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ALCOHOL REGULATION AND CRIME

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ABSTRACT

We provide a critical review of research in economics that has examined causal relationships between alcohol use and crime. We lay out several causal pathways through which alcohol regulation and alcohol consumption may affect crime, including: direct pharmacological effects on aggression, reaction time, and motor impairment; excuse motivations; venues and social interactions; and victimization risk. We focus our review on four main types of alcohol regulations: price/tax restrictions, age-based availability restrictions, spatial availability restrictions, and temporal availability restrictions. We conclude that there is strong evidence that tax- and age-based restrictions on alcohol availability reduce crime, and we discuss implications for policy and practice.

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1. Introduction

A substantial body of research in economics, criminology, and public health documents an association between alcohol availability, alcohol consumption, and crime.[1] Though much of the literature is focused on violent crime, many studies have also examined the link between alcohol and property crimes, nuisance crimes, and crimes that result directly from alcohol consumption such as driving under the influence (DUI). Most of these have found a large and statistically significant relationship between alcohol consumption and crime.

The very strong correlations between alcohol consumption and crime raise the possibility that alcohol regulations may be effective crime reduction tools. Specifically, if alcohol consumption causes people to commit crimes or increases the chance they will be victimized, then laws and regulations designed to reduce the availability of alcohol may reduce crime. However, if the correlation between alcohol use and crime is due in total or in part to unobserved individual factors (such as risk preferences) or to unobserved local factors (such as neighborhood quality), then alcohol regulation may not be an effective crime prevention tool. And even if alcohol consumption by some people does increase the probability that they will commit crimes, it is not obvious that alcohol regulations will significantly reduce consumption in this population; thus it would be possible for certain policies to impose social costs without significantly reducing crime. Despite these concerns, alcohol regulation as a crime reduction strategy is worth

[1] While some crimes such as driving under the influence (DUI) and public intoxication are clearly linked to alcohol use, here we concern ourselves primarily with crimes for which a causal role for alcohol is possible but not obvious. One reason for this is that the effects of alcohol control policies on DUI and alcohol-related fatalities have been extensively studied and reviewed elsewhere. We focus primarily on the Federal Bureau of Investigation's index crimes, which consist of violent crimes (murder, rape, robbery, and aggravated assault) and property crimes (larceny, burglary, and motor vehicle theft).

examining because alcohol consumption is so strongly associated with crime and because there is wide latitude for changing alcohol regulations in the United States.[2]

In this chapter we offer a critical review of existing research on alcohol and crime from the perspective of economics. Notably, we are not the first to apply economic perspectives to alcohol control policies (see, e.g., Manning 1989; Pogue and Sgontz 1989; Kenkel 1996; Kenkel and Manning 1996; Cook 2007). We review most of the serious policy options that use alcohol control to bring about reductions in crime, but other, less commonly debated policy options exist as well. We are not the first to review the evidence on the effects of alcohol control policies on alcohol consumption and adverse outcomes; indeed, we draw heavily from previous reviews of alcohol control from Cook and Moore 2000, Chaloupka et al. 2002, Chaloupka 2004, and others. Those reviews, however, differ from ours in at least two important ways. First, previous reviews have largely (but not exclusively) focused on the monetary price of alcohol and associated tax-based interventions. Second, most earlier reviews have focused on a wider range of alcohol-related outcomes, such as motor vehicle fatalities, accidental injury, risky sexual behavior, and productivity, in addition to crime. This focus on a broader set of outcomes is a natural way to research the effect of price and tax interventions. Here, however, we expand the scope of alcohol regulations under review but focus entirely on their effect on crime. This broader look incorporates a body of evidence from other countries and regions (especially Scandinavia) that have used a variety of natural experiment designs to study the effects of non-price restrictions on alcohol availability and crime. It also permits examination of several recent high-quality studies that have provided important

[2] Potential changes include alcohol increasing the excise tax, raising the drinking age, adopting tougher drunk-driving laws, restricting liquor license availability, further restricting the hours/days/locations of alcohol sales, and more strictly enforcing existing laws against underage drinking.

information about alcohol's causal role in crime using variation in liquor store density, bar closing hours, and age-based alcohol restrictions, among others.

We pay particular attention to studies that take the causal inference problem seriously. This focus is appropriate given the chapter's focus on policy-relevant crime control strategies. It is important to accurately assess the strengths and weaknesses of the evidence before deciding to continue using a particular type of regulation, taking into account endogeneity, simultaneity, unobserved heterogeneity, and associated evaluation problems. We also pay close attention to whether alcohol regulations reduce crime rates or just displace crimes to different times or places, and we highlight the need to distinguish between the effects of alcohol restrictions on criminals and the effects on victims.

The chapter proceeds as follows. First, we review evidence from laboratory studies on the pharmacological effects of alcohol consumption on the brain and behavior. We use this and other evidence to lay out several pathways by which alcohol consumption could lead to the commission of crime. We also address the relationship between alcohol and other drugs, with respect to both consumption and regulation. We then review studies on the relationship between alcohol regulations and crime, grouping them by the type of alcohol regulation examined (tax/price restrictions, age-based restrictions, spatial restrictions, temporal restrictions, and regulations not otherwise classified). We summarize what is known about the value of each type of regulation and conclude with a discussion of economic considerations in assessing the importance of alcohol regulations as part of an effective crime control strategy.

2. The Pharmacological Effects of Alcohol

Although pharmacological effects are not the only mechanism by which alcohol consumption causes crime, it is a major one. People's "blood-alcohol concentration" (BAC) from drinking affects their level of impairment differently, according to their individual characteristics. The most important is the size of the dose. The number of drinks consumed, the speed with which they are consumed, and the alcohol content of the drinks are the major determinants of the dose. Dose size is moderated by numerous individual characteristics. Heavier and more muscular individuals have more water mass and as a consequence will reach a lower BAC than a smaller, less muscular individual who has consumed the same amount of alcohol. Individuals also differ substantially in the rate at which the liver metabolizes alcohol. For example, there is evidence that older individuals metabolize alcohol more slowly than younger individuals and chronic drinkers metabolize alcohol more rapidly than less frequent drinkers.

Generally speaking, a 160-pound man will reach a BAC of .02% (or 2 grams per 100 milliliters of blood) after one standard-sized drink (roughly one shot [1–1.5 ounces] of liquor, one 12-ounce beer, or one 5-ounce glass of wine). That same man will reach a BAC of .05%, .07%, .09%, and .12% after two, three, four, and five drinks, respectively, and will accordingly reach increasingly higher blood alcohol concentrations with successive drinks (assuming no time between drinks). A similarly sized woman will, on average, reach a higher BAC after the same number of drinks due to sex-specific differences in body composition.

Though the exact level of impairment at a given BAC varies from person to person, intoxication due to alcohol usually follows several stages associated with

different BAC levels. At low BACs (below .05%), alcohol can induce enjoyment, happiness, and euphoria characterized by increased sociability and talkativeness. Loss of inhibitions and reduced attention are also characteristic of this level of intoxication. At higher BACs (.06%–.10%), disinhibition is more apparent, as are impairments in judgment, coordination, concentration, reflexes, depth perception, distance acuity, and peripheral vision. Because these impairments can be dangerous in certain environments, many countries set the BAC at which a driver is considered legally impaired at either .05% or .08% (often lower for younger or less experienced drivers). In the range .11%–.30%, individuals experience exaggerated emotional states, including anger and sadness; they may also have a higher pain threshold, reduced reaction time, loss of balance, slurred speech, and moderate to severe motor impairment. At extremely high BACs (above .35%), individuals are likely to suffer from incontinence or impaired respiration, or they may lose consciousness and even die from respiratory arrest. For lower levels of BAC, many of the effects have been documented in controlled laboratory settings, particularly impairments of driving-related skills and tasks.

Laboratory experiments have been used extensively to estimate whether and to what extent acute alcohol consumption increases aggressive behaviors in humans.[3] In the most common experimental design, individuals are told they will be competing against a competitor in a different location, with the winner of each task choosing the severity of the electric shock that the loser receives. In reality, there is no competitor in these experiments, and the outcomes (i.e., whether the subject wins or loses the timed task) are predetermined. The magnitude and severity of the shocks chosen by the subjects provide

[3] Notably, the increases in aggression attributable to alcohol have also been documented in controlled experiments with mice and primates, suggesting a fundamental biological link between alcohol and aggression.

a measure of aggression. The subjects are told either that they will be served an alcoholic beverage or that they will be served a non-alcoholic beverage. Individuals in each group are randomly served an alcoholic beverage or a non-alcoholic beverage. Comparing the level of the electric shocks chosen by the people in the four groups allows researchers to separate the effects of alcohol from the effects due to alcohol-based expectations. These experiments typically find that people who consume the beverage with alcohol, whether they were expecting it or not, choose to give larger shocks than those that did not get alcohol.[4]

3. Possible Causal Pathways and Mechanisms from Alcohol to Crime

In this section we examine several of the causal pathways through which alcohol consumption leads to crime (Fagan 1990, Pernanen 1981).[5] Most public policies targeting alcohol consumption have the potential to influence more than one causal pathway, making it challenging to determine exactly how important each pathway is. As a result, it is not possible to rank them perfectly; however, we discuss the pathways in what we believe is roughly their order of importance: (1) direct pharmacological effects

[4] It is worth noting that the majority of the experiments examining how alcohol affects aggression have studied either college undergraduates or alcoholics, somewhat limiting the generalizability of the findings. Also, the dose-response relationship observed between alcohol consumption and aggression is highly non-linear: although through much of the range of BAC alcohol increases aggression, at very high levels it results in sedation. Finally, the pharmacological effects of alcohol can differ markedly from the effects of other drugs; we discuss this below in Section 4.

[5] It is important to acknowledge that some sociologists and criminologists suggest that the links among alcohol, crime, and violence do not result from a causal mechanism of the type we describe here. Some common alternative hypotheses include the possibility that unobserved factors such as risk preference or taste for deviance cause both variation in alcohol consumption and variation in crime or that some income-producing crime is itself a cause (not a consequence) of alcohol and other drugs (Fagan 1993 and others). That these types of explanations would produce the same observed associations between alcohol use and crime as a true causal effect (defined below) is why we focus on studies that seriously address the possibility of omitted variables bias.

on aggression and cognitive functioning; (2) the “excuse” mechanism; and (3) the role of social interactions and venues.

Arguably the most direct pathway from alcohol consumption to crime is through its direct pharmacological effects. By increasing aggression and heightening emotional responses, acute alcohol use may cause increases in interpersonal violence, including murder, rape, robbery, and assault. And by reducing cognitive functioning and altering normal judgment and decision-making abilities, drinking may lead to alcohol-induced myopia or short-sightedness. Individuals so afflicted may engage in criminal activity because they fail to recognize the social and legal consequences of their actions. The pharmacological effects of alcohol may also have a causal effect on crime by increasing the risk of victimization. Excessively large doses of alcohol, for example, lead to sedation rather than aggression, which may make intoxicated individuals easy targets of various types of crime. And impaired cognitive functioning and decision-making may place individuals in situations where they are at increased risk of victimization. Unfortunately, for reasons outlined below it is difficult to disentangle alcohol's effect on crime commission from its effect on criminal victimization, and as a result very little research has been conducted on this important question.

Alcohol may also increase crime by providing an “excuse motive” for crime commission. It is conceivable that someone considering a criminal act could believe that being inebriated would lessen the punishment for a crime if the case came to trial. Alcohol consumption may also lead a person to justify antisocial activities to himself or the people around him. Policies that succeeded in reducing alcohol availability could remove the “excuse motive” in some instances.

Alcohol consumption may also increase the incidence of interpersonal violence by increasing social contact, which again may be relevant both for crime commission and for criminal victimization. A closely related mechanism is venue: alcohol regulations can dictate the location and setting of alcohol use either directly (e.g., through bar closing hours or licensing of on-premises outlets where alcohol is consumed at the same location after purchase) or indirectly (e.g., by lowering the drinking age and thereby increasing the number of public venues where youths can legally consume alcohol). When people consume alcohol in public places such as bars they will have more interactions than they would if they had stayed home. Even in private venues alcohol is often enjoyed in social group settings. Indeed, one of alcohol's pharmacological effects is to make individuals more talkative and outwardly social in the short term. Alcohol use may therefore increase the number of interpersonal interactions at risk for a criminal incident, and in public venues those interactions are more likely to be with strangers and involve negotiations over personal space, further increasing the risk of a violent conclusion to the interaction.

Most of the studies of the impact of alcohol regulations are ecological studies, which find that regulations can plausibly impact more than one of the mechanisms described above. Because the individual effect of each mechanism is difficult to determine, it is also difficult to predict how a new regulation that targets only one mechanism is likely to affect crime rates. However, the reduced-form findings are still of substantial value since most new policies are likely to have characteristics similar to those of existing policies.

4. Alcohol and Crime versus Other Drugs and Crime

Our focus here is on the relationship between alcohol regulations and crime as distinct from the relationship between the regulation of illicit drugs (such as marijuana, methamphetamines, heroin, and cocaine) and crime, which is the focus of other work in this volume (see chapter by Pollack and Reuter). One reason for this distinction is that the production, purchase, sale, and distribution of alcohol are not criminogenic in the same way as the production, purchase, sale, and distribution of illicit drugs such as opiates and cocaine. Because marijuana and hard drugs are illegal, a substantial portion of the crime associated with these drugs is caused by the dangerous nature of the illicit markets. Evaluating policies that target illegal drugs is therefore even more complex than evaluating policies aimed at alcohol abuse.

Another reason to distinguish alcohol from other drugs when considering the effects of their consumption on crime is that the pharmacological and behavioral effects of alcohol differ significantly from those of the most commonly consumed illicit drugs. While several laboratory studies and reviews of the literature (described above) have documented a causal effect of alcohol consumption on aggression and disinhibition, controlled experiments in animals and humans that examine other substances indicate a range of behavioral effects. Probably the closest to alcohol in its pharmacological effects is cocaine, which has similarly been shown to increase aggression, reduce self-control, and increase irritability (Washton 1987). Amphetamines can also produce an increase in aggression; however, unlike the aggression induced by alcohol it is sometimes accompanied by a paranoid psychotic state that independently may contribute to violent acts. In contrast, marijuana has generally been found to inhibit (rather than promote)

aggressive behavior in humans, mice, fish, and primates (Miczek et al. 1994). Similarly, opiates have been shown to decrease aggressive behavior and hostility in animals and humans, though the period of opiate withdrawal is usually characterized as increasing risk for aggressive behaviors. Thus alcohol has a pharmacological profile that is significantly different from that of the most commonly consumed illicit drugs.

The differential pharmacological effects of alcohol and other drugs on human behavior raise a potentially important issue regarding the role of alcohol regulation and crime control. Specifically, it is possible that alcohol use is related to the use of other drugs in an underlying structural way. Specifically, if alcohol and other drugs are complements in consumption, then an increase in the price of alcohol (through, for example, stricter regulations) will reduce not only drinking (through the own-price effect) but also use of other drugs (through a cross-price effect). In contrast, if alcohol and other drugs are substitutes in consumption, then an increase in the price of alcohol will reduce drinking but will lead to an increase in the use of other drugs. Existing research is mixed on this question and has focused primarily on the study of marijuana. While some studies find evidence that alcohol and marijuana are substitutes in consumption (DiNardo and Lemieux 2001; Conlin et al. 2005), others find that the two are complements (Pacula 1988).[6] The relationship is potentially important because some research has suggested a

[6] DiNardo and Lemieux (2001) use variation induced by state drinking age changes (described in more detail below in the review of age-based alcohol restrictions) and find that exposure to a more restrictive drinking age significantly reduced alcohol consumption by high school seniors but significantly increased marijuana consumption, suggesting that alcohol and marijuana are substitutes in consumption among youths. Conlin et al. (2005) use changes between "wet" and "dry" status from local prohibition referenda in Texas in a quasi-experimental framework and find a significant inverse relationship between alcohol availability and illicit drug-related crimes, suggesting that alcohol and illicit drugs are substitutes. Pacula (1998) uses data from the National Longitudinal Survey of Youths and finds that increases in the beer tax reduce both drinking and marijuana use among young adults, suggesting the two goods are complements in consumption.

direct causal effect of marijuana on the commission of income-producing and property crimes (Pacula and Kilmer 2003).[7]

Why might this matter? The vast majority of the empirical research reviewed below relies on estimating reduced-form relationships between alcohol control policies and crime. If alcohol control policies influence both the consumption of alcohol and illicit drugs, we will observe the total or net effect of these two mechanisms.

5. A Critical Evaluation and Summary of Research on the Effects of Alcohol Regulations on Alcohol Consumption and Crime

A key reason that alcohol control merits attention as a possible crime control strategy is that access to alcohol is highly manipulable by public policy through various types of regulations. Indeed, much of the research we review compares the effects of regulations across areas and changes within areas over time as a way of identifying alcohol's causal role in the commission of crime. We group our review by types of alcohol regulation, which broadly correspond to different types of research designs that have been used to identify the effects of alcohol on crime. These include: excise taxes on alcohol; age-based restrictions such as minimum legal drinking ages; spatial restrictions on alcohol outlet density and availability; temporal restrictions on alcohol sale; and other

[7] Pacula and Kilmer (2003) use ADAM and UCR data to estimate fixed-effects models of crime with controls for alcohol, cocaine, and marijuana prices. They find that higher marijuana prices (which should be associated with lower marijuana use) were associated with lower rates of income-producing and property crime but not violent crime, which is consistent with pharmacological evidence suggesting that marijuana decreases aggression in the short term. As noted above, there is less research addressing whether alcohol and illicit drugs other than marijuana are substitutes or complements. Again, this may be important because some previous research suggests a causal effect of cocaine consumption on crime. Desimone (2001), for example, uses data from 29 large cities in the period 1981-1995 and instruments for the endogeneity of cocaine prices with wholesale supply factors and retail enforcement intensity and finds a significant negative association between the price of cocaine and every index crime except assault. His results suggest that there are independent causal effects of cocaine use (i.e., consumption) on crime apart from effects on criminality associated with the production, sale, and distribution of the drug.

"circumstance" regulations that combine elements of spatial and temporal alcohol availability restrictions. We discuss each of these—and the relevant literature employing each design—in turn.[8]

5.1 Regulations on the tax/price of alcohol

Economists studying the relationship between alcohol and crime have largely focused on alcohol control policies that change the full price of alcohol either directly (through alcohol excise taxes) or indirectly (through other non-price availability restrictions). In this section we review and evaluate studies that have leveraged variation in excise taxes to identify alcohol's role in crime. Studies of this variety have natural appeal to economists because they are firmly grounded in economic theory: a tax-induced increase in the price of alcohol should reduce alcohol consumption by the law of demand. Moreover, there is a great deal of variation across states and countries in alcohol excise taxes, and there is also some variation within areas over time, which allows estimation of more credible fixed-effects models of the effects of taxes on alcohol prices, drinking, and crime. Indeed, previous research confirms that alcohol taxes are passed through to prices (a necessary condition for taxes to affect alcohol consumption and crime). Young and Bielinska-Kwapisz (2002) and Stehr (2007) both use quasi-experimental approaches to document that taxes are fully shifted to prices by matching tax information to commonly used local price data from the ACCRA (the American Chamber of Commerce

[8] In addition to these categories, there are others that may prove fruitful in future research, including: restrictions on alcohol advertising, alcohol education regulations in schools, requirements that parolees abstain from alcohol, restrictions on price promotions and other point-of-sale regulations such as "Happy Hours," laws that assign liability to bar owners for serving intoxicated persons, alcohol-involved driving regulations, fines and penalties for alcohol violations, and others. To our knowledge these regulations have not been studied extensively in the context of crime or violence outcomes (other than crime and violence related to driving under the influence, which is substantial but not in the scope of our review), so we do not discuss them here.

Researchers Association, now known as the Council on Community Economic Research, or C2ER). Kenkel (2005) also finds that taxes are more than fully shifted to prices using original survey data from before and after a large alcohol tax hike in Alaska. Finally, a focus on alcohol taxes has substantial policy relevance as several states have debated and implemented significant increases in alcohol taxes in the past few years.

There is a now enormous body of evidence showing an inverse relationship between alcohol taxes and various measures of alcohol consumption; it has been reviewed extensively elsewhere (Chaloupka et al. 2002; Cook and Moore 2001; and others). Some of this evidence comes from cross-sectional studies that use tax-induced price variation (Grossman et al. 1987, 1994; Coate and Grossman 1988; and others), but some of the earliest work on this topic used more credible panel data evaluation methods. Cook and Tauchen (1982), for example, showed that increases in state liquor taxes significantly reduced mortality from cirrhosis of the liver, a common proxy for chronic heavy drinking. This study effectively introduced the two-way fixed effects design to studies of alcohol control policies, and it has become the standard for these types of evaluations. Although some research has called the relationship between state beer taxes and alcohol consumption into question, particularly for young adults (see, Dee 1999; Mast et al. 1999; and others), other recent research has confirmed that tax-induced price increases for ethanol are associated with decreases in drinking (Cook and Moore 2001; Carpenter et al. 2007; and others).

Several studies in economics by Markowitz and colleagues have used the inverse relationship between alcohol taxes and consumption to estimate models of the effect of alcohol consumption on violence by using alcohol taxes as instruments or by directly

estimating the reduced-form association between alcohol taxes and individual measures of violence. Markowitz and Grossman (1998), for example, using one cross section of data from the 1976 National Family Violence Survey, found that state excise taxes on beer were significantly negatively related to the probability of child abuse. In a related study Markowitz and Grossman (2000) added data from the 1985 wave of the same survey, which allowed them to estimate the sensitivity of the beer tax estimates to the inclusion of state fixed effects (thus identifying the effects of beer taxes from changes in state tax rates). They found that beer taxes were negatively related to child abuse committed by women, but these results were only statistically significant when state fixed effects were excluded. These studies do not include direct information on alcohol consumption, so the first stage relationship cannot be directly tested. Markowitz (2000b) also examined spousal violence using the 1985, 1986, and 1987 waves of the National Family Violence Survey. In models with individual fixed effects, she estimated that a 1% increase in price would reduce abuse aimed at wives by 5.34%. Markowitz (2005) analyzes panel data on individuals from the 1992, 1993, and 1994 National Crime Victimization Surveys. She finds that higher beer taxes have a (marginally) significant inverse relationship with physical assault but no substantive relationship with rape/sexual assault or robbery. Markowitz (2001a) uses data from the 1989 and 1992 cross sections of the International Victimization Survey. Controlling for the country-specific price of alcohol, she finds that these prices exhibit significant negative associations with the rates of assault, robbery, and sexual assault against women in the cross-section but that the associations are no longer statistically significant when country fixed effects are included in the regressions.

Markowitz and colleagues have also used similar strategies to relate beer taxes to violence among youths and young adults. Markowitz (2001b), for example, uses data from the 1991, 1993, and 1995 Centers for Disease Control and Prevention's Youth Risk Behavior Surveys (YRBS) to examine the relationship between alcohol use and the probability of being in a physical fight or carrying a weapon. An advantage of the YRBS data is that they include information on youth drinking participation and heavy episodic drinking. Using beer prices as instruments for youth drinking, Markowitz estimates that alcohol consumption significantly increases the probability of being in a physical fight but does not affect weapon-carrying behavior. However, the first-stage and reduced-form relationships are estimated entirely from cross-sectional variation across states (net of state economic and religious characteristics). Finally, Grossman and Markowitz (2001), using data from the 1989, 1990, and 1991 Core Alcohol and Drug Surveys of College students, find that state beer taxes are inversely related to the probability that college students reported getting into trouble with the police, being involved in property damage, getting into a verbal or physical fight, and being involved in violence. They control directly for observed measures of anti-drinking sentiment in a state (such as religiosity) but do not include state (or other area) fixed effects in their models.

In a related analysis, using data on individuals age 12 and older from the 1991 National Household Survey on Drug Abuse, Saffer (2001) found that state beer taxes were significantly and inversely related to the probability of being arrested, the probability of engaging in property crime, the probability of engaging in property damage, and the probability of an individual using force to obtain something from someone. These effects were generally larger for youths under age 21. Apart from individual

demographic characteristics, however, no other state policies or economic/demographic characteristics were included in the model except for state drug control spending.

Arguably the most compelling direct evidence that higher alcohol taxes would reduce crime rates comes from a series of panel evaluations. The first of these was conducted by Cook and Moore (1993), who regressed state violent crime rates for the period 1979–1987 on state excise taxes on beer, state fixed effects, and year fixed effects. In this parsimonious specification, they found a significant inverse relationship between a state's beer tax and rates of rape and robbery (but not homicide or assault) within the state. They did not consider property or nuisance crimes, however, and they did not control for other state demographic characteristics or other relevant prices or policies. To the extent that these omitted characteristics were invariant within states over this period, however, the inclusion of state fixed effects largely shields Cook and Moore's 1993 study from these criticisms.

More recently, Desimone (2001) effectively replicates and extends the findings of Cook and Moore (1993), despite the fact that Desimone's focus is on the money price of cocaine, not alcohol. Using panel data on crimes in 29 large cities for the period 1981–1995, Desimone estimates fixed-effects models that include controls for the excise tax on beer in addition to a host of city-level demographic characteristics such as the local age structure, unemployment rates, per capita income, the fraction that is female, and the fraction that is racial and ethnic minorities. Like Cook and Moore (1993), he too finds that beer taxes are significantly negatively related to rape, and he also finds a significant negative association between beer taxes and rates of assault, larceny, and motor vehicle theft.

Finally, Matthews et al. (2006) and Sivarajasingam et al. (2006) study the relationship between beer prices and rates of injury-related violence using a panel of emergency department (ED) admissions over the period 1995–2000 in England and Wales. They estimated fixed-effects models of outcomes and found that higher beer prices were significantly associated with lower rates of violence-related injury as proxied by ED admissions, consistent with a causal relationship between alcohol prices and violence. These studies did not, however, address the mechanisms that drive regional or temporal price variations in beer.

Critiques of the literature on tax/price restrictions

Although the literature on alcohol taxes and criminal outcomes is extensive, most studies focus exclusively on violent crimes, so much less is known about the impact of alcohol taxes on nuisance or property crimes. In addition, these studies cannot disentangle crime commission from criminal victimization since taxes should theoretically affect consumption among both populations.

A more serious problem is that over the 1980s and 1990s (the period studied in most existing research) there were very few changes in state alcohol tax rates, making it difficult to precisely measure the effect of taxes on drinking and crime using models with fixed effects. Therefore much of the research using taxes to identify the effects of alcohol on crime—particularly the studies using individual level data to examine violence—has been cross-sectional, which raises standard concerns that unobserved factors associated with both the level of the state's beer tax and the state's crime rate cannot be ruled out as an alternative explanation. This critique has been previously

articulated by Dee (2001) and others in the context of youths. A related issue is that since alcohol taxes affect everyone within the taxed jurisdiction there is no clean way to define within-area treatment and control groups that would make it possible to difference out these possible biases.

Indeed, a central question for evaluating the usefulness of tax/price strategies is: what causes the observed variation in the level of alcohol taxes across space and time? Studies of taxes and prices may be biased if population preferences about alcohol control (or changes in these preferences) are correlated with tax rates (or changes in these rates). Arguably, these preferences are more likely problematic for evaluations of direct restrictions on spatial and temporal alcohol availability (reviewed below), in part because concern about alcohol-related problems directly underlies many of those policies. Taxes are somewhat shielded from this concern because they are often imposed as revenue-raising devices. Researchers must still be careful, however, because other determinants of crime rates such as resources available to police are also likely to be affected by budget shortfalls.

Overall evaluation: tax/price restrictions

Alcohol taxes in real terms have been falling steadily for decades, and economists commonly argue that alcohol taxes are "too low" (Manning et al. 1989; Cook 2007; and others). Economic theory predicts that higher alcohol taxes should reduce both alcohol consumption and crime, to the extent that crime is caused by drinking. Yet the evidence on the effects of alcohol taxes has been limited by the fact that there have not been many significant changes in the rate at which alcohol is taxed. As a result, most of the papers

in the literature on the effect of alcohol taxes on violence are identified off cross-sectional variation, so omitted variables bias is a serious concern. However, the two studies that leverage within-state variation find that alcohol taxes are negatively related to rates of various violent crimes. Several recent alcohol tax increases may be fruitful subjects for future panel evaluations of the effects of taxes on alcohol consumption crime and may help to shed light on previous debates in this literature about the importance of price.

5.2 Age-based alcohol restrictions

One of the most direct forms of alcohol regulation in the United States and elsewhere is the minimum legal drinking age (MLDA). Most economists view drinking-age policies as affecting the full price of obtaining alcohol. Studies of age-based restrictions benefit from several advantages. First, like alcohol taxes, they are highly policy-relevant. The country has been actively engaged in a debate about the appropriateness of an age-21 drinking age, led by the Amethyst Initiative, a group of college and university leaders who have called for a re-examination of U.S. minimum drinking age policies. Second, studies of drinking ages benefit from naturally sharp predictions about which groups of people should be affected by the policy in question: youths under the drinking age should have their consumption (and by implication, criminal activity) constrained by the MLDA, while youths at or above the drinking age should be largely unaffected. Third, drinking age studies benefit from numerous changes in the drinking age, which can be used to estimate the effects of age-based alcohol regulations. In the early 1970s, for example, all states had an MLDA of 21, but later that decade several states experimented with reducing the MLDA to 18, 19, or 20. In response to research showing that youth alcohol-

related fatalities increased following these age-based liberalizations, the federal government passed the 1984 Uniform Minimum Drinking Age Act, which required states to adopt an age-21 MLDA or risk losing 10% of their federally provided highway funds. By 1988, all states had returned to an MLDA of 21. Because states adopted lower and then higher drinking ages in a staggered fashion (i.e., different states changed their laws in different years), it is possible to leverage within-state variation in the drinking age for identification. Moreover, the age-based criteria for the laws allows researchers to use people too young or too old to be affected by the laws as a control group.

Given the strengths of age-based research designs, it is not surprising that the effect of these laws on alcohol consumption has been clearly documented. Multiple studies have shown that exposure to a lower drinking age increased both drinking participation and heavy episodic drinking. Using pooled cross sections of reports of alcohol consumption by high school seniors from the school-based Monitoring the Future (MTF) study of 1976–1992, Dee (1999) found that exposure to a permissive drinking age significantly increased drinking participation and heavy episodic drinking; this result was confirmed in analyses of other policies that also used MTF data (see, e.g., DiNardo and Lemieux 2001 and Carpenter et al. 2007). Cook and Moore (2001) found a similar result using data on young adults from the National Longitudinal Survey of Youths. Multiple studies have also shown that states' drinking age experiments were predictably associated with significant changes in alcohol-related traffic fatalities, which is partly what prompted the 1984 federal action (see Wagenaar and Toomey 2001 for a comprehensive review).

Two studies have documented that the state drinking age changes—in addition to affecting alcohol use—also affected crime. Using age-specific arrest data from the Uniform Crime Reports, Joksch and Jones (1993) found that increasing the drinking age reduced nuisance crimes and simple assaults among young adults in the affected age groups. Carpenter (2005) also found that drinking age increases were associated with significant reductions in arrests for nuisance crimes among youths in the targeted group. Taken together, these two studies suggest a causal relationship between alcohol availability, alcohol consumption, and the commission of some types of crime.

More recently, Carpenter and Dobkin (2009b) used a different approach to evaluate drinking ages and their effect on alcohol consumption and crime. Specifically, their regression discontinuity (RD) design uses the fact that the costs of obtaining alcohol fall discretely at the MLDA. Since all other observed and unobserved determinants of crime are likely to trend smoothly across the MLDA threshold, the observed changes in drinking and crime precisely at age 21 (net of birthday effects) can be used to identify the effect of easier alcohol availability on crime. Using alcohol consumption data from the 2001–2007 California Health Interview Surveys (which gives subjects' exact age in months at the time of they were interviewed), they find that drinking participation increases sharply at age 21 by about 30%.[9] They then use data on the universe of arrests in California for the period 2000–2008 (including subjects' exact age in days at time they were arrested) from the Monthly Arrest and Citation Register and find significant increases in arrest rates for nuisance and violent crimes precisely at age 21. Assaults accounted for most of the sharp increase in arrests for violent crimes exactly at age 21.

[9] In related work, Carpenter and Dobkin (2009a) also document a similar discontinuity nationally using data from the 1997-2003 National Health Interview Surveys.

The RD approach in Carpenter and Dobkin (2009) leverages an abrupt change in alcohol availability; as a result the estimates from this research design are less likely to be contaminated by population policy preferences than estimates from research designs that leverage statewide changes in policies regulating alcohol.

Critiques of the literature on age-based restrictions

Although studies of age-based restrictions on alcohol have several strengths, they also have limitations. First, there is limited evidence on enforcement of drinking age laws, either historically or present day. However, numerous researchers have documented the first-stage effect of age-based restrictions on alcohol consumption, somewhat easing this concern. Second, most of the studies using age-based alcohol regulations use arrest data instead of reported crimes data (since the age of the offender is typically not known). This raises the usual concern that alcohol use independently increases the probability of being arrested if one has committed a crime, thus resulting in an overstatement of the effect of age-based restrictions. The fact that these studies generally find effects for some crime types but not others mitigates this concern somewhat.

Third, the alcohol/crime relationship using variation in drinking age laws is complicated by the fact that individuals legally old enough to drink not only can obtain alcohol more cheaply, but also can obtain it in more places and venues (an additional dimension of the "full price" of alcohol). This means that it is difficult to distinguish whether it is the alcohol consumption, the increased social interaction with other potentially intoxicated individuals, or the interaction of the two that is the key causal

determinant in increased crime.[10] Since very few large-scale surveys in the United States ask detailed questions about the location and circumstances of alcohol use, these age-based studies usually cannot rule out the possibility that changes in both the quantity of alcohol consumed and the circumstances of consumption independently contribute to changes in the incidence of crime.

Finally, there are two related critical issues dealing with external validity and whether drinking age laws induce temporary displacements of drinking and crime outcomes or permanent reductions. Carpenter and Dobkin's RD design suggests that the increases in crime attributable to easier alcohol access at age 21 persist through at least age 23, though extrapolation away from the discontinuity for the purposes of informing policy is particularly difficult in this setting. Indeed, one of the most salient challenges to age-based designs is that their key advantage—tight information about which individuals should and should not have been affected by the restrictions according to single year of age (in the panel evaluation design) or exact age (in the RD design)—comes at a cost: questionable generalizability beyond young adults. That is, it is not obvious that a proportional reduction in alcohol consumption at age 15, 35, or 50 would have the same effects on crime as indicated in the drinking age studies (whose focus is ages 18–21). The weight of this limitation, however, is somewhat mitigated by the fact that the age profiles for both drinking and crime peak in late adolescence and early adulthood. This

[10] This limitation is less salient for the evidence on property and nuisance crimes, since they are less likely to involve interpersonal interactions.

suggests that though estimates are specific to young adults, they are still of substantial general interest.[11]

Overall evaluation: age-based alcohol restrictions

In the 1970s and 1980s many states lowered their drinking age to 18 or 19 before raising it back to 21; the minimum legal drinking age in every state has been set at 21 since then. The literature has produced a great deal of evidence that lowering the drinking age would increase youth drinking, and results from both panel evaluations and a regression discontinuity design indicate that lowering the drinking age would also increase several types of crime, including violent crime, committed by young adults.

5.3 Place-based restrictions on the availability and density of alcohol outlets

There are numerous studies in the addiction, criminology, and public health literature that have found a strong spatial relationship between alcohol and crime. If these correlations are due to an underlying causal relationship between alcohol availability and crime, then local restrictions on the availability of alcohol–such as prohibiting the sale of alcohol in residential zones or within a certain radius of schools–may be effective crime control tools.

Studies of this type typically use sophisticated geographic information system (GIS) methods and very detailed data on locations of alcohol outlets and the places where crimes are committed to estimate spatial correlations. Since these studies typically focus on crime committed in very small areas around where alcohol is available, concerns

[11] Note that if age-based alcohol control policies have longer-term effects on drinking outcomes (as suggested by Norberg et al. 2009 and Kaestner and Yarnoff 2009), then there may also be an additional rationale for age-based restrictions.

about ecological inference problems are mitigated. Also, some of the studies allow disaggregation by type of alcohol outlet (e.g., liquor store versus bar) to provide more detailed evidence on which types of outlets are more strongly associated with crime. The pharmacological evidence reviewed above suggests that on-premises outlets should be more strongly related to aggression-related crimes than off-premises outlets in the presence of a true causal effect.

Most of the early research using spatial variation relied on cross-sectional variation in alcohol outlets in particular cities. One of the earliest and most often cited studies of this kind was conducted by Scribner et al. (1995) using detailed information on the location of assaults in 75 communities in Los Angeles. They found that one additional alcohol outlet was associated with 3.4 additional violent assaults (from a base of 570). In a related study, Scribner et al. (1999) examined 155 neighborhoods in New Orleans and found that a 10% increase in off-premises alcohol outlets was significantly associated with a 2.4% higher homicide rate.[12] Livingston (2008) performed a similar study for 223 neighborhoods in Melbourne, Australia, and found statistically significant relationships between general alcohol outlet density and assault and between on-premises alcohol outlet density and assault, but other research that has disaggregated alcohol outlets by type has reached different conclusions.[13] A 2006 study by Gruenewald and colleagues, for example, examined California hospital admission for assaults and found that assaults were more common in areas with many alcohol outlets that required off-premises consumption such as liquor stores than in areas with many outlets where alcohol

[12] Not all such studies have found similar effects, however. Gorman et al. (1998) perform a similar analysis for New Jersey and find no spatial relationship between alcohol outlets and violence.

[13] We review only a handful of representative articles from the public health and criminology literature here. For others see, for example, Lipton and Gruenewald 2002; Roncek and Maier 1991; Zhu, Gorman, and Horel 2004; Gorman et al. 1998.

is consumed on the premises, such as bars. In fact, Gruenewald et al. (2006) found that bar density increased the assault rate only in low-income poor communities and rural communities, but not in stable, wealthy communities.

The relationships between space-based restrictions, alcohol consumption, and crime-related outcomes have also been studied extensively in the context of college students. Multiple studies have used the Harvard College Alcohol Study (CAS)—a large, nationally representative survey of college youths' risk behaviors related to alcohol use—to show that proximity of the campus to alcohol outlets is significantly related to drinking participation and heavy episodic drinking (see, e.g., Chaloupka and Wechsler 1996; Weitzman et al. 2002). Wechsler et al. (2002) fielded a telephone survey of households around college campuses with varying rates of binge drinking. They found that people living near college campuses with high binge-drinking rates reported significantly more nuisance crimes such as drunkenness, vandalism, public urination, and other disorderly conduct than people living farther away.

Of course, a significant issue with these cross-sectional analyses of spatial variation in liquor outlets is that unobserved local factors that contribute to both crime and alcohol outlet density may bias the estimates presented in these studies. Researchers have tried to address this challenge in a variety of ways. The most common approach is to include tract-level controls for multiple observable dimensions of local neighborhood quality. Adjusting for observable differences between neighborhoods in this fashion may reduce the problem of omitted variables bias.

An alternative way to address the problem that neighborhood characteristics are probably correlated with both liquor outlet availability and crime is to identify an

instrumental variable that affects liquor availability but does not directly affect crime (i.e., a variable that only affects crime through its effect on liquor outlets). Gyimah-Brempong (2003) uses the number of gas stations in a census tract as an instrument for alcohol outlet density in Detroit, with the idea that the fixed costs of operating an alcohol outlet are lower in commercial zones. He uses median rent as an additional instrument with the reasoning that higher rents will make it more expensive to operate an alcohol outlet. He finds that both uncorrected and corrected specifications suggest a strong positive relationship between alcohol outlets and property and violent crimes. A difficulty with this approach is that it is not clear that the instruments satisfy the necessary exclusion restrictions for valid identification.

A third approach for addressing concerns about omitted variables bias is to examine how the opening and closing of alcohol outlets affects crime rates. Teh (2008) employs this type of strategy in an event-study framework using data on liquor outlets in Los Angeles. Her empirical specifications include area fixed effects, which ease concerns about the time-invariant characteristics of neighborhoods that might affect both crime rates and the probability that a liquor store is located in the neighborhood. In her main specifications the effects of liquor availability are identified from liquor store openings and closings. She finds that both property and violent crimes increase immediately after an alcohol outlet opens, and these effects are larger in the immediate vicinity of the outlet and in low-income neighborhoods.

Critiques of the space-based literature on alcohol outlets

Despite these innovations for addressing the evaluation problem in the context of spatial alcohol availability and crime, several challenges remain. First, it is not obvious that the designs used in this literature adequately address concerns about omitted variables bias. As noted above, the cross-sectional comparisons are vulnerable to omitted variables bias. Even the panel and interrupted time-series designs in this literature may still produce biased estimates if changing population preferences toward alcohol contribute to political processes that generate spatial restrictions.

Second, the mechanisms underlying any crime reduction in these studies are not well understood. For example, spatial studies face the challenge that alcohol outlets may *attract* crime (in addition to or instead of causing it), so estimates from this design may overstate the benefits of reducing liquor store or bar density. Similarly, it is difficult to disentangle whether it is variation in alcohol consumption, variation in social interaction around liquor outlets, or the interaction of the two that causes crime. Also, little is known about how liquor store placement and alcohol outlet density affect alcohol consumption. Most survey data on alcohol use are not well suited for these types of detailed spatial analyses since surveys generally do not ask about the precise location of consumption. Studies have used data on the location of alcohol sales (Stevenson et al. 1999), though this may be a poor measure of the location of consumption, particularly for consumption away from the location where the alcohol was purchased.

Third, most studies in the spatial availability literature have focused on violent crime; a more comprehensive analysis of all crime types would be useful, particularly an examination of alcohol-related crimes (e.g., public intoxication) to strengthen the evidence that alcohol outlets actually increase consumption. Fourth, most studies lack

information on spatial dimensions of police enforcement. Such studies may produce biased estimates of the effects of, say, liquor outlets on crime if enforcement is systematically higher around liquor outlets (either at a point in time or over time in a way that is correlated with alcohol outlet openings and closings). Fifth, studies in this literature cannot disentangle whether alcohol use causes crime commission, criminal victimization, or both. Having an alcohol outlet in a neighborhood could plausibly increase the alcohol consumption of both perpetrators and victims, and there is no way to separately identify these two effects.

Finally, and perhaps most important, few of the studies in the spatial availability literature provide evidence on the critical question of whether the policies simply shift the location of alcohol consumption and crime or whether they actually affect the overall crime rate. While social welfare may be enhanced in the presence of consumption or crime displacement across space (if, for example, alcohol consumed and crime committed away from schools were less socially harmful than the same level of consumption and crime near schools), a full accounting of the costs and benefits of these policies requires a complete understanding of the total effects of the regulations on outcomes.

Overall evaluation: space-based regulations on alcohol outlets

The existing evidence suggests a link between alcohol outlets and some types of crime, but strong conclusions are not yet warranted because studies of spatial availability restrictions have yet to convincingly overcome key identification problems. Even the best studies in this literature do not establish that the opening of an alcohol outlet will

increase crime rather than just attract it. We conclude that the evidence is not sufficient to establish that spatial availability restrictions are effective at reducing crime.

5.4 Temporal restrictions on the hours and/or days of alcohol sales

In addition to explicitly space-based regulations, there are also several types of alcohol regulations that specify when alcohol can be purchased or consumed. These include laws that prevent Sunday sales of alcohol in the United States and Australia and Saturday sales in Scandinavia, and laws that regulate the hours of operation of venues where alcohol is consumed, such as bars and restaurants.

Studies of temporal availability restrictions have the advantage that there have been many changes in laws governing when alcohol can be sold. This allows researchers to implement fairly compelling pre-/post- and interrupted time-series designs, thus reducing some concerns about omitted variables bias to the extent that time-invariant unobservables are controlled for through the inclusion of location fixed effects. Since these types of policies are routinely debated in many jurisdictions, studies that can evaluate their efficacy are of particular interest.

Much of the research exploiting temporal variation in alcohol availability has focused on repealing prohibitions of alcohol sales, particularly restrictions on Sunday alcohol sales. Commonly termed "blue laws," these policies are still prevalent in many areas in the southern United States. In the past decade, however, several places have lifted their Sunday sales restrictions, in part to increase state revenue from sales taxes. Evidence suggests that such policy liberalizations do increase alcohol sales. Stehr (2008) uses aggregate data on beer, wine, and spirits sales to study the effect of various U.S.

states' repeal of Sunday sales restrictions. His fixed-effects estimates indicate that Sunday sales policies significantly increased spirits sales. Evidence that such policies affect actual alcohol consumption (as distinct from alcohol sales) is more limited, though Carpenter and Eisenberg (2009) show that Sunday sales liberalization in Ontario, Canada, increased Sunday-specific drinking by 7–15%, with some evidence of substitution away from consumption on Fridays and Saturdays.[14]

Evidence that these types of temporal restrictions on alcohol sales and availability reduce crime rates is fairly limited. Ligon and Thyer (1993) showed that a ban on Sunday alcohol sales reduced arrests for driving under the influence (DUI). McMillan and Lapham (2006), using data on day-specific traffic fatalities in New Mexico before and after its 1995 Sunday sales liberalization, found very large increases in Sunday fatalities, though more recent analyses have suggested far more modest effects (Maloney and Rudbeck 2009; Lovenheim and Steefel 2009). A related series of papers by Smith (1988, 1990, and others) showed that increases in the hours/days of availability in Australia were associated with increases in alcohol-related fatalities.

Some of the best evidence of the impact of temporal availability restrictions on crime rates comes from Scandinavia, where multiple policy changes make it possible to implement relatively compelling interrupted time-series designs.[15] Olsson and Wikstrom (1982) studied Sweden's short-term closing of its state monopoly liquor stores on Saturdays in the summer of 1981. They found that offenses related to drunkenness,

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[14] Gruber and Hungerman (2008) examine earlier repeals of blue laws in the United States that increased a variety of economic activities on Sundays (not just alcohol sales). While alcohol consumption was not the primary focus of their paper, they did find that blue law repeals increased drinking by 16 percent among individuals who had previously attended church services (and whose behavior would be most likely affected by the blue law repeal).

[15] In addition to the peer-reviewed studies on Scandinavian policy changes described here and in the next section, see the comprehensive treatment by Room (2002).

domestic disturbances, public disturbances, and assaults fell on both Saturdays and Sundays relative to other days of the week during the period of the experiment. Interestingly, they found no effects on outcomes related to vandalism, acute medical care, and road accidents, and there is also no evidence from this policy experiment that the reduction in Saturday availability reduced total alcohol consumption. These results raise the possibility of across-day substitution (e.g., a displacement of consumption and/or crime from weekends to weekdays) and suggest the need for more detailed data on the circumstances of alcohol consumption.[16]

In a series of studies examining the effects of Sweden's staggered adoption of longer Saturday retail hours in its monopoly-run liquor stores (first in a limited geographic area in February 2000 and then countrywide in 2001), Norstrom and Skog (2003, 2005) found that increased alcohol availability was associated with modest increases in sales of beer and spirits (but not wine) of about 3.6% during both phases of the experiment. The increased alcohol sales from the first liberalization (but not the second) were associated with significant increases in alcohol-involved driving, while assaults exhibited no significant change in either period. Norstrom and Skog argue that the increase in drunk-driving arrests that resulted from the 2000 policy change is at least partly attributable to changes in police enforcement. They also note that such modest changes in overall alcohol sales make it difficult to precisely estimate the relationship between alcohol sales and assaults. Norstrom and Skog did not examine whether their

[16] The lack of effects on overall alcohol consumption from modest changes in the days or hours of sale is a common finding in this literature. Duffy and Plant (1986), for example, studied Scotland's 1976 alcohol liberalization, which extended bar closing hours from 10 p.m. until 11 p.m. and allowed "public houses" (not just bars in hotels) to be open on Sundays. Using changes in alcohol-related outcomes in England and Wales as a control condition, they found no evidence that these temporal restrictions affected overall alcohol consumption or alcohol-related harm except for a reduction in public order offenses such as drunkenness.

outcomes exhibited cross-day substitution in response to the Saturday-specific increase in availability.

Finally, we review studies of laws that regulate the hours of alcohol availability. Chikritzhs and Stockwell (2002) studied later closing times for bars in licensed hotels in Perth, Australia, using data on assaults reported to the police. Over their 1991–1997 sample period, about a quarter of hotels applied for and were granted a permit to extend alcohol sales from midnight until 1 a.m. They found that, relative to hotels that were not granted such permits, hotels with extended trading hours exhibited significantly greater wholesale alcohol purchases (their proxy for increased alcohol consumption) and had significantly higher rates of assault in the immediate vicinity of the hotel. Vingilis et al. (2007) studied the effects of Ontario, Canada's 1996 extension of its bar closing hours from 1 a.m. to 2 a.m. They examined administrative data on hospital admissions for each hour of availability from 11 p.m. to 3 a.m. for the four years before and three years after the policy change. They found reductions in motor vehicle collision admissions between 11 p.m. and midnight and between 1 and 2 a.m. but no significant change for the 2–3 a.m. period, which they attribute to concurrent increased enforcement and road safety initiatives. For other types of injuries not related to motor vehicle collisions, they found significant increases for the 2–3 a.m. period.[17]

One of the strongest examples of this type of research used the adoption of mandatory bar and restaurant closing hours in Sao Paolo, Brazil. Biderman et al. (2009 forthcoming) use a difference-in-differences design to examine the effects of these "dry"

[17] A study related to research on bar closing hours is by Jackson and Owens (2009), who use the extension of late-night hours of Metro rail service in Washington, D.C., which allowed individuals to stay at bars and restaurants until those establishments closed. They show that a series of these policy changes predictably affected DUIs and alcohol-related crimes.

laws on violent crime. Between March 2001 and August 2004, 16 of 39 municipalities in the Sao Paolo metropolitan area prohibited alcohol sales during the late-night and early-morning hours; prior to this most bars were allowed to remain open 24 hours a day. In models with city and period fixed effects as well as controls for varying city/period demographic characteristics, police enforcement, and lags in the homicide rate, Biderman et al. found that adoption of the dry law was associated with a statistically significant reduction in both homicides and battery of about 10%.[18]

Changes in temporal restrictions are not uniformly effective, however, as evidenced by recent evaluations of another high-profile policy experiment regarding bar closing hours that was intended to reduce crime—England and Wales's Licensing Act of 2003—but that have not returned uniformly strong evidence of crime reductions (Hough and Hunter 2008; Humphreys and Eisner 2010; and others). That policy liberalized restrictions on bar closing hours and encouraged licensed premises to apply for rights to serve alcohol later than previously allowed. The rationale for the policy was to reduce the problems associated with a fixed, common bar closing time, which generally resulted in large numbers of inebriated individuals coming into close contact with each other in small spaces. Staggering bar closing hours, it was thought, would dramatically reduce crimes such as assaults that occur at very high rates when all bars close at once in the early hours of the morning. Results have generally shown no meaningful effects on either alcohol consumption or measures of violent crime (Hough and Hunter 2008). Some scholars have noted that because the law did not require staggered closing hours

[18] Biderman et al.'s (2009) results confirm the findings of an earlier single-city analysis of the Brazilian city of Diadema, which found that homicides fell significantly after the imposition of a mandatory 11 p.m. closing hour (Duailibi et al. 2007).

but rather made them voluntary, in reality there was rather limited variation in the treatment of interest (Treno 2010; Frattaroli 2010).

Critiques of the literature on temporal availability restrictions

Studies of temporal availability restrictions have several of the same limitations as the spatial availability research described above. While these studies arguably do a better job of dealing with the possibility of omitted variables correlated with both the temporal availability restriction and outcomes, one might still worry that changing attitudes towards alcohol will drive the adoption of these policies and also directly affect outcomes. Though Sunday sales liberalizations seem to be driven by a desire to increase state tax revenues, endogenous policy adoption may be an issue for the other studies examined in this subsection, such as the imposition of mandatory bar closing times in Brazil or liberalized/staggered bar closing times in England and Wales.[19] A related question about generalizability arises in examining changes in temporal availability restrictions: while a handful of studies have examined the effects of changes in the days and hours of alcohol availability on alcohol consumption and DUI outcomes in the United States, none of these has examined crime or violence more generally. The studies that do examine crime leverage policy changes in countries and time periods where the basic patterns of alcohol control and crime differ markedly from those in the United States.

There are other limitations as well. As with the spatial availability studies, the research on temporal restrictions provides almost no insight into how these policies affect the extent or conditions of alcohol consumption, as most evidence comes from imperfect

[19] Duailibi et al. (2007) note, for example, that the mandatory bar closing hour policy in Diadema was instituted because the mayor was concerned about the high murder rate, and police records showed that most murders occurred in or near bars.

proxies such as aggregate sales. And, like the spatial literature, most studies of restrictions on the days or hours of alcohol sales lack information on temporal dimensions of police enforcement. If enforcement is systematically lower during the periods when alcohol sales are restricted, the observed effect of a change in temporal availability restrictions on crime will be dampened. Studies in the temporal literature also cannot separately identify whether alcohol use causes crime commission, criminal victimization, or both. Finally, and again perhaps most important, few of the temporal availability studies provide compelling evidence on whether the policies simply shift the timing of alcohol consumption and crime or permanently reduce them.

Overall evaluation: temporal availability restrictions

Research examining temporal restrictions on the hours and days on which alcohol may be sold share many of the strengths and weaknesses of studies that exploit detailed spatial variation in the availability of alcohol outlets. These studies tend to examine a limited number of types of crime and do not address the broader question of whether temporal availability restrictions simply redistribute crime across different time periods or reduce it. We conclude that there is compelling evidence that temporal availability restrictions change the times at which crimes are committed, but that the evidence that they actually reduce overall alcohol consumption and crime rates is weaker.

5.5 Other "circumstance" regulations not elsewhere classified and indirect evidence on alcohol and crime

The preceding sections reviewed evidence from studies of alcohol regulations that were either spatial or temporal in nature. There are, in addition, other types of alcohol restrictions that combine elements of both. These "circumstance" regulations control the conditions under which alcohol is made available and include alcohol rationing and privatizations of government liquor sales monopolies (which generally lead to increases in alcohol availability through increases in outlet density and reductions in prices). In this section we also review a handful of studies that do not explicitly evaluate alcohol control policies but do provide important and useful evidence on the nature and extent of the causal links between alcohol availability, alcohol consumption, and the commission of crime.

One type of "circumstance" regulation that has been studied in the context of crime is drunk-driving policy. Specifically, Carpenter (2008) examined the reduced-form relationship between age-targeted “zero tolerance” (ZT) drunk-driving laws and age-specific crime (as proxied by age-specific arrests).[20] ZT laws were adopted by every U.S. state during the 1990s; they make it illegal for youths under age 21 to drive with *any* alcohol in their blood (the relevant standard for adults in most states at the time was a BAC of 0.08 or 0.10); thus, these laws dramatically lowered the relevant BAC threshold for youths under age 21 but had no independent effect on the legal environment for young adults over age 21. Carpenter (2008) showed that adoption of ZT laws, which reduced youth drinking among 18–20 year-olds by 13% (Carpenter 2005), also reduced property and nuisance crime arrests among young adults age 18–20 by 5% but had no

[20] Alcohol-involved driving regulations may be useful for future studies of alcohol and crime, but we are not aware of any research that has used them in this way other than that by Carpenter (2008).

effect on arrests among slightly older adults age 22–24 who were unaffected by ZT laws. He did not find significant effects on violent crime.

Another type of circumstance regulation is a local prohibition on alcohol (i.e., whether a city or county is “wet” or “dry”). Such restrictions remain especially common in the southern United States. Conlin, Dickert-Conlin, and Pepper (2005) studied the effects of changes in local prohibitions at the county level in Texas using data from 1978 to 1996. In fixed-effects models with county and year fixed effects, they found that a county's movement to “wet” status *decreased* illicit drug crimes (and mortality associated with illicit drug use). They interpret this as evidence that alcohol and illicit drugs are substitutes.

Several studies have examined large-scale reductions in alcohol consumption associated with major world or country-specific events. During Russia's anti-alcohol campaign of 1985–1988, alcohol was banned in public places, prices quadrupled, and state production and sale of alcohol was dramatically limited. Over this period, population alcohol consumption fell by 25%, and the homicide rate fell by about 40% (Shkolnikov and Nemtsov 1997). Lenke (1982) reports on a series of studies in Scandinavia that have suggested a strong link between availability and crime using historical records. First, he shows that alcohol rationing as part of a broader food shortage during World War I resulted in a two-thirds decline in alcohol consumption. This was soon followed by significant reductions in assault but overall increases in other types of crime. Lenke also reports on the end of Sweden's rationing system in 1955, which led to a sharp increase in alcohol consumption of about 25% and a more modest increase in assaults (8%). In another study, Lenke (1982) reports on a policy experiment

that introduced "middle-strong" beer into general food stores in 1965. Prior to 1965, beer above a certain alcohol content could only be sold in government-run monopoly stores. From 1960 to 1970, alcohol consumption increased in Sweden by 40% and assaults increased by 50–100%, with particularly large increases among youths and in rural areas. Lenke reports similar patterns from a closely related policy experiment in Finland in 1969. Notably, however, when Sweden ended the sale of "middle-strong" beer in general food stores in 1977, alcohol consumption fell only modestly—on the order of 10%—and assaults did not fall. These studies do not include explicit control groups, raising some concerns that the estimates may be biased.

Temporary suspensions of alcohol availability associated with labor strikes affecting monopoly-run alcohol outlets have also provided strong evidence of a link between alcohol availability and crime. Lenke (1982) reports on a three-month strike at the state monopoly liquor stores in Sweden in 1963. During the strike (when alcohol was much more difficult to obtain), aggravated assault fell. There were similar reductions in Finland during strikes by workers at state liquor stores in 1972, which caused an abrupt and large reduction in alcohol availability. Over this period, it is estimated that alcohol consumption fell by 30%, with concomitant reductions in public drunkenness, resisting arrest, disturbing the peace, and aggravated assault. Inflicted injuries as measured by admissions to emergency clinics for murder, manslaughter, assault, and battery also fell sharply coincident with the reduced availability (Makela 1980; Karaharju and Stjernvall 1974; Takala 1973). Similar strikes in Norway's monopoly liquor stores in 1978 also reduced alcohol consumption by 5–10% and reduced domestic violence (Horverak 1981, as reported in Room 1983). Although these circumstances were not brought about by

intentional policy manipulations, the studies do provide strong evidence that liberalizing alcohol availability through increased retail availability would increase alcohol consumption and crime.

Finally, we note that several studies have shed light on a possible link between alcohol and crime while not directly studying alcohol control or alcohol consumption. Dahl and DellaVigna (2009), for example, find that the release of violent movies is associated with reductions in violent crime during evening hours; they hypothesize that movie-going may displace alcohol consumption, which would result in fewer acts of violence if alcohol use causes violent behavior. Rees and Schnepel (2009) examine the relationship between college football games and crime, finding that cities hosting Division I-A football games experience sharp increases in arrests for assault, vandalism, and disorderly conduct on game days. Importantly, they find some of the largest effects on liquor law violations which, combined with anecdotal evidence, suggests that alcohol may play a causal role in game-day crime. Card and Dahl (2009) study the effects of emotional cues on domestic violence; they examine the days on which professional football games are played and show that unexpected home-team losses significantly increase acts of domestic violence. Again, alcohol use may be one mechanism contributing to this effect. These and other studies suggest alcohol is a substantial cause of violence and that situation-specific regulations may be effective at reducing alcohol consumption and some of the crime it generates.

6. Common Limitations and Proposed Future Directions

While each of the literatures we have reviewed above has specific strengths and weaknesses, there are also some broad gaps in the literature on alcohol control policy and crime that are worth mentioning in the hope of spurring researchers to address them.

One of the most serious gaps in the literature is that little is known about how much and in what ways alcohol regulations affect alcohol consumption. Understanding how regulations affect the levels, patterns, and circumstances of alcohol use would be of substantial value. For example, information on how each regulation affects total drinking, binge drinking, and drinking by high-risk groups would provide more insight into the mechanisms through which the policies reduce crime (if indeed they do) and would subsequently allow policymakers to craft more effective regulations. An additional benefit of having estimates of how different policies affect the level of alcohol consumption by different groups (e.g., heavy versus light drinkers) is that we would be able to better characterize specific policies' social costs. Existing evidence suggests that even "heavy" and "problem" drinkers respond to price (Cook and Tauchen 1982), though which groups are particularly sensitive to tax-induced price changes is not well documented.

Another limitation of the literature reviewed here is that it is focused largely on homicides and other violent crimes such as rape and assault, presumably because of the strong pharmacological links between alcohol and aggression. Property crimes (e.g., larceny, motor vehicle theft), drug crimes (e.g., possession, sale, use), and social nuisance crimes (e.g., vandalism, disorderly conduct, prostitution) have received much less attention, perhaps because they are considered less serious. However, these crimes are

far more common than violent crimes, so failing to examine them closely may lead us to miss a substantial part of the social costs of alcohol consumption.

A final shortcoming of the entire literature on alcohol and crime is the disproportionate focus on alcohol's role in the commission of crime as opposed to criminal victimization (with the exception of homicide victimization, for which there is a substantial public health and criminology literature). The lack of research attention to how alcohol affects the probability of criminal victimization is probably due in part to the lack of high-quality victimization data. The National Crime Victimization Surveys, for example, provide some of the best data for studying criminal victimization, but they do not include geographic identifiers. It is not possible therefore to conduct state/year panel evaluations of the effects of alcohol control policies on criminal victimization. Yet even with credible victimization data there is another methodological and conceptual challenge for understanding how alcohol regulations affect criminal victimization: specifically, most alcohol control policies probably affect the alcohol consumption of both criminals and their victims, but it is impossible to distinguish between these two causal mechanisms without very rich individual-level data. Beer taxes, for example, likely affect drinking among both individuals committing crime and their victims, as do spatial and temporal restrictions on alcohol availability.

There are at least two recent encouraging developments for researchers interested in the important question of how alcohol affects crime. The first is that excellent data sources such as the National Epidemiologic Survey on Alcohol Related Conditions (NESARC) are becoming more broadly available. The NESARC is a two-wave panel (completed in 2001 and 2004/05) of the alcohol consumption behaviors of over 40,000

adult residents of the United States performed by the National Institute on Alcohol Abuse and Alcoholism (NIAAA). Limited-access data files with geographic identifiers will be made available to researchers. These data are likely to be particularly useful for understanding the settings in which alcohol is consumed because the survey includes detailed questions about the location, timing, and frequency of recent alcohol consumption. Most U.S. surveys such as the National Health Interview Survey contain far less detailed information on alcohol consumption and/or lack a longitudinal component. Another useful dataset increasingly being used by economists is the National Incident Based Reporting System (NIBRS). It contains detailed information on crimes known to the police (not just crimes resulting in an arrest) and includes among other variables one that records if the crime involved alcohol. With this information, NIBRS can support state/year panel evaluations of alcohol control regulations on various measures of crime and violence, including incidents of criminal victimization. Finally, we are encouraged by the increased availability of large administrative datasets at the state and federal levels on mortality, arrests, and hospital and emergency room admissions; they broaden the set of outcomes that researchers can examine in the contexts of crime and violence.

A second encouraging development is the substantial number of recent state- and province-level policy changes. Some of the policy changes are the consequence of the recent economic downturn, which seriously diminished state budget coffers and led many states to increase excise taxes on beer, wine, and spirits as revenue-raising devices. For example, the Tax Foundation lists 19 legislated increases in state beer taxes in the period 2002–2008, including several very large tax hikes (e.g., in Alaska from 35 cents to $1.07

per gallon in 2002); in the entire prior decade there were just 15 increases (and most of these were small). These changes are particularly welcome for analyses of the effects of beer taxes, since one limitation of research in this area is that there historically have been too few large-scale state-specific tax increases to get precise estimates from panel regression models (Dee 1999). Budget shortfalls have also led 12 states since 2000 to repeal longstanding restrictions on Sunday alcohol sales (Stehr 2007). The staggered timing of adoption of these policies at the state level should prove useful for understanding not only how these regulations affect alcohol consumption but also how they affect crime. Finally, a handful of U.S. states and Canadian provinces have begun to privatize their systems of liquor control. Iowa, West Virginia, and Alberta did so in the late 1980s and early 1990s with dramatic effect on alcohol sales (Holder and Wagenaar 1995; Cook 2007). More recently, British Columbia undertook a partial privatization that has resulted in very large increases in alcohol availability; when combined with detailed information on consumption, violence, and crime, this should provide useful new insight into both the structural relationships underlying alcohol and crime as well as direct policy guidance for other states, such as Virginia, that are actively debating privatization (Stockwell et al. 2009).

In summary, the increased availability of high-quality survey and administrative data and the numerous recent policy changes suggest that future work on alcohol regulations, alcohol consumption, and crime can address some of the limitations that are common to the now substantial literature on this important and complex topic.

7. Policy Options and Economic Considerations

We conclude our review with a discussion of the economic considerations that are relevant when considering whether alcohol regulations should be implemented in an attempt to reduce crime. Doing so requires us to identify the main feature distinguishing the public health perspective from the economic perspective: the latter takes into account the valuation of the utility loss borne by moderate drinkers whose responsible alcohol consumption is not criminogenic. In a typical policy analysis from a public health perspective, these utility losses are not included in the cost/benefit calculations of alcohol control policies. Instead, the value of the crime reduction (as measured by the direct dollar values of reduced property and nuisance crimes and the present value of the stream of increased quality-adjusted life years gained from reducing crime) is compared with the direct costs of the stricter regulation (e.g., the personnel and administrative costs associated with increased enforcement of alcohol control regulations). The economic approach, however, recognizes that adoption of stricter alcohol control policies for the purposes of crime reduction imposes deadweight loss on moderate, responsible consumers. Higher taxes, for example, may reduce alcohol consumption by people whose drinking leads them to commit crimes, but may also reduce the consumption by law-abiding drinkers. Since the majority of the population consumes alcohol and does so in a responsible way, the foregone value of alcohol consumption by this group should not be easily dismissed.[21]

The situation is further complicated by the fact that alcohol may confer nontrivial benefits to drinkers and to society. There is a large body of medical research suggesting

[21] Cook (2008), however, argues that this concern about alcohol taxes—that they penalize moderate drinkers—is weakened to the extent that revenue raised from alcohol taxes can be used to reduce other tax rates or create public programs that benefit those same moderate drinkers. Moreover, the costs associated with implementing more "targeted" interventions (e.g., setting up counseling programs for drinkers, enforcing stronger sanctions against alcohol-related crimes) also impose costs on moderate drinkers.

that moderate drinking reduces heart disease risk among men, although Cook et al. (2005) find no long-term net effect on mortality for moderate drinkers. There is also evidence that moderate and social drinkers have higher earnings than abstainers, raising the possibility that responsible consumption of alcohol might make people more productive. If these literatures document a causal relationship, regulations that lead to lower alcohol consumption would impose costs on society.

Notably, stricter regulations of alcohol such as higher excise taxes are likely to have effects on a range of important social outcomes. Although our review here has focused on studies that convincingly tackle alcohol's causal role in crime, there are large bodies of research on other adverse outcomes of alcohol consumption, including: drunk-driving mortality, mortality due to accidents and other causes, accidental injury, and risky sexual behavior (for reviews, see Cook and Moore 2000; Chaloupka 2004; and others). Measured in economic terms, the value of the changes in these other alcohol-related outcomes may be substantially greater than the direct and indirect costs of stricter alcohol control.

What, then, are we to conclude about the relative value of alcohol control policies available to local, state, and federal lawmakers? First, changing the availability of alcohol with marginal spatial or temporal restrictions (or some combination of these) is unlikely to yield major reductions in crime because individuals can—and do—respond to such restrictions by shifting the venue or circumstances of their consumption, thus undoing the effects of the regulation. The most credible existing evidence on the repeal of Sunday sales, for example, suggests that it has no effect on overall alcohol consumption and very modest effects on the outcome that is arguably the most direct

consequence of problem alcohol consumption: alcohol-related fatalities (Carpenter and Eisenberg 2009; Lovenheim and Steefel 2009). And while there is credible evidence that Sao Paolo's introduction of mandatory bar closing times significantly reduced violent crime, the change was of a very different type and magnitude than is relevant for the vast majority of the United States (where very few bars are open 24 hours a day). We do see potential value in mandating staggered bar closing hours (a voluntary variant of which was tried with seemingly little success in the UK Licensing Act of 2003), but the political viability of this option is unclear. Moreover, while most alcohol outlet policies with respect to licensing disrupt the location or timing of on-premises drinking, in theory a larger-scale supply-side restriction on alcohol licenses (such as increasing licensing fees or directly restricting the supply of available licenses) would reduce alcohol-related adverse outcomes such as crime and violence by driving up alcohol prices, though this is an understudied research area.

Second, we conclude that lowering the minimum drinking age in the United States—a policy option being considered by several states (Florida, Wisconsin, Vermont, and others) at this writing in 2010—would lead to significant increases in crime and violence among young adults and overall. Both the state/year panel evaluation based on historical state policy changes (Joksch and Jones 1993) and evidence from a regression discontinuity design examining a more recent time period (Carpenter and Dobkin 2009b) confirm that drinking ages have large and economically meaningful effects on alcohol consumption and crime among young adults (who are at the peak of the age-crime profile). Were individual U.S. states to return to the 1970s-era regime of allowing youth age 18–21 to legally drink alcohol, the results from these studies indicate that nuisance

and violent crime would significantly increase among this age group.[22] And while a drinking age of 21 may shift some alcohol consumption to later in the life-cycle, this shift is still likely to result in less crime overall because: 1) by this age youths are on the downward slope of the age-crime profile; and 2) brain development and maturation in young adults continues through the mid-20s, such that the degree of crime-inducing cognitive deficits from alcohol use later in life is likely to be lower than in the late teens and early 20s (Brown et al. 2000; Pyapali et al. 1999; and others).

In contrast to the active discussion about lowering drinking ages in some states, there currently is no political interest in raising the minimum drinking age above 21 in the United States, despite the fact that such an increase would likely reduce crime and other adverse outcomes in the affected age range. How, then, could current drinking age policy be improved for the purpose of further reducing crime? One possibility is to increase enforcement of the law. Little research, however, has credibly evaluated the effects of better drinking age enforcement, in part because enforcement is likely strongly endogenous to youth drinking and crime outcomes (e.g., communities may be expected to "get tough" on drinking age violations after spikes in underage drinking problems). Some recent work, however, has used more credible multi-community controlled time-series methods to evaluate the effects of drinking age enforcement training for employees at alcohol outlets and police enforcement checks of these outlets. That research shows no evidence that training programs are effective and some evidence that increased enforcement checks reduce sales to minors (and, by implication, underage drinking)

[22] Different drinking ages across states also raise serious issues about adverse consequences (including crime, alcohol-related traffic fatalities, and other events) for youths traveling across state lines to purchase and consume alcohol. For evidence on this, see Lovenheim and Slemrod 2010. These issues are also relevant for analyses of differential excise taxes across states; for recent evidence on this, see Stehr 2007.

(Wagenaar et al. 2005). This research suggests that increasing enforcement of current drinking ages through police checks would further reduce youth drinking and associated crime.

Third, our research suggests that increases in excise taxes on alcohol are likely to reduce crime. As others have shown, the real price of ethanol has fallen steadily over the past two decades (see, e.g., Cook 2007, 2008). While our review of the literature examining the impact of alcohol taxes on crime illustrates some important limitations to those studies, we think there is sufficient evidence on the underlying structural relationship between taxes and prices, prices and alcohol consumption, and alcohol consumption and crime to conclude that a tax-induced price increase would reduce drinking and crime.[23] Such an increase is likely to be most effective when it is sizable and does not exacerbate cross-state tax differentials.

As noted earlier, alcohol tax increases are increasingly common revenue-raising devices in states facing serious budget shortfalls. A natural question is: how much crime reduction would a representative tax increase buy? One recent estimate is that a 10% increase in the price of alcohol would reduce youth drinking by a similar amount (Carpenter et al. 2007); price elasticity estimates for older adults are generally smaller in magnitude. Carpenter and Dobkin (2009b) estimate that a 10% increase in drinking probability increases the likelihood of being arrested for any crime by about 2% for young adults, with most of these effects attributable to reductions in violent crime, such as aggravated assault. These estimates imply modest but meaningful effects on overall

[23] We do not think the literature is definitive with respect to taxation of, say, beer versus wine or spirits. In part this is because the most common empirical approach is to control for beer taxes, since this is the most commonly consumed alcoholic beverage in the United States. This is an important issue for future work.

crime given common estimates that about 40% of violent crimes involve alcohol (Greenfield and Hennenberg 2001).

Finally, although we have reviewed most of the serious policy options that use alcohol control to bring about reductions in crime, less commonly debated policy options also exist. Most of the alcohol restrictions we have discussed here work by altering the costs of alcohol consumption (e.g., directly, by raising prices, or more commonly, through sanctions and availability restrictions). Other potential interventions might target changes in actual or perceived benefits of alcohol consumption, such as responsible drinking campaigns, alcohol advertising regulations, and/or product placements in mainstream media. If these approaches were effective at reducing irresponsible drinking they might reduce crime with less deadweight loss than other policies.

8. Conclusion

We began this review with the goal of identifying whether the strong observed associations between alcohol and crime reflect true causal effects of alcohol consumption (pharmacologically driven or otherwise) or whether they reflect unobserved heterogeneity, with the goal of informing alcohol regulation-based efforts to reduce crime. Our final assessment is that there is ample evidence to conclude that at least some of the extensively documented correlations between alcohol availability, alcohol consumption, crime, and violence do, in fact, represent "true" causal effects of alcohol use on crime commission. This seems especially true for interventions that induce very large and stark changes in alcohol consumption (e.g., large price or availability changes), as well as for alcohol control policies that effectively manipulate not only alcohol consumption but also

potential and realized social interactions (e.g., mandatory closing hours and drinking ages). Taken together, our review suggests that, to the extent that inebriation plays a causal role in crime, the control of alcohol consumption should be taken seriously and deserves a place on the policy agenda.

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