)
SUS	0.0003** (2.42)	0.0002** (2.51)
ILDUM	-0.1143** (2.38)	-0.1150*** (2.83)
CONSTANT		-6.2999*** (5.69)
STATE F.E.	Yes	Yes
YEAR F.E.	No	Yes
N	300	300
Adj. R^2	.9650	.9755
F-stat.	153.59	186.82

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 3.3
Per-Capita Wine Consumption Regression
(1985-1994)

	ONE-WAY F.E.	TWO-WAY F.E.
LM1844	1.7051*** (3.82)	-0.4793 (0.97)
LNONMET	0.0682 (1.49)	0.0697* (1.73)
LPOPDEN	-0.3730** (2.30)	0.6987*** (3.64)
LPERBLK	0.0022 (0.47)	-0.0014 (0.34)
LUR	0.0067 (0.51)	0.0102 (0.54)
LINCOME	-0.1334 (1.32)	0.1913** (2.03)
LGINI	-0.3998*** (3.24)	-0.2616** (2.32)
LEGAL	0.0477 (1.58)	0.0305 (1.05)
LWINETAX	-0.0702*** (4.88)	-0.0057 (0.27)
DRYPER	0.1259 (1.44)	0.2892*** (3.72)
LDRINK	0.0588** (2.57)	0.0026 (0.11)
LTOURPCT	-0.3480*** (3.25)	-0.2882*** (3.11)
NOSIGN	-0.0302 (1.32)	-0.0160 (0.81)
NOPRINT	-0.0003 (0.01)	0.0041 (0.11)
LMRM	-0.5585*** (4.99)	0.2489* (1.92)
LSOBAP	0.0729 (0.72)	0.3067*** (3.33)
LCATH	-0.1623 (1.10)	-0.0543 (0.43)
LPROT	0.2605* (1.94)	0.3359*** (2.84)
PBT	0.0219 (0.74)	0.0188 (0.72)
NOPLEA	-0.0819** (2.18)	-0.0775** (2.41)
DRAM	-0.0561** (2.29)	-0.0363* (1.70)

Table 3.3 (continued)

	ONE-WAY F.E.	TWO-WAY F.E.
JAIL	0.1379** (2.31)	0.1176** (2.23)
FINE	0.0004*** (3.93)	0.0003*** (3.33)
SUS	-0.0002 (1.18)	-0.0007E-01 (0.47)
ILDUM	-0.0497 (1.21)	-0.0273 (0.76)
CONSTANT		-8.8845*** (6.52)
STATE F.E.	Yes	Yes
YEAR F.E.	No	Yes
N	450	450
Adj. R^2	.9765	.9828
F-stat.	270.79	325.81

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 3.4
Per-Capita Total Alcohol Consumption Regression
(1985-1994)

	ONE-WAY F.E.	TWO-WAY F.E.
LM1844	0.0293 (0.11)	-0.6908*** (2.78)
LNONMET	0.0265 (1.26)	0.0162 (0.93)
LPOPDEN	-0.2416** (2.17)	0.2081* (1.94)
LPERBLK	-0.0045* (1.80)	-0.0044** (2.12)
LUR	-0.0336*** (5.40)	-0.0745*** (8.65)
LINCOME	0.1090** (2.02)	0.1841*** (4.13)
LGINI	-0.0965 (1.50)	0.0458 (0.85)
LEGAL	0.0014 (0.07)	0.0042 (0.27)
FORCE	-0.0407 (1.45)	0.0227 (0.98)
CASHLAW	-0.0231*** (2.87)	-0.0236*** (3.36)
MINDIST	-0.0003E-01 (0.63)	0.0001E-01 (0.28)
MANDATE	0.0382 (1.03)	0.0714** (2.40)
LTOTTAX	0.1216** (2.44)	-0.0639 (1.39)
DRYPER	0.0219 (0.53)	0.0738** (2.16)
LDRINK	0.0266** (2.09)	0.0468*** (3.90)
LTOURPCT	-0.1021** (2.16)	-0.1131*** (2.96)
NOSIGN	0.0224** (2.20)	0.0121 (1.47)
NOPRINT	-0.0325 (0.83)	-0.0343 (1.10)
LMRM	-0.2234*** (3.37)	0.1987*** (2.63)
LSOBAP	0.0757 (1.24)	0.1268*** (2.59)
LCATH	0.1736* (1.94)	0.1022 (1.43)

Table 3.4 (continued)

	ONE-WAY F.E.	TWO-WAY F.E.
LPROT	0.0021 (0.03)	0.0894 (1.40)
PBT	-0.0098 (0.78)	0.0001 (0.01)
NOPLEA	-0.0382** (2.19)	-0.0128 (0.89)
DRAM	0.0524*** (3.52)	0.0256** (2.11)
JAIL	0.0100 (0.41)	0.0142 (0.73)
FINE	-0.0001** (2.07)	0.0001E-01 (0.20)
SUS	0.0009E-01 (1.30)	0.0001** (2.10)
JLDUM	0.0426 (1.35)	-0.0112 (0.44)
CONSTANT		-4.5312*** (7.16)
STATE F.E.	Yes	Yes
YEAR F.E.	No	Yes
N	290	290
Adj. R^2	.9687	.9805
F-stat.	157.94	218.28

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.1
Per-Capita Murder Regression:
Full Sample 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRMUR	-0.0859*** (3.26)	-0.0921*** (3.48)	-0.0887*** (3.36)	-0.0857*** (3.24)	-0.0911*** (3.44)
LPOLICE	0.0804 (0.27)	-0.0271 (0.09)	-0.0224 (0.08)	0.0815 (0.27)	0.0424 (0.14)
LPRISON	-0.0352 (0.29)	0.0128 (0.11)	-0.0184 (0.15)	-0.0310 (0.25)	-0.0164 (0.13)
$P_{c\|a}$	-0.5522 (1.62)	-0.5689* (1.68)	-0.5871* (1.73)	-0.5653* (1.66)	-0.5071 (1.49)
$P_{e\|c}$	-0.0662 (1.53)	-0.0692 (1.60)	-0.0685 (1.57)	-0.0655 (1.51)	-0.0699 (1.61)
LNRA	0.1664 (0.76)	0.1483 (0.67)	0.1702 (0.77)	0.1713 (0.78)	0.1214 (0.55)
LM1844	2.5110* (1.71)	3.0489** (2.07)	2.8008* (1.91)	2.5089* (1.71)	2.9219** (1.98)
LNONMET	-0.0162 (0.16)	-0.0320 (0.31)	-0.0111 (0.11)	-0.0115 (0.11)	-0.0506 (0.49)
LPOPDEN	0.2035 (0.58)	0.3166 (0.91)	0.2581 (0.74)	0.1889 (0.54)	0.2497 (0.71)
LPERBLK	-0.0200* (1.74)	-0.0186 (1.62)	-0.0186 (1.61)	-0.0202* (1.76)	-0.0188 (1.64)
LUR	-0.0311 (0.56)	0.0283 (0.54)	-0.0008 (0.02)	-0.0265 (0.47)	-0.0111 (0.20)
LINCOME	-0.1485 (0.59)	-0.2413 (0.96)	-0.2155 (0.86)	-0.1501 (0.59)	-0.1849 (0.73)
LGINI	-0.5766* (1.85)	-0.5490* (1.76)	-0.5672* (1.81)	-0.5901* (1.89)	-0.5064 (1.61)
LWAGELOW	0.2407 (0.73)	0.3166 (0.96)	0.2596 (0.79)	0.2310 (0.70)	0.3058 (0.93)
LROBBERY	0.2255*** (2.95)	0.1815** (2.37)	0.2037*** (2.70)	0.2244*** (2.92)	0.1992*** (2.58)
LBURGLARY	0.1913** (2.28)	0.2129** (2.51)	0.1885** (2.23)	0.1922** (2.29)	0.2116** (2.49)
LBEER	-0.4882 (1.36)				-0.6604* (1.80)
LLIQ		0.3685 (1.63)			0.4487* (1.92)
LWINE			0.0829 (0.61)		0.0635 (0.47)
LTOTALC				-0.4242 (1.12)	
CONSTANT	-5.5240 (1.43)	-1.3308 (0.34)	-3.3413 (0.89)	-5.1403 (1.34)	-2.6743 (0.65)

Table 4.1 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	480	480	480	480
Adj. R^2	.9135	.9137	.9132	.9134	.9140
F-stat.	69.35	69.50	69.08	69.24	67.94

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.2
Per-Capita Rape Regression:
Full Sample 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRRAPE	-0.0707*** (3.57)	-0.0717*** (3.68)	-0.0704*** (3.53)	-0.0713*** (3.62)	-0.0720*** (3.69)
LPOLICE	0.1598 (0.89)	0.1842 (1.06)	0.2335 (1.29)	0.1006 (0.56)	0.1371 (0.76)
LPRISON	-0.1880*** (2.59)	-0.1713** (2.40)	-0.2221*** (3.08)	-0.1757** (2.43)	-0.1558** (2.16)
LNRA	0.4005*** (2.97)	0.3406** (2.56)	0.3828*** (2.82)	0.4002*** (2.99)	0.3531*** (2.65)
LM1844	-1.7835** (2.00)	-1.4551* (1.65)	-1.9081** (2.12)	-1.6740* (1.88)	-1.4015 (1.58)
LNONMET	0.1340** (2.14)	0.0826 (1.33)	0.1224* (1.94)	0.1315** (2.11)	0.0922 (1.47)
LPOPDEN	-0.7223*** (3.38)	-0.7118*** (3.40)	-0.7893*** (3.70)	-0.6570*** (3.06)	-0.6783*** (3.22)
LPERBLK	-0.0011 (0.16)	-0.0005 (0.08)	-0.0021 (0.31)	0.0003 (0.04)	0.0002 (0.03)
LUR	-0.0281 (0.83)	-0.0204 (0.65)	-0.0652** (2.14)	-0.0143 (0.42)	-0.0024 (0.07)
LINCOME	0.1628 (1.06)	0.1702 (1.13)	0.2114 (1.37)	0.1299 (0.84)	0.1417 (0.93)
LGINI	0.0018 (0.01)	0.0888 (0.47)	0.0166 (0.09)	0.0241 (0.13)	0.0794 (0.42)
LWAGELOW	-0.1959 (0.97)	-0.1252 (0.63)	-0.2100 (1.03)	-0.1695 (0.84)	-0.1236 (0.62)
LBEER	0.5242** (2.43)				0.3117 (1.42)
LLIQ		0.5816*** (4.37)			0.5342*** (3.85)
LWINE			0.0491 (0.59)		-0.0074 (0.09)
LTOTALC				0.7373*** (3.26)	
CONSTANT	-5.3354** (2.25)	-3.1479 (1.31)	-6.8532*** (2.95)	-4.8643** (2.08)	-2.4351 (0.98)
N	480	480	480	480	480
Adj. R^2	.9052	.9081	.9039	.9062	.9081
F-stat.	66.31	68.59	65.35	67.13	66.72

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.3
Per-Capita Robbery Regression:
Full Sample 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRROB	-0.0036 (0.24)	-0.0058 (0.39)	-0.0029 (0.19)	-0.0045 (0.31)	-0.0075 (0.51)
LPOLICE	0.5706*** (2.85)	0.6699*** (3.40)	0.6586*** (3.27)	0.4991** (2.48)	0.5019** (2.49)
LPRISON	-0.3816*** (4.72)	-0.3915*** (4.86)	-0.4461*** (5.54)	-0.3694*** (4.60)	-0.3636*** (4.50)
LNRA	0.0259 (0.17)	-0.0424 (0.28)	-0.0134 (0.09)	0.0224 (0.15)	-0.0224 (0.15)
LM1844	2.9510*** (2.98)	3.1287*** (3.13)	2.8226*** (2.81)	3.0841*** (3.13)	3.3617*** (3.40)
LNONMET	-0.0102 (0.15)	-0.0676 (0.96)	-0.0341 (0.48)	-0.0165 (0.24)	-0.0482 (0.69)
LPOPDEN	-0.0850 (0.36)	-0.1300 (0.55)	-0.2031 (0.85)	0.0014 (0.01)	-0.0571 (0.24)
LPERBLK	-0.0370*** (4.86)	-0.0377*** (4.96)	-0.0378*** (4.88)	-0.0353*** (4.65)	-0.0347*** (4.56)
LUR	-0.0087 (0.23)	-0.0309 (0.87)	-0.0767** (2.25)	0.0048 (0.13)	0.0049 (0.13)
LINCOME	-0.2690 (1.57)	-0.2152 (1.27)	-0.2003 (1.16)	-0.3073* (1.80)	-0.3027* (1.78)
LGINI	-0.5520*** (2.62)	-0.4676** (2.20)	-0.4959** (2.30)	-0.5166** (2.46)	-0.4491** (2.12)
LWAGELOW	0.1582 (0.70)	0.2139 (0.95)	0.1331 (0.59)	0.1959 (0.88)	0.2128 (0.95)
LBEER	0.9236*** (3.85)				0.7339*** (2.99)
LLIQ		0.5508*** (3.65)			0.4055*** (2.61)
LWINE			0.1896** (2.04)		0.1318 (1.44)
LTOTALC				1.1409*** (4.54)	
CONSTANT	2.9516 (1.12)	3.5376 (1.31)	0.9208 (0.36)	3.2877 (1.27)	5.9142** (2.14)
N	480	480	480	480	480
Adj. R^2	.9819	.9819	.9815	.9822	.9823
F-stat.	372.77	371.41	363.29	377.95	369.58

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.4
Per-Capita Aggravated Assault Regression:
Full Sample 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRASS	-0.0079 (0.42)	-0.0166 (0.85)	-0.0143 (0.73)	-0.0111 (0.59)	-0.0071 (0.37)
LPOLICE	0.1283 (0.72)	0.3014* (1.68)	0.3113* (1.71)	0.1022 (0.57)	0.1358 (0.75)
LPRISON	0.0150 (0.21)	-0.0328 (0.45)	-0.0466 (0.64)	0.0146 (0.20)	0.0149 (0.21)
LNRA	-0.1430 (1.08)	-0.1790 (1.31)	-0.1691 (1.24)	-0.1501 (1.13)	-0.1400 (1.05)
LM1844	3.7702*** (4.30)	3.5619*** (3.93)	3.4501*** (3.82)	3.8218*** (4.34)	3.7425*** (4.22)
LNONMET	-0.3502*** (5.66)	-0.3784*** (5.90)	-0.3688*** (5.80)	-0.3586*** (5.78)	-0.3483*** (5.53)
LPOPDEN	-0.3524* (1.68)	-0.4589** (2.13)	-0.4799** (2.24)	-0.2981 (1.40)	-0.3534* (1.67)
LPERBLK	-0.0177*** (2.60)	-0.0198*** (2.84)	-0.0203*** (2.89)	-0.0166** (2.43)	-0.0179*** (2.61)
LUR	-0.1654*** (4.98)	-0.2229*** (6.85)	-0.2349*** (7.62)	-0.1661*** (4.95)	-0.1655*** (4.83)
LINCOME	-0.0635 (0.42)	0.0349 (0.23)	0.0450 (0.29)	-0.0767 (0.50)	-0.0601 (0.39)
LGINI	0.3635* (1.95)	0.3865** (2.00)	0.3684* (1.90)	0.3948** (2.11)	0.3549* (1.87)
LWAGELOW	0.1052 (0.53)	0.0977 (0.48)	0.0764 (0.37)	0.1329 (0.66)	0.1033 (0.52)
LBEER	1.0268*** (4.82)				1.0387*** (4.69)
LLIQ		0.1502 (1.09)			-0.0169 (0.12)
LWINE			0.0147 (0.18)		-0.0202 (0.25)
LTOTALC				1.0250*** (4.56)	
CONSTANT	6.0996*** (2.62)	3.5863 (1.46)	2.6377 (1.13)	5.7233** (2.46)	5.8963** (2.37)
N	480	480	480	480	480
Adj. R^2	.9657	.9638	.9637	.9655	.9655
F-stat.	193.49	183.34	182.81	192.37	187.23

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.5
Per-Capita Total Violent Crime Regression:
Full Sample 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRTVC	0.0167 (1.16)	0.0091 (0.62)	0.0119 (0.80)	0.0149 (1.04)	0.0137 (0.95)
LPOLICE	0.2295* (1.76)	0.3532*** (2.69)	0.3572*** (2.66)	0.1880 (1.43)	0.2117 (1.60)
LPRISON	-0.0913* (1.74)	-0.1176** (2.18)	-0.1460*** (2.72)	-0.0867* (1.65)	-0.0830 (1.56)
LNRA	-0.1217 (1.25)	-0.1665* (1.66)	-0.1495 (1.48)	-0.1267 (1.31)	-0.1382 (1.41)
LM1844	2.9258*** (4.54)	2.8958*** (4.36)	2.7122*** (4.07)	3.0035*** (4.67)	3.0648*** (4.72)
LNONMET	-0.1898*** (4.18)	-0.2265*** (4.82)	-0.2084*** (4.44)	-0.1965*** (4.35)	-0.2033*** (4.42)
LPOPDEN	-0.3588** (2.33)	-0.4273*** (2.71)	-0.4675*** (2.95)	-0.2981* (1.92)	-0.3462** (2.24)
LPERBLK	-0.0188*** (3.79)	-0.0201*** (3.94)	-0.0205*** (3.96)	-0.0177*** (3.55)	-0.0180*** (3.60)
LUR	-0.1075*** (4.42)	-0.1439*** (6.05)	-0.1681*** (7.39)	-0.1025*** (4.19)	-0.1011*** (4.04)
LINCOME	-0.0666 (0.60)	0.0027 (0.02)	0.0153 (0.13)	-0.0883 (0.79)	-0.0771 (0.69)
LGINI	0.0682 (0.50)	0.1137 (0.80)	0.0909 (0.64)	0.0974 (0.71)	0.1020 (0.74)
LWAGELOW	0.0463 (0.32)	0.0658 (0.44)	0.0230 (0.15)	0.0750 (0.51)	0.0673 (0.46)
LBEER	0.8670*** (5.55)				0.7946*** (4.92)
LLIQ		0.2945*** (2.92)			0.1587 (1.55)
LWINE			0.0728 (1.18)		0.0325 (0.54)
LTOTALC				0.9523*** (5.81)	
CONSTANT	4.3275** (2.53)	3.3482* (1.86)	1.7709 (1.03)	4.2742** (2.52)	5.3742*** (2.96)
N	480	480	480	480	480
Adj. R^2	.9820	.9811	.9807	.9821	.9821
F-stat.	374.48	355.25	349.08	377.02	364.95

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 4.6
DWH Endogeneity Test F-Statistics and p-values for Violent Crime Models (1985-1994)

Murder		
Specification:	**F-stat.**	**p-value**
Beer	1.1351	3.4
Liquor	1.0637	3.8
Wine	1.1119	3.5
Total	1.1523	3.3
Inclusive	0.8472	5.6
Rape		
Specification:	**F-stat.**	**p-value**
Beer	4.2029	2.4
Liquor	8.1777	2.3
Wine	7.0807	1.6
Total	6.6745	3.2
Inclusive	3.1824	4.6
Robbery		
Specification:	**F-stat.**	**p-value**
Beer	3.3561	1.0
Liquor	4.0712	3.0
Wine	3.8174	4.6
Total	3.5519	7.3
Inclusive	2.2813	3.5
Aggravated Assault		
Specification:	**F-stat.**	**p-value**
Beer	6.9591	1.9
Liquor	8.5453	1.2
Wine	9.0822	4.9
Total	7.0093	1.8
Inclusive	4.6363	1.4
Total Violent Crime		
Specification:	**F-stat.**	**p-value**
Beer	4.1307	2.7
Liquor	6.4022	5.2
Wine	6.5563	4.0
Total	4.6885	1.0
Inclusive	2.8587	9.7

Table 4.7 is found on page 124

Table 5.1
Per-Capita Murder Regression:
Semi-Reduced-Form 2WFE-OLS Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LARRMUR	-0.0908*** (3.32)	-0.1219*** (3.68)	-0.0744** (2.55)	-0.1236*** (3.57)	-0.1185*** (3.33)
LPOLICE	-0.5635*** (3.14)	-0.3128 (1.19)	-0.4531** (2.44)	-0.6696** (2.47)	-0.5014* (1.82)
LPRISON	0.4638*** (6.47)	0.4676*** (5.67)	0.4639*** (6.34)	0.4180*** (4.70)	0.3749*** (3.99)
$P_{c\|a}$	-0.8673** (2.23)	-0.6966 (1.56)	-0.7846* (1.88)	-0.7024 (1.51)	-0.7148 (1.53)
$P_{e\|c}$	-0.1050** (1.98)	-0.1071* (1.87)	-0.1035* (1.86)	-0.1055* (1.73)	-0.0796 (1.32)
LNRA	0.2990*** (2.71)	0.2392* (1.65)	0.2760** (2.33)	0.2282 (1.41)	0.0943 (0.58)
LM1844	0.0267 (0.05)	-0.1653 (0.24)	0.2946 (0.47)	-0.5639 (0.73)	-0.3765 (0.48)
LNONMET	0.1184** (2.20)	0.1997*** (3.47)	0.1708*** (3.26)	0.1044 (1.58)	0.1940*** (2.72)
LPOPDEN	-0.4441 (0.88)	-0.4260 (0.49)	-0.3203 (0.58)	-0.3194 (0.32)	-0.1770 (0.18)
LPERBLK	0.0093 (0.61)	-0.0110 (0.56)	0.0139 (0.84)	-0.0101 (0.50)	-0.0109 (0.53)
LUR	0.1205*** (3.50)	0.1461*** (3.57)	0.1098*** (2.94)	0.1515*** (3.70)	0.1665*** (3.95)
LINCOME	-0.0634 (0.22)	-0.2606 (0.76)	-0.2180 (0.71)	0.1000 (0.29)	-0.0287 (0.08)
LGINI	-0.2040 (0.70)	0.7874** (1.99)	-0.3971 (1.27)	0.6080 (1.52)	1.0141** (2.47)
LWAGELOW	0.2266 (0.67)	-0.1384 (0.40)	-0.1188 (0.35)	-0.0448 (0.12)	0.0428 (0.12)
LROBBERY	0.5277*** (10.43)	0.5667*** (8.42)	0.5542*** (10.63)	0.6173*** (9.00)	0.6239*** (8.48)
LBURGLARY	0.2733*** (3.56)	0.3229*** (3.35)	0.2948*** (3.68)	0.2539** (2.52)	0.2846*** (2.64)
LEGAL	-0.1772* (1.82)	-0.1770 (1.23)	-0.1779* (1.73)	-0.1817 (1.21)	-0.1873 (1.24)
LBEERTAX	0.0887 (0.78)				0.0840 (0.36)
LLIQTAX		-0.7392** (2.22)			-1.2776*** (2.76)
LWINETAX			-0.0154 (0.24)		0.1076 (1.12)

Table 5.1 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LTOTTAX				-0.5443*** (2.99)	
MANDATE	-0.0486 (0.89)			-0.1559** (2.06)	-0.2306*** (2.92)
CASHLAW	-0.0316 (0.59)			0.0168 (0.23)	0.0432 (0.58)
FORCE	-0.2022*** (3.33)			-0.1900** (1.98)	-0.2245** (2.34)
MINDIST	0.0002 (1.06)			0.0002 (0.62)	0.0004 (1.63)
DRYPER	-0.2962 (1.15)	-0.0728 (0.24)	-0.2255 (0.81)	-0.0079 (0.03)	-0.0573 (0.19)
LDRINK	-0.0605 (0.62)	-0.2435 (1.48)	-0.0437 (0.43)	-0.3144* (1.88)	-0.2721 (1.60)
LTOURPCT	-0.4477 (1.41)	-0.7047* (1.92)	-0.6218* (1.80)	-0.5755 (1.54)	-0.5526 (1.47)
NOSIGN	-0.0164 (0.26)	0.0586 (0.86)	0.0043 (0.06)	0.0002 (0.00)	0.0210 (0.28)
NOPRINT	-0.0929 (0.89)	-0.1983 (1.54)	-0.1489 (1.10)	-0.0771 (0.38)	-0.0549 (0.26)
LMRM	-0.1325 (0.26)	-0.5452 (0.68)	-0.0140 (0.03)	-0.4872 (0.56)	-0.3359 (0.38)
LSOBAP	-0.5844** (1.96)	-0.1346 (0.39)	-0.4671 (1.46)	-0.1576 (0.44)	-0.1958 (0.54)
LCATH	0.0330 (0.06)	-0.8045 (1.09)	-0.0458 (0.09)	-0.7013 (0.85)	-0.8395 (1.01)
LPROT	-0.7200 (1.39)	-0.4842 (0.71)	-0.8010 (1.48)	-0.6472 (0.85)	-0.1899 (0.25)
PBT	-0.0226 (0.48)	-0.0605 (1.10)	-0.0409 (0.83)	-0.0231 (0.40)	-0.0286 (0.49)
NOPLEA	0.0959 (1.54)	0.0618 (0.95)	0.1015 (1.64)	0.0626 (0.88)	0.0173 (0.24)
DRAM	-0.1310*** (2.77)	-0.1275** (2.18)	-0.1234** (2.47)	-0.1654*** (2.68)	-0.1188* (1.83)
JAIL	-0.0323 (0.89)	-0.0397 (0.98)	-0.0200 (0.51)	-0.0036 (0.08)	-0.0421 (0.94)
FINE	-0.0004 (1.56)	-0.0004 (1.04)	-0.0007** (2.08)	-0.0004 (0.73)	-0.0006 (1.07)
SUS	-0.0004 (0.80)	-0.0004 (0.59)	0.0001 (0.20)	-0.0006 (0.87)	-0.0013* (1.88)
ILDUM	0.1846 (1.50)	-0.1177 (0.55)	0.2034 (1.58)	-0.0942 (0.42)	-0.0443 (0.20)
CONSTANT	-6.0857 (1.55)	1.0413 (0.23)	-2.5128 (0.72)	-7.8855* (1.66)	-1.8688 (0.38)

Table 5.1 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	300	450	290	290
Adj. R^2	.8924	.9024	.8850	.9039	.9035
F-stat.	44.16	40.49	41.66	38.25	37.07

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.2
Per-Capita Rape Regression:
Subsample OLS and 2SLS Estimates
(1985-1994)

	BEER		LIQUOR		WINE	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRRAPE	-0.0707*** (3.57)	-0.0409 (1.55)	-0.0223 (1.00)	0.0076 (0.24)	-0.0697*** (3.48)	-0.0260 (0.95)
LPOLICE	0.1598 (0.89)	1.2695*** (3.29)	-0.2379 (1.04)	-0.0259 (0.06)	0.2778 (1.52)	1.4715*** (3.96)
LPRISON	-0.1880*** (2.59)	0.0210 (0.17)	-0.3916*** (4.59)	0.1443 (0.96)	-0.2017*** (2.85)	-0.0589 (0.49)
LNRA	0.4005*** (2.97)	0.2002 (1.31)	0.0467 (0.32)	-0.1460 (0.81)	0.2494* (1.82)	0.1190 (0.73)
LM1844	-1.7835** (2.00)	-3.8754*** (4.03)	-1.4925 (1.43)	-2.6542** (2.03)	-1.6662* (1.83)	-3.0397*** (2.98)
LNONMET	0.1340** (2.14)	0.1627** (2.36)	-0.6804 (1.10)	-0.0099 (0.14)	0.1068* (1.73)	0.1855*** (2.64)
LPOPDEN	-0.7223*** (3.38)	-0.7946*** (3.07)	-1.5094*** (6.57)	-1.1740*** (4.06)	-0.8203*** (3.87)	-0.9124*** (3.99)
LPERBLK	-0.0011 (0.16)	-0.0036 (0.48)	0.0058 (0.67)	0.0019 (0.18)	-0.0074 (1.05)	-0.0147* (1.85)
LUR	-0.0281 (0.83)	0.0341 (1.31)	-0.0141 (0.37)	-0.0209 (0.68)	-0.0618** (2.02)	0.0423* (1.71)
LINCOME	0.1628 (1.06)	0.5090*** (3.23)	0.5317*** (3.04)	0.8852*** (4.56)	0.1625 (1.06)	0.3454** (2.14)
LGINI	0.0018 (0.01)	0.0780 (0.38)	-0.0358 (0.16)	0.0200 (0.08)	0.1255 (0.66)	-0.0823 (0.37)
LWAGELOW	-0.1959 (0.97)	-0.3686 (1.61)	-0.1301 (0.68)	-0.3842 (1.61)	-0.1372 (0.69)	-0.3263 (1.44)
LBEER	0.5242** (2.43)	-0.0280 (0.06)				
LLIQ			0.6290*** (4.24)	0.0264 (0.09)		
LWINE					0.0958 (1.16)	-0.4215*** (2.65)
CONSTANT	-5.3050** (2.11)	-8.7562*** (2.94)	-7.0481** (2.50)	-13.5385*** (3.35)	-5.9378** (2.46)	-8.6054*** (2.98)
N	480	480	300	300	450	450
Adj. R^2	.9052	.8954	.9102	.8857	.9116	.9064
F-stat.	66.31	62.16	59.25	48.29	70.10	68.92

Table 5.2 (extended)

	TOTAL		INCLUSIVE	
	OLS	2SLS	OLS	2SLS
LARRRAPE	-0.0125 (0.56)	0.0018 (0.06)	-0.0145 (0.66)	0.0005 (0.02)
LPOLICE	-0.3870* (1.65)	0.3354 (0.92)	-0.3015 (1.30)	0.4638 (1.21)
LPRISON	-0.3568*** (4.19)	0.0044 (0.03)	-0.3434*** (4.09)	-0.1131 (0.86)
LNRA	-0.0135 (0.09)	-0.2185 (1.27)	-0.0228 (0.15)	0.0268 (0.15)
LM1844	-0.3567 (0.34)	-3.1240** (2.50)	-0.4482 (0.43)	-3.3995*** (2.79)
LNONMET	-0.0384 (0.63)	-0.0272 (0.39)	-0.0574 (0.95)	-0.0700 (1.02)
LPOPDEN	-1.2465*** (5.21)	-1.1452*** (3.71)	-1.3505*** (5.76)	-1.2411*** (4.16)
LPERBLK	0.0036 (0.41)	0.0014 (0.14)	0.0047 (0.53)	-0.0011 (0.11)
LUR	-0.0211 (0.52)	-0.0178 (0.58)	0.0061 (0.15)	-0.0003 (0.01)
LINCOME	0.3912** (2.18)	0.8677*** (4.44)	0.4046** (2.30)	0.7854*** (4.07)
LGINI	0.0167 (0.07)	0.1449 (0.57)	-0.0049 (0.02)	-0.0412 (0.16)
LWAGELOW	-0.1261 (0.67)	-0.2676 (1.18)	-0.0826 (0.44)	-0.0281 (0.13)
LBEER			0.7077*** (2.94)	0.5696 (1.49)
LLIQ			0.5213*** (3.43)	0.5302** (2.19)
LWINE			-0.1126 (1.00)	-0.7590*** (3.80)
LTOTALC	1.0395*** (4.08)	0.0633 (0.14)		
CONSTANT	-6.8490** (2.47)	-13.3501*** (3.86)	-4.7067 (1.63)	-12.0749*** (3.24)
N	290	290	290	290
Adj. R^2	.9144	.8896	.9167	.8982
F-stat.	61.53	49.52	60.96	52.01

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.3
Per-Capita Robbery Regression:
Subsample OLS and 2SLS Estimates
(1985-1994)

	BEER		LIQUOR		WINE	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRROB	-0.0036 (0.24)	-0.0140 (0.59)	-0.0019 (0.12)	-0.0191 (0.75)	-0.0055 (0.34)	0.0137 (0.50)
LPOLICE	0.5706*** (2.85)	2.0594*** (4.54)	-0.2229 (0.95)	1.2770*** (2.79)	0.6674*** (3.11)	3.2132*** (6.75)
LPRISON	-0.3816*** (4.72)	0.1938 (1.37)	-0.4635*** (5.34)	0.0570 (0.36)	-0.4383*** (5.28)	-0.1256 (0.82)
LNRA	0.0259 (0.17)	-0.2270 (1.27)	0.2507* (1.67)	-0.0010 (0.00)	-0.0491 (0.31)	-0.3914* (1.89)
LM1844	2.9510*** (2.98)	0.1044 (0.09)	0.4086 (0.38)	-3.1858** (2.31)	2.9842*** (2.80)	-0.4032 (0.31)
LNONMET	-0.0102 (0.15)	0.0201 (0.25)	0.0558 (0.89)	0.0788 (1.01)	-0.0457 (0.63)	0.0398 (0.44)
LPOPDEN	-0.0850 (0.36)	0.3800 (1.25)	-0.2406 (1.03)	0.3503 (1.18)	-0.1498 (0.60)	-0.1034 (0.35)
LPERBLK	-0.0370*** (4.86)	-0.0368*** (4.19)	-0.0243*** (2.75)	-0.0220** (2.02)	-0.0393*** (4.77)	-0.0436*** (4.32)
LUR	-0.0087 (0.23)	0.0712** (2.35)	0.0207 (0.52)	0.0529* (1.66)	-0.0924** (2.57)	0.0427 (1.35)
LINCOME	-0.2690 (1.57)	0.0324 (0.18)	-0.1650 (0.92)	0.2862 (1.40)	-0.2036 (1.14)	0.0641 (0.31)
LGINI	-0.5520*** (2.62)	-0.3229 (1.34)	-0.4033* (1.75)	-0.2041 (0.73)	-0.4847** (2.16)	-0.5115* (1.77)
LWAGELOW	0.1582 (0.70)	0.0310 (0.12)	0.1818 (0.93)	0.1820 (0.73)	0.1445 (0.62)	-0.1009 (0.35)
LBEER	0.9236*** (3.85)	1.6911*** (3.06)				
LLIQ			0.5528*** (3.63)	0.2783 (0.95)		
LWINE					0.1866* (1.93)	0.0121 (0.06)
CONSTANT	3.0536 (1.10)	6.9194** (1.99)	-4.0027 (1.40)	-7.2820* (1.76)	0.9378 (0.33)	5.1412 (1.39)
N	480	480	300	300	450	450
Adj. R^2	.9819	.9809	.9850	.9801	.9814	.9810
F-stat.	372.77	368.34	377.93	301.00	353.99	364.01

Table 5.3 (extended)

	TOTAL		INCLUSIVE	
	OLS	2SLS	OLS	2SLS
LARRROB	0.0030 (0.19)	-0.0071 (0.29)	-0.0026 (0.16)	-0.0144 (0.54)
LPOLICE	-0.3539 (1.42)	0.9003** (2.26)	-0.2809 (1.15)	1.0447** (2.36)
LPRISON	-0.4834*** (5.36)	-0.0466 (0.33)	-0.4539*** (5.13)	-0.0205 (0.13)
LNRA	0.2734* (1.74)	0.0087 (0.05)	0.2256 (1.44)	0.2124 (1.01)
LM1844	0.5411 (0.48)	-2.7847** (2.06)	0.5331 (0.48)	-2.1720 (1.53)
LNONMET	0.0743 (1.15)	0.1012 (1.35)	0.0468 (0.73)	0.0563 (0.70)
LPOPDEN	-0.3002 (1.19)	0.0242 (0.07)	-0.2996 (1.22)	-0.0695 (0.20)
LPERBLK	-0.0294*** (3.15)	-0.0292*** (2.67)	-0.0259*** (2.80)	-0.0318*** (2.70)
LUR	-0.0369 (0.85)	0.0137 (0.41)	0.0001E-01 (0.00)	0.0229 (0.67)
LINCOME	-0.1425 (0.74)	0.3266 (1.55)	-0.1519 (0.81)	0.2329 (1.03)
LGINI	-0.4229* (1.77)	-0.2620 (0.94)	-0.3726 (1.56)	-0.5340* (1.72)
LWAGELOW	0.1011 (0.50)	0.0334 (0.14)	0.1777 (0.90)	0.2377 (0.93)
LBEER			-0.1497 (0.59)	-0.2668 (0.60)
LLIQ			0.5513*** (3.43)	0.8983*** (3.18)
LWINE			-0.0222 (0.19)	-1.0440*** (4.48)
LTOTALC	0.1332 (0.49)	-0.4778 (0.99)		
CONSTANT	-7.2792** (2.46)	-11.2367*** (3.03)	-4.6708 (1.53)	-8.3729* (1.95)
N	290	290	290	290
Adj. R^2	.9845	.9803	.9851	.9824
F-stat.	361.08	301.11	361.78	323.59

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.4
Per-Capita Aggravated Assault Regression:
Subsample OLS and 2SLS Estimates
(1985-1994)

	BEER		LIQUOR		WINE	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRASS	-0.0079 (0.42)	0.0193 (0.66)	0.0106 (0.55)	0.0311 (1.01)	-0.0115 (0.56)	0.0563* (1.82)
LPOLICE	0.1283 (0.72)	-0.4602 (1.09)	0.1455 (0.66)	0.8799** (2.04)	0.2577 (1.34)	0.9743** (2.44)
LPRISON	0.0150 (0.21)	0.8437*** (6.41)	-0.0631 (0.78)	0.5050*** (3.42)	-0.0332 (0.45)	0.2153* (1.67)
LNRA	-0.1430 (1.08)	-0.3410** (2.04)	0.1930 (1.38)	-0.1326 (0.77)	-0.2073 (1.44)	-0.4482*** (2.58)
LM1844	3.7702*** (4.30)	3.4010*** (3.25)	0.1547 (0.16)	-0.9695 (0.77)	3.8936*** (4.08)	3.7721*** (3.45)
LNONMET	-0.3502*** (5.66)	-0.3260*** (4.30)	-0.1593*** (2.71)	-0.0938 (1.32)	-0.3661*** (5.61)	-0.2723*** (3.61)
LPOPDEN	-0.3524* (1.68)	0.3344 (1.18)	-0.5503** (2.51)	-0.1522 (0.54)	-0.4061* (1.82)	-0.6391*** (2.59)
LPERBLK	-0.0177*** (2.60)	-0.0162* (1.95)	-0.0165** (1.99)	-0.0143 (1.41)	-0.0214*** (2.88)	-0.0296*** (3.43)
LUR	-0.1654*** (4.98)	-0.0276 (0.97)	-0.1290*** (3.46)	-0.0285 (0.95)	-0.2492*** (7.70)	-0.0894*** (3.35)
LINCOME	-0.0635 (0.42)	0.1866 (1.09)	0.0194 (0.12)	0.2282 (1.22)	0.0596 (0.37)	0.2861* (1.65)
LGINI	0.3635* (1.95)	0.5799*** (2.58)	0.0177 (0.08)	0.0583 (0.23)	0.3818* (1.90)	0.1191 (0.49)
LWAGELOW	0.1052 (0.53)	0.0121 (0.05)	0.0417 (0.23)	-0.0922 (0.40)	0.0527 (0.25)	-0.2339 (0.96)
LBEER	1.0268*** (4.82)	2.9231*** (5.82)				
LLIQ			0.1308 (0.92)	0.1521 (0.55)		
LWINE					0.0104 (0.12)	-0.5077*** (2.92)
CONSTANT	6.0611** (2.45)	8.8022*** (2.72)	-0.6294 (0.23)	-0.1814 (0.05)	2.3271 (0.92)	0.6277 (0.20)
N	480	480	300	300	450	450
Adj. R^2	.9657	.9638	.9719	.9681	.9640	.9609
F-stat.	193.49	191.43	199.56	185.97	180.28	173.28

Table 5.4 (extended)

	TOTAL		INCLUSIVE	
	OLS	2SLS	OLS	2SLS
LARRASS	0.0145 (0.76)	0.0237 (0.85)	0.0159 (0.81)	0.0407 (1.31)
LPOLICE	-0.0198 (0.09)	0.7463** (2.08)	0.0005 (0.00)	0.6210 (1.50)
LPRISON	-0.0077 (0.10)	0.5850*** (4.65)	-0.0237 (0.29)	0.4045*** (2.90)
LNRA	0.1761 (1.25)	-0.1305 (0.77)	0.1705 (1.18)	0.1711 (0.90)
LM1844	0.9942 (0.98)	-0.2840 (0.23)	0.8924 (0.88)	-0.1943 (0.15)
LNONMET	-0.1493** (2.57)	-0.1053 (1.55)	-0.1497** (2.54)	-0.1266* (1.76)
LPOPDEN	-0.3371 (1.48)	0.2019 (0.66)	-0.3901* (1.71)	-0.1096 (0.35)
LPERBLK	-0.0144* (1.72)	-0.0122 (1.23)	-0.0150* (1.76)	-0.0188* (1.76)
LUR	-0.0861** (2.21)	-0.0110 (0.36)	-0.0992** (2.46)	-0.0090 (0.29)
LINCOME	-0.1260 (0.73)	0.1345 (0.71)	-0.0986 (0.57)	0.0239 (0.12)
LGINI	0.0301 (0.14)	0.1489 (0.59)	0.0378 (0.17)	-0.1587 (0.57)
LWAGELOW	0.1124 (0.62)	0.0241 (0.11)	0.0861 (0.47)	0.1787 (0.78)
LBEER			0.7468*** (3.17)	1.5118*** (3.66)
LLIQ			-0.0232 (0.16)	0.3254 (1.23)
LWINE			0.0863 (0.79)	-0.8107*** (3.87)
LTOTALC	0.8688*** (3.57)	0.9812** (2.28)		
CONSTANT	1.9473 (0.74)	1.6319 (0.48)	1.9800 (0.70)	1.7202 (0.44)
N	290	290	290	290
Adj. R^2	.9734	.9693	.9731	.9710
F-stat.	208.35	191.26	197.89	194.62

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.5
Per-Capita Total Violent Crime Regression:
Subsample OLS and 2SLS Estimates
(1985-1994)

	BEER		LIQUOR		WINE	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRTVC	0.0167 (1.16)	0.0158 (0.74)	0.0168 (1.15)	0.0210 (0.85)	0.0133 (0.85)	0.0433* (1.78)
LPOLICE	0.2295* (1.76)	0.5653* (1.79)	-0.1729 (1.08)	0.8076** (2.27)	0.3254** (2.30)	1.6383*** (5.08)
LPRISON	-0.0913* (1.74)	0.5714*** (5.80)	-0.2303*** (3.87)	0.3393*** (2.75)	-0.1314** (2.40)	0.1486 (1.43)
LNRA	-0.1217 (1.25)	-0.3560*** (2.85)	0.1402 (1.37)	-0.1522 (1.05)	-0.2066* (1.95)	-0.4641*** (3.30)
LM1844	2.9258*** (4.54)	1.7253** (2.20)	0.2990 (0.41)	-1.7110 (1.62)	3.1017*** (4.43)	2.0411** (2.31)
LNONMET	-0.1898*** (4.18)	-0.1516*** (2.69)	-0.0808* (1.88)	-0.0192 (0.32)	-0.2103*** (4.40)	-0.1099* (1.80)
LPOPDEN	-0.3588** (2.33)	0.0969 (0.46)	-0.6822*** (4.26)	-0.2107 (0.90)	-0.4115** (2.51)	-0.5324*** (2.67)
LPERBLK	-0.0188*** (3.79)	-0.0180*** (2.91)	-0.0135** (2.24)	-0.0121 (1.45)	-0.0219*** (4.01)	-0.0284*** (4.10)
LUR	-0.1075*** (4.42)	0.0042 (0.19)	-0.0728*** (2.68)	-0.0054 (0.22)	-0.1807*** (7.63)	-0.0319 (1.49)
LINCOME	-0.0666 (0.60)	0.2255* (1.76)	0.0528 (0.43)	0.3738** (2.41)	0.0150 (0.13)	0.2439* (1.74)
LGINI	0.0682 (0.50)	0.2428 (1.45)	-0.1123 (0.72)	-0.0294 (0.14)	0.1203 (0.81)	-0.0876 (0.45)
LWAGELOW	0.0463 (0.32)	-0.1106 (0.59)	0.0054 (0.04)	-0.1228 (0.64)	0.0163 (0.11)	-0.2689 (1.36)
LBEER	0.8670*** (5.55)	1.8526*** (4.91)				
LLIQ			0.2925*** (2.81)	0.0736 (0.32)		
LWINE					0.0767 (1.20)	-0.3802*** (2.72)
CONSTANT	4.3462** (2.40)	6.4744*** (2.68)	-1.9797 (1.01)	-3.8938 (1.21)	1.7581 (0.94)	2.0088 (0.80)
N	480	480	300	300	450	450
Adj. R^2	.9820	.9793	.9851	.9785	.9811	.9790
F-stat.	374.48	339.03	380.67	278.42	348.48	328.08

Table 5.5 (extended)

	TOTAL		INCLUSIVE	
	OLS	2SLS	OLS	2SLS
LARRTVC	0.0235 (1.64)	0.0161 (0.71)	0.0219 (1.50)	0.0260 (1.03)
LPOLICE	-0.3416** (2.09)	0.7338** (2.45)	-0.3000* (1.83)	0.5931* (1.76)
LPRISON	-0.1966*** (3.33)	0.3423*** (3.24)	-0.1962*** (3.32)	0.2321** (2.00)
LNRA	0.1256 (1.23)	-0.1766 (1.25)	0.1191 (1.14)	0.1191 (0.75)
LM1844	1.0033 (1.36)	-1.3429 (1.30)	0.9379 (1.28)	-1.0098 (0.95)
LNONMET	-0.0677 (1.61)	-0.0262 (0.46)	-0.0749* (1.76)	-0.0578 (0.96)
LPOPDEN	-0.5436*** (3.28)	-0.1308 (0.51)	-0.5973*** (3.61)	-0.3367 (1.26)
LPERBLK	-0.0137** (2.26)	-0.0130 (1.57)	-0.0135** (2.18)	-0.0183** (2.05)
LUR	-0.0624** (2.21)	-0.0086 (0.34)	-0.0563* (1.93)	-0.0030 (0.11)
LINCOME	-0.0533 (0.43)	0.3515** (2.21)	-0.0376 (0.30)	0.2351 (1.39)
LGINI	-0.1008 (0.65)	0.0306 (0.15)	-0.1032 (0.65)	-0.2627 (1.12)
LWAGELOW	0.0311 (0.24)	-0.0871 (0.47)	0.0383 (0.29)	0.1001 (0.52)
LBEER			0.5266*** (3.09)	0.8356** (2.37)
LLIQ			0.1888* (1.75)	0.4726** (2.16)
LWINE			-0.0076 (0.10)	-0.9218*** (5.28)
LTOTALC	0.6929*** (3.92)	0.2482 (0.69)		
CONSTANT	-1.1583 (0.60)	-3.7644 (1.34)	-0.3109 (0.15)	-3.1907 (0.98)
N	290	290	290	290
Adj. R^2	.9860	.9790	.9859	.9815
F-stat.	400.21	281.39	382.86	307.55

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.6 is found on page 142

Table 5.7
Per-Capita Rape Regressions:
Fully Specified Reduced-Form Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LNRA	0.0847 (0.68)	-0.0644 (0.43)	-0.0131 (0.10)	-0.0764 (0.50)	0.1304 (0.84)
LM1844	0.7157 (0.71)	-0.6291 (0.47)	0.8588 (0.80)	0.4140 (0.29)	0.6315 (0.47)
LNONMET	0.1741*** (2.83)	0.0609 (0.78)	0.1632** (2.56)	0.0435 (0.56)	0.1313* (1.80)
LPOPDEN	0.0931 (0.39)	-0.7068** (2.06)	0.0733 (0.30)	-0.0067 (0.02)	0.6280 (1.46)
LPERBLK	-0.0017 (0.26)	-0.0089 (0.98)	-0.0082 (1.21)	-0.0031 (0.34)	-0.0025 (0.29)
LUR	0.0135 (0.67)	-0.0226 (0.85)	-0.0056 (0.26)	-0.0208 (0.81)	-0.0191 (0.76)
LINCOME	-0.0671 (0.48)	0.2235 (1.20)	-0.0633 (0.44)	0.2485 (1.30)	0.1463 (0.81)
LGINI	0.0431 (0.25)	-0.0201 (0.09)	0.1099 (0.62)	0.0729 (0.33)	0.0691 (0.33)
LWAGELOW	-0.2459 (1.34)	-0.4933*** (2.60)	-0.5275*** (2.87)	-0.3164* (1.66)	-0.1410 (0.78)
LEGAL	0.0570 (1.26)	0.0302 (0.45)	0.0296 (0.62)	0.1104 (1.62)	0.0681 (1.07)
LBEERTAX	-0.6139*** (7.59)				-0.6301*** (4.36)
LLIQTAX		0.1349 (0.52)			1.2790*** (3.34)
LWINETAX			-0.1283*** (3.96)		0.0377 (0.77)
LTOTTAX				0.3311* (1.78)	
MANDATE	-0.1066 (1.21)			-0.1311 (1.07)	-0.1069 (0.94)
CASHLAW	-0.0168 (0.78)			-0.0694** (2.12)	-0.0535* (1.76)
FORCE	0.0011 (0.01)			0.0313 (0.32)	0.1952* (1.95)
MINDIST	-0.0004** (2.51)			-0.0005*** (3.05)	-0.0004** (2.49)
DRYPER	0.3178*** (2.70)	0.1582 (1.11)	0.3042** (2.45)	0.1688 (1.20)	0.2830** (2.16)
LDRINK	0.0523 (1.44)	0.0381 (0.76)	0.0455 (1.21)	0.0416 (0.83)	0.0360 (0.78)
LTOURPCT	-0.4351*** (3.18)	-0.0930 (0.55)	-0.2998** (2.02)	-0.1268 (0.77)	-0.0472 (0.30)

Table 5.7 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
NOSIGN	0.0557* (1.80)	-0.0070 (0.21)	0.0140 (0.44)	0.0366 (1.07)	0.0144 (0.44)
NOPRINT	-0.0486 (0.89)	0.0055 (0.08)	-0.1965*** (3.15)	0.0509 (0.39)	0.0179 (0.15)
LMRM	1.0225*** (6.19)	1.0410*** (4.17)	0.9278*** (5.59)	1.2626*** (4.50)	2.0457*** (6.69)
LSOBAP	0.3132** (2.35)	-0.1395 (0.70)	0.4603*** (3.20)	0.0035 (0.02)	-0.0243 (0.13)
LCATH	-0.7686*** (3.75)	-0.4738 (1.62)	-0.4981** (2.39)	-0.3386 (1.03)	-0.7402** (2.39)
LPROT	-0.5953*** (3.19)	-1.1904*** (4.66)	-0.4931*** (2.59)	-0.6841** (2.47)	-0.8001*** (3.15)
PBT	-0.0228 (0.56)	-0.0481 (1.17)	-0.0475 (1.15)	-0.0564 (1.37)	-0.0143 (0.36)
NOPLEA	0.1407*** (2.66)	0.0036 (0.06)	0.0687 (1.30)	0.0106 (0.17)	0.1341** (2.25)
DRAM	0.0447 (1.40)	0.0960* (1.91)	0.0268 (0.78)	0.0705 (1.33)	0.0275 (0.55)
JAIL	0.0129 (0.16)	0.0069 (0.09)	0.0876 (1.06)	-0.0109 (0.14)	-0.0314 (0.41)
FINE	-0.0004*** (3.91)	-0.0003* (1.75)	-0.0002* (1.68)	-0.0003 (1.58)	-0.0007E-01 (0.39)
SUS	0.0007*** (3.16)	0.0004 (1.52)	0.0007*** (2.96)	0.0003 (1.44)	0.0004* (1.72)
ILDUM	0.0462 (0.80)	0.2074** (2.01)	0.0280 (0.48)	0.1484 (1.35)	0.0537 (0.55)
LPOP45P	2.1144*** (5.64)	1.7745*** (3.11)	1.5622*** (3.72)	1.6436*** (3.14)	2.4531*** (4.78)
LATPC	0.0297 (1.54)	0.0357 (1.50)	0.0231 (1.13)	0.0259 (1.07)	0.0288 (1.30)
LTAX	0.1715* (1.76)	-0.2038 (1.35)	0.1294 (1.27)	-0.0485*** (2.67)	-0.0528* (1.90)
PREDEC	-0.1609 (1.38)		-0.2047* (1.71)		
FINAL	0.0043 (0.08)	0.0843 (1.22)	0.0330 (0.57)	0.0636 (0.94)	0.0607 (0.96)
FURTHER	-0.0573 (1.13)	-0.0018 (0.03)	-0.0288 (0.56)	-0.0203 (0.39)	-0.0602 (1.22)
FINAL23		0.1224* (1.95)		0.1499** (2.46)	0.1283** (2.25)
FUR23		0.0594 (1.34)		0.0409 (0.90)	0.0279 (0.66)
CONSTANT	-1.6974 (0.72)	-3.1584 (1.05)	-0.4481 (0.18)	-0.0032 (0.00)	-1.4206 (0.52)

Table 5.7 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	300	450	290	290
Adj. R^2	.9304	.9199	.9291	.9277	.9378
F-stat.	71.39	50.06	71.05	51.76	59.08

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.8
Per-Capita Robbery Regressions:
Fully Specified Reduced-Form Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LNRA	0.1801 (1.19)	0.3357** (2.18)	0.1012 (0.65)	0.3284** (2.12)	0.3292* (1.92)
LM1844	0.9125 (0.75)	2.1011 (1.53)	2.6170** (2.03)	2.2022 (1.50)	2.0774 (1.40)
LNONMET	0.0412 (0.55)	-0.0626 (0.79)	0.0153 (0.20)	-0.0787 (1.00)	-0.0654 (0.81)
LPOPDEN	0.1850 (0.64)	-0.1353 (0.39)	0.4313 (1.48)	-0.3063 (0.67)	-0.4805 (1.02)
LPERBLK	-0.0194** (2.50)	-0.0103 (1.11)	-0.0151* (1.86)	-0.0126 (1.37)	-0.0129 (1.39)
LUR	0.0084 (0.35)	0.0009 (0.03)	-0.0125 (0.49)	-0.0011 (0.04)	0.0093 (0.34)
LINCOME	0.2660 (1.56)	0.2439 (1.28)	0.2640 (1.53)	0.1305 (0.67)	0.0521 (0.26)
LGINI	-0.4915** (2.37)	-0.1309 (0.57)	-0.3207 (1.51)	-0.1607 (0.70)	-0.1801 (0.79)
LWAGELOW	0.2614 (1.18)	0.2726 (1.41)	0.1511 (0.69)	0.4222** (2.17)	0.4612** (2.33)
LEGAL	-0.0649 (1.18)	-0.2114*** (3.08)	-0.0674 (1.19)	-0.1402** (2.02)	-0.1561** (2.23)
LBEERTAX	0.1010 (1.03)				-0.2419 (1.52)
LLIQTAX		-0.0635 (0.24)			0.0223 (0.05)
LWINETAX			0.0137 (0.35)		0.0186 (0.34)
LTOTTAX				0.1473 (0.77)	
MANDATE	0.0737 (0.69)			0.0928 (0.74)	0.0955 (0.76)
CASHLAW	-0.0320 (1.22)			-0.1190*** (3.57)	-0.1160*** (3.47)
FORCE	0.1979* (1.77)			0.3018*** (2.99)	0.3086*** (2.79)
MINDIST	-0.0003 (1.51)			-0.0003 (1.57)	-0.0002 (1.18)
DRYPER	0.2703* (1.89)	0.0716 (0.49)	0.1696 (1.14)	-0.0156 (0.11)	0.0103 (0.07)
LDRINK	0.0821* (1.86)	0.0989* (1.93)	0.0540 (1.20)	0.1227** (2.39)	0.1186** (2.33)
LTOURPCT	-1.0292*** (6.19)	-0.6777*** (3.92)	-0.9677*** (5.44)	-0.6145*** (3.64)	-0.6140*** (3.60)

Table 5.8 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
NOSIGN	-0.0914** (2.44)	-0.1558*** (4.49)	-0.1046*** (2.73)	-0.1187*** (3.39)	-0.1210*** (3.33)
NOPRINT	0.0472 (0.71)	0.1911*** (2.84)	0.0074 (0.10)	0.0048 (0.04)	-0.0028 (0.02)
LMRM	0.4108** (2.05)	0.8929*** (3.51)	0.3694* (1.86)	0.6096** (2.13)	0.6465* (1.92)
LSOBAP	-0.3931** (2.43)	-1.0733*** (5.25)	-0.3757** (2.18)	-0.9983*** (4.64)	-1.0762*** (5.23)
LCATH	-0.2033 (0.82)	-0.7681** (2.57)	0.0069 (0.03)	-1.0668*** (3.19)	-1.2683*** (3.72)
LPROT	0.7621*** (3.36)	-0.4456* (1.71)	0.7828*** (3.43)	0.1659 (0.59)	0.0055 (0.02)
PBT	0.1139** (2.31)	0.1486*** (3.54)	0.1153** (2.32)	0.1416*** (3.37)	0.1507*** (3.43)
NOPLEA	0.0183 (0.29)	0.1249** (2.01)	0.0187 (0.30)	0.0806 (1.27)	0.1204* (1.84)
DRAM	0.0628 (1.62)	0.1165** (2.27)	0.0779* (1.89)	0.1298** (2.40)	0.1222** (2.23)
JAIL	0.0573 (0.57)	0.1020 (1.26)	0.1231 (1.24)	0.0761 (0.92)	0.0619 (0.73)
FINE	0.0001 (1.10)	0.0004*** (2.69)	0.0003* (1.81)	0.0009*** (4.33)	0.0009*** (4.85)
SUS	0.0005** (2.00)	0.0007E-01 (0.31)	0.0005* (1.67)	0.0003 (1.12)	0.0003 (1.30)
ILDUM	-0.1659** (2.36)	-0.3066*** (2.92)	-0.1421** (2.01)	-0.4717*** (4.22)	-0.5086*** (4.76)
LPOP45P	0.6361 (1.40)	2.8602*** (4.92)	1.1759** (2.34)	2.3402*** (4.38)	2.4267*** (4.30)
LATPC	0.0062 (0.26)	0.0151 (0.62)	0.0035 (0.14)	0.0293 (1.18)	0.0339 (1.39)
LTAX	0.0699 (0.59)	-0.5983*** (3.90)	0.0477 (0.39)	0.0130 (0.70)	0.0345 (1.13)
PREDEC	-0.2991** (2.11)		-0.3038** (2.11)		
FINAL	0.0423 (0.62)	0.0010 (0.02)	0.0435 (0.63)	0.0154 (0.22)	0.0128 (0.18)
FURTHER	-0.0594 (0.97)	-0.1126** (2.07)	-0.0649 (1.04)	-0.0840 (1.56)	-0.0898* (1.66)
FINAL23		0.0107 (0.17)		0.0526 (0.84)	0.0428 (0.68)
FUR23		-0.0868* (1.93)		-0.0567 (1.22)	-0.0560 (1.21)
CONSTANT	-6.6780** (2.33)	-7.1373** (2.32)	-4.1457 (1.41)	-3.3678 (1.14)	-3.5656 (1.18)

Table 5.8 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	300	450	290	290
Adj. R^2	.9842	.9867	.9844	.9880	.9880
F-stat.	328.75	318.88	337.14	326.23	317.74

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.9
Per-Capita Aggravated Assault Regressions:
Fully Specified Reduced-Form Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LNRA	-0.0629 (0.47)	0.1482 (1.07)	-0.1387 (1.00)	0.2493* (1.70)	0.2152 (1.33)
LM1844	4.4782*** (4.11)	2.9883** (2.41)	5.9140*** (5.14)	2.7586** (1.99)	2.8078** (2.00)
LNONMET	-0.3528*** (5.29)	-0.0858 (1.20)	-0.3194*** (4.67)	-0.1053 (1.41)	-0.0700 (0.92)
LPOPDEN	-0.5742** (2.22)	0.3349 (1.06)	-0.2833 (1.09)	-0.1211 (0.28)	0.1714 (0.38)
LPERBLK	-0.0095 (1.37)	-0.0076 (0.91)	-0.0057 (0.79)	-0.0074 (0.84)	-0.0067 (0.77)
LUR	-0.1228*** (5.65)	-0.0727*** (2.97)	-0.1365*** (5.99)	-0.0747*** (2.99)	-0.0724*** (2.79)
LINCOME	0.2593* (1.70)	-0.0054 (0.03)	0.2751* (1.78)	-0.0007 (0.00)	-0.0451 (0.24)
LGINI	0.1968 (1.06)	-0.1991 (0.95)	0.2506 (1.32)	-0.1667 (0.77)	-0.1629 (0.75)
LWAGELOW	0.0345 (0.17)	0.0174 (0.10)	0.1065 (0.54)	0.0811 (0.44)	0.1114 (0.59)
LEGAL	0.1407*** (2.86)	0.1553** (2.50)	0.1708*** (3.37)	0.1670** (2.53)	0.1532** (2.31)
LBEERTAX	0.2362*** (2.69)				-0.2322 (1.54)
LLIQTAX		-0.2092 (0.88)			-0.2676 (0.67)
LWINETAX			-0.0312 (0.90)		-0.0321 (0.63)
LTOTTAX				-0.2582 (1.43)	
MANDATE	-0.0613 (0.64)			-0.0085 (0.07)	-0.0290 (0.25)
CASHLAW	-0.0110 (0.47)			-0.0388 (1.23)	-0.0265 (0.84)
FORCE	0.0542 (0.54)			0.1732* (1.81)	0.1759* (1.68)
MINDIST	0.0003 (1.55)			0.0002 (1.01)	0.0002 (0.94)
DRYPER	0.0253 (0.20)	0.0975 (0.74)	0.0442 (0.33)	0.0662 (0.49)	0.0803 (0.59)
LDRINK	-0.0019 (0.05)	0.0677 (1.46)	0.0039 (0.10)	0.0499 (1.03)	0.0651 (1.35)
LTOURPCT	-0.1872 (1.26)	-0.3717** (2.38)	-0.3107* (1.96)	-0.3924** (2.45)	-0.3852** (2.38)

Table 5.9 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
NOSIGN	0.0394 (1.18)	0.0397 (1.26)	0.0535 (1.56)	0.0297 (0.89)	0.0249 (0.72)
NOPRINT	-0.0183 (0.31)	-0.0650 (1.07)	0.0345 (0.52)	-0.0450 (0.36)	-0.0507 (0.41)
LMRM	0.0959 (0.54)	0.0744 (0.32)	0.0551 (0.31)	-0.0323 (0.12)	0.1837 (0.58)
LSOBAP	-0.3141** (2.18)	0.4552** (2.46)	-0.1607 (1.04)	0.3242 (1.59)	0.4073** (2.09)
LCATH	0.6830*** (3.07)	0.2779 (1.03)	0.5971*** (2.67)	-0.0326 (0.10)	-0.1207 (0.37)
LPROT	0.1790 (0.88)	0.4261* (1.81)	0.2528 (1.24)	0.3193 (1.19)	0.3997 (1.51)
PBT	-0.1256*** (2.85)	-0.1201*** (3.16)	-0.0615 (1.39)	-0.1300*** (3.26)	-0.1005** (2.42)
NOPLEA	0.1175** (2.05)	0.1527*** (2.72)	0.1299** (2.30)	0.1716*** (2.84)	0.1801*** (2.90)
DRAM	0.0587* (1.69)	0.0953** (2.05)	0.0391 (1.06)	0.1065** (2.08)	0.0916* (1.77)
JAIL	0.0298 (0.33)	0.0322 (0.44)	0.0619 (0.70)	0.0279 (0.36)	0.0512 (0.64)
FINE	-0.0002* (1.70)	-0.0002 (1.33)	-0.0003** (2.02)	0.0009E-01 (0.47)	0.0004E-01 (0.22)
SUS	0.0001 (0.45)	0.0004* (1.77)	0.0007E-01 (0.29)	0.0004* (1.69)	0.0004* (1.79)
ILDUM	0.0095 (0.15)	0.0822 (0.87)	0.0160 (0.25)	-0.0054 (0.05)	0.0215 (0.21)
LPOP45P	2.0543*** (5.05)	2.1764*** (4.14)	2.9067*** (6.46)	2.3422*** (4.62)	2.3933*** (4.48)
LATPC	0.0749*** (3.58)	0.0435** (1.98)	0.0794*** (3.63)	0.0527** (2.25)	0.0481** (2.08)
LTAX	0.2007* (1.90)	0.1178 (0.85)	0.1744 (1.59)	0.0001 (0.01)	0.0286 (0.99)
PREDEC	-0.0218 (0.17)		-0.0380 (0.30)		
FINAL	-0.0478 (0.78)	-0.1033 (1.62)	-0.0738 (1.19)	-0.0936 (1.42)	-0.1108* (1.69)
FURTHER	-0.0544 (0.99)	-0.0750 (1.52)	-0.0741 (1.33)	-0.0644 (1.27)	-0.0827 (1.62)
FINAL23		0.0235 (0.41)		0.0223 (0.38)	0.0010 (0.02)
FUR23		-0.0580 (1.42)		-0.0384 (0.87)	-0.0565 (1.29)
CONSTANT	4.5220* (1.77)	5.0271* (1.81)	6.3520** (2.42)	3.7084 (1.32)	4.7123* (1.65)

Table 5.9 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	300	450	290	290
Adj. R^2	.9694	.9766	.9700	.9769	.9770
F-stat.	167.95	179.53	173.78	168.14	164.77

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.10
Per-Capita Total Violent Crime Regressions:
Fully Specified Reduced-Form Estimates
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LNRA	-0.0673 (0.66)	0.1192 (1.10)	-0.1533 (1.48)	0.1663 (1.49)	0.1755 (1.44)
LM1844	3.2460*** (3.94)	2.4254** (2.51)	4.5661*** (5.29)	2.4123** (2.29)	2.4335** (2.30)
LNONMET	-0.1613*** (3.19)	-0.0470 (0.84)	-0.1505*** (2.93)	-0.0649 (1.14)	-0.0316 (0.55)
LPOPDEN	-0.2203 (1.13)	0.0885 (0.36)	0.0081 (0.04)	-0.1702 (0.52)	0.0574 (0.17)
LPERBLK	-0.0094* (1.80)	-0.0071 (1.09)	-0.0069 (1.26)	-0.0072 (1.08)	-0.0067 (1.03)
LUR	-0.0660*** (4.01)	-0.0423** (2.21)	-0.0825*** (4.82)	-0.0432** (2.28)	-0.0402** (2.06)
LINCOME	0.2198* (1.91)	0.0773 (0.58)	0.2307** (1.99)	0.0534 (0.38)	0.0062 (0.04)
LGINI	-0.0317 (0.23)	-0.1614 (0.99)	0.0528 (0.37)	-0.1428 (0.87)	-0.1423 (0.88)
LWAGELOW	0.0018 (0.01)	-0.0217 (0.16)	-0.0178 (0.12)	0.0754 (0.54)	0.1214 (0.86)
LEGAL	0.0626* (1.68)	0.0303 (0.63)	0.0789** (2.07)	0.0677 (1.35)	0.0529 (1.06)
LBEERTAX	0.0948 (1.43)				-0.2474** (2.19)
LLIQTAX		-0.1222 (0.66)			0.0218 (0.07)
LWINETAX			-0.0299 (1.15)		-0.0064 (0.17)
LTOTTAX				-0.0871 (0.64)	
MANDATE	-0.0344 (0.47)			-0.0099 (0.11)	-0.0183 (0.21)
CASHLAW	-0.0184 (1.04)			-0.0597** (2.49)	-0.0500** (2.10)
FORCE	0.0811 (1.07)			0.1902*** (2.62)	0.2132*** (2.72)
MINDIST	0.0003E-01 (0.21)			-0.0005E-01 (0.37)	-0.0002E-01 (0.18)
DRYPER	0.1117 (1.15)	0.1068 (1.04)	0.0963 (0.96)	0.0677 (0.66)	0.0934 (0.91)
LDRINK	0.0323 (1.08)	0.0772** (2.13)	0.0277 (0.91)	0.0737** (2.00)	0.0808** (2.23)
LTOURPCT	-0.3662*** (3.25)	-0.3384*** (2.78)	-0.4013*** (3.37)	-0.3413*** (2.81)	-0.3257*** (2.68)

Table 5.10 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
NOSIGN	0.0078 (0.31)	-0.0210 (0.86)	0.0081 (0.31)	-0.0112 (0.44)	-0.0162 (0.63)
NOPRINT	-0.0068 (0.15)	0.0077 (0.16)	-0.0065 (0.13)	-0.0097 (0.10)	-0.0175 (0.19)
LMRM	0.4229*** (3.11)	0.5628*** (3.14)	0.3774*** (2.83)	0.4405** (2.14)	0.6725*** (2.81)
LSOBAP	-0.2224** (2.03)	-0.0734 (0.51)	-0.1070 (0.93)	-0.1168 (0.76)	-0.0840 (0.57)
LCATH	0.2469 (1.47)	-0.1649 (0.78)	0.2875* (1.71)	-0.4173* (1.74)	-0.5437** (2.24)
LPROT	0.2170 (1.41)	-0.0500 (0.27)	0.2724* (1.78)	0.1077 (0.53)	0.1148 (0.58)
PBT	-0.0506 (1.52)	-0.0391 (1.32)	-0.0151 (0.46)	-0.0482 (1.59)	-0.0260 (0.83)
NOPLEA	0.1056** (2.43)	0.1312*** (3.00)	0.1035** (2.44)	0.1305*** (2.85)	0.1566*** (3.36)
DRAM	0.0427 (1.63)	0.0985*** (2.72)	0.0307 (1.11)	0.1051*** (2.70)	0.0907** (2.33)
JAIL	0.0176 (0.26)	0.0347 (0.61)	0.0626 (0.94)	0.0191 (0.32)	0.0260 (0.43)
FINE	-0.0001 (1.20)	-0.0005E-01 (0.43)	-0.0009E-01 (0.85)	0.0002 (1.63)	0.0002* (1.78)
SUS	0.0003 (1.60)	0.0003* (1.65)	0.0002 (1.31)	0.0003** (1.97)	0.0004** (2.11)
ILDUM	-0.0354 (0.74)	0.0034 (0.05)	-0.0275 (0.58)	-0.1015 (1.26)	-0.1021 (1.34)
LPOP45P	1.6110*** (5.23)	2.3353*** (5.69)	2.1966*** (6.51)	2.2711*** (5.91)	2.4094*** (6.00)
LATPC	0.0534*** (3.37)	0.0351** (2.05)	0.0541*** (3.30)	0.0435** (2.45)	0.0419** (2.41)
LTAX	0.1625** (2.03)	-0.1212 (1.12)	0.1363* (1.66)	-0.0016 (0.12)	0.0162 (0.74)
PREDEC	-0.0924 (0.96)		-0.1099 (1.14)		
FINAL	-0.0141 (0.30)	-0.0415 (0.83)	-0.0268 (0.58)	-0.0358 (0.72)	-0.0466 (0.95)
FURTHER	-0.0503 (1.21)	-0.0692* (1.80)	-0.0598 (1.43)	-0.0576 (1.49)	-0.0735* (1.91)
FINAL23		0.0423 (0.94)		0.0550 (1.23)	0.0387 (0.87)
FUR23		-0.0476 (1.49)		-0.0304 (0.91)	-0.0420 (1.27)
CONSTANT	1.9548 (1.01)	2.2729 (1.05)	3.8317* (1.95)	2.8538 (1.34)	3.1717 (1.48)

Table 5.10 (continued)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
N	480	300	450	290	290
Adj. R^2	.9830	.9859	.9835	.9868	.9871
F-stat.	304.49	299.29	320.22	295.90	295.40

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 5.11 is found on page 154

Table 5.12
Estimated Effects of Alcohol Control Policies on Per-Capita Rapes (1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LEGAL	-0.0667	0.0000	-0.0944	0.0000	-0.0639
LBEERTAX	-0.0834				-0.1171
LLIQTAX		0.0000			0.1222
LWINETAX			0.0206		0.0000
LTOTTAX				0.0000	
MANDATE	0.0000			0.0000	0.0539
CASHLAW	0.0000			0.0000	0.0177
FORCE	0.0629			0.0000	0.0873
MINDIST	0.0002			0.0000	0.0000
DRYPER	0.0000	0.0000	-0.0874	0.0000	-0.1321
LDRINK	0.0000	0.0000	0.0000	0.0000	0.0000
LTOURPCT	-0.1826	0.0000	-0.1284	0.0000	0.0000
NOSIGN	0.0000	0.0000	0.0000	0.0000	-0.0436
NOPRINT	0.0000	0.0000	0.0000	0.0000	0.0000
PBT	0.0000	0.0000	0.0000	0.0000	0.0000
NOPLEA	0.0000	0.0000	0.0363	0.0000	0.1058
DRAM	0.0000	0.0000	0.0186	0.0000	0.0420
JAIL	0.0000	0.0000	-0.0457	0.0000	-0.0765
FINE	0.0000	0.0000	-0.0001	0.0000	0.0002
SUS	0.0000	0.0000	0.0002	0.0000	0.0000
ILDUM	0.0000	0.0000	-0.0462	0.0000	-0.0947

Table 5.13
Estimated Effects of Alcohol Control Policies on Per-Capita Robberies (1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LEGAL	-0.1082	0.0000	-0.1547	0.0000	-0.0879
LBEERTAX	-0.2721				-0.1984
LLIQTAX		-0.3047			-0.1319
LWINETAX			0.0000		0.0000
LTOTTAX				-0.1275	
MANDATE	0.0000			0.0000	0.0913
CASHLAW	0.0000			0.0000	0.0177
FORCE	0.1020			0.0000	0.0837
MINDIST	0.0003			0.0001	0.0002
DRYPER	0.0000	-0.1006	0.0000	-0.0671	-0.2592
LDRINK	0.0492	0.0000	0.0000	0.0000	0.0000
LTOURPCT	-0.4612	-0.1123	-0.5718	0.0000	0.0000
NOSIGN	0.0340	0.0000	0.0000	0.0000	-0.0600
NOPRINT	0.0000	0.0000	0.0000	0.0000	0.0000
PBT	0.0000	-0.0389	0.0000	-0.0257	-0.0330
NOPLEA	0.0000	0.0000	0.0000	0.0000	0.1592
DRAM	0.0430	0.0000	0.0000	-0.0290	0.0316
JAIL	0.0000	0.0000	0.0000	0.0000	-0.1052
FINE	0.0000	-0.0001	0.0000	0.0000	0.0002
SUS	0.0000	0.0001	0.0005	0.0000	0.0000
ILDUM	0.0000	0.0000	-0.1009	0.0000	-0.1605

Table 5.14
Estimated Effects of Alcohol Control Policies
on Per-Capita Aggravated Assaults
(1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LEGAL	0.0000	0.0000	-0.0905	0.0000	-0.0646
LBEERTAX	-0.0809				0.0983
LLIQTAX		-0.7263			-0.0677
LWINETAX			0.0441		-0.0117
LTOTTAX				-0.3204	
MANDATE	0.1020			0.1495	0.0710
CASHLAW	0.0000			-0.0161	0.0607
FORCE	0.2678			0.1751	0.0761
MINDIST	0.0000			0.0004	0.0003
DRYPER	0.2653	0.0555	-0.0171	0.1040	-0.1793
LDRINK	0.0850	0.0000	0.0000	-0.0039	0.0381
LTOURPCT	-0.0552	0.0899	0.0842	0.1160	0.1463
NOSIGN	0.0588	0.0000	0.0106	0.0329	-0.0257
NOPRINT	0.0000	0.0000	0.0250	0.0000	0.0000
PBT	0.0000	-0.0268	0.0000	-0.0213	-0.0197
NOPLEA	0.0000	0.0000	0.0597	-0.0440	-0.0109
DRAM	0.0743	0.0000	0.0224	0.0171	0.0362
JAIL	0.0000	0.0000	-0.0550	0.0000	-0.0795
FINE	0.0001	-0.0007E-01	-0.0006E-01	0.0008E-01	-0.0006E-01
SUS	-0.0005	-0.0003	-0.0007E-02	-0.0004	-0.0003
ILDUM	0.0000	0.0000	-0.0306	0.0000	0.1109

Table 5.15
Estimated Effects of Alcohol Control Policies on Total Violent Crimes Per-Capita (1985-1994)

	BEER	LIQUOR	WINE	TOTAL	INCLUSIVE
LEGAL	-0.0297	0.0000	-0.1092	0.0000	-0.0741
LBEERTAX	-0.0816				-0.1304
LLIQTAX		-0.5397			0.1095
LWINETAX			0.0319		-0.0070
LTOTTAX				-0.3106	
MANDATE	0.0691			0.0875	0.0973
CASHLAW	0.0000			0.0000	0.0284
FORCE	0.2093			0.1024	0.0772
MINDIST	0.0008E-01			0.0003	0.0002
DRYPER	0.1796	0.0202	-0.0163	0.0387	-0.2383
LDRINK	0.0539	0.0000	0.0000	-0.0239	0.0203
LTOURPCT	-0.1063	0.0414	-0.1043	0.1362	0.0712
NOSIGN	0.0373	0.0000	0.0073	0.0000	-0.0342
NOPRINT	0.0000	0.0000	0.0150	0.0000	0.0000
PBT	0.0000	-0.0246	0.0000	-0.0209	-0.0210
NOPLEA	0.0000	0.0000	0.0438	0.0000	0.0864
DRAM	0.0471	0.0000	0.0168	-0.0236	0.0541
JAIL	0.0000	0.0000	-0.0412	0.0000	-0.0960
FINE	0.0001	-0.0007E-01	-0.0007E-01	0.0001	0.0001
SUS	-0.0003	-0.0002	0.0001	-0.0002	-0.0001
ILDUM	0.0000	0.0000	-0.0514	0.0000	-0.0240

Table 5.16 is found on page 177

Table 6.1
Per-Capita Beer Consumption:
OLS Estimates with Metropolitan Data
(1994-1998)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
LM184490	1.9748*** (3.52)	1.8942*** (3.50)	3.1674*** (6.25)
LUR	-0.0963 (1.03)	-0.6293*** (3.53)	-0.6013*** (3.91)
LURB90	-0.2181* (1.81)	-0.2513** (2.18)	-0.1244 (1.04)
LINCOME	-4.2584*** (5.77)	-2.6605*** (2.90)	4.6423*** (3.38)
LPOV90	-1.3114*** (2.58)	-0.7424 (1.48)	0.6944 (1.39)
LFHH90	1.9806*** (3.17)	2.3768*** (3.97)	4.1338*** (6.62)
LBEERTAX	3.7975*** (6.43)	3.7103*** (5.23)	4.6303*** (7.16)
MANDATE	0.3495** (2.44)	0.3501** (2.58)	-0.0046 (0.03)
CASHLAW	-0.4887** (2.46)	-0.3545* (1.83)	-0.5158** (2.48)
FORCE	-0.5473*** (2.92)	-0.6711*** (3.67)	-0.3524** (2.03)
MINDIST	-0.0011 (1.54)	-0.0012* (1.77)	0.0021** (1.98)
LDRINK	0.4862*** (6.22)	0.5744*** (7.21)	0.7818*** (10.08)
LTOURPCT	-1.5117*** (6.16)	-1.6286*** (6.77)	-1.1794*** (5.03)
NOSIGN	-0.6168*** (4.30)	-0.5013*** (3.45)	0.5043** (2.46)
NOPRINT	-0.6774*** (2.74)	-0.9973*** (4.03)	-0.7549*** (2.82)
LMRM	-0.7055*** (5.20)	-0.7523*** (5.77)	0.8469*** (2.80)
LSOBAP	0.0275 (0.45)	0.0234 (0.36)	-0.3043*** (2.93)
LCATH	-0.9061*** (4.97)	-1.0318*** (5.80)	-0.9675*** (4.70)
LPROT	-3.1143*** (5.66)	-3.7593*** (6.93)	-2.9355*** (5.10)
YEAR94		-0.6896*** (4.22)	-1.3143*** (7.85)
YEAR95		-0.0734 (0.59)	-0.5227*** (4.29)

Table 6.1 (continued)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
YEAR96		-0.0890 (0.93)	-0.3526*** (3.98)
YEAR97		-0.0978 (1.20)	-0.1985*** (2.83)
WEST			-3.6770*** (6.78)
SOUTH			-1.6638*** (5.88)
MIDWEST			-1.2285*** (5.64)
CONSTANT	31.6209*** (4.04)	16.3974 (1.61)	-43.0864*** (3.38)
N	150	150	150
Adj. R^2	.8146	.8339	.8818
F-stat.	35.46	33.53	43.73

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.2
Per-Capita Liquor Consumption:
OLS Estimates with Metropolitan Data
(1991-1998)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
LM184490	2.0347*** (2.85)	1.7289*** (2.67)	-0.8049 (0.83)
LUR	-0.0809 (1.34)	-0.5819*** (4.34)	-0.3262*** (3.08)
LURB90	0.3303*** (3.26)	0.4757*** (4.85)	0.2834** (2.33)
LINCOME	-6.6886*** (9.35)	-10.1732*** (11.22)	-14.7480*** (18.58)
LPOV90	-3.3957*** (3.97)	-3.2800*** (4.34)	-3.3188*** (3.72)
LFHH90	2.2261*** (2.69)	1.4682* (1.85)	-1.2408 (0.98)
LLIQTAX	6.3257*** (8.38)	3.4431*** (4.20)	2.7336*** (3.12)
LDRINK	-0.2621*** (4.45)	-0.2671*** (4.62)	-0.3186*** (5.33)
LTOURPCT	-0.5290*** (2.62)	-0.8616*** (4.61)	-1.2618*** (6.66)
NOSIGN	1.2321*** (9.46)	1.0822*** (9.26)	1.8225*** (13.42)
NOPRINT	-1.2803*** (4.91)	-0.3808 (1.37)	-1.4568*** (2.69)
LMRM	0.1873** (2.19)	0.4167*** (4.57)	0.7891*** (3.46)
LSOBAP	-0.1136*** (3.23)	-0.0836*** (2.61)	-0.1892** (2.27)
LCATH	0.1122 (0.72)	-0.0816 (0.57)	-0.2989 (1.48)
LPROT	-0.6692*** (3.36)	-0.4177** (2.04)	0.2830 (0.66)
YEAR91		1.3661*** (6.02)	2.3086*** (12.04)
YEAR92		1.3081*** (6.60)	2.0622*** (12.50)
YEAR93		1.0558*** (6.52)	1.6147*** (11.82)
YEAR94		0.2615 (1.52)	0.9817*** (6.52)
YEAR95		0.5526*** (5.17)	0.8936*** (10.58)

Table 6.2 (continued)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
YEAR96		0.3538*** (4.20)	0.5507*** (8.63)
YEAR97		0.1341* (1.83)	0.2255*** (4.21)
WEST			-0.4888 (0.95)
SOUTH			-0.1062 (0.28)
MIDWEST			-1.1060*** (3.70)
CONSTANT	52.9670*** (5.86)	93.2108*** (8.94)	132.7997*** (14.16)
N	160	160	160
Adj. R^2	.8448	.8828	.9399
F-stat.	58.71	55.43	100.38

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.3
Per-Capita Wine Consumption:
OLS Estimates with Metropolitan Data
(1991-1998)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
LM184490	0.3092 (0.37)	0.9127 (0.59)	2.1854 (1.35)
LUR	-0.1285 (1.43)	-0.6345*** (3.13)	-0.7088*** (3.38)
LURB90	-0.0517 (0.49)	-0.0345 (0.20)	-0.0474 (0.24)
LINCOME	-1.6206*** (3.10)	-0.2249 (0.08)	4.1426 (1.38)
LPOV90	-0.0426 (0.10)	0.2164 (0.35)	0.8763 (0.95)
LFHH90	-0.6296 (0.96)	-0.2623 (0.38)	1.1701 (0.73)
LWINETAX	0.5132** (2.48)	0.5020** (2.29)	0.9492*** (2.62)
LDRINK	0.4742*** (7.80)	0.5317*** (6.41)	0.7164*** (6.54)
LTOURPCT	-0.8575*** (5.37)	-0.9491*** (5.56)	-1.1800*** (4.46)
NOSIGN	0.3091* (1.89)	0.3946 (1.42)	1.0288** (2.29)
NOPRINT	-1.5778*** (6.46)	-1.6246*** (6.70)	-1.7180*** (5.28)
LMRM	-0.5265*** (4.05)	-0.6013*** (3.38)	-0.0870 (0.12)
LSOBAP	-0.0279 (0.53)	-0.0660 (0.55)	-0.2272 (1.12)
LCATH	-1.3882*** (6.09)	-1.5564*** (4.73)	-1.2389*** (3.45)
LPROT	-2.2001*** (7.91)	-2.5705*** (8.62)	-1.7340*** (3.32)
YEAR91		-0.1304 (0.24)	-0.8068 (1.40)
YEAR92		-0.1532 (0.31)	-0.7439 (1.46)
YEAR93		0.0733 (0.19)	-0.4418 (1.11)
YEAR94		-0.6513** (2.11)	-1.0748*** (3.29)
YEAR95		-0.0956 (0.40)	-0.3442 (1.41)

Table 6.3 (continued)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
YEAR96		-0.1002 (0.58)	-0.2532 (1.46)
YEAR97		-0.0807 (0.62)	-0.1433 (1.11)
WEST			-1.1991 (0.81)
SOUTH			-1.0187 (1.09)
MIDWEST			-1.2635** (2.28)
CONSTANT	3.7429 (0.63)	-8.7402 (0.32)	-41.0421 (1.36)
N	174	174	174
Adj. R^2	.7361	.7448	.7597
F-stat.	33.16	23.95	22.87

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.4
Per-Capita Total Alcohol Consumption:
OLS Estimates with Metropolitan Data
(1994-1998)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
LM184490	-9.3112*** (7.73)	-8.2859*** (5.15)	-9.3204*** (6.94)
LUR	0.0273 (0.79)	0.2266** (2.47)	0.0232 (0.66)
LURB90	-0.2262 (1.45)	-0.1219 (0.73)	-0.1957 (1.21)
LINCOME	0.3082 (0.29)	-2.0046 (0.44)	0.2545 (0.21)
LPOV90	11.0903*** (7.17)	8.8989** (2.26)	11.0975*** (6.22)
LFHH90	-17.2488*** (7.50)	-14.3116** (2.24)	-17.5215*** (6.44)
LTOTTAX	-1.0626 (1.47)	-0.4679 (0.59)	-0.8572 (1.02)
MANDATE	4.1092*** (5.40)	3.0802* (1.76)	2.9418* (1.79)
FORCE	-0.8053** (2.08)	-0.5652 (1.35)	0.1186 (0.06)
MINDIST	0.0186*** (2.91)	0.0178* (1.68)	0.0267* (1.70)
LDRINK	0.1131** (2.04)	0.0543 (0.90)	0.0886 (1.32)
LTOURPCT	-2.2566*** (5.57)	-2.0498*** (5.05)	-1.0873 (0.53)
NOSIGN	-4.3513*** (3.81)	-3.4506*** (2.88)	-2.5481 (0.68)
NOPRINT	-0.0626 (0.08)	-0.1781 (0.14)	-2.0598 (1.07)
LMRM	-0.0927 (0.58)	-0.0737 (0.19)	0.5212 (0.35)
LSOBAP	-0.2404*** (2.96)	-0.1442 (0.67)	-0.8726 (1.17)
LCATH	0.5380 (1.10)	0.1152 (0.20)	-0.0046 (0.00)
LPROT	-2.8021*** (4.13)	-2.3730*** (2.94)	-2.2045 (1.40)
YEAR94		0.2736 (0.56)	
YEAR95		0.0525 (0.15)	

Table 6.4 (continued)

	NO FIXED EFFECTS REMOVED	YEAR FIXED EFFECTS REMOVED	YEAR AND REGION FIXED EFFECTS REMOVED
YEAR96		0.0213 (0.11)	
YEAR97		-0.0108 (0.12)	
WEST			-0.0460 (0.02)
SOUTH			2.9176 (1.02)
MIDWEST			0.9794 (0.52)
CONSTANT	-82.5491*** (4.59)	-48.3333 (0.69)	-84.7568*** (3.83)
N	85	85	85
Adj. R^2	.9896	.9900	.9894
F-stat.	446.38	380.52	374.87

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.5
Per-Capita Murder Rates:
OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRMUR	0.0002 (0.57)	0.0332 (0.43)	-0.0145 (0.17)	0.0966 (0.78)	0.0199 (0.16)
LPOLICE	-0.2390*** (3.31)	-0.0087 (0.15)	-0.0459 (0.79)	-0.2079*** (2.83)	-0.4199*** (4.21)
$P_{c\|a}$	5.2183 (1.54)	0.9032 (0.49)	1.1757 (0.64)	4.7307 (1.04)	5.2087 (1.19)
$P_{e\|c}$	-0.0003 (0.91)	-0.0004 (0.98)	-0.0004 (1.09)	-0.0002 (0.60)	-0.0001 (0.25)
LM184490	0.7511** (2.04)	0.5409 (1.50)	1.1271*** (3.47)	0.3588 (0.85)	0.3070 (0.75)
LUR	0.3436** (2.05)	0.3324** (2.06)	0.1229 (0.82)	0.4168** (2.23)	0.0495 (0.23)
LURB90	0.0117 (0.17)	0.0327 (0.58)	0.1523** (2.51)	0.0733 (0.76)	0.0419 (0.43)
LINCOME	1.0564 (1.28)	6.6963*** (4.95)	1.9404** (2.36)	3.2834** (2.11)	0.5534 (0.27)
LPOV90	0.5388* (1.68)	2.5438*** (5.35)	1.1599*** (4.04)	1.6142*** (3.04)	0.2630 (0.34)
LFHH90	2.7074*** (6.03)	2.6046*** (7.06)	3.3051*** (8.35)	3.1227*** (6.07)	4.4440*** (6.71)
LWAGELOW	-0.8015 (1.04)	-1.0656 (1.60)	-1.9325** (2.28)	-2.9303** (2.32)	-3.8091*** (2.94)
LBEER	0.1627** (2.49)				0.7563*** (3.47)
LLIQ		0.2757*** (4.58)			-1.1379*** (2.84)
LWINE			0.1175** (1.98)		0.5984** (2.01)
LTOTALC				0.1934** (2.27)	
YEAR91		-0.5436** (2.07)	0.4618** (2.52)		
YEAR92		-0.4797** (2.02)	0.4196** (2.28)		
YEAR93		-0.2343 (1.17)	0.5141*** (3.18)		
YEAR94	0.6669*** (3.71)	0.1856 (1.00)	0.5858*** (2.96)	0.6877*** (2.93)	0.8883*** (3.69)
YEAR95	0.2982*** (2.63)	-0.0309 (0.22)	0.3665*** (2.78)	0.2930* (1.80)	0.6636*** (3.31)

Table 6.5 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
YEAR96	0.2266** (2.20)	-0.0282 (0.23)	0.2120* (1.82)	0.1830 (1.44)	0.4013*** (2.82)
YEAR97	0.1766* (1.85)	0.0614 (0.55)	0.1483 (1.39)	0.1631 (1.57)	0.2436** (2.35)
WEST	0.5916*** (3.95)	0.1158 (0.76)	0.6350*** (3.20)	0.7277*** (2.88)	1.2890*** (4.09)
SOUTH	0.5289*** (3.58)	-0.2332 (1.32)	0.3964*** (2.70)	0.3811 (1.58)	1.0597*** (3.25)
MIDWEST	0.3725*** (2.71)	-0.1271 (1.00)	0.2066 (1.64)	0.0590 (0.24)	0.6618** (2.04)
CONSTANT	-13.4236* (1.65)	-68.5895*** (5.10)	-17.1677* (1.96)	-30.2634* (1.86)	1.6991 (0.08)
N	120	181	171	95	95
Adj. R^2	.7007	.6836	.6395	.6874	.7141
F-stat.	15.66	18.68	14.71	11.88	12.18

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.6
Per-Capita Rape Rates:
OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRRAPE	0.0003 (1.60)	-0.0771 (1.21)	-0.0647 (0.92)	-0.0578 (0.81)	-0.0798 (0.97)
LPOLICE	-0.0958*** (2.73)	-0.0527* (1.71)	-0.0859*** (2.67)	-0.1260*** (3.18)	-0.1299** (2.22)
LM184490	-0.7795*** (3.93)	-1.2135*** (6.36)	-1.0737*** (5.92)	-1.1854*** (5.22)	-1.1848*** (5.16)
LUR	-0.0004*** (4.60)	-0.0002E-01 (0.32)	0.0273 (0.31)	0.2022** (2.01)	0.2065 (1.63)
LURB90	-0.0579 (1.29)	0.0354 (1.06)	0.0440 (1.18)	-0.1066** (2.08)	-0.1168** (2.14)
LINCOME	0.4578 (1.00)	1.5789** (2.00)	-0.0130 (0.03)	1.8580** (2.14)	1.3043 (1.00)
LPOV90	0.4351*** (2.62)	1.1984*** (4.50)	0.6897*** (3.95)	0.8875*** (3.23)	0.6874 (1.44)
LFHH90	-0.7312*** (3.38)	-0.8280*** (4.35)	-0.7909*** (3.52)	-0.7302*** (2.81)	-0.7116** (1.96)
LWAGELOW	1.3424*** (2.67)	1.2423*** (3.16)	1.4261*** (2.83)	1.2382* (1.86)	1.3609* (1.86)
LBEER	0.0798** (2.00)				0.1607 (1.27)
LLIQ		0.0208 (0.61)			0.0401 (0.17)
LWINE			-0.0142 (0.38)		-0.0750 (0.43)
LTOTALC				0.1473*** (3.05)	
YEAR91		0.0010 (0.01)	0.2830** (2.39)		
YEAR92		0.0434 (0.32)	0.2603** (2.23)		
YEAR93		-0.0066 (0.06)	0.1529 (1.49)		
YEAR94	0.0226 (0.28)	-0.0335 (0.34)	0.1060 (0.92)	0.0575 (0.45)	0.0935 (0.64)
YEAR95	0.0142 (0.21)	-0.0171 (0.21)	0.0494 (0.62)	-0.0831 (0.90)	-0.0603 (0.49)
YEAR96	0.0098 (0.17)	-0.0295 (0.43)	0.0256 (0.38)	-0.0449 (0.63)	-0.0314 (0.36)
YEAR97	0.0196 (0.38)	-0.0236 (0.38)	-0.0041 (0.07)	-0.0123 (0.22)	-0.0041 (0.07)

Table 6.6 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
WEST	0.2473*** (2.72)	0.3048*** (3.23)	0.4176*** (3.58)	0.2076 (1.50)	0.2474 (1.33)
SOUTH	0.3758*** (4.43)	0.2922*** (2.76)	0.4788*** (5.65)	0.2875** (2.21)	0.2907 (1.54)
MIDWEST	0.2901*** (3.82)	0.2109*** (2.72)	0.3066*** (4.44)	0.2497** (2.54)	0.2221 (1.43)
CONSTANT	-19.9688*** (4.18)	-33.0897*** (4.17)	-17.6343*** (3.15)	-34.5777*** (3.79)	-29.3464** (2.18)
N	120	178	168	95	95
Adj. R^2	.6070	.5981	.6010	.5636	.5536
F-stat.	11.81	14.17	13.58	8.14	7.13

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.7
Per-Capita Robbery Rates:
OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRROB	-0.0002 (0.64)	0.0054 (0.07)	-0.0733 (0.83)	0.0394 (0.36)	-0.0326 (0.30)
LPOLICE	0.0075 (0.10)	0.0241 (0.40)	-0.0366 (0.61)	-0.1154 (1.47)	-0.3378*** (3.15)
LM184490	0.7603* (1.87)	0.6505* (1.77)	0.3991 (1.21)	0.3078 (0.70)	0.3026 (0.71)
LUR	0.5185*** (2.80)	0.3876** (2.29)	0.4819*** (3.06)	0.5950*** (3.02)	0.1949 (0.84)
LURB90	-0.1542** (2.05)	-0.2528*** (4.31)	-0.1113* (1.70)	-0.0834 (0.83)	-0.0942 (0.93)
LINCOME	1.9779** (2.15)	2.5143* (1.79)	2.6599*** (2.95)	0.3123 (0.19)	-2.3484 (1.06)
LPOV90	0.0057 (0.02)	0.2929 (0.59)	0.8943*** (2.95)	0.1727 (0.32)	-1.1380 (1.38)
LFHH90	2.6668*** (5.83)	2.8198*** (7.62)	3.5661*** (8.86)	4.0503*** (7.94)	5.3845*** (8.04)
LWAGELOW	-0.6980 (0.81)	0.4551 (0.65)	-2.2500** (2.48)	-3.6995*** (2.82)	-4.8824*** (3.58)
LBEER	0.1045 (1.44)				0.6865*** (2.94)
LLIQ		0.1743*** (2.78)			-1.2537*** (2.87)
LWINE			0.1801*** (2.81)		0.7022** (2.18)
LTOTALC				0.1073 (1.16)	
YEAR91		0.0803 (0.30)	0.2241 (1.12)		
YEAR92		-0.0102 (0.04)	0.1933 (0.97)		
YEAR93		0.0770 (0.37)	0.2941* (1.67)		
YEAR94	0.6925*** (3.47)	0.4577** (2.37)	0.7716*** (3.68)	1.1575*** (4.59)	1.3715*** (5.21)
YEAR95	0.2152* (1.70)	0.1438 (0.97)	0.3242** (2.19)	0.5172*** (2.83)	0.9305*** (4.06)
YEAR96	0.1694 (1.50)	0.1306 (1.02)	0.2007* (1.66)	0.3402** (2.49)	0.5658*** (3.69)
YEAR97	0.1431 (1.34)	0.1225 (1.05)	0.1505 (1.34)	0.2277** (2.04)	0.3125*** (2.79)

Table 6.7 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
WEST	-0.1432 (0.87)	-0.2393 (1.52)	0.2591 (1.26)	0.7013*** (2.59)	1.2726*** (3.75)
SOUTH	-0.0112 (0.07)	-0.1996 (1.08)	0.0961 (0.63)	0.4807* (1.89)	1.2154*** (3.49)
MIDWEST	-0.0319 (0.21)	-0.3073** (2.38)	-0.2751** (2.32)	-0.2699 (1.42)	0.3439 (1.18)
CONSTANT	-16.3518* (1.81)	-23.9358* (1.71)	-18.4999* (1.90)	9.2874 (0.53)	41.2546* (1.79)
N	120	180	170	95	95
Adj. R^2	.5300	.5830	.6104	.6430	.6712
F-stat.	8.89	13.51	14.24	10.96	11.10

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.8
Per-Capita Aggravated Assault Rates:
OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRASS	-0.0002 (0.77)	-0.2338*** (4.40)	-0.1106* (1.94)	-0.3498*** (4.37)	-0.2161*** (2.78)
LPOLICE	0.0388 (0.61)	0.0979** (2.52)	0.0815** (2.15)	0.0714 (1.38)	0.0551 (0.87)
LM184490	0.2276 (0.68)	0.5160** (2.18)	0.2166 (1.03)	1.0615*** (3.59)	1.0451*** (3.95)
LUR	0.4816*** (3.18)	0.2418** (2.23)	0.2300** (2.32)	0.2536** (1.99)	0.2328* (1.69)
LURB90	-0.1342** (2.17)	-0.3172*** (8.45)	-0.3672*** (9.28)	-0.3795*** (6.10)	-0.2949*** (5.04)
LINCOME	2.8496*** (3.79)	2.3582*** (2.64)	6.1633*** (10.72)	0.4146 (0.40)	3.9398*** (3.00)
LPOV90	0.5769** (2.05)	0.6020* (1.91)	1.6514*** (8.47)	-0.7354** (2.20)	0.5586 (1.16)
LFHH90	1.4555*** (3.89)	1.6482*** (6.87)	1.2973*** (5.11)	2.1567*** (6.77)	2.0479*** (5.36)
LWAGELOW	0.5112 (0.73)	2.7400*** (6.18)	3.3075*** (6.06)	3.2390*** (4.00)	2.0777*** (2.61)
LBEER	0.1441** (2.42)				-0.0092 (0.07)
LLIQ		0.2326*** (5.68)			-0.3361 (1.35)
LWINE			0.4153*** (9.36)		0.6870*** (3.55)
LTOTALC				0.1756*** (2.75)	
YEAR91		-0.3455** (1.99)	-0.9654*** (7.61)		
YEAR92		-0.4496*** (2.87)	-0.9860*** (7.98)		
YEAR93		-0.2962** (2.23)	-0.6923*** (6.32)		
YEAR94	0.3253** (1.99)	-0.0280 (0.23)	-0.3964*** (3.10)	0.1027 (0.66)	-0.0597 (0.40)
YEAR95	0.0013 (0.01)	-0.1252 (1.35)	-0.3456*** (3.97)	-0.0223 (0.20)	-0.1204 (0.96)
YEAR96	-0.0304 (0.33)	-0.0883 (1.08)	-0.2255*** (3.00)	-0.0450 (0.53)	-0.1013 (1.16)
YEAR97	0.0677 (0.78)	0.0146 (0.20)	-0.0329 (0.47)	0.0207 (0.30)	-0.0117 (0.18)

Table 6.8 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
WEST	-0.1115 (0.83)	-0.2971*** (2.95)	-0.7491*** (5.76)	-0.2637 (1.57)	-0.5215*** (2.67)
SOUTH	-0.0190 (0.14)	-0.3501*** (2.99)	-0.5856*** (6.26)	-0.1977 (1.26)	-0.1028 (0.52)
MIDWEST	-0.3240*** (2.62)	-0.5267*** (6.45)	-0.5028*** (6.87)	-0.0989 (0.82)	0.1563 (0.95)
CONSTANT	-31.8366*** (4.29)	-31.0395*** (3.49)	-70.9366*** (11.43)	-10.3908 (0.95)	-42.3825*** (3.14)
N	120	180	170	95	95
Adj. R^2	.5215	.7002	.7407	.7274	.7805
F-stat.	8.63	21.90	25.14	15.75	18.59

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.9
Per-Capita Total Violent Crime Rates:
OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRTVC	-0.2794*** (3.55)	-0.1430** (2.31)	-0.1043 (1.54)	-0.1498* (1.80)	-0.0953 (1.15)
LPOLICE	0.0562 (1.11)	0.0635* (1.68)	0.0314 (0.83)	-0.0212 (0.42)	-0.1111* (1.72)
LM184490	0.6076** (2.29)	0.5007** (2.20)	0.2608 (1.25)	0.6014** (2.16)	0.6115** (2.34)
LUR	0.4029*** (3.32)	0.2738*** (2.61)	0.3000*** (3.01)	0.4050*** (3.28)	0.2257 (1.60)
LURB90	-0.1858*** (3.99)	-0.2622*** (7.29)	-0.2303*** (5.56)	-0.2556*** (4.39)	-0.2105*** (3.62)
LINCOME	2.0263*** (3.38)	2.3792*** (2.73)	4.2600*** (7.53)	0.5092 (0.50)	1.5772 (1.18)
LPOV90	0.1289 (0.56)	0.4854 (1.58)	1.2359*** (6.34)	-0.3249 (0.99)	-0.0530 (0.11)
LFHH90	2.0768*** (6.77)	1.9821*** (8.61)	2.1309*** (8.47)	2.6853*** (8.66)	3.1448*** (8.05)
LWAGELOW	-0.0250 (0.05)	1.4389*** (3.34)	0.6367 (1.11)	0.2679 (0.34)	-0.8705 (1.07)
LBEER	0.0885* (1.87)				0.2652** (1.96)
LLIQ		0.1921*** (4.91)			-0.6818*** (2.69)
LWINE			0.2747*** (6.48)		0.6813*** (3.53)
LTOTALC				0.1666*** (2.84)	
YEAR91		-0.1436 (0.85)	-0.3750*** (2.99)		
YEAR92		-0.2144 (1.40)	-0.3864*** (3.12)		
YEAR93		-0.1010 (0.78)	-0.1954* (1.79)		
YEAR94	0.4433*** (3.41)	0.1812 (1.52)	0.1271 (0.98)	0.5355*** (3.52)	0.5112*** (3.35)
YEAR95	0.0956 (1.17)	0.0089 (0.10)	-0.0301 (0.34)	0.1981* (1.84)	0.2913** (2.27)
YEAR96	0.0756 (1.03)	0.0071 (0.09)	-0.0275 (0.36)	0.1037 (1.25)	0.1600* (1.78)
YEAR97	0.0995 (1.47)	0.0554 (0.79)	0.0487 (0.71)	0.1056 (1.60)	0.1252* (1.91)

Table 6.9 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
WEST	-0.0406 (0.38)	-0.2295** (2.36)	-0.2358* (1.78)	0.1135 (0.69)	0.1846 (0.93)
SOUTH	-0.0351 (0.33)	-0.2654** (2.31)	-0.2552*** (2.64)	0.0908 (0.59)	0.4269** (2.10)
MIDWEST	-0.1260 (1.28)	-0.3992*** (4.99)	-0.3583*** (4.52)	-0.1534 (1.29)	0.2384 (1.40)
CONSTANT	-19.3403*** (3.26)	-26.4866*** (3.05)	-43.4387*** (7.11)	-3.2644 (0.31)	-9.9065 (0.72)
N	120	179	169	97	97
Adj. R^2	.6483	.6990	.7097	.7373	.7664
F-stat.	13.90	21.66	21.53	16.85	17.58

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.10
Per-Capita Murder Rates:
Reduced-Form OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRMUR	0.0218 (0.17)	0.1211 (0.79)	0.2027 (1.38)		
LPOLICE	-0.1157 (0.49)	0.1919 (0.99)	-0.2662 (1.39)		
$P_{c\|a}$	9.6086* (1.90)	0.2739 (0.05)	2.2924 (0.58)		
$P_{e\|c}$	-0.0001 (0.33)	-0.0271 (0.19)	0.0219 (0.18)		
LM184490	1.4549 (1.55)	-2.3024 (0.98)	-4.2106** (2.06)		
LUR	0.0161 (0.09)	0.0538 (0.24)	-0.2086 (1.16)		
LURB90	-0.1184 (0.51)	-1.2479*** (4.73)	-0.5356*** (2.59)		
LINCOME	4.8004** (1.97)	1.4090 (0.89)	-4.3356 (1.13)		
LPOV90	2.0926*** (2.78)	1.4315 (0.67)	1.3936 (1.52)		
LFHH90	2.4163** (2.47)	1.9674 (0.63)	1.0349 (0.70)		
LWAGELOW	1.4370 (0.54)	1.9856 (0.92)	-0.0920 (0.04)		
LBEERTAX	-1.1279 (1.39)				
LLIQTAX		2.9035 (1.29)			
LWINETAX			-0.8833** (2.40)		
LTOTTAX					
MANDATE	-0.0563 (0.13)				
CASHLAW	-0.5641* (1.65)				
FORCE	0.2304 (0.63)				
MINDIST	0.0043 (1.20)				
LDRINK	0.1242 (0.62)	0.0397 (0.23)	0.1081 (0.71)		
LTOURPCT	-0.2342 (0.56)	-0.2104 (0.38)	-0.7203** (2.39)		

Table 6.10 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
NOSIGN	0.2567 (0.70)	0.4619 (0.64)	0.1915 (0.36)		
NOPRINT	0.4890 (0.57)	-4.5084*** (2.76)	-1.0981** (2.33)		
LMRM	1.0251 (1.51)	1.4282 (1.47)	0.3924 (0.52)		
LSOBAP	-0.3255 (1.13)	-0.3426 (1.37)	0.2233 (0.95)		
LCATH	-0.0113 (0.03)	-0.7035 (1.45)	0.2041 (0.51)		
LPROT	1.4688 (1.33)	-1.3358 (1.25)	-0.4374 (0.67)		
YEAR91		-0.1846 (0.46)	1.7310** (2.22)		
YEAR92		-0.2442 (0.71)	1.4684** (2.00)		
YEAR93		-0.0311 (0.11)	1.3371** (2.20)		
YEAR94	0.0741 (0.22)	-0.0414 (0.12)	0.8984* (1.72)		
YEAR95	0.0995 (0.52)	-0.0290 (0.15)	0.8303** (2.18)		
YEAR96	0.0924 (0.70)	0.0131 (0.09)	0.4737** (2.03)		
YEAR97	0.0932 (0.96)	0.0465 (0.39)	0.2559* (1.88)		
WEST	-1.3256 (1.09)	-3.6354** (1.97)	0.0544 (0.03)		
SOUTH	-0.1156 (0.26)	-0.2759 (0.32)	0.3873 (0.38)		
MIDWEST	-0.4061 (1.00)	-0.2986 (0.43)	-0.3905 (0.69)		
CONSTANT	-51.1953** (2.21)	-30.0664 (1.34)	30.3832 (0.77)		
N	118	138	148		
Adj. R^2	.7527	.7460	.6954		
F-stat.	12.49	14.41	12.19		

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.11
Per-Capita Rape Rates:
Reduced-Form OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRRAPE	-0.0477 (0.56)	0.0024 (0.02)	-0.0807 (0.72)		
LPOLICE	0.1056 (0.88)	-0.0083 (0.07)	-0.1556 (1.46)		
LM184490	-0.4302 (0.90)	-2.0486 (1.63)	-3.7866*** (3.62)		
LUR	0.0036 (0.04)	-0.0971 (0.73)	0.0260 (0.28)		
LURB90	-0.0441 (0.40)	-0.2525 (1.47)	-0.1747* (1.65)		
LINCOME	2.5226** (2.05)	1.4132 (1.60)	-1.4525 (0.77)		
LPOV90	1.2216*** (3.14)	1.2978 (1.10)	1.3547*** (2.89)		
LFHH90	-0.1194 (0.25)	-1.1558 (0.67)	-1.9425*** (2.59)		
LWAGELOW	1.7837 (1.52)	2.4432** (2.00)	1.8943* (1.83)		
LBEERTAX	-1.3548*** (3.22)				
LLIQTAX		0.2197 (0.17)			
LWINETAX			-0.2181 (1.20)		
LTOTTAX					
MANDATE	-0.4508** (2.21)				
CASHLAW	0.4413** (2.45)				
FORCE	-0.1260 (0.68)				
MINDIST	0.0039** (2.17)				
LDRINK	0.0143 (0.16)	0.1009 (1.11)	0.0261 (0.30)		
LTOURPCT	0.0264 (0.12)	0.0400 (0.13)	-0.1162 (0.75)		
NOSIGN	0.1714 (0.88)	1.9901E+14 (0.21)	0.2247 (0.73)		
NOPRINT	0.6998 (1.57)	-1.9901E+14 (0.21)	-0.7884** (2.53)		

Table 6.11 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LMRM	1.1325*** (3.27)	0.0894 (0.17)	-0.0128 (0.04)		
LSOBAP	-0.2892** (1.98)	0.0080 (0.06)	0.1666 (1.35)		
LCATH	-0.2760 (1.51)	-0.0766 (0.31)	0.0402 (0.20)		
LPROT	-0.0323 (0.06)	-0.0305 (0.06)	-0.3815 (1.11)		
YEAR91		-0.0765 (0.34)	0.4819 (1.29)		
YEAR92		-0.0285 (0.14)	0.4054 (1.17)		
YEAR93		-0.0721 (0.44)	0.2871 (0.99)		
YEAR94	-0.0082 (0.05)	-0.2148 (1.11)	0.1863 (0.76)		
YEAR95	-0.0007 (0.01)	-0.0778 (0.72)	0.0914 (0.51)		
YEAR96	0.0006 (0.01)	-0.0449 (0.56)	0.0711 (0.64)		
YEAR97	0.0180 (0.37)	-0.0117 (0.18)	0.0138 (0.21)		
WEST	-1.6950*** (2.80)	-0.3486 (0.34)	0.3246 (0.37)		
SOUTH	0.4471** (2.11)	0.0353 (0.07)	0.3727 (0.67)		
MIDWEST	0.1852 (1.01)	-0.1878 (0.47)	0.2261 (0.78)		
CONSTANT	-36.7662*** (3.12)	-35.2169*** (2.73)	-11.5506 (0.61)		
N	118	136	145		
Adj. R^2	.7005	.6349	.7063		
F-stat.	10.44	9.38	13.37		

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.12
Per-Capita Robbery Rates:
Reduced-Form OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRROB	-0.0318 (0.28)	0.0293 (0.21)	-0.0319 (0.28)		-0.0035 (0.02)
LPOLICE	0.1477 (0.60)	0.3734* (1.94)	-0.3724* (1.92)		-0.4193 (0.14)
LM184490	0.9370 (0.97)	1.5254 (0.62)	-4.7042** (2.24)		-2.4017E+10 (0.01)
LUR	0.4586*** (2.58)	0.1200 (0.48)	-0.0796 (0.41)		0.1964 (0.42)
LURB90	0.0514 (0.23)	-1.0225*** (3.41)	-0.5035** (2.23)		-1.3744E+09 (0.01)
LINCOME	4.4823* (1.77)	3.7302** (2.05)	-5.0071 (1.25)		-1.1844E+09 (0.01)
LPOV90	1.9538** (2.46)	-0.6101 (0.27)	0.9452 (0.96)		1.8147E+10 (0.01)
LFHH90	3.0431*** (3.07)	5.7642* (1.75)	2.3262 (1.48)		-1.8632E+10 (0.01)
LWAGELOW	-3.0477 (1.23)	0.8027 (0.34)	-2.1162 (0.97)		5.9905E+10 (0.01)
LBEERTAX	-0.2910 (0.33)				3.0033 (0.40)
LLIQTAX		3.4440 (1.44)			-4.3060E+10 (0.01)
LWINETAX			-1.1313*** (3.10)		-0.2464 (0.12)
LTOTTAX					
MANDATE	0.3974 (0.93)				-6.3993E+09 (0.01)
CASHLAW	-0.5241 (1.43)				
FORCE	-0.2298 (0.61)				-9.9694E+09 (0.01)
MINDIST	-0.0017 (0.47)				2.5781E+07 (0.01)
LDRINK	-0.1541 (0.83)	-0.0330 (0.18)	0.1189 (0.73)		-0.0322 (0.11)
LTOURPCT	-0.6492 (1.48)	0.4083 (0.68)	-0.1111 (0.36)		-1.8785E+10 (0.01)
NOSIGN	0.2702 (0.68)	0.7053 (0.89)	0.4409 (0.87)		1.0393E+10 (0.01)
NOPRINT	-0.0575 (0.06)	-3.3727* (1.92)	-0.7202 (1.44)		1.1959E+10 (0.01)

Table 6.12 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LMRM	0.0538 (0.08)	1.3833 (1.31)	1.9673** (2.55)		2.4961 (0.34)
LSOBAP	-0.1085 (0.36)	-0.4133 (1.54)	-0.0926 (0.37)		-1.0557 (0.23)
LCATH	-0.1603 (0.44)	-0.2402 (0.48)	0.7303* (1.87)		2.2224 (0.47)
LPROT	-0.4843 (0.41)	-0.6690 (0.60)	-1.4929** (2.27)		-2.4923 (0.52)
YEAR91		-0.5991 (1.36)	1.9736** (2.50)		
YEAR92		-0.5211 (1.33)	1.7675** (2.41)		
YEAR93		-0.3119 (0.95)	1.5524** (2.54)		
YEAR94	0.6213* (1.96)	-0.1371 (0.36)	1.2662** (2.44)		-1.4907E+09 (0.01)
YEAR95	0.1973 (1.00)	-0.1451 (0.67)	1.0039*** (2.67)		-1.0530E+09 (0.01)
YEAR96	0.1648 (1.20)	0.0211 (0.13)	0.6116*** (2.60)		-5.9731E+08 (0.01)
YEAR97	0.1240 (1.23)	0.0618 (0.47)	0.2944** (2.10)		-2.4205E+08 (0.01)
WEST	0.0405 (0.03)	-3.6504* (1.82)	-2.1501 (1.16)		2.4627E+09 (0.01)
SOUTH	-0.2483 (0.57)	-0.5322 (0.59)	1.1719 (1.07)		-7.7584E+09 (0.01)
MIDWEST	-0.2439 (0.65)	-0.4248 (0.59)	0.4297 (0.76)		-5.2594E+09 (0.01)
CONSTANT	-38.6829 (1.60)	-29.1101 (1.17)	54.1326 (1.35)		-1.3774E+11 (0.01)
N	118	137	147		80
Adj. R^2	.6397	.6480	.6495		.5977
F-stat.	8.16	9.94	10.66		4.91

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.13
Per-Capita Aggravated Assault Rates:
Reduced-Form OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRASS	-0.0797 (0.95)	0.0359 (0.43)	-0.3384*** (4.49)		-0.0040 (0.02)
LPOLICE	-0.0639 (0.45)	0.0890 (0.82)	-0.2790** (2.36)		-1.4316 (1.12)
LM184490	0.2731 (0.52)	2.2673 (1.58)	-2.4180* (1.88)		1.8974E+10 (0.01)
LUR	0.0933 (0.90)	-0.0469 (0.34)	-0.2298* (1.87)		0.0914 (0.46)
LURB90	-0.2054* (1.93)	-0.5187*** (3.04)	-0.6959*** (4.82)		1.0858E+09 (0.01)
LINCOME	1.3038 (0.97)	-1.7412* (1.68)	-5.5579** (2.21)		9.3570E+08 (0.01)
LPOV90	1.4123*** (3.17)	-2.5768** (2.01)	-1.2446** (2.01)		-1.4336E+10 (0.01)
LFHH90	-0.0407 (0.08)	4.8787** (2.58)	3.8056*** (3.86)		1.4719E+10 (0.01)
LWAGELOW	1.0314 (0.74)	0.8811 (0.67)	-0.2741 (0.20)		-4.7325E+10 (0.01)
LBEERTAX	-1.2834** (2.54)				2.9036 (0.88)
LLIQTAX		2.8141** (2.07)			3.4018E+10 (0.01)
LWINETAX			0.2948 (1.22)		-0.0008 (0.00)
LTOTTAX					
MANDATE	0.1666 (0.85)				5.0555E+09 (0.01)
FORCE	0.2206 (1.01)				7.8758E+09 (0.01)
MINDIST	0.0012 (0.58)				-2.0367E+07 (0.01)
LDRINK	0.0987 (0.93)	0.0165 (0.16)	0.2641** (2.53)		-0.0137 (0.11)
LTOURPCT	-0.4196* (1.66)	0.0105 (0.03)	-0.3241* (1.66)		1.4840E+10 (0.01)
NOSIGN	-0.3889* (1.94)	1.2179*** (3.11)	-1.0333*** (3.34)		-8.2105E+09 (0.01)
NOPRINT	0.4225 (0.78)	-1.8624** (1.99)	-0.2910 (0.98)		-9.4477E+09 (0.01)

Table 6.13 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LMRM	-0.2739 (0.63)	1.1822** (2.03)	0.3460 (0.69)		5.5537* (1.79)
LSOBAP	-0.0562 (0.33)	-0.0346 (0.23)	0.5600*** (3.56)		-1.4266 (0.75)
LCATH	-0.4168*** (2.94)	0.1643 (0.58)	0.4049 (1.64)		2.6923 (1.37)
LPROT	-0.8160 (1.29)	0.0125 (0.02)	-1.3434*** (3.27)		-0.7191 (0.35)
YEAR91		0.2573 (1.02)	1.4026*** (2.84)		
YEAR92		0.2124 (0.94)	1.1308** (2.46)		
YEAR93		0.2107 (1.13)	1.0399*** (2.72)		
YEAR94	0.1994 (1.11)	0.1609 (0.75)	0.6598** (2.02)		1.1776E+09 (0.01)
YEAR95	0.1223 (1.14)	0.1879 (1.53)	0.6547*** (2.75)		8.3189E+08 (0.01)
YEAR96	0.0473 (0.61)	0.1378 (1.51)	0.3777** (2.55)		4.7188E+08 (0.01)
YEAR97	0.0624 (1.07)	0.1117 (1.50)	0.1852** (2.10)		1.9122E+08 (0.01)
WEST	0.5172 (0.68)	-2.7990** (2.53)	-0.4821 (0.40)		-1.9455E+09 (0.01)
SOUTH	1.0182*** (4.15)	-1.2943** (2.53)	-0.5743 (0.82)		6.1292E+09 (0.01)
MIDWEST	0.0217 (0.10)	-1.7439*** (4.21)	-0.4522 (1.25)		4.1550E+09 (0.01)
CONSTANT	-28.1702** (2.17)	29.4095** (2.12)	61.8059** (2.44)		1.0881E+11 (0.01)
N	118	137	147		80
Adj. R^2	.8103	.7716	.7396		.8401
F-stat.	18.85	17.41	15.81		14.84

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.14
Per-Capita Total Violent Crime Rates:
Reduced-Form OLS Estimates with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LARRTVC	-0.0144 (0.16)	0.0544 (0.57)	0.0638 (0.65)	0.2216 (0.03)	
LPOLICE	0.0257 (0.18)	0.2544** (1.98)	0.0104 (0.08)	-0.8874 (0.01)	
LM184490	0.6930 (1.20)	1.6840 (1.06)	-1.0928 (0.82)	-5.4160E+12 (0.00)	
LUR	0.2367** (2.32)	-0.0539 (0.33)	-0.0512 (0.44)	2.0986 (0.13)	
LURB90	-0.0256 (0.20)	-0.8042*** (4.13)	-0.5262*** (3.90)	-3.5675E+11 (0.00)	
LINCOME	3.3008** (2.28)	0.8527 (0.74)	-2.7347 (1.14)	-2.7973E+11 (0.00)	
LPOV90	1.8670*** (4.04)	-1.5368 (1.08)	-0.5844 (0.99)	4.1859E+12 (0.00)	
LFHH90	0.8363 (1.49)	4.7198** (2.24)	3.4958*** (3.71)	-4.6767E+12 (0.00)	
LWAGELOW	-0.7918 (0.57)	1.0259 (0.70)	0.3629 (0.27)	-7.3911E+12 (0.00)	
LBEERTAX	-0.8736 (1.61)				
LLIQTAX		2.5937* (1.70)			
LWINETAX			-0.1450 (0.63)		
LTOTTAX				5.5036 (0.17)	
MANDATE	0.3358 (1.33)			1.4076E+12 (0.00)	
CASHLAW	-0.3682* (1.68)				
FORCE	0.0674 (0.31)			2.0376E+12 (0.00)	
MINDIST	0.0007 (0.35)			3.2846E+10 (0.00)	
LDRINK	-0.0246 (0.23)	-0.0022 (0.02)	0.0103 (0.10)	-3.8341 (0.36)	
LTOURPCT	-0.4595* (1.82)	0.2069 (0.54)	-0.3339* (1.83)	2.9302E+12 (0.00)	
NOSIGN	0.0006 (0.00)	1.9626E+15*** (3.42)	-1.4335*** (3.58)	3.2299E+17 (0.18)	
NOPRINT	0.3290 (0.61)	-1.9626E+15*** (3.42)	0.7183* (1.74)	-3.2300E+17 (0.18)	

Table 6.14 (continued)

	BEER (94-98)	LIQUOR (91-98)	WINE (91-98)	TOTAL (94-98)	INCLUSIVE (94-98)
LMRM	-0.0109 (0.03)	0.9540 (1.44)	0.9127** (1.96)	-9.3646 (0.03)	
LSOBAP	-0.1351 (0.78)	-0.1446 (0.86)	0.1951 (1.32)	5.3598 (0.05)	
LCATH	-0.1758 (0.78)	0.0154 (0.05)	-0.0484 (0.18)	-21.7048 (0.10)	
LPROT	-0.4084 (0.62)	-0.0106 (0.02)	-0.4770 (1.10)	12.0977 (0.05)	
YEAR91		-0.0795 (0.28)	1.0367** (2.18)		
YEAR92		-0.0454 (0.18)	0.8780** (1.98)		
YEAR93		0.0121 (0.06)	0.7837** (2.12)		
YEAR94	0.3235* (1.78)	-0.0389 (0.16)	0.5897* (1.89)	7.3017E+11 (0.01)	
YEAR95	0.1201 (1.07)	0.0442 (0.32)	0.4868** (2.11)	5.1580E+11 (0.01)	
YEAR96	0.0746 (0.95)	0.0725 (0.71)	0.2953** (2.08)	2.9258E+11 (0.01)	
YEAR97	0.0819 (1.41)	0.0767 (0.92)	0.1732** (2.09)	1.1856E+11 (0.10)	
WEST	0.1930 (0.26)	-2.6289** (2.09)	-1.9985* (1.74)	1.3007E+12 (0.01)	
SOUTH	0.4672* (1.87)	-0.8979 (1.57)	-1.0201 (1.43)	3.7523E+12 (0.00)	
MIDWEST	-0.1382 (0.64)	-1.1082** (2.41)	-0.7118* (1.94)	3.1062E+12 (0.00)	
CONSTANT	-38.2871*** (2.77)	0.9039	34.8196 (1.45)	-6.7379E+12 (0.00)	
N	118	135	144	69	
Adj. R^2	.7859	.7007	.7475		
F-stat.	15.81	12.20	16.12		

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.15 is found on page 218

Table 6.16
OLS and 2SLS Murder Rate Estimates
with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRMUR	0.0668 (0.68)	0.0921 (1.00)	0.0571 (0.53)	-0.0111 (0.11)	-0.0908 (0.85)	-0.1147 (1.15)
LPOLICE	-0.2390*** (3.40)	-0.2265*** (3.48)	0.0843 (0.97)	0.0431 (0.53)	-0.0222 (0.35)	-0.0204 (0.35)
$P_{c\|a}$	5.6083* (1.67)	5.7016* (1.86)	-2.1836 (0.48)	-4.0129 (0.96)	1.4641 (0.42)	2.7220 (0.83)
$P_{e\|c}$	-0.0003 (0.90)	-0.0003 (1.03)	0.2016 (1.46)	0.1870 (1.47)	0.2210* (1.74)	0.1532 (1.26)
LM184490	0.8034** (2.19)	0.8212** (2.44)	1.0529* (1.71)	0.9585* (1.68)	1.4540*** (3.11)	1.3137*** (2.99)
LUR	0.3297** (2.01)	0.2945* (1.93)	0.2512 (1.17)	0.1140 (0.57)	0.0408 (0.24)	0.0175 (0.11)
LURB90	0.0378 (0.51)	0.0742 (1.00)	-0.0014 (0.02)	0.0572 (0.92)	0.1634** (2.16)	0.2409*** (3.10)
LINCOME	1.0006 (1.24)	0.8898 (1.20)	8.4649*** (4.24)	6.0417*** (3.19)	3.5053*** (3.41)	2.2752** (2.09)
LPOV90	0.5601* (1.78)	0.5879** (2.04)	2.9313*** (4.59)	2.4269*** (4.06)	1.2431*** (3.57)	1.0414*** (3.10)
LFHH90	2.6419*** (5.91)	2.5878*** (6.29)	3.6582*** (4.95)	3.2705*** (4.76)	4.0903*** (7.35)	4.0212*** (7.73)
LWAGELOW	-0.9547 (1.22)	-1.1261 (1.54)	-0.3076 (0.35)	0.0961 (0.12)	-2.7075*** (2.67)	-2.9897*** (3.14)
LBEER	0.1436** (2.13)	0.0911 (1.21)				
LLIQ			0.2316** (2.23)	0.0490 (0.48)		
LWINE					0.1695** (2.47)	0.0343 (0.40)
LTOTALC						
YEAR91			-0.8312** (2.18)	-0.4012 (1.11)	0.3548* (1.76)	0.5586*** (2.70)
YEAR92			-0.7137** (2.07)	-0.3319 (1.02)	0.3992** (2.02)	0.5768*** (2.90)
YEAR93			-0.4322 (1.50)	-0.1370 (0.51)	0.5075*** (2.87)	0.6454*** (3.69)
YEAR94	0.6707*** (3.83)	0.6658*** (4.15)	-0.0640 (0.26)	0.0176 (0.08)	0.5299** (2.48)	0.6245*** (3.08)
YEAR95	0.3128*** (2.79)	0.3310*** (3.19)	-0.1635 (0.88)	-0.0195 (0.11)	0.4166*** (2.93)	0.4932*** (3.62)

Table 6.16 (continued)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
YEAR96	0.2318** (2.30)	0.2395*** (2.60)	-0.0410 (0.27)	0.0460 (0.33)	0.2601** (2.05)	0.2943** (2.47)
YEAR97	0.1801* (1.94)	0.1786** (2.10)	0.0004 (0.00)	0.0325 (0.27)	0.1359 (1.13)	0.1488 (1.33)
WEST	0.5866*** (4.01)	0.6209*** (4.54)	-0.0567 (0.26)	0.1721 (0.83)	0.6989*** (3.09)	0.8932*** (3.94)
SOUTH	0.5295*** (3.68)	0.5599*** (4.18)	-0.4610 (1.62)	-0.1286 (0.48)	0.3905* (1.93)	0.5088*** (2.60)
MIDWEST	0.3287** (2.20)	0.3201** (2.35)	0.1882 (0.90)	0.3959** (2.01)	0.2680* (1.73)	0.2618* (1.81)
CONSTANT	-12.6701 (1.58)	-11.4837 (1.55)	-85.4216*** (4.53)	-64.9727*** (3.65)	-27.4486*** (2.66)	-15.8755 (1.47)
N	118	118	138	138	148	148
Adj. R^2	.7156	.7138	.6832	.6746	.6352	.6238
F-stat.	16.49	16.36	14.43	13.91	12.63	12.08

Table 6.16 (extended)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
LARRMUR	0.1784 (0.77)		0.2986 (1.29)	
LPOLICE	-0.1930 (1.28)		0.0895 (0.25)	
$P_{c\|a}$	10.3467 (1.27)		3.0204 (0.36)	
$P_{e\|c}$	0.0074 (0.04)		-0.1638 (0.88)	
LM184490	1.7481 (1.57)		7.1938** (2.55)	
LUR	0.4895 (1.30)		-0.2079 (0.48)	
LURB90	-0.1098 (0.63)		-0.6955** (2.09)	
LINCOME	6.6404** (2.25)		4.5751 (0.80)	
LPOV90	0.9150 (1.20)		-3.9634** (2.05)	
LFHH90	5.7861*** (4.28)		11.5368*** (4.42)	
LWAGELOW	-4.7835* (1.67)		-3.4453 (0.91)	
LBEER			1.3567*** (3.23)	
LLIQ			1.2355 (1.03)	
LWINE			-2.1456* (1.87)	
LTOTALC	0.5671*** (3.33)			
YEAR91				
YEAR92				
YEAR93				
YEAR94	0.6676 (1.08)		0.0345 (0.03)	
YEAR95	0.2349 (0.74)		0.1719 (0.26)	

Table 6.16 (extended, continued)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
YEAR96	0.1567 (0.74)		0.0809 (0.20)	
YEAR97	0.1721 (1.11)		0.2290 (1.14)	
WEST	0.6256 (1.14)		0.3723 (0.34)	
SOUTH	0.1272 (0.25)		-1.0243 (0.76)	
MIDWEST	-0.0358 (0.08)		-1.6684* (1.75)	
CONSTANT	-45.8193 (1.48)		1.6433 (0.03)	
N	69		69	
Adj. R^2	.6606		.6966	
F-stat.	7.97		8.43	

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.17
OLS and 2SLS Rape Rate Estimates
with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRRAP	-0.1969*** (3.21)	-0.1923*** (3.38)	0.0245 (0.30)		-0.1943** (2.28)	-0.2070** (2.57)
LPOLICE	-0.1093*** (2.81)	-0.1036*** (2.84)	-0.0032 (0.07)		-0.0807*** (2.77)	-0.0794*** (2.94)
LM184490	-0.7043*** (3.44)	-0.7057*** (3.74)	-1.6120*** (5.21)		-1.4777*** (6.29)	-1.4930*** (6.84)
LUR	0.1309 (1.41)	0.1178 (1.36)	-0.0345 (0.32)		-0.0085 (0.10)	-0.0127 (0.16)
LURB90	0.0043 (0.11)	0.0169 (0.42)	0.0587* (1.74)		0.0960** (2.52)	0.1047*** (2.83)
LINCOME	-0.5390 (1.16)	-0.5718 (1.33)	1.7679* (1.73)		-0.5301 (1.04)	-0.6649 (1.32)
LPOV90	0.3365* (1.90)	0.3476** (2.12)	1.5562*** (4.90)		0.8149*** (4.41)	0.7923*** (4.57)
LFHH90	-0.7786*** (3.31)	-0.7929*** (3.65)	-1.2000*** (3.39)		-0.9978*** (3.54)	-0.9900*** (3.79)
LWAGELOW	0.3558 (0.82)	0.3000 (0.74)	1.5040*** (3.29)		1.5010*** (2.86)	1.4361*** (2.92)
LBEER	0.0153 (0.42)	-0.0046 (0.11)				
LLIQ			-0.0304 (0.58)			
LWINE					-0.0690* (1.87)	-0.0839** (2.15)
LTOTALC						
YEAR91			-0.0884 (0.45)		0.3368*** (2.95)	0.3643*** (3.28)
YEAR92			0.0059 (0.03)		0.3010*** (2.65)	0.3271*** (2.97)
YEAR93			-0.0504 (0.35)		0.1924* (1.92)	0.2133** (2.21)
YEAR94	0.3276*** (3.28)	0.3243*** (3.51)	-0.1104 (0.88)		0.1027 (0.95)	0.1178 (1.16)
YEAR95	0.1375** (2.17)	0.1430** (2.43)	-0.0454 (0.49)		0.0493 (0.64)	0.0621 (0.85)
YEAR96	0.0839 (1.47)	0.0864 (1.64)	-0.0269 (0.36)		0.0518 (0.80)	0.0586 (0.96)
YEAR97	0.0348 (0.65)	0.0336 (0.68)	-0.0046 (0.07)		0.0026 (0.05)	0.0040 (0.07)

Table 6.17 (continued)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
WEST	0.4077*** (4.93)	0.4238*** (5.40)	0.1736 (1.62)		0.4557*** (4.14)	0.4792*** (4.51)
SOUTH	0.5779*** (7.04)	0.5913*** (7.66)	0.1789 (1.35)		0.4870*** (5.85)	0.4983*** (6.36)
MIDWEST	0.5293*** (6.99)	0.5320*** (7.61)	-0.0025 (0.03)		0.1981*** (2.62)	0.1919*** (2.72)
CONSTANT	-8.4229* (1.84)	-8.0770* (1.91)	-37.6621*** (3.95)		-14.6230*** (2.68)	-13.2281** (2.47)
N	118	118	136		145	145
Adj. R^2	.5853	.5841	.6298		.6708	.6704
F-stat.	10.71	10.66	12.49		15.67	15.64

Table 6.17 (extended)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
LARRRAP	0.0371 (0.34)		0.0336 (0.31)	
LPOLICE	-0.0492 (0.96)		-0.0623 (0.46)	
LM184490	-1.0424** (2.44)		-0.4669 (0.46)	
LUR	-0.0069 (0.05)		-0.1272 (0.77)	
LURB90	0.0471 (0.73)		-0.0211 (0.17)	
LINCOME	0.7115 (0.62)		-0.6304 (0.28)	
LPOV90	0.7948*** (2.97)		-0.1542 (0.20)	
LFHH90	-0.5278 (1.02)		0.3157 (0.31)	
LWAGELOW	-0.9563 (0.96)		-1.1959 (0.82)	
LBEER			0.2196 (1.28)	
LLIQ			-0.0085 (0.02)	
LWINE			-0.2047 (0.51)	
LTOTALC	0.0296 (0.45)			
YEAR91				
YEAR92				
YEAR93				
YEAR94	0.2066 (0.95)		0.2607 (0.67)	
YEAR95	0.1259 (1.05)		0.2340 (0.92)	
YEAR96	0.1002 (1.24)		0.1595 (1.02)	
YEAR97	0.0445 (0.79)		0.0746 (0.93)	

Table 6.17 (extended, continued)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
WEST	0.4521** (2.30)		0.5969 (1.42)	
SOUTH	0.4936*** (2.65)		0.5132 (1.01)	
MIDWEST	-0.1931 (1.33)		-0.3341 (1.13)	
CONSTANT	-16.4980 (1.43)		1.2162 (0.05)	
N	69		69	
Adj. R^2	.7057		.7039	
F-stat.	10.59		9.51	

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.18
OLS and 2SLS Robbery Rate Estimates
with Metropolitan Data
(1994-1998 or 1991-1998)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRROB	-0.0894 (0.88)	-0.0976 (1.03)	0.1232 (1.31)	0.0797 (0.91)	-0.1356 (1.25)	-0.1997* (1.88)
LPOLICE	0.0112 (0.15)	0.0041 (0.06)	0.2371*** (2.73)	0.1991** (2.45)	-0.0315 (0.49)	-0.0122 (0.20)
LM184490	0.7832* (1.93)	0.7854** (2.10)	2.4779*** (4.06)	2.4335*** (4.29)	0.5425 (1.11)	0.3140 (0.67)
LUR	0.5201*** (2.82)	0.5388*** (3.12)	0.1713 (0.80)	0.0370 (0.18)	0.4669*** (2.65)	0.4520*** (2.73)
LURB90	-0.1785** (2.21)	-0.1966** (2.45)	-0.3716*** (5.69)	-0.3102*** (5.02)	-0.0455 (0.58)	0.0312 (0.38)
LINCOME	1.9021** (2.07)	1.9473** (2.29)	9.3399*** (4.71)	7.1131*** (3.76)	2.9250*** (2.79)	1.6441 (1.42)
LPOV90	-0.0727 (0.21)	-0.0907 (0.28)	1.5466** (2.48)	1.0462* (1.78)	0.8977** (2.51)	0.7195** (2.07)
LFHH90	2.7351*** (5.91)	2.7558*** (6.45)	5.6840*** (8.23)	5.3946*** (8.37)	3.9500*** (6.99)	3.8703*** (7.26)
LWAGELOW	-0.5969 (0.69)	-0.5182 (0.64)	0.6682 (0.77)	0.9558 (1.18)	-3.1358*** (2.82)	-3.6072*** (3.37)
LBEER	0.1157 (1.56)	0.1438* (1.74)				
LLIQ			0.2602*** (2.58)	0.0950 (0.96)		
LWINE					0.1710** (2.37)	0.0242 (0.25)
LTOTALC						
YEAR91			-1.0735*** (2.87)	-0.6695* (1.88)	0.2344 (1.08)	0.4623** (2.00)
YEAR92			-0.9370*** (2.79)	-0.5776* (1.81)	0.2532 (1.16)	0.4661** (2.04)
YEAR93			-0.6277** (2.25)	-0.3490 (1.32)	0.3636* (1.88)	0.5240*** (2.65)
YEAR94	0.6946*** (3.50)	0.6991*** (3.82)	-0.3447 (1.39)	-0.2672 (1.16)	0.8305*** (3.60)	0.9648*** (4.27)
YEAR95	0.2303* (1.81)	0.2240* (1.90)	-0.3435* (1.81)	-0.1935 (1.08)	0.3870** (2.31)	0.5036*** (3.02)
YEAR96	0.1678 (1.49)	0.1636 (1.57)	-0.0685 (0.47)	0.0132 (0.10)	0.2277* (1.66)	0.2789** (2.12)
YEAR97	0.1375 (1.29)	0.1385 (1.41)	0.0499 (0.39)	0.0773 (0.65)	0.1715 (1.35)	0.1804 (1.51)

Table 6.18 (continued)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
WEST	-0.1531 (0.93)	-0.1753 (1.13)	-0.9140*** (4.29)	-0.7198*** (3.57)	0.3422 (1.50)	0.5509** (2.33)
SOUTH	-0.0298 (0.18)	-0.0488 (0.32)	-1.0073*** (3.83)	-0.7455*** (2.99)	0.1488 (0.84)	0.2446 (1.42)
MIDWEST	-0.0249 (0.17)	-0.0282 (0.20)	-0.2807 (1.59)	-0.1464 (0.88)	-0.3461** (2.31)	-0.3897*** (2.73)
CONSTANT	-15.5821* (1.72)	-16.0530* (1.92)	-81.5745*** (4.37)	-62.2846*** (3.51)	-17.6712 (1.63)	-5.3241 (0.45)
N	118	118	137	137	147	147
Adj. R^2	.5316	.5310	.6467	.6385	.5885	.5751
F-stat.	8.81	8.79	13.45	13.01	11.44	10.88

Table 6.18 (extended)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
LARRROB	0.0789 (0.46)		0.1698 (0.99)	
LPOLICE	0.0732 (0.51)		0.6500* (1.78)	
LM184490	1.9665* (1.65)		8.2353*** (2.97)	
LUR	0.3384 (0.88)		-0.2366 (0.53)	
LURB90	-0.3605** (2.03)		-1.0988*** (3.20)	
LINCOME	7.1448** (2.29)		10.5127* (1.74)	
LPOV90	0.6242 (0.83)		-3.1809 (1.52)	
LFHH90	6.2876*** (4.41)		12.4853*** (4.50)	
LWAGELOW	-0.8433 (0.29)		3.1826 (0.81)	
LBEER			0.7012 (1.53)	
LLIQ			2.2486* (1.84)	
LWINE			-2.6613** (2.38)	
LTOTALC	0.3165* (1.81)			
YEAR91				
YEAR92				
YEAR93				
YEAR94	0.1304 (0.21)		-1.2382 (1.18)	
YEAR95	-0.0906 (0.27)		-0.7624 (1.10)	
YEAR96	0.0312 (0.14)		-0.3738 (0.89)	
YEAR97	0.0996 (0.62)		-0.0433 (0.20)	

Table 6.18 (extended, continued)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
WEST	-0.3271 (0.58)		-1.5460 (1.37)	
SOUTH	-0.5096 (0.97)		-2.8264** (2.05)	
MIDWEST	-0.0941 (0.25)		-1.9048** (2.30)	
CONSTANT	-55.2575* (1.70)		-63.8699 (1.02)	
N	69		69	
Adj. R^2	.6317		.6621	
F-stat.	7.86		8.01	

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.19
OLS and 2SLS Aggravated Assault Rate Estimates with Metropolitan Data (1994-1998 or 1991-1998)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRASS	-0.3803*** (4.49)	-0.3960*** (4.96)	-0.1924*** (3.20)	-0.2028*** (3.65)	-0.1316** (2.28)	-0.1384** (2.36)
LPOLICE	0.0948 (1.62)	0.1062* (1.92)	0.0789 (1.52)	0.0680 (1.42)	0.0853** (2.41)	0.0870*** (2.61)
LM184490	0.6584** (2.08)	0.6761** (2.31)	1.1005*** (3.02)	1.0929*** (3.25)	0.4321 (1.58)	0.4267* (1.68)
LUR	0.3573** (2.56)	0.3296** (2.50)	0.2034 (1.61)	0.1663 (1.42)	0.2142** (2.21)	0.2116** (2.35)
LURB90	-0.1723*** (3.05)	-0.1546*** (2.80)	-0.3229*** (8.39)	-0.3065*** (8.51)	-0.3523*** (8.80)	-0.3488*** (8.93)
LINCOME	2.3047*** (3.34)	2.2187*** (3.45)	2.3354** (1.98)	1.6928 (1.52)	6.3139*** (10.70)	6.2238*** (9.86)
LPOV90	0.3149 (1.21)	0.3172 (1.32)	0.2986 (0.79)	0.1489 (0.42)	1.5711*** (7.56)	1.5534*** (7.68)
LFHH90	1.9253*** (5.42)	1.9271*** (5.89)	2.0943*** (5.11)	2.0194*** (5.33)	1.6526*** (5.30)	1.6544*** (5.73)
LWAGELOW	0.6181 (0.97)	0.5379 (0.91)	2.0167*** (3.86)	2.0872*** (4.32)	2.6427*** (4.58)	2.6138*** (4.80)
LBEER	0.0683 (1.21)	0.0323 (0.50)				
LLIQ			0.2180*** (3.70)	0.1710*** (3.00)		
LWINE					0.4160*** (9.68)	0.4058*** (7.58)
LTOTALC						
YEAR91			-0.2742 (1.22)	-0.1558 (0.73)	-0.9773*** (7.96)	-0.9607*** (7.52)
YEAR92			-0.3520* (1.76)	-0.2485 (1.32)	-0.9731*** (8.20)	-0.9585*** (7.90)
YEAR93			-0.2256 (1.36)	-0.1455 (0.93)	-0.6774*** (6.43)	-0.6660*** (6.32)
YEAR94	0.2680* (1.81)	0.2604* (1.90)	0.0069 (0.05)	0.0311 (0.23)	-0.3938*** (3.21)	-0.3858*** (3.30)
YEAR95	-0.0038 (0.04)	0.0054 (0.06)	-0.0846 (0.78)	-0.0450 (0.44)	-0.3565*** (4.23)	-0.3497*** (4.28)
YEAR96	0.0028 (0.03)	0.0090 (0.12)	-0.0239 (0.27)	0.0003 (0.00)	-0.1956*** (2.62)	-0.1918*** (2.72)
YEAR97	0.0512 (0.65)	0.0486 (0.67)	0.0584 (0.76)	0.0667 (0.95)	-0.0188 (0.27)	-0.0180 (0.28)

Table 6.19 (continued)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
WEST	0.0025 (0.02)	0.0331 (0.28)	-0.3166** (2.38)	-0.2563** (2.06)	-0.7165*** (5.73)	-0.7017*** (5.52)
SOUTH	-0.0853 (0.71)	-0.0671 (0.59)	-0.3131** (2.00)	-0.2362 (1.60)	-0.5878*** (6.25)	-0.5825*** (6.54)
MIDWEST	-0.2685** (2.39)	-0.2613** (2.51)	-0.6180*** (5.81)	-0.5772*** (5.82)	-0.5390*** (6.66)	-0.5402*** (7.20)
CONSTANT	-25.0546*** (3.64)	-24.1156*** (3.76)	-26.4849** (2.35)	-20.8322** (1.97)	-69.1855*** (11.28)	-68.2804*** (10.49)
N	118	118	137	137	147	147
Adj. R^2	.5989	.5973	.7459	.7445	.7764	.7763
F-stat.	11.28	11.21	20.96	20.81	26.35	26.33

Table 6.19 (extended)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
LARRASS	-0.0028 (0.02)		0.0144 (0.12)	
LPOLICE	0.1123* (1.80)		0.0622 (0.38)	
LM184490	2.8055*** (5.60)		3.0165** (2.43)	
LUR	-0.0119 (0.07)		-0.0899 (0.45)	
LURB90	-0.4612*** (5.88)		-0.4782*** (3.14)	
LINCOME	3.3460** (2.53)		1.8217 (0.68)	
LPOV90	-0.9846*** (3.08)		-1.6969* (1.78)	
LFHH90	4.1231*** (6.69)		4.5944*** (3.58)	
LWAGELOW	1.7201 (1.19)		1.1064 (0.58)	
LBEER			0.4542** (2.12)	
LLIQ			-0.1315 (0.24)	
LWINE			-0.0210 (0.04)	
LTOTALC	0.3202*** (4.00)			
YEAR91				
YEAR92				
YEAR93				
YEAR94	-0.2054 (0.73)		-0.0546 (0.12)	
YEAR95	-0.0985 (0.65)		0.0540 (0.17)	
YEAR96	-0.0416 (0.41)		0.0462 (0.24)	
YEAR97	0.0576 (0.82)		0.1015 (1.02)	

Table 6.19 (extended, continued)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
WEST	-0.6150*** (2.58)		-0.3969 (0.79)	
SOUTH	-0.4111* (1.78)		-0.2429 (0.40)	
MIDWEST	-0.5643*** (2.59)		-0.6249 (1.50)	
CONSTANT	-25.2331* (1.83)		-6.6627 (0.24)	
N	69		69	
Adj. R^2	.8350		.8302	
F-stat.	21.24		18.49	

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.20
OLS and 2SLS Total Violent Crime Rate Estimates with Metropolitan Data (1994-1998 or 1991-1998)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
LARRTVC	-0.2791*** (3.62)	-0.2789*** (3.92)	-0.0279 (0.37)		-0.1122 (1.52)	-0.1296* (1.88)
LPOLICE	0.0556 (1.12)	0.0553 (1.19)	0.1334** (2.55)		0.0349 (0.95)	0.0409 (1.20)
LM184490	0.6560** (2.47)	0.6559*** (2.69)	1.4259*** (3.82)		0.3696 (1.26)	0.3135 (1.15)
LUR	0.3960*** (3.33)	0.3968*** (3.56)	0.1579 (1.24)		0.2855*** (2.83)	0.2785*** (2.98)
LURB90	-0.1671*** (3.38)	-0.1677*** (3.49)	-0.2967*** (7.17)		-0.1817*** (3.85)	-0.1609*** (3.58)
LINCOME	1.9961*** (3.40)	1.9982*** (3.67)	4.9443*** (4.14)		4.4549*** (7.45)	4.0878*** (7.00)
LPOV90	0.1228 (0.55)	0.1225 (0.59)	0.8651** (2.27)		1.2440*** (5.66)	1.1931*** (5.81)
LFHH90	2.1062*** (6.97)	2.1065*** (7.57)	3.2236*** (7.62)		2.4498*** (7.64)	2.4363*** (8.19)
LWAGELOW	-0.0714 (0.13)	-0.0689 (0.14)	1.2811** (2.43)		-0.1350 (0.22)	-0.2832 (0.49)
LBEER	0.0810* (1.74)	0.0820 (1.60)				
LLIQ			0.2032*** (3.42)			
LWINE					0.2642*** (6.13)	0.2213*** (4.89)
LTOTALC						
YEAR91			-0.5519** (2.43)		-0.3819*** (3.06)	-0.3149*** (2.62)
YEAR92			-0.5049** (2.50)		-0.3564*** (2.91)	-0.2954** (2.51)
YEAR93			-0.3276* (1.95)		-0.1612 (1.49)	-0.1146 (1.11)
YEAR94	0.4448*** (3.50)	0.4450*** (3.80)	-0.1236 (0.83)		0.1492 (1.16)	0.1865 (1.54)
YEAR95	0.1013 (1.26)	0.1010 (1.36)	-0.1476 (1.34)		-0.0244 (0.28)	0.0050 (0.06)
YEAR96	0.0754 (1.05)	0.0753 (1.14)	-0.0367 (0.41)		-0.0037 (0.05)	0.0120 (0.17)
YEAR97	0.0858 (1.26)	0.0859 (1.37)	0.0526 (0.68)		0.0616 (0.86)	0.0648 (0.98)

Table 6.20 (continued)

	BEER (94-98)		LIQUOR (91-98)		WINE (91-98)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
WEST	-0.0245 (0.23)	-0.0254 (0.25)	-0.5368*** (4.05)		-0.1807 (1.38)	-0.1165 (0.93)
SOUTH	-0.0355 (0.34)	-0.0361 (0.37)	-0.5679*** (3.58)		-0.2369** (2.40)	-0.2105** (2.27)
MIDWEST	-0.1281 (1.33)	-0.1283 (1.45)	-0.4639*** (4.00)		-0.4417*** (4.58)	-0.4576*** (5.10)
CONSTANT	-18.8531*** (3.24)	-18.8759*** (3.50)	-46.7216*** (4.12)		-42.3348*** (6.84)	-38.7173*** (6.44)
N	118	118	135		144	144
Adj. R^2	.6557	.6557	.7300		.7322	.7300
F-stat.	14.10	14.10	19.12		20.55	20.34

Table 6.20 (extended)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
LARRTVC	0.0401 (0.37)		0.0946 (0.87)	
LPOLICE	0.0851 (1.25)		0.2263 (1.32)	
LM184490	2.2543*** (4.07)		4.4669*** (3.41)	
LUR	0.1144 (0.64)		-0.1373 (0.65)	
LURB90	-0.3668*** (4.29)		-0.6170*** (3.84)	
LINCOME	4.4271*** (3.05)		4.1678 (1.47)	
LPOV90	-0.2995 (0.85)		-2.1607** (2.16)	
LFHH90	4.6439*** (6.97)		7.0988*** (5.32)	
LWAGELOW	0.2653 (0.18)		0.9189 (0.47)	
LBEER			0.5312** (2.41)	
LLIQ			0.6123 (1.07)	
LWINE			-0.8937* (1.69)	
LTOTALC	0.2733*** (3.32)			
YEAR91				
YEAR92				
YEAR93				
YEAR94	-0.0057 (0.02)		-0.3006 (0.60)	
YEAR95	-0.0366 (0.22)		-0.1029 (0.32)	
YEAR96	0.0121 (0.11)		-0.0367 (0.18)	
YEAR97	0.0812 (1.07)		0.0760 (0.73)	

Table 6.20 (extended, continued)

	TOTAL (94-98)		INCLUSIVE (94-98)	
	OLS	2SLS	OLS	2SLS
WEST	-0.4089 (1.56)		-0.6046 (1.14)	
SOUTH	-0.3614 (1.44)		-0.9538 (1.48)	
MIDWEST	-0.3553* (1.71)		-1.0187** (2.44)	
CONSTANT	-32.4949** (2.14)		-18.5266 (0.63)	
N	69		69	
Adj. R^2	.8091		.8183	
F-stat.	17.95		17.12	

Note: *L* denotes the natural logarithm. Absolute values of t-ratios appear in parentheses. * indicates statistical significance at the ten-percent level, ** at the five-percent level, and *** at the one-percent level in a two-tailed test.

Table 6.21 is found on page 227

Table 6.22 is found on page 232