

Alcohol Regulation and Crime¹

1. Introduction

There are substantial literatures in economics, criminology, and public health that document the association between alcohol availability, alcohol consumption, and crime.² Though much of this literature is focused on violent crime there is also a substantial body of work that examines the link between alcohol and property crimes, nuisance crimes, and crimes that result directly from alcohol consumption such as driving under the influence. Most of these papers find 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 targeting alcohol availability 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) correlated with both drinking and crime or unobserved local factors (such as neighborhood quality) associated with both alcohol availability and crime, then alcohol regulation may not be an effective crime prevention tool. And even if alcohol consumption does increase the probability that some people will commit crimes, it is not obvious that alcohol regulations will significantly reduce consumption in this population; thus, some policies can impose substantial social costs without reducing crime. Despite these concerns, alcohol regulation as a crime reduction strategy is worth examining because alcohol consumption is so strongly associated with crime and there is wide latitude for changing alcohol regulations in the US.³

In this chapter we offer a critical review from the perspective of economics of existing research on alcohol and crime to complement the numerous reviews in other disciplines on this topic. Notably, we are not the first to apply economic perspectives to alcohol control policies (see, for example, Manning 1989, Pogue and Sgontz 1989, Kenkel 1996, Kenkel and Manning 1996, Cook 2007, and others). Nor are we 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, earlier reviews generally focused on a wider range of alcohol-

¹ We thank Phil Cook, Jens Ludwig, Steve Raphael, and participants at the pre-conference on the Economics of Crime Control at the 2009 NBER Summer Institute for useful comments on a previous draft. This review has benefitted greatly -- and draws heavily -- from earlier reviews of alcohol and alcohol control policies, including: Cook and Moore (2000), Chaloupka, Grossman, and Saffer (2002), Room (1983), and others. We gratefully acknowledge grant support from NIH/NIAAA #RO1 AA017302-01. The usual caveats apply.

² While some crimes such as driving under the influence (DUI) and public intoxication are clearly linked to alcohol use, we will concern our selves 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 will instead be primarily interested in 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).

³ Potential changes include alcohol excise tax increases, drinking age changes, adoption of tougher drunk driving laws, changing liquor license availability, further restricting the hours/days/locations of alcohol sales, or tougher enforcement of existing laws targeting underage drinking.

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 naturally followed the focus on price and tax interventions: research using monetary variation in the price of alcohol has indeed disproportionately focused on outcomes such as motor vehicle fatalities and unintentional injuries in comparison to crime outcomes. As a result, the treatment of alcohol and crime in these reviews was naturally more limited than our review which focuses entirely on alcohol regulations and crime but which expands the scope of alcohol control regulations under review. This allows us to incorporate into our review a body of older evidence from other countries (especially Scandinavia) that used a variety of natural experiment designs to study non-price restrictions on alcohol availability and their effects on crime, and it also permits examination of several recent high-quality studies that have provided important information on alcohol's causal role in crime using variation in liquor store density, bar closing hours, and age-based alcohol restrictions, among others.

A common theme in our review is that we are focused on papers that take the causal inference problem seriously. This focus is particularly appropriate given the chapter's focus on policy-relevant crime control strategies. Given that endogeneity, simultaneity, unobserved heterogeneity, and associated evaluation problems are likely to be serious concerns in studying alcohol and crime, papers without good research designs are at best providing biased estimates of the effects of regulation. Due to the difficulty in determining how much some types of regulation reduce crime as part of summarizing what is known about the return to a particular type of regulation we assess the strength and weaknesses of the evidence.

Our chapter proceeds as follows. First, we review evidence on the pharmacological effects of alcohol consumption on the brain and behavior from laboratory studies. We then use this and other evidence to lay out several possible causal pathways through which alcohol consumption could lead to the commission of crime. We also address the relationship between alcohol and other drugs, both with respect to consumption and regulation. We then review studies on the relationship between alcohol regulations and crime. We group the papers by the type of alcohol regulation examined (tax/price restrictions, age-based restrictions, spatial restrictions, temporal restrictions, and other evidence not elsewhere 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

In this section we review what is known about the pharmacological effects of alcohol. Though this is not the only mechanism through which alcohol consumption causes crime (we discuss this below), it is one of the major ones. We begin by describing how alcohol consumption affects "blood-alcohol concentration" (BAC) and how this varies based on individual characteristics. Then we summarize what is known about the relationship between BAC and a person's level of impairment.

Several factors determine how consuming alcohol affects an individual's BAC. 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 have 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 standardized drink (roughly one shot 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% for the second through fifth drink, respectively, and will accordingly have increasingly higher blood alcohol concentrations with successive drinks (assuming no time between drinks). A similarly sized woman will on average have a higher BAC at the same number of drinks due to sex-specific differences in body composition.

Though the exact level of impairment at a given BAC varies across individuals, intoxication due to alcohol generally follows several stages associated with different BAC levels. At low BACs (below .05%), alcohol can induce senses of enjoyment, happiness, and euphoria characterized by increased sociability and talkativeness. Loss of inhibitions and reduced attention can also result at this level of drinking. 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 of this, many countries set the BAC at which a driver is considered legally impaired at either .05% or .08% often with a lower threshold for younger or less experienced drivers. In the range .11%-.30%, individuals experience exaggerated emotional states including anger and sadness, increased 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, unconsciousness, impaired respiration, and possible death due to respiratory arrest. For lower levels of BAC many of the effects have been documented in controlled laboratory settings, particularly in the context of tests of the impact of alcohol on 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.⁴ 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 a level of electronic shock that will be delivered to the loser. 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 a measure of aggression. The subjects are either told they will be served an alcoholic beverage or that they will be served a non-alcoholic beverage. Some individuals in each group are randomly served an alcoholic beverage and some are served 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.⁵

⁴ 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.

⁵ It is worth noting that the majority of the experiments examining how alcohol affects aggression are studying 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:

3. Possible Causal Pathways and Mechanisms from Alcohol to Crime

In this section we examine several of the major causal pathways through which policy-induced alcohol consumption may cause crime (Fagan 1990, Pernaenen 1981).⁶ Most public policies targeting alcohol have the potential to impact 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 their general order of importance. These include: direct pharmacological effects on aggression and cognitive functioning, the 'excuse' mechanism, and the roles of social interaction and venue.

Arguably the most direct pathway from alcohol consumption to crime is through its direct pharmacological effects. By increasing aggression and generally heightening emotional responses, acute alcohol use may cause increases in interpersonal violence, including murder, rape, robbery, and simple and aggravated assault. And by reducing cognitive functioning, affecting reasoning abilities, and altering normal judgment and decision-making, drinking may lead to alcohol-induced myopia or short-sightedness which may make individuals fail to realize the social and legal consequences of their actions, thus increasing a range of types of crime. The pharmacological effects of alcohol may also have causal effects on crime by increasing the risk of victimization. Excessively large doses of alcohol, for example, lead to sedation as opposed to aggression, which may make intoxicated individuals easy targets of a variety of types of crime. Direct effects on cognitive functioning and decision-making may also place individuals in situations prone to increased risk of victimization. Unfortunately, for reasons outlined below associated with the nature of most alcohol availability restrictions, in practice it is difficult to disentangle alcohol's role in crime commission as distinct from criminal victimization, and as a result very little research has provided evidence on this important question. Despite this limitation, however, most of the regulations discussed below are likely to reduce alcohol consumption and therefore directly reduce crime that results from the pharmacological effects of alcohol consumption.

Alcohol may also increase crime by providing an 'excuse motive' for crime commission. Someone considering a criminal act may believe that alcohol use will reduce the costs of crime conditional on being caught, since potential perpetrators may be able to appeal to the inebriating effects of alcohol as a mitigating factor in the criminal incident. Alcohol consumption may also allow a person to justify antisocial activities to themselves or the people around them. These excuse motives are a causal mechanism that may be affected by policy as reducing alcohol availability may remove the excuse in some cases.

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 in comparison to other drugs; we discuss this below in Section 4.

⁶ 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 in the presence of a true causal effect (defined below) is precisely why we focus below on studies that seriously address the problem of correlated unobservables.

Alcohol may also increase the chance of interpersonal violence by increasing social contact, which again may be relevant both for crime commission and criminal victimization. A closely related mechanism is venue: many alcohol regulations can change the location and setting of alcohol use either directly (e.g., bar closing hours or on-premise outlet licensing) or indirectly (e.g., minimum drinking ages that increase the range of venues in which youths can legally consume alcohol). Venue and social contact strongly interact, since when people consume alcohol in public places such as bars where 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 both 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 in many cases involve potentially stressful negotiations over personal space, further increasing the risk of a violent interaction.

Most of the studies of the impact of alcohol regulations are ecological studies, and most regulations can plausibly impact more than one of the mechanisms described above. As a result it is challenging to determine exactly how much of the crime caused by alcohol is due to each mechanism. This limits our ability to predict how a completely novel regulation that is targeting just one mechanism is likely to affect crime rates. However, the reduced form findings are still of substantial value as most new policies are likely to have fairly similar characteristics to 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 regulations of other drugs (such as marijuana, methamphetamines, and others) and crime, which is the focus of other work in this volume (see, for example, Pollack and Reuter). One reason for this distinction is that the production, purchase, sale, and distribution of alcohol is not criminogenic in the same way as the production, purchase, sale, and distribution of illicit drugs such as opiates and crack-cocaine. Because marijuana and hard drugs are illegal, much of the crime associated with these drugs is caused not by consumption of these substances but rather by the dangerous nature of the illicit markets themselves. Alcohol, in contrast, suffers from none of these challenges (except for illicit provision of alcohol to minors) and has the associated benefit that there is ample state-sanctioned policy variation that can be used to plausibly identify the effects of alcohol regulations on availability, consumption, and crime. As a result, observed relationships between alcohol control policies and crime are far more likely to reflect the behavioral and pharmacological effects of alcohol consumption as compared with crimes associated with production, sale, and distribution of alcohol—which is not true for the case of illicit drugs.

The other key reason to distinguish alcohol from other drugs in their relationships with crime is that the pharmacological and behavioral effects of alcohol can differ markedly in comparison to other 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 terms of pharmacological effects is cocaine, which has similarly been shown to increase aggression, reduce self-control, and increase irritability (Washton 1987). Amphetamines also can produce an increase in aggression (as with alcohol) but are also accompanied by a paranoid

psychotic state which 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 has been generally characterized as increasing risk for aggressive behaviors. Thus, alcohol has a quite unique pharmacological profile with respect to its possible links with crime as compared to other substances.

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 fairly mixed on this question and has generally limited attention to the study of marijuana. While some studies find evidence that alcohol and marijuana are substitutes in consumption (DiNardo and Lemeux 2001, Conlin et. al. 2005), others find that the two are complements in consumption (Pacula 1988).⁷ The relationship is potentially important because some research has suggested a direct causal effect of marijuana on the commission of income-producing and property crimes (Pacula and Kilmer 2003).⁸

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; to the extent that alcohol control policies influence not only consumption of alcohol but also use of other drugs, then the observed effect on crime should properly be attributed to the total or net effect of both the causal effect of drinking on crime as well as the causal effect of the drug use (either reduced or increased, depending on the underlying relationship) on crime.

⁷ DiNardo and Lemeux (2001) use variation induced by state drinking age changes (described in more detail below in the review of age-based alcohol restrictions) and finds that exposure to a more restrictive drinking age significantly reduced alcohol consumption by high school seniors but significantly increased marijuana consumption, suggesting alcohol and marijuana are substitutes in consumption among youths. Conlin et. al. (2005) uses changes between 'wet' and 'dry' status from local prohibition referenda in Texas in a quasi-experimental framework and finds 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.

⁸ 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. There is less research that addressed 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 on 29 large cities from 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.

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 relies on variation across areas and in changes within areas over time in these regulations for identifying alcohol's causal role in the commission of crime. We group our review according to these different types of alcohol regulations, 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 of the hours/days of alcohol sale; and other '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.⁹

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 any study of taxes and outcomes related to drinking and/or 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 Researchers Association, now known as C2ER, or the Council on Community Economic Research). 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 in the current policy environment, as several states have recently 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 and has been reviewed

⁹ In addition to these categories of alcohol regulations that have been used in research on alcohol and crime, 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.

extensively elsewhere (Chaloupka et al. 2002, Cook and Moore 2001, and others).¹⁰ Some of this evidence comes from cross-section studies that use tax-induced price variation (Grossman et al., 1987, 1994; Coate and Grossman, 1988; and others), but in fact 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 the state liquor taxes significantly reduced liver cirrhosis mortality, a common proxy for heavy chronic drinking, from 1962-1977. This study effectively introduced the two-way fixed effects design to studies of alcohol control policies, and it has become the relevant standard in 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, for example, 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, used one cross section of data from the 1976 National Family Violence Survey and found that state excise taxes on beer were significantly negatively related to the probability of child abuse. In a related extension, 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 individual panel data from the 1985, 1986, and 1987 waves of the National Family Violence Survey. In models with individual fixed effects, she estimated that a one percent increase in price would significantly reduce abuse aimed at wives by 5.34 percent, with no effects for husband abuse. 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. She controls for the country-specific price of alcohol and finds that these prices exhibit significant negative associations with the probability of being the victim of assault, robbery, and sexual assault against women in the cross-section but are not related to these outcomes when country fixed effects are included.

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

¹⁰ Note that in order for alcohol taxes to affect alcohol consumption, it must be the case that taxes affect prices. Young and Bielinska-Kwapisz (2002) and Stehr (2007) document that taxes are fully passed through (or 'shifted') to prices by matching tax information to commonly used local price data from the ACCRA (the American Chamber of Commerce Researchers Association, now known as C2ER, or the Council on Community Economic Research). 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.

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 and weapon carrying. 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 not weapon carrying behavior. The first stage and reduced form relationships are estimated entirely off of cross-sectional variation across states (net of state economic and religious characteristics), however. Finally, Grossman and Markowitz (2001) use data from the 1989, 1990, and 1991 Core Alcohol and Drug Surveys of College students and 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, Saffer (2001) used data on individuals age 12 and older from the 1991 National Household Survey on Drug Abuse and 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 the individual used 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 three panel evaluations. The first of these was 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 from Cook and Moore (1993), despite that Desimone's focus is on the money price of cocaine as opposed to alcohol. Using panel data on crimes in 29 large cities from 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 female, and the fraction 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, suggesting a wider range of criminal outcomes that may be causally related to alcohol use.

Finally, Matthews et al. (2006) and Sivarajasingam et al. (2006) study the relationship between beer prices and rates of injury-related violence using a 5 year 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 there is a natural appeal of using tax-induced price variation for understanding the economic effects of price on consumption and resulting crime, studies in this literature do have some weaknesses. For example, the tax/price studies focus nearly exclusively on violent crimes but do not generally study nuisance or property crimes (with the exception of Desimone 2001). These studies also cannot disentangle the crime commission/criminal victimization distinction since prices should theoretically affect consumption among both populations.

A more salient issue is the problem of institutional history that over the 1980s and 1990s (the period under study 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 outcomes such as drinking and crime, a critique that has been previously articulated by Dee (2001) and others in the context of youths. A consequence of this is that 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. And since alcohol taxes affect everyone within the taxed jurisdiction, even in panel evaluations there is no natural way to identify within-area treatment and control groups for differencing away these possible biases, unlike the case of age-based alcohol restrictions or temporal restrictions on the hours/days of alcohol sales (discussed below).

Indeed, a central question for evaluating the usefulness of tax/price strategies is: what causes the observed variation across space and time in the level of alcohol taxes? Studies of taxes and prices may be biased if population preferences toward 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 direct restrictions on spatial and temporal alcohol availability (reviewed below), in part because concern about alcohol problems directly underlie many of those policies. Thus, taxes are somewhat shielded from this concern in this respect because they are largely imposed as revenue raising devices, which is advantageous from a researcher's perspective. For example, the fact that in recent years states facing budget shortfalls have chosen to raise alcohol taxes does lend support to the idea that tax changes are plausibly uncorrelated with the unobserved determinants of crime (though clearly observed determinants such as police force size are likely to systematically covary with 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 mainly by the policy environment: few tax policy changes have resulted in mostly cross-sectional associations between the level of a state's alcohol tax and violence, meaning that omitted variables bias may be a serious concern. Despite this, two fixed-effects studies do indicate that alcohol taxes are negatively related to rates of various violent crimes. Several recent alcohol tax increases may be fruitful avenues 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 US and elsewhere is the minimum legal drinking age (MLDA). Economists conceptualize drinking age policies as affecting the full price of obtaining alcohol, much like an explicit price increase associated with alcohol excise taxes. Studies of age-based restrictions benefit from several advantages. First, like alcohol taxes, they are highly policy relevant, as the country has been actively engaged in a debate about the appropriateness of an age-21 drinking age brought about by the Amethyst Initiative, in which numerous college and university leaders have called for a re-examination of US 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 unaffected. Third, drinking age studies benefit from numerous state policy experiments that can be used for identification of the effects of easier age-based alcohol access. Specifically, all states had an MLDA of 21 in the early 1970s, but several states experimented with reducing the MLDA down to 18, 19, or 20 in the mid to late 1970s. In response to research showing increases in youth alcohol-related fatalities following these age-based liberalizations, the federal government adopted the 1984 Uniform Minimum Drinking Age Act which required states to adopt an age 21 MLDA or risk losing ten percent of federal highway funds. By 1986, 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), there is a great deal of historical within-state variation in age-based restrictions. Moreover, this variation in the context of drinking ages is in both directions and allows for natural within-state treatment and control groups (unlike alcohol excise taxes, which effectively treat everyone within the state).

Given the strengths of age-based research designs, then, it is perhaps not surprising that the literature has generated a solid understanding of the alcohol consumption mechanism: multiple previous studies have shown that exposure to a lower drinking age increased drinking participation and heavy episodic drinking. Dee (1999) used pooled cross-sections of reports of alcohol consumption of high school seniors from the school-based Monitoring the Future (MTF) study from 1976-1992 and found that exposure to a permissive drinking age significantly increased drinking participation and heavy episodic drinking, and this result was also confirmed in analyses of other policies that also used MTF data (see, for example, 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 the state 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 review).

Two studies have documented that the state drinking age changes--in addition to predictably affecting alcohol use--also predictably affected crime. Joksch and Jones (1993) used age-specific arrest data from the Uniform Crime Reports and found that nuisance crimes were significantly reduced among the affected age groups coincident with drinking age increases. Joksch and Jones did not examine other types of crimes. Carpenter (2008) also used age-specific arrest data and found that drinking age increases were associated with

significant reductions in arrests for property 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 non-violent crimes.

More recently, Carpenter and Dobkin (2009) use a different approach to evaluate drinking ages and their effect on alcohol consumption and crime. Specifically, they use a regression discontinuity (RD) design that uses the fact that the costs of obtaining alcohol fall discretely at the MLDA (Carpenter and Dobkin 2009). 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 therefore be used to identify the effect of easier alcohol availability on crime. Using alcohol consumption data from the 2001-2007 California Health Interview Surveys with information on exact age in months at time of interview, they find that drinking participation increases sharply at age 21 by about 30 percent. They then use data on the universe of arrests in California from 2000-2008 with information on exact age in days at time of the arrest and find significant increases in arrest rates for nuisance and violent crimes precisely at age 21. The sharp increase in violent crimes exactly at age 21 is largely attributable to increases in assaults. Relative to previous work, the RD approach in Carpenter and Dobkin (2009) provides an unbiased estimate of the causing variable of interest – alcohol availability – at the discontinuity, and as such it is less likely to be contaminated by population policy preferences than state alcohol policy changes – including changes to drinking ages in previous decades.

Finally, we note the results from another study that studied an alcohol-involved driving restriction that combined age-based elements to study the relationship between alcohol and crime. 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).¹¹ ZT laws were adopted by every state over the 1990s and make it illegal for youths under age 21 to drive with *any* alcohol in their blood (the relevant BAC standard for adults in most states at this time was .10 or .08 BAC); 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 percent but had no effects on arrests among slightly older adults age 22-24 who were unaffected by ZT laws. He did not find significant effects on violent crime.

Critiques of the literature on age-based restrictions

Although studies of age-based restrictions on alcohol benefit from several strengths described above, they also face some important limitations. First, there is not good evidence on enforcement of drinking ages, either historically or present day. Fortunately, it is likely that enforcement varies little for youths very near drinking age thresholds (i.e., for 20 versus 22 year olds), so this is unlikely to be a serious concern. Second, most of the studies using age-based alcohol regulations use arrest data instead of reported crimes data (since offender age is typically not known). This raises usual concerns about the possibility that alcohol use may independently increase the probability of being arrested conditional on committing a crime, thus resulting in an overstatement of the effect of age-based restrictions. The fact

¹¹ Alcohol-involved driving regulations may be useful for future studies of alcohol and crime, but we are not aware of other research that has used them in this way other than Carpenter (2008).

that these studies generally find effects on some crime types but not others mitigates but does not completely invalidate this limitation.

Third, the alcohol/crime relationship using variation in drinking ages is complicated by the fact that individuals legally old enough to drink not only can obtain alcohol more cheaply, they can also 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.¹² This problem is potentially more problematic in Carpenter's (2008) study of age-targeted drunk driving laws, since those policies only change alcohol consumption in the context of driving; since no large scale surveys in the US ask about the detailed locations and conditions of alcohol use, these age-based studies generally cannot rule out that changes in both the quantity of consumption as well as the conditions of consumption independently contribute to changes in crime.

Finally, there are two related critical issues dealing with external validity and whether drinking ages 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 on 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 of drinking and crime both exhibit peaks in late adolescence and early adulthood, meaning that research studying the drinking age is likely to have good power in detecting effects on outcomes. This also suggests that drinking age restrictions could generate meaningful reductions in population crime since youths in this age range commit a disproportionate share.¹³

Overall evaluation: age-based alcohol restrictions

Drinking ages were commonly changed in the 1970s and 1980s but have been stable for over two decades. Recently, this policy debate has reignited, increasing the relevance of studies that have evaluated the effects of drinking ages on alcohol consumption and crime outcomes. 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 a variety of types of crime, including violent crime, committed by young adults.

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

¹² This limitation is less salient for the evidence on property and nuisance crimes, since they are less likely to involve interpersonal interactions.

¹³ 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 additional rationale for age-based restrictions.

There are numerous articles in the addiction, criminology, and public health literature that have focused on the spatial relationship between alcohol and crime and have found a strong spatial correlation. 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 GIS methods and very detailed data on locations of alcohol outlets and adverse crime events to establish evidence of spatial correlations. Since these studies typically focus on crime committed in very small areas around where alcohol is available, concerns about ecological inference problems are mitigated. Also, some of the studies allow disaggregation of 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-premise outlets should be more strongly related to aggression-related crimes than off-premise 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 is Scribner et al. (1995) who use detailed information on the location of assaults in 75 communities in Los Angeles and find that an additional alcohol outlet is associated with 3.4 additional violent assaults (off a base of 570). In a related study, Scribner et al. (1999) examined 155 neighborhoods in New Orleans and found that a 10 percent increase in off-premise alcohol outlets was significantly associated with a 2.4 percent higher homicide rate.¹⁴ 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-premise alcohol outlet density and assault, but other research that has disaggregated alcohol outlets by type has reached different conclusions.¹⁵ 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 'off-premise' alcohol outlets such as liquor stores as opposed to areas with many 'on-premise' outlets such as bars. In fact, Gruenewald et al. (2006) find that bar density increased the risk of assaultive violence only on 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, for example, Chaloupka and Wechsler 1996, Weitzman et al. 2002, and others), and this spatial availability has also been linked to various crime outcomes. Wechsler et al. (2002) fielded a telephone survey of households near and far from college campuses with varying rates of binge drinking. They found that residents nearby college campuses with high binge drinking rates reported significantly more neighborhood

¹⁴ Not all such studies have found effects, however. Gorman et al. (1998) perform a similar analysis for New Jersey and find no spatial relationship between alcohol outlets and violence.

¹⁵ We review only a handful of representative articles from this public health and criminology literature here. For other examples, see, for example, Lipton and Gruenewald (2002), Roncek and Maier (1991), Zhu, Gorman, and Horel (2004), Gorman et. al. (1998), and others.

nuisance crimes such as drunkenness, vandalism, public urination, and other disorderly conduct.

Of course, a central difficulty with these cross-sectional analyses of spatial variation in liquor outlets is that unobserved local factors that contribute both to crime and alcohol outlet density may bias the estimates in these studies. Researchers have tried to address this challenge in a variety of ways. The most common approach is simply to include tract-level controls for multiple observable dimensions of local neighborhood quality. To the extent that these controls absorb the confounding variation between outlets and crime, this is a useful approach, though it is difficult to evaluate across studies.

An alternative approach for addressing unobserved variation across neighborhoods that may be 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 commercialized zones. He also 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 at all clear that the instruments satisfy the necessary exclusion restrictions for valid identification.

A third approach for addressing concerns about correlated unobservables in these space-based types of alcohol outlet studies is to control longitudinally for alcohol outlet openings and closings. Teh (2008) employs this type of strategy in an event-study framework using data on liquor outlets in Los Angeles. Her empirical specifications include outlet fixed effects, thus eliminating confounders that are time-invariant about the precise location of an alcohol outlet. 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-SES neighborhoods, as suggested by the cross-sectional results in Gruenewald et al. (2006).

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 these literatures adequately address concerns about omitted variables biases. The instrumental variables approach for understanding spatial variation in liquor outlets may be problematic if, say, the location of gas stations have independent effects on crime other than through their effect on liquor stores, and the interrupted time-series designs in both literatures may still produce biased estimates to the extent that population preferences toward alcohol (and changes in these preferences) contribute to the political processes generating spatial and temporal restrictions (and changes in these restrictions) on alcohol availability and crime outcomes.

Second, the mechanisms underlying any crime reduction in these studies are not well understood. For example, spatial studies face that 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, and most survey data on alcohol use is not well suited for these types of detailed spatial analyses since surveys generally do not ask about precise location of consumption. Data on the location of alcohol sales has been used (Stevenson et. al. 1999), though sales are imperfect proxies for consumption.

Third, studies in these literatures have focused on fairly narrow type of crimes. Most studies in the spatial availability literature have focused on violent crime, while most studies in the temporal availability literature focus on DUI; a more comprehensive analysis of all crime types would be useful, particularly an examination of alcohol-related crimes (e.g. public intoxication) to increase confidence that alcohol outlets actually increase consumption. Fourth, most studies lack information on spatial dimensions of police enforcement. This 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 correlated with alcohol outlet openings and closings). Fifth, studies in this literature cannot disentangle whether alcohol use causes crime commission, criminal victimization, or both. This is because the policy levers under study should plausibly change consumption of both perpetrators and victims; thus, there is no way to separately identify these two effects.

Finally, and perhaps most importantly, few of the studies in the spatial or temporal availability literatures 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 otherwise similar consumption or 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.¹⁶

Overall evaluation: space-based regulations on alcohol outlets

Existing evidence on space-based alcohol outlet availability is suggestive of a link between alcohol outlets and some types of crime, but strong conclusions are not yet warranted based on the weight of limitations of this research. Studies of spatial availability restrictions have yet to convincingly overcome key identification problems and provide limited insight into the mechanisms underlying the observed associations. These studies also face challenges in interpretation of overall crime reductions versus crime displacement. We conclude that the evidence is not sufficient to conclude that spatial availability restrictions are effective at reducing crime.

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

In contrast to explicitly space-based regulations, there are also several types of alcohol regulations that regulate the times during which alcohol can be purchased or consumed. Examples include historical restrictions on Sunday alcohol availability in the US and Australia and Saturday alcohol availability in Scandinavia, as well as changes in the hours of operation of venues where alcohol is consumed, such as bars and restaurants.

¹⁶ Related to this, the very local nature of these approaches can be a double-edged sword: while they allow very precise examination of spatial patterns in one city or at a point in time, they also raises concerns about generalizability and external validity.

Studies of temporal availability restrictions benefit from several strengths. Perhaps most importantly, these studies allow very strong prior beliefs as to how the laws should affect alcohol related crimes: specifically, these laws should restrict consumption and resulting crime during the times when alcohol sales are restricted or prohibited. And unlike much of the space-based alcohol outlet literature, regulations of temporal availability benefit from a greater number of changes in these laws, which allows for the implementation of fairly compelling pre/post interrupted time series designs. This reduces some concerns about omitted variables bias to the extent that time-invariant unobservables are controlled for through the inclusion of location fixed effects. Finally, like spatial availability restrictions, these policies are also actively being debated and adopted, increasing the policy relevance of these types of alcohol regulations.

Much of the research exploiting temporal variation in alcohol availability has focused on repeals of 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 tax-paid sales. Evidence suggests that such policy liberalizations do increase alcohol sales. Stehr (2008) uses aggregate data on beer, wine, and spirits sales and uses the timing of various US state repeals of Sunday sales restrictions. His fixed-effects estimates indicate that Sunday sales policies significantly increased spirits sales. Evidence that such policies affected actual alcohol consumption (as distinct from alcohol sales) is more limited, though Carpenter and Eisenberg (2009) show that Ontario, Canada's Sunday sales liberalization increased Sunday-specific drinking by 7-15 percent, with some evidence of substitution away from consumption on Fridays and Saturdays.¹⁷

Evidence that these types of temporal restrictions on alcohol sales and availability is related to crime and violence outcomes in a systematic way is fairly limited. Ligon and Thyer (1993) show that a ban on Sunday alcohol sales reduced DUIs. McMillan and Lapham (2006) use data on day-specific traffic fatalities in New Mexico before and after its 1995 Sunday sales liberalization, and they find 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) shows that increases in the hours/days of availability in Australia were predictably associated with increases in alcohol-related fatalities.

Some of the best evidence on temporal availability restrictions and crime outcomes comes from Scandinavia, where multiple policy experiments allow relatively clean identification of changes in temporal dimensions of alcohol availability using interrupted-time series designs.¹⁸ Olsson and Wikstrom (1982) study Sweden's short-term closing of its state monopoly liquor stores on Saturdays in the summer of 1981. They find that offenses related to drunkenness, 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 find no effects on outcomes related to vandalism, acute

¹⁷ 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).

¹⁸ In addition to the peer reviewed studies on Scandinavian policy changes described here and in the next section, see Room (2002) for a comprehensive treatment.

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. This raises the possibility of across-day substitution (e.g. a displacement of consumption and/or crime from weekends to weekdays) and suggests the need for more detailed data on alcohol consumption.¹⁹

In a series of studies examining the effects of Sweden's staggered adoption of increased Saturday retail hours in its monopoly-run 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 attribute the drunk driving increase associated with the 2000 policy change as at least partly attributable to changes in police enforcement, and they also recognize that such modest changes in overall alcohol sales make it difficult to precisely identify effects on assaults even if they truly existed. Norstrom and Skog do not examine whether their outcomes exhibited across-day substitution in response to the Saturday-specific increase in availability.

Finally, we review here studies that regulate the hours of alcohol availability through policies such as bar closing times. Chikritzhs and Stockwell (2002) studied extended closing times for 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 12 midnight until 1am. 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 monthly 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 1am to 2am. They examined administrative data on hospital admissions for each hour of availability from 11pm to 3am for the four years before and three years after the policy change. They found reductions in motor vehicle collision admissions between 11-12 midnight and 1-2am but no significant change for the 2-3am 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 2-3am period, consistent with availability theory.

One of the strongest examples of this type of research used the adoption of mandatory bar and restaurant closing hours in Sao Paulo, Brazil. Biderman et al. (2009 forthcoming) use a difference-in-differences design to examine the effects of these 'dry' laws on violent crime. Between March 2001 and August 2004, 16 of 39 municipalities in the Sao Paulo Metropolitan area prohibited alcohol during the late night and early morning hours; prior to this most bars were allowed to remain open 24 hours. In models with city and period fixed effects as well as controls for city/period varying demographic characteristics,

¹⁹ 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, study Scotland's 1976 alcohol liberalization which extended bar closing hours from 10pm until 11pm 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 find 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.

police enforcement, and lags of the homicide rate, Biderman et al. find that adoption of a dry law was associated with a statistically significant reduction in both homicides and battery of about 10 percent.²⁰

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 go further at reducing concerns about omitted variables biases correlated with both the temporal availability restriction and outcomes, one still may worry that population preferences toward alcohol may confound adoption and repeal of these policies. Fortunately, Sunday sales liberalizations seem to be driven by desires to increase state tax revenues (similar to alcohol tax increases), but other examples in this group—such as the imposition of mandatory bar closing hours in Brazil—are more likely to be subject to concerns about endogenous policy adoption.²¹ 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 US, none of these has examined crime or violence more generally. And those that have examined more relevant outcomes come from other countries and time periods where the basic patterns of alcohol control differ markedly from the US, which has a much weaker role for government monopolies in determining alcohol availability than Canada, Australia, or Scandinavia.

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 affected the extent or conditions of alcohol consumption, as most evidence comes from imperfect proxies such as aggregate sales. Another shared limitation is that the temporal availability studies have also focused on a narrow set of crimes.²² 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 importantly, none of the temporal availability studies provide compelling evidence on whether the policies are simply shifting the timing of alcohol consumption and crime as opposed to permanently reducing adverse outcomes. Again, it may certainly be the case that social welfare is enhanced in the presence of consumption or crime displacement across time (if, for example, alcohol consumed and crime committed on Mondays were less socially harmful than otherwise similar consumption

²⁰ 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 imposition of a mandatory 11pm closing hour (Duailibi et al. 2007).

²¹ 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.

²² An exception is recent work by Jackson and Owens (2009) who use the extension of late night hours of metro rail service in Washington DC which allowed individuals to stay at on-premise alcohol locations (e.g. bars, restaurants) until closing hours. They show that a series of these policy changes predictably affected DUIs, alcohol-related crimes, and others.

or crime on Saturdays), but again more information is needed for a full accounting of the costs and benefits of these policies.

Overall evaluation: temporal availability restrictions

Research examining temporal restrictions on the hours and days of alcohol availability share many of the same benefits and challenges as the studies that exploit detailed spatial variation in alcohol outlet availability. These studies tend to be limited with respect to the types of crime outcomes studied and do not address the broader question of whether temporal availability restrictions simply redistribute crime across different time periods. We conclude that temporal availability restrictions are likely to change alcohol consumption and crime outcomes in the immediate vicinity of the timing of the policy change, but broader statements about overall effects are not warranted by the literature, except in cases where the change in availability is very large.

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

While the preceding sections reviewed evidence from studies of alcohol regulations that were primarily or exclusively spatial or temporal in nature, there are several other types of alcohol restrictions that combine important elements of both. These "circumstance" regulations regulate the conditions under which alcohol is made more or less easily available and include examples such as alcohol rationing and government privatizations of liquor 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 per se but do provide important and useful evidence on the nature and extent of the causal linkages between alcohol availability, alcohol consumption, and the commission of crime.

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 were doubled twice, and state production and sale of alcohol was dramatically limited. Over this period, population alcohol consumption fell by 25 percent, and homicide victimizations also fell by about 40 percent (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 led to a sharp decline in alcohol consumption of about two-thirds. 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 percent and a more modest increase in assaults (8 percent). 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 percent and assaults increased by 50-100 percent, 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 percent—and assaults did not fall. These studies do not include explicit control groups, however.

Temporary suspensions of alcohol availability associated with strikes of monopoly-run alcohol outlets have also provided quite 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. Similar findings have been shown for Finland surrounding strikes of its alcohol monopoly stores in 1972 which generated abrupt and large reductions in alcohol availability. Over this period, alcohol consumption was estimated to fall by 30 percent, with concomitant reductions in public drunkenness, resisting arrest, disturbing the peace, and especially aggravated assault. Violent victimization also fell sharply coincident with the reduced availability as measured by admissions to emergency clinics for murder, manslaughter, assault, and battery (Makela 1980, Karaharju and Stjernvall 1974, Takala 1973). Similar strikes in Norway's alcohol monopoly stores in 1978 also reduced alcohol consumption by 5-10 percent and reduced domestic violence (Horverak 1981, as reported in Room 1983). While not the result of intentional policy manipulations, these studies do provide strong evidence consistent with the idea that liberalizing alcohol availability through increased retail availability would increase alcohol consumption and crime.

Finally, we note that several recent 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 use professional football as the relevant institutional setting and show that unexpected home-team losses significantly increase acts of domestic violence. Again, alcohol use may be one mechanism contributing to the effect. These and other studies suggesting a role for alcohol as a mechanism for violence – while not studying alcohol regulations per se – may suggest that alcohol control policy should consider other situation-specific regulations as alternative ways to address the adverse effects of alcohol on crime.

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 gaps in the literature on alcohol control policy and crime that are worth mentioning in the hopes of spurring researchers to address them.

One of the most serious gaps in the literature is that although there are many estimates of the impact that alcohol regulations have on crime rates, much less is known about how much and in what ways they 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 are reducing crime 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 the social cost of various policies. Existing evidence suggests that even 'heavy' or 'problem' drinkers respond

to price (Cook and Tauchen 1982), though the evidence on which groups are particularly sensitive to tax-induced price changes is not completely developed.

Another limitation of the literature reviewed above is that it is focused largely on homicides and other violent crimes such as rape and assault, presumably due to 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 limitation to 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). Mostly this lack of research attention to alcohol's role in criminal victimization stems from the lack of quality victimization data that would allow implementation of a credible research design to disentangle causality. The National Crime Victimization Surveys, for example, are some of the best data for studying criminal victimization, but geographic identifiers have not historically been available in these data, rendering state/year panel evaluations of the effects of alcohol control policies on criminal victimization impossible. Yet even with credible victimization data there is another methodological and conceptual challenge for understanding how alcohol regulations affect criminal victimization as opposed to crime commission: specifically, most alcohol control policies probably affect the alcohol consumption of both criminals and their victims making it impossible to distinguish between these two causal mechanisms without very rich individual level data. Beer taxes, for example, plausibly affect drinking among both individuals committing crime and individuals who are being victimized by crime. Spatial and temporal restrictions on alcohol availability suffer from the same problem. The regression discontinuity design proposed by Carpenter and Dobkin (2009b) poses a potentially useful way to separately identify these effects, since a discrete increase in victimization rates at 21 years of age is probably due to alcohol increasing victimization rather than increasing criminal behavior. This is a fruitful area for future research.

Despite these limitations, there are at least two recent and encouraging developments for researchers interested in the important question of how alcohol affects crime. First, there are several new datasets that help address the limitations of previous work described above. For example, the National Epidemiologic Survey on Alcohol Related Conditions (NESARC) is a recently completed two-wave panel of the alcohol consumption behaviors of over 40,000 US adults performed by the National Institute on Alcohol Abuse and Alcoholism (NIAAA). Limited-access datafiles with geographic identifiers will be made available to researchers. These data are likely to be particularly useful for understanding the settings of alcohol use because the survey includes detailed questions on the location, timing, and frequency of recent alcohol consumption. Most US 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), which contains detailed information on crimes known to the police (not just crimes resulting in an arrest), limited information on alcohol and drug involvement in the incident, and key information on geographic location. NIBRS can therefore 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 increasing trend toward availability of large administrative datasets at the state and federal levels on mortality, arrests, hospital and emergency room admissions, and related outcomes that should also be useful for applying a variety of research designs in understanding alcohol's role in 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 has seriously diminished state budget coffers leading 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 from 2002-2008, including several very large tax hikes (e.g., Alaska from 35 cents to \$1.07 per gallon in 2002); while in the entire prior decade there were just 15 increases (and most of these were small). This increased variation is 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 regressions models (Dee 1999). The same budget shortfall motivation has 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 may affect crime. Finally, a handful of 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 has undertaken 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 and others 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 substantive review with a discussion of the economic considerations that are relevant for whether alcohol regulations should be implemented to reduce crime, and if so which ones. 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 traditional public health perspective, these utility losses are not generally included into 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 reduced violent crime) is weighted mainly against 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, reduce not

only the consumption of drinkers who would otherwise commit crime in the presence of lower taxes but also the consumption of drinkers whose criminal propensity is unaffected by alcohol. 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.

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 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 are documenting a causal relationship it is likely that regulations that reduce alcohol consumption will impose costs on society through these two mechanisms.

Despite the fact that there is a dead weight loss and possibly even some health costs to reducing alcohol consumption, there is still a strong case for appropriately targeted increases in alcohol regulations. The main reason is that stricter regulations of alcohol such as higher excise taxes are likely to have beneficial 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 outcomes related to drunk-driving mortality, mortality due to accidents and other causes, accidental injury, risky sexual behavior and its attendant consequences, and others (for reviews, see Cook and Moore 2000, Chaloupka 2004, and others). Reductions in alcohol-related harms for each of these other outcomes likely result in benefits to society that dwarf the direct and indirect costs due to stricter alcohol control.

Given this, what are we to conclude about choosing among the range of alcohol control policies currently being debated at the local, state, and federal levels? First, changing the availability of alcohol through spatial or temporal restrictions (or some combination of these) is unlikely to yield major crime reduction overall because individuals can respond to such restrictions by shifting their behavior, thus undoing the effects of the regulation. The most credible existing evidence on Sunday sales repeals, for example, suggest no effects 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). Second, recent debates about lowering the drinking age in the US are likely to lead to significant increases in crime and violence, both among young adults and overall. Both the state/year panel evaluation based on historical state policy changes (Joksch and Jones 1993) and evidence from an RD design using more recent samples (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). And while there may be some shifting of alcohol consumption to later in the life-cycle, this shifting is still likely to result in less crime overall because: 1) by this time 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).²³ Finally, our results suggest that increases in excise taxes on alcohol are likely

²³ With policies such as drinking ages as excise taxes, there are also concerns about equalization of policies across state boundaries, since another way individuals can undo the effects of some alcohol restrictions is to

to reduce crime. As other researchers have shown, the real price of ethanol has been falling steadily over the past two decades (Cook 2007, and others), and taking into account the adverse consequences of alcohol in a variety of domains others have estimated that excise taxes on alcohol are too low (Manning et al. 1989 and others). While our review of the direct literature relating taxes to crime illustrated 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 from these and other studies to conclude that a tax-induced price increase would reduce drinking and crime.²⁴ Such an increase is likely to be most effective when it is sizable in magnitude and does not exacerbate cross-state tax differentials.

8. Summary and 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 toward crime reduction. 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., extended bar closing hours and drinking ages). Taken together, our review suggests that to the extent that inebriation plays a causal role in crime, alcohol consumption and alcohol control should be taken seriously and deserve a role on the policy agenda.

travel to a nearby jurisdiction with a more permissive policy. For recent evidence on excise taxes see Stehr (2007); for recent evidence on drinking ages, see Lovenheim and Slemrod (2009 forthcoming).

²⁴ We do not think the literature is firm enough 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 common alcoholic beverage consumed in the US. This is an important issue for future work in this area.

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