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THE MINIMUM LEGAL DRINKING AGE AND CRIME VICTIMIZATION

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ABSTRACT

For every crime there is a victim. However nearly all studies in the economics of crime have focused the causal determinants of criminality. We present novel evidence on the causal determinants of victimization, focusing on legal access to alcohol. The social costs of alcohol use and abuse are sizable and well-documented. We find criminal victimization for both violent and property crimes increases noticeably at age 21. Effects are not present at other birthdays and do not appear to be driven by a birth-day "celebration effect." The effects are particularly large for sexual assaults, especially those that occur in public locations. Our results suggest prior research which has focused on criminality has understated the true social costs associated with increased access to alcohol.

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1 Introduction

For every crime committed there is both an offender and a victim. Yet, economics research on crime dating back to (Becker, 1968) has primarily focused on policy levers that modify the behavior of potential offenders (Nagin, 2013; Chalfin and McCrary, 2017). Perhaps this is natural because the chief policy levers at the disposal of a social planner, namely police (which change the certainty of punishment) and prisons (which change the severity of punishment), are intended to influence the actions of the agents committing crimes. Given that by offending offenders violate society's explicit social norms, this perspective is largely consistent with the normative views of the general public which does not wish to blame the victim for having been victimized (Crawford, 1977; Eigenberg and Garland, 2008). We agree with this view — that victims are, in no way, to blame when a crime occurs — and note that, since markets require the voluntary transfer of property rights, there can be neither a market nor a price for victimization. (Carnis, 2004). At the same time, victim behavior can be an important input into the cost function of potential offenders and, as such, providing information can be a critical means through which social planners can empower victims while, at the same time, potentially reducing the cost of crime control (Ben-Shahar and Harel, 1995). In this paper we provide some of the first evidence on the causal determinants of victimization, focusing on abrupt change in legal access to alcohol at age 21 in the United States.

Empirical evidence suggests that both law enforcement (Sherman and Weisburd, 1995; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Evans and Owens, 2007; Chalfin and McCrary, 2018; Mello, 2019) and harsher punishments (Helland and Tabarrok, 2007; Drago et al., 2009; Hansen, 2015; Tahamont and Chalfin, 2016) can reduce offending, at

least under some circumstances. However, most research suggests that the effect of police on crime is, at best, modest and that the effect of incarceration is small and has declined at the margin as the incarceration rate has risen (Liedka et al., 2006; Cullen et al., 2011; Durlauf and Nagin, 2011; Johnson and Raphael, 2012; Lofstrom and Raphael, 2016). The picture that emerges from this research is that traditional policy levers, while effective, are not necessarily cost-effective, at least at current levels. Beyond the large financial cost to the government, collateral harms associated with the use of police and incarceration, including direct economic and social impacts as well as effects on families and communities, fall disproportionately on low income, racially segregated neighborhoods, further raising the cost of traditional policy levers (Western et al., 2001; Aizer and Doyle Jr, 2015; Mueller-Smith, 2015).[1]

Given that traditional forms of crime control are costly, there are potentially important benefits to identifying alternative means of crime control that have lower financial and collateral costs. Recently, a number of alternatives have emerged, including improving the quality of labor market market opportunities (Yang, 2017; Schnepel, 2017; Agan and Makowsky, 2018), cognitive behavioral therapy (Heller et al., 2017), summer job programs for youth(Heller, 2014; Davis and Heller, 2017), and investments in local capital infrastructure like street lighting (Doleac and Sanders, 2015; Chalfin et al., 2019) and the quality of public space (Branas et al., 2011; Kondo et al., 2016; Branas et al., 2018). Though all of these potential policy levers show promise, there are inevitable concerns regarding the scalability of local programs as well as external validity (Davis et al., 2017).

Each of the aforementioned alternatives seeks primarily to modify the behavior of offend-

[1]We further note that the low clearance rates for most serious crimes make crime reduction through deterrence particularly challenging. Low clearance rates may also reduce the scope for incapacitation to have appreciable effects on crime.

ers. However, a factor in nearly every crime is the presence of a victim. To date, the literature in economics focusing on victimization is sparse and focuses primarily on a small number of investments that make committing a crime more difficult including auto theft prevention technology (Ayres and Levitt, 1998), security guards (Maheshri and Mastrobuoni, 2017) and the introduction of business improvement districts (Cook and MacDonald, 2011). However, the scope of victim precaution is large. Indeed, private markets supply everything from low tech security devices like locks, to the increasing presence of private alarms and security cameras, GPS trackers, and credit monitoring. Victim precaution also includes behavioral modifications that potentially affect the probability of victimization such as the choice to leave ones home at night, hailing a taxi vs. walking while in a high-crime area, carrying valuables on ones person, or the awareness and knowledge of a new environment. As such, there is a great deal of ground for the empirical literature to cover.

Identifying the determinants of victimization is both important and promising for several reasons. First, recent literature has found that becoming a crime victim has a wide range of impacts and includes effects as diverse as mental well-being (Cornaglia et al., 2014; Dustmann and Fasani, 2015), labor market earnings and benefits receipt (Bindler and Ketel, 2019) and health outcomes for newborn infants (Currie et al., 2018). Accordingly, the costs of victimization are likely to be large and especially concentrated among the most vulnerable members of society. Second, for many crimes, especially those that have low clearance rates, abating crime through deterrence-based strategies is costly. As such, victimization-oriented strategies might reduce crime at lower cost. While the normative implications of this hypothesis are potentially controversial, given the high collateral costs of the criminal justice system, this policy option may be worth further exploration. Third, victims often have rela-

tively little information about their probability of being victimized as well as the effectiveness of private investments in crime control. Indeed, given that the academic literature has yet to offer evidence on the causal effect of actions or policies on victimization, we might expect that individuals will have difficulty accurately forecasting on their own. Accordingly, it stands to reason that victims might not optimally invest in precaution in wide variety of situations.[2] Finally, while programmatic interventions typically have high variable costs, we note that informational interventions often have low marginal costs and, as such, are easier to scale. As such, we note that there may be considerable promise in providing information to victims — as well as law enforcement.

Studying the determinants of victimization has proven elusive for at least two reasons. First, it is difficult to identify policies which affect the probability of victimization without also affecting the supply side of the market. Second, victimization research is hampered by the extremely limited availability of microdata, especially U.S. microdata at the sub-national level. While a large research literature in criminology identifies some demographic and situational correlates of victimization (Gottfredson, 1986), finding exogenous variation upon which a causal claim may be made about an actionable policy lever has proven elusive.

In this paper, we study one prominent policy lever could plausibly affect outsize influence on victimization: legal access to alcohol. A large body of research has found evidence of significant social costs associated with legal access to alcohol (Carpenter, 2005, 2007; Carpenter and Dobkin, 2009, 2011; Heaton, 2012; Kilmer et al., 2013; Carpenter and Dobkin, 2015; Anderson et al., 2017). These papers have used a variety of approaches, utilizing both

[2]We further note that individual choices may result in externalities to others as investments in precaution may change the relative returns of crime to potential offenders.

age-based discontinuities in access to alcohol and geographic variation in state or local policy. Their consensus suggests that legal access to alcohol is associated with increased traffic fatalities, suicides, violent behavior and injuries. Despite the substantial body of evidence documenting the negative public health impacts of alcohol use and abuse, these impacts might be even larger if alcohol use also increases the risk of of becoming a victim of a crime, which none of the previous studies have been able to address. In particular, one of the most intriguing possibilities is that legal access to alcohol might be an important driver of sexual assaults, a relationship that has received wide speculation in the literature in criminology and public health (Kantor and Straus, 1989; Dembo et al., 1992; Miller et al., 1993; Abbey et al., 2001; Abbey, 2002; Champion et al., 2004; Felson and Burchfield, 2004). These studies, while suggestive, are largely correlational and lack credible research designs, though recent research has intriguingly linked sexual assaults to local culture of drinking and alcohol abuse or "college party culture." (Lindo et al., 2018).

The primary empirical challenge involved in identifying a causal impact of alcohol use on crime is that using alcohol, particularly to excess, is an endogenous choice. As a result, there are many reasons why a correlation between alcohol use and victimization might exist either among individuals or, for a given individual, over time. We study a related research question — and one which pairs naturally with a potential policy lever — and estimate the extent to which *legal access to alcohol* causes a discrete change in victimization.

In order to identify a causal effect of legal access to alcohol on victimization, we utilize the fact that legal access to alcohol in the United States changes discretely at age 21 and, using a sharp regression discontinuity design, estimate the likelihood that an individual is victimized just after her 21st birthday relative to the period before her 21st birthday. In order

to estimate the model, we build a unique administrative dataset that contains the exact date of birth for all crime victims known to law enforcement in eight major U.S. cities and find strong evidence that certain types of victimization — sexual assault and burglary for women, assault and robbery for men and larceny for both genders — increase considerably at age 21. This effect is found only at age 21 (and not on prior or subsequent birthdays) and is unlikely to be driven by celebrating one's 21st birthday itself.[3]

On the whole, our estimates suggest that legal access to alcohol changes the landscape of victimization considerably and that a sizable share of serious crime could be abated by policies that change legal access to alcohol or modify the parameters of public intoxication. Our findings also provide additional insights into the complex and controversial relationship between alcohol and sexual assault (Lindo et al., 2018). In particular, while both Carpenter and Dobkin (2015) and Hansen and Waddell (2018) fail to find evidence that arrests or criminal charges for rape increase at age 21, we find sexual assault victimization at age 21 increases by nearly 25 percent in our preferred specifications. Taken together, these findings are more consistent with a model of crime in which perpetrators of sexual assault seek out vulnerable populations than with a model where sexual assault perpetrators lose control due to increased alcohol use.

The remainder of this paper proceeds as follows: Section 2 provides a brief institutional history of the minimum legal drinking age and its effects on alcohol consumption. Section 3 provides detail on the unique administrative dataset collected for this study; Section 4 provides an overview of the econometric models; Section 5 presents results and Section 6

[3]We explicitly study the role of birthday effects. Victimization is consistently great deal higher on one's birthday, even into one's thirties.

concludes.

2 Background

2.1 Private Actions and Victimization

There are potentially many ways through which potential victims can reduce their likelihood of becoming the victim of a crime. With respect to property crimes, these include investments in traditional target-hardening strategies (e.g., locks and deadbolts) and technology (e.g., surveillance cameras and security systems) as well as labor inputs such as private security services. In the case of violent crimes which drive an outsize share of the social costs of offending (Chalfin, 2015), private precautions are, to a greater extent, driven by behavioral modifications by potential victims — modifications that are perceived to change an individual's probability of victimization. Such behavioral modifications might include avoiding leaving one's home at night, hailing a taxi instead of walking while in a high-crime area, carrying fewer valuables on one's person or maintaining a generally higher level of vigilance or situational awareness. Each of these actions has the potential to make crime less profitable to a potential offender.

While investments in private precaution are costly with often unknown benefits potential victims, they are potentially attractive to a social planner for a number of reasons. First, an individual victim may have more information about how to successfully abate his or her risk of being victimized than law enforcement which must devise crime control strategies on the basis of typical patterns of victim and offender behavior and which cannot easily

tailor these strategies to a given individual's needs (Ben-Shahar and Harel, 1995; Felson and Clarke, 1995). Second, in most cities in the United States, there is approximately one sworn police officer for every 250 residents and so there are natural limits to the ability of law enforcement to deter offending. Finally, investments in private precaution may raise search costs for offenders, thus making crime less attractive overall (Shavell, 1991). Thus, private precautions, even when observable to potential offenders, may generate positive spillovers to society.

Taken as a whole, the theory suggests that it may be possible for potential victims to abate crime more efficiently than can the government – at least at the margin. Consider, for instance, crimes such as larceny or burglary which often involve belongings left unattended or homes that were unlocked at the time of the crime, both of which are extremely common and which could be abated through low-cost changes in behavior among potential victims. These crimes are only marginally responsive to police manpower (Chalfin and McCrary, 2018). Yet, for a variety of reasons — because individuals do not fully internalize the cost of victimization (Clotfelter, 1978; Ayres and Levitt, 1998), because public spending on crime control may be treated as a subsidy (Guha and Guha, 2012) or because individuals are myopic or misinformed — victims may under-invest in precaution, relative to what is socially optimal.[4] This raises the possibility that there may yet be a number of low hanging fruits to pick with respect to addressing crime through private victim action.

[4]Some of the earliest thinking about the role of private precaution in the crime production function can be found in the seminal work of (Erhlich, 1973; Ehrlich, 1981) who conceives of the "derived demand" for crime as the willingness of market participants to invest in private precautions.

2.2 Alcohol Use and Victimization

Literature outside of economics has linked alcohol abuse and victimization, either as a correlate of victimization risk (Champion et al., 2004; Felson and Burchfield, 2004) or as a predictor of subsequent victimization (Kantor and Straus, 1989; Widom, 2001), particularly in the context of domestic violence and sexual assault (Abbey, 2002). However, none of these studies utilizes exogenous variation to identify a causal effect of substance use.

While the evidence is predominantly correlational, there are a number of reasons why alcohol use and crime victimization might be causally related. First, there is evidence that the use and, particularly, the abuse of alcohol causes individuals to exhibit fewer inhibitions (Mulvihill et al., 1997; Easdon and Vogel-Sprott, 2000; Fillmore and Vogel-Sprott, 2000) which may lead them to take on risks that they otherwise would not have taken (Ryb et al., 2006). Thus victimization might rise with alcohol abuse due to a change in the risk tolerance of potential victims. Second, intoxication may affect an individual's situational awareness and therefore increase the ease with which a victim can be identified and approached by a motivated offender. For instance, an intoxicated victim might be less likely to notice a risky situation (Parks and Miller, 1997) or take actions to mitigate that risk. Third, a large literature establishes that intoxication increases aggression (Giancola and Zeichner, 1995; Graham et al., 2006), which itself is a predictor of victimization, especially for assaults. We note, for example, that the difference between an assault victim and the perpetrator of an assault can simply be which party strikes the first blow (Chalfin et al., 2018). Finally, intoxicated victims may be less able to defend themselves effectively, thus reducing the cost to a potential offender.

2.3 The MLDA and Alcohol Consumption

In the United States, the minimum legal drinking age — the age at which individuals are legally allowed to purchase alcohol – has historically oscillated between 18 and 21 years of age. Many states initially lowered their minimum legal drinking ages only to raise them again later in the 1980s. Now, essentially every state implements a minimum legal drinking age of 21.[5]

While the law does not prevent minors from securing access to alcohol (Freisthler et al., 2003), there is ample evidence that *legal access* to alcohol nevertheless increases drinking and, in particular, problematic drinking. For example, recent research uses information from National Health Interview Survey to show that drinking increases at both the extensive and intensive margins when individuals turn 21, based on local regression discontinuity based estimates (Carpenter and Dobkin, 2009). Other evidence shows that it is precisely the most problematic types of drinking that increase at age 21 — for example, binge drinking — as opposed to moderate levels of drinking (Carpenter et al., 2016). This research, and other related studies on youth zero tolerance laws (Carpenter, 2007), suggest that alcohol use, including consumption patterns consistent with alcohol abuse, increases with legal access to alcohol.

Is the same relationship true in our sample? In this research, we rely on the sharp discontinuity in legal access to alcohol at age 21 to identify a causal effect of legal access to alcohol on victimization. Since data that contain the exact date of victimization for the entire United States are not available, we focus on city-level administrative records of vic-

[5]There are a few very limited exceptions. For instance, some states permit alcohol use with one's parents at restaurants (in Wisconsin for instance).

timizations with the exact date of birth and exact date of victimization for all crime victims in eight large cities in the United States. Our data cover the following jurisdictions: Charlotte, NC (Charlotte-Mecklenburg), Dallas, TX, Denver, CO, Houston, TX, Kansas City, MO, Milwaukee, WI, San Diego, CA and St. Louis, MO.

While this gives us a large number of victimizations upon which to draw inferences, this subsample of the United States is small enough we would ill-powered to do a subsample analysis of the National Health Interview Survey (NHIS). To verify alcohol abuse increases at age 21 for our subsample, we focus on a public health outcome shown to be correlated with alcohol use: drunk driving. Indeed, prior research on the MLDA using both both differences-in-differences designs (Carpenter and Dobkin, 2011) and regression discontinuity designs (Carpenter and Dobkin, 2009) suggests that traffic fatalities increase with legal access to alcohol due to the MLDA. Moreover, both (Carpenter and Dobkin, 2015) and (Hansen and Waddell, 2018) find that drunk driving increases by 40 percent at the MLDA for two completely independent states.[6]

This motivates our proxy for alcohol use and abuse: the fraction of fatal accidents involving alcohol from the Fatal Analysis and Reporting System (FARS), a census of every fatal car crash in the United States. We focus on the fraction of fatal car crashes and its evolution over the age distribution. The FARS collects age instead of exact date of birth. As shown in Figure 1, the fraction of accidents involving alcohol increases monotonically with age among teenagers before exhibiting a discrete jump at age 21, after which it declines monotonically. A similar pattern emerges for the subset of cities that provided victimization reports. This

[6]These estimated increases are essentially equal in magnitude to the estimated increase in alcohol consumption found in the NHIS by Carpenter and Dobkin (2009). As such, it may be reasonable to infer that alcohol consumption and drunk driving increase at similar percentages with legal access to alcohol.

suggests that there are similar "first stage" effects of alcohol access on alcohol use and abuse for our subset of cities and the nation as a whole.

3 Data

This research considers whether individuals who have legal access to alcohol are more likely to become crime victims. As national microdata on crime victims are unavailable, we construct a unique dataset on crime victimization, using administrative data obtained from eight municipal police departments in the United States. The eight police departments are the municipal law enforcement agencies for the following cities: Charlotte, NC (Charlotte-Mecklenburg), Dallas, TX, Denver, CO, Houston, TX, Kansas City, MO, Milwaukee, WI, San Diego, CA and St. Louis, MO.[7] These departments cover a population of approximately 8 million residents, represent a number of different U.S. regions and include three of the ten largest cities in the United States — Houston, Dallas and San Diego.[8] Table 1 explores the extent to which the cities in our analytic sample differ from other U.S. cities and the population as a whole with respect to their crime rates. The cities in our sample have higher than average crime rates, approximately 50 percent higher than other large cities, depending on the crime type. St. Louis, in particular, has an extremely high crime rate and had the highest homicide rate in the United States in 2016.

[7]Note that not every crime has a person-victim — for example, crimes against businesses. We focus on crimes with a person-victim.

[8]In total, we reached out to twenty-two police departments. We received no reply from municipal law enforcement agencies in the following cities: Cincinnati, OH Cleveland, OH, Detroit, MI Memphis,TN, Nashville,TN, Washington DC, Atlanta, GA, Sacramento, CA, Tuscon, AZ, Cambridge MA, Baton Rouge, LA, Seattle, WA and Las Vegas, NV. The following departments declined or request for data: Baltimore, MD, Miami, FL, Orlando, FL, Philadelphia, PA, Boston, MA, Columbus, OH, Portland, OR, Phoenix, AZ and Newark NJ.

In each city, the data contains information on the type of crime, the date of victimization and the victim's exact date of birth and gender. We focus on crimes that, with a few exceptions, largely correspond to the Federal Bureau of Investigation's list of "index crimes" which are collected annually and reported in the FBI's *Uniform Crime Reports*. Specifically, we focus on the following crimes: assault,[9] burglary, homicide, larceny, motor vehicle theft, robbery and sex-related crimes which are an aggregate of rape and other sexually-related offenses, in cities for which they are available.[10] Overall, data cover the years 2007 through 2018 though exact years of data availability vary by department.[11] In all subsequent analyses, we aggregate the data from our eight cities in order to produce a national estimate of the effect of legal exposure to alcohol on crime.

We supplement our main analyses of administrative data using microdata from the U.S. National Crime Victimization Survey (NCVS), a survey of a random sample of between 49,000 and 77,000 U.S. residents, collected by the Bureau of Justice Statistics (BJS) since 1977. Residents are asked to indicate whether they have been the victim of a crime during the past six months; they are also asked whether they reported that crime to law enforcement. We use the NCVS to explore whether crime reporting changes at the age of 21 which might be true if intoxicated victims who are under the minimum legal drinking age are less likely to report a crime to law enforcement. If true, this would lead us to conclude that victimization increases at age 21 even though this effect might merely be an artifact of differential crime reporting. We report the results of this analysis in Section 5.2 and conclude that there is

[9]This includes both aggravated and simple assaults, but not sexual assaults.

[10]While specific offense types vary by city, we include the following offenses in our sexual assault aggregate: fondling, rape, sexual assault or battery and sodomy. See Appendix B for the universe of sex offenses by police department.

[11]See Appendix C for department-specific date ranges.

little evidence of differential reporting.

4 Methods

We estimate the causal effect of legal access to alcohol on victimization using a regression discontinuity design in which the effect of alcohol access is attributed to the discrete change in victimization that occurs at the age of 21. The identifying assumption is that individuals who are just below age 21 and individuals who are just above age 21 are exchangeable — that is, they do not differ, on average, with respect to both observable and unobservable characteristics.

Because all individuals are subject to the treatment age age 21, without exception, we estimate treatment effects using a "sharp" RD design. In keeping with standard empirical practice, we estimate treatment effects using the following general specification:

$$Y_i = \alpha + \tau D_i + \beta(X_i - c) + \gamma(X_i - c)D_i + \varepsilon_i \tag{1}$$

In (1), Y_i is the count of victimizations occurring on relative age i, (X_i-c) is the number of days relative to a given crime victim's birth date and D_i is an indicator variable for whether or not the criminal incident occurred prior to or after the victim's 21st birthday. The coefficient on D_i, τ identifies the causal effect of legal alcohol access. Because the evolution of victimization over the life cycle may be nonlinear in age relative to 21, in practice, we specify a model that also includes $(X_i - c)^j$ and the product of this term and D_i for polynomials of order j=2 and 3. These non-linearities could pick up numerous different factors which can affect

victimization, such as criminality which is known to vary over the life course (Loeber and Farrington, 2014), an age gradient to alcohol consumption or likelihood that an individual lives alone. Given that all of these factors shift as individuals age, we focus on on identifying the causal effect of alcohol access at age 21. As other factors directly related to victimization are unlikely to discretely change at 21, this offers a reasonable approach to estimating the causal effect of alcohol use on victimization.

Equation (1) is estimated for a given bandwidth, h so that the regression is estimated for those observations within $c - h \leq X_i \leq c + h$. All models are estimated using robust standard errors which accommodate the possibility that there is heteroskedasticity among the individual error terms within age bins. In Section 5.4, we describe a number of robustness checks which test the sensitivity of the results to alternative modeling strategies.

In addition to estimating a standard RD effect of the impact of the minimum legal drinking age, we also estimate a "birthday effect" — that is, the change in victimization risk on a victim's birthday itself or on the following weekend when an individual might celebrate his or her birthday. Estimating this effect is important for two reasons. First, it helps to ensure that our estimates of the causal effect of alcohol access are not merely due to birthday celebration effects. Second, birthday celebration effects are interesting in their own right and serve to bolster our interpretation that legal access to alcohol explains discontinuities in the probability of victimization that we document in Section 5.1.

In practice, we estimate (and control for) birthday celebration effects by adding a dummy variable to (1) that indicates whether date i was the victim's birthday or on the subsequent weekend. We show that the risk of victimization increases dramatically on a victim's 21st birthday as well as on other birthdays. This finding further bolsters our confidence that

legal access to alcohol, and the corresponding increase in alcohol consumption previously documented at age 21, is a predominant mechanism driving these large effects.

5 Results

5.1 Main Results

We study the effect of the reaching the legal drinking age separately for violent crimes (murder, robbery, sexual assault and other assaults) and property crimes (burglary, larceny and motor vehicle theft). We also estimate models separately by gender. Tables 2 and 3 present Poisson regression estimates of the effect of legal access to alcohol on victimization for males and female, respectively.[12]

In each cell, we report the incidence rate ratio (IRR) from the Poisson regression model and the robust standard error around the estimate. The first column reports coefficient estimates for the regression outlined in equation (1) using an order 1 polynomial in age. Columns (2) and (3) include second order polynomials (column 2) and third order polynomials (column 3) in age, fully interacted with an indicator variable for whether the victim's age was greater than or equal to 21. In column (4), we focus on the order 2 polynomial specification and add a dummy variable for whether an individual is victimized on his or her birthday. Recognizing that birthdays are not always celebrated on an individual's exact birthday, in column (5), we include the birth date itself and the three following days. In column (6), we include the entire week around the individual's birthday.

[12]To address the concern that results may be sensitive to our choice of a Poisson link function, we present corresponding log-linear estimates of the RD treatment effect in Appendix Tables A1 and A2. Results are substantively very similar to the Poisson regression estimates reported in Tables 2 and 3.

For males, legal access to alcohol causally increases each of violent and property victimizations by roughly 7 percent. Effects are especially large for sex offenses (12-120 percent) though these are not precisely estimated as sexual assaults with male victims are uncommon. Effects are also meaningful and significant at conventional levels for robbery (5-8 percent), non-sexual assault (6-7 percent) and larceny (3-10 percent). Effects for motor vehicle theft and burglary are positive in most specifications but are imprecisely estimated. For females, legal access to alcohol does not, in general, increase the likelihood of a violent victimization. However, sex assaults increase considerably, by approximately 25 percent. Property crimes likewise increase — by approximately 10 percent for burglary and larceny. The estimated effects are, for the most part, not sensitive to our choice of polynomial and persist regardless of how we estimate birthday effects.

The estimated effects can be seen graphically in Figure 2 which presents Poisson estimates of the age profile of victimizations with average victimization counts in fourteen-day bins for violent and property crimes, respectively.[13] Both panels in this figure provide visual evidence consistent with the results in Tables 2 and 3. As is evident from Tables 2 and 3, there are noticeable increases in sex assault and larceny victimizations for both men and women as well as non-sexual assault victimizations for men.[14]

5.2 Robustness to Differential Reporting Behavior

A natural concern in ascribing a causal interpretation to the results reported in Section 5.1 is that these estimates could be an artifact of differential reporting behavior among

[13]Figure 2 is produced using the specification from column (2) in Tables 2 and 3

[14]Using data from San Diego, Denver, St. Louis and Dallas — the cities for which we have time stamps — we explore the extent to which effects are driven by weekday versus weekend victimizations in Appendix Table 5.

individuals who have reached the age of drinking majority. This might be the case, for instance, if underage victims are less inclined to report a crime to law enforcement due to concerns about being arrested or detained as a result of their own illegal use of alcohol. Such a story is especially worrisome insofar as it could rationalize our principal finding — that victimization increases at age 21.

This differential reporting story is not possible to rule out using our administrative data as these data include only crimes that are known to law enforcement. In order to investigate the plausibility of differential reporting, we use 18 to 35 year old respondents from the 2006-2016 waves of the National Crime Victimization Survey and provide visual evidence that shows that changes in reporting behavior at 21 are unlikely to be contributing to our results. Leveraging the fact that the NCVS captures whether an individual was victimized as well as whether or not he reported a given crime to law enforcement, we observe the extent to which reporting rates change discretely at age 21. Figure 3 shows average reporting rates for crime victimization for males and females, respectively. While the estimates are noisy,[15] there is no evidence of a discrete change in reporting behavior at 21 for any of our crime types. This supports our claim that the changes in victimization at the MLDA are being driven by legal access to alcohol and not by differential reporting patterns of crime victims.

5.3 Other Robustness Checks

Columns (1) and (3) through (5) of Tables 2 and 3 show that our estimates of the effect of the MLDA on criminal victimization are relatively stable with respect to the polynomial in

[15]The NCVS records age at the time of survey, not victimization. As a result, there are likely many instances where the victim's age is mislabeled in terms of when the victimization occurred. Figure 3 was produced assuming that each respondent was the same age at survey and victimization.

age as well as with respect to differing controls for when an individual might celebrate his or her birthday. We next turn to whether results are sensitive to choice of bandwidth and we also present evidence from a powerful placebo test in which we re-estimate treatment effects for birthdays at other ages at which no change in legal access to alcohol is present.

Figure 4 shows the degree to which our results are sensitive to bandwidth selection. We estimate models with bandwidths from 180 to 730 days, in 10 day increments. In Panel A of Figure 4 we see that the RD effect on sex offense victimization attenuates as bandwidth increases for males and stays quite stable for females. The RD effect on both male robbery and assault victimizations attenuates slightly as bandwidth increases. In the case of property crimes (Panel B of Figure 4), we see that larger bandwidths dampen MLDA effect on female larceny victimizations whereas the effect on males persists throughout the range of bandwidths. Overall, the evidence suggests that results are not driven by a strategic choice of bandwidth.

Next we show that the increase in victimization that we observe at age 21 is unique and not present at other ages that are unaffected by the age of drinking majority. Figure 7 presents RD treatment effects graphically for each age between 19 and 35, using an order 2 polynomial. In the figure, age is plotted on the x-axis and the incidence rate ratio from the sharp RD regression presented in equation (1) is plotted on the y-axis. Graphs are presented for estimates that were significant at conventional levels in Tables 2 and 3. In each graph, the treatment effects cluster around an IRR of 1, indicating that there is no average treatment effect of legal access to alcohol at ages other than 21. This is exactly what we would expect given that legal alcohol access does not change at any of the other ages. Critically, in all cases, the treatment effect at age 21 is the largest among all of the ages estimated which

indicates that the RD effect at age 21 is unusual and therefore provides key support for the prior estimates.

Finally, we conduct a randomization inference exercise in order to provide further support for the estimates derived from our parametric models. We hold the observation window fixed at two years on either side of the MLDA cutoff at age 21 and randomly shuffle each observation's age relative to the cutoff. We then estimate our preferred model which uses an order 2 polynomial in age fully interacted with an indicator for age greater than or equal to 21 and no birthday effects. This model corresponds to Column (2) of Tables 2 and 3.

We perform randomization inference using 1,000 replications for each of the crime type by gender combinations for which we estimate a significant effect of the MLDA on victimization. Figure 6 presents histograms of the distributions of the t-ratios for each estimate of the RD effect with a dashed gray line indicating the 95th percentile of the distribution of randomization and a black line indicating the t-ratio for our true model. The ranking of the true t-ratio in the distribution of placebo estimates yields an implied p-value based on the simulated sampling distribution. It is evident in Figure 6 that our true t-ratio is well to the right of the 95th percentile of the distribution of randomization for all sub-plots in each panel indicating that these results are unlikely to be due to chance.

5.4 Location of Victimization

In order to better understand the mechanisms through which the MLDA affects victimization, we estimate treatment effects separately for crimes that occur in residential versus

non-residential locations — which include both outdoor and commercial locations.[16] In Table 5, we report effects for residential and non-residential crimes both with (columns 3 and 4) and without (columns 1 and 2) a control variable for the birthday effect. As in previous analyses, we further disaggregate results by crime type and gender.

For males, effects on violent victimization are driven by crimes that occur in non-residential locations. This is especially true for assaults and also, to a lesser extent, for robberies.[17] For females, the large effects for sex offenses are likewise driven by non-residential locations while effects for larceny are equally large in both location types. Taken as a whole, the data suggest that increases in victimization are, at least to an extent, driven by the fact that alcohol use is more likely to occur in non-residential settings after individuals have reached the legal drinking age.

5.5 Birthday Celebration Effects

A large literature in public health establishes that individuals are more likely to consume alcohol in both public and private on their birthdays — especially at age 21 (Neighbors et al., 2005; Brister et al., 2010). We therefore investigate whether there are "birthday effects," that is, an increase in victimization on or around an individual's birthday, independent of an intercept shift in the incidence of victimization that occurs at age 21 and endures in the ensuing weeks and months.

We estimate birthday effects by adding three different sets of birthday-related indicators to our main RD models — an indicator for an individual's birthday, an indicator for a date

[16]Location information was shared by the following 5 police departments in our subsample: Dallas, TX, Denver, CO, Houston, TX, Milwaukee, WI and St. Louis, MO.

[17]The same is true for sex offenses though sparse data means that the results are estimated with only limited precision.

that is within three days of an individual's birthday and an indicator for a date that is within one week of an individual's birthday.[18] Table 4 presents Poisson estimates of the change in the likelihood of victimization on or around an individual's birthday, an effect we describe as a birthday celebration effect and as distinct from the broader and more enduring effect of legal access to alcohol.[19]

In Table 4, the first three columns present estimates for males, the next three columns for females. Each column corresponds to a different definition of the birthday celebration window. We report incidence rate ratios and robust standard errors in parentheses. Birthday celebration effects are very large — overall, men are nearly 30 percent more likely to suffer a violent victimization and 10 percent more likely to suffer a property victimization. Effects are similar for women. Both genders are more likely to be assaulted. For women, sexual assault effects are particularly large with a 60 percent increase in the likelihood of suffering a sexual assault on one's birthday.

In order to investigate whether the birthday celebration effect is unique to age 21, we re-estimate birthday celebration effects (using the exact birthday) for all ages between 19 and 35. These estimates are presented in Figure 8, which plots incidence rate ratios on the y-axis against the victim's birthday in years on the x-axis. These figures support the idea that birthday celebration effects are not unique to age 21 and are instead universal, persisting throughout an individual's life. Indeed, celebration effects at age 21 are not of unusual magnitude and fall roughly in the middle of the distribution of estimated effects. This finding provides further support for the idea that alcohol use is a driver of crime

[18] All birthday effects are estimated with a quadratic polynomial in age interacted with an indicator for being 21 or older at the time of victimization.

[19] Appendix Table A3 reports log-linear estimates of the birthday effect.

victimization and that this effect persists beyond age 21.

6 Conclusion

A large body of research has explored the causal determinants of criminality. While victimization is an equally important side of the same coin, due to data constraints, this topic has received far less attention in the literature. Given that recent media attention and research related to criminal justice highlight the drawbacks to punishments (either through time incarcerated or fines) and over-policing, there is increasing appeal to understanding other policies which can affect crime that potentially impose fewer costs.

In this paper, we study one prominent policy lever that operates through private precaution and which could plausibly have an outsize influence on victimization: legal access to alcohol. We construct a unique administrative dataset with the exact date of birth and date of victimization for crime victims in eight large cities in the United States and use a regression discontinuity design to estimate the change in victimization that occurs at age 21, the minimum legal drinking age in the United States.

We find evidence that victimization increases at age 21 for for both males and females, though in subtly different ways. Males experience a greater number of assaults and robberies; females experience a large increase in the risk of a sexual assault. Results are robust to empirical specification, bandwidth selection and controls for birthday celebration effects. The likely mechanisms behind these increases in victimization are varied and include differences in the amount of alcohol that is consumed after reaching the legal drinking age and differences in the environment in which alcohol is consumed. Given that effects are largest in non-

residential locations, there is some evidence for the latter of these two mechanisms. Effects do not appear to be an artifact of increased reporting of crimes at age 21.

This research provides some of the first causal that alcohol increases crime victimization. Our findings suggest that prior estimates based on arrests (Carpenter and Dobkin, 2015) or criminal charges (Hansen and Waddell, 2018) likely underestimate the effect of alcohol on total crime. Our findings can also potentially reconcile the reason why regression discontinuity based estimates of arrests using the minimum legal drinking age are typically smaller than recent diffrences-in-difference estimates (Anderson et al., 2017). Finally, these findings provide additional insights into the complex and controversial relationship between alcohol and sexual assault (Lindo et al., 2018). In particular, while both Carpenter and Dobkin (2015) and Hansen and Waddell (2018) fail to find evidence that arrests or criminal charges for rape increase at age 21, we find sexual assault victimization at age 21 increases by nearly 25 percent in our preferred specifications. Taken together, these findings are more consistent with a model of crime in which perpetrators of sexual assault seek out vulnerable populations than with a model where sexual assault perpetrators lose control due to increased alcohol use.

More generally, information interventions that educate the public about its increased risk of victimization and encourage individuals to invest in private precautions to prevent victimization may help mitigate the effects of alcohol access on criminal victimization. Behavioral changes such as remaining cautious of one's surroundings, avoiding walking home alone or taking a taxi in lieu of walking, avoiding violence when faced with conflict, locking one's door immediately after returning home and being particular about the degree to which one associates with strangers while drinking all have the potential to reduce criminal victim-

ization. To be clear, we are not suggesting a campaign of victim-blaming. On the contrary, information is a means of empowering potential victims to better protect themselves.

The possibility of raising the drinking age to reduce the social cost of alcohol use is a possibility that should be taken with caution as it is unclear whether the United States' unique cultural relationship with alcohol is a by-product of its drinking age being 21.[20] As it stands, our estimates suggest that the increased consumption of alcohol at age 21 is met with additional costs previously not considered.

Moreover, the choices and precautions of individuals could carry externalities to others. As an individual engages in precautions, this has a small effect on the returns and costs to engaging in crime for potential offenders. Aggregated, this would suggest the private supply precautions would be under-supplied relative to what is socially optimal, even if we assumed individuals were privately optimizing. Thus private precautions like locks, private security cameras, alarms or GPS anti-theft trackers might merit subsidies. Moreover, this is further justification for taxes on alcohol, which have remained largely unchanged in nominal value since the 1990s and whose externality offsetting effects have likely been eroded by inflation (Cook and Durrance, 2013). Future research could investigate whether other alcohol control policies such as taxes are also effective in reducing victimization.

[20]If this is the case, a policy that raises the drinking age might have a negative general equilibrium impact on the binge-drinking culture that is common in the US.

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Table 1: Crime Rates in the Study Sample (2016)

	Study Sample	Cities > 250K Population	United States
Population	7,850,000	75,300,000	321,000,000
Violent Crimes			
Murder	14.6	9.9	4.4
Rape	62.0	49.2	26.6
Robbery	356.7	226.3	101.3
Assault	1213.8	388.3	229.2
Property Crimes			
Burglary	730.7	542.3	537.2
Larceny	2621.8	2135.0	1821.5
Motor vehicle theft	624.3	388.9	215.4

Note: Data were obtained from the compilation of the Federal Bureau of Investigation's Uniform Crime Reports made available on ICPSR by Kaplan (2019).

Table 2: Poisson Male RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	1.054***	1.069***	1.098***	1.067***	1.064***	1.068***
	(0.0165)	(0.0251)	(0.0343)	(0.0250)	(0.0241)	(0.0246)
Homicide	0.998	0.781	0.705	0.782	0.771	0.780
	(0.121)	(0.148)	(0.165)	(0.148)	(0.145)	(0.147)
Sex Offenses	1.119	1.744*	2.204*	1.753*	1.762*	1.742*
	(0.234)	(0.531)	(0.948)	(0.534)	(0.539)	(0.529)
Robbery	1.048*	1.078*	1.139**	1.079*	1.081**	1.079*
	(0.0278)	(0.0424)	(0.0585)	(0.0424)	(0.0427)	(0.0425)
Assault	1.058***	1.067**	1.083*	1.064**	1.059*	1.066**
	(0.0212)	(0.0325)	(0.0452)	(0.0321)	(0.0309)	(0.0317)
Property						
All	1.023	1.074***	1.077***	1.072***	1.069***	1.072***
	(0.0142)	(0.0222)	(0.0302)	(0.0219)	(0.0217)	(0.0215)
Burglary	1.003	1.047	0.976	1.045	1.046	1.047
	(0.0307)	(0.0493)	(0.0620)	(0.0492)	(0.0494)	(0.0493)
Larceny	1.032*	1.079***	1.108***	1.078***	1.075***	1.077***
	(0.0181)	(0.0280)	(0.0385)	(0.0278)	(0.0276)	(0.0275)
Motor Vehicle Theft	1.007	1.091	1.089	1.087	1.077	1.087
	(0.0402)	(0.0682)	(0.0914)	(0.0679)	(0.0665)	(0.0662)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Poisson Female RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	1.008	1.017	1.002	1.015	1.013	1.016
	(0.0122)	(0.0190)	(0.0251)	(0.0189)	(0.0187)	(0.0188)
Homicide	1.274	1.437	2.007	1.428	1.418	1.429
	(0.409)	(0.685)	(1.389)	(0.676)	(0.669)	(0.674)
Sex Offenses	1.170***	1.249***	1.137	1.239**	1.232**	1.246***
	(0.0649)	(0.108)	(0.137)	(0.106)	(0.103)	(0.106)
Robbery	1.014	1.034	1.015	1.033	1.034	1.034
	(0.0360)	(0.0568)	(0.0771)	(0.0570)	(0.0572)	(0.0567)
Assault	0.998	1.002	0.992	1.001	0.999	1.002
	(0.0129)	(0.0198)	(0.0258)	(0.0197)	(0.0196)	(0.0198)
Property						
All	1.036***	1.108***	1.091***	1.102***	1.098***	1.105***
	(0.0135)	(0.0216)	(0.0289)	(0.0205)	(0.0199)	(0.0203)
Burglary	1.010	1.116***	1.068	1.111***	1.108***	1.113***
	(0.0259)	(0.0424)	(0.0525)	(0.0421)	(0.0421)	(0.0421)
Larceny	1.049***	1.117***	1.114***	1.111***	1.105***	1.115***
	(0.0166)	(0.0270)	(0.0371)	(0.0255)	(0.0246)	(0.0253)
Motor Vehicle Theft	0.988	1.022	0.976	1.025	1.022	1.022
	(0.0413)	(0.0638)	(0.0803)	(0.0639)	(0.0637)	(0.0637)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on female victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Poisson Birthday Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	1.148***	1.128***	1.074**	1.167***	1.093***	1.054**
	(0.0437)	(0.0421)	(0.0341)	(0.0547)	(0.0332)	(0.0246)
Homicide	0.850	1.429	1.243	1.567	1.417	1.439
	(0.519)	(0.369)	(0.244)	(1.430)	(0.762)	(0.737)
Sex Offenses	0.637	0.777	1.045	1.661***	1.377**	1.207*
	(0.535)	(0.340)	(0.370)	(0.252)	(0.174)	(0.131)
Robbery	0.945	0.942	0.950	1.075	1.007	1.048
	(0.132)	(0.0652)	(0.0525)	(0.219)	(0.0899)	(0.0695)
Assault	1.261***	1.218***	1.131***	1.149***	1.087***	1.045*
	(0.0515)	(0.0522)	(0.0425)	(0.0434)	(0.0342)	(0.0264)
Property						
All	1.133**	1.114***	1.099***	1.368***	1.232***	1.142***
	(0.0623)	(0.0400)	(0.0278)	(0.0481)	(0.0387)	(0.0331)
Burglary	1.190***	1.031	1.042	1.329***	1.169***	1.135***
	(0.0349)	(0.0731)	(0.0613)	(0.0869)	(0.0659)	(0.0461)
Larceny	1.088	1.099**	1.086***	1.461***	1.279***	1.162***
	(0.0951)	(0.0441)	(0.0281)	(0.0585)	(0.0504)	(0.0422)
Motor Vehicle	1.281***	1.322***	1.253***	0.730***	1.002	1.005
Theft	(0.0524)	(0.110)	(0.0707)	(0.0581)	(0.0801)	(0.0722)

This table contains IRR estimates for the birthday effect on male and female victimization rates for each crime type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Poisson RD Effects – Residential vs. Non-Residential

	(1)	(2)	(3)	(4)
	No Birthday Control		Birthday Effect Control	
	Residential	Non-Residential	Residential	Non-Residential
		Panel A: Males		
Violent				
All	0.997	1.097***	0.989	1.094***
	(0.0522)	(0.0349)	(0.0500)	(0.00345)
Homicide	0.857	0.713	0.797	0.717
	(0.347)	(0.171)	(0.299)	(0.173)
Sex Offenses	0.425	4.464***	0.429	4.603***
	(0.267)	(1.944)	(0.271)	(2.024)
Robbery	1.070	1.102*	1.056	1.109***
	(0.126)	(0.0567)	(0.119)	(0.0573)
Assault	0.989	1.094**	0.985	1.085**
	(0.0545)	(0.0455)	(0.0538)	(0.0444)
Property				
All	1.058	1.073**	1.058	1.067**
	(0.0474)	(0.0305)	(0.0476)	(0.0300)
Burglary	1.070	1.260*	1.073	1.260*
	(0.0670)	(0.152)	(0.0674)	(0.152)
Larceny	1.022	1.036	1.017	1.032
	(0.0748)	(0.0367)	(0.0672)	(0.0362)
Motor Vehicle Theft	1.110	1.127**	1.111	1.124*
	(0.142)	(0.0622)	(0.141)	(0.0614)
		Panel B: Females		
Violent				
All	0.993	1.040	0.992	1.037
	(0.0287)	(0.0292)	(0.0288)	(0.00289)
Homicide	1.141	1.973	1.134	1.894
	(1.121)	(1.275)	(1.118)	(1.214)
Sex Offenses	1.030	1.324***	1.015	1.313***
	(0.149)	(0.168)	(0.145)	(0.164)
Robbery	0.986	1.025	1.000	1.022
	(0.131)	(0.0658)	(0.133)	(0.0656)
Assault	0.992	1.026	0.992	1.024
	(0.0300)	(0.0327)	(0.0301)	(0.0325)
Property				
All	1.108***	1.113***	1.098**	1.102***
	(0.0419)	(0.0307)	(0.0409)	(0.0293)
Burglary	1.109**	1.210*	1.099*	1.201
	(0.0551)	(0.137)	(0.0546)	(0.136)
Larceny	1.144**	1.131***	1.130**	1.118***
	(0.0687)	(0.0365)	(0.0667)	(0.0343)
Motor Vehicle Theft	0.934	1.034	0.931	1.031
	(0.134)	(0.0531)	(0.134)	(0.0531)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime and location type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Columns (3) and (4) include indicator variables for exact birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

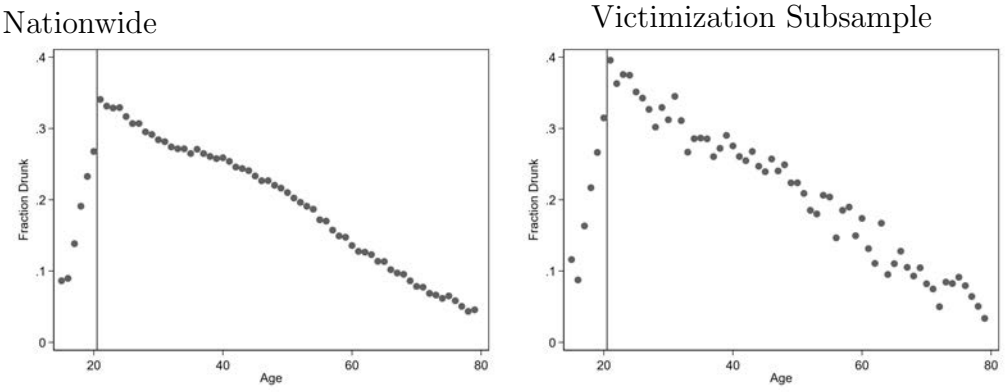

Figure 1: This figures shows that the fraction of of fatal accidents involving alcohol both nationwide, in the figure to the left, and for the subsample of cities which shared victimization data. Both series of data are based on fatal accidents from 2000 to 2016.

Figure 2: This figure contains fitted Poisson estimates and average victimization rates in 14 day bins. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

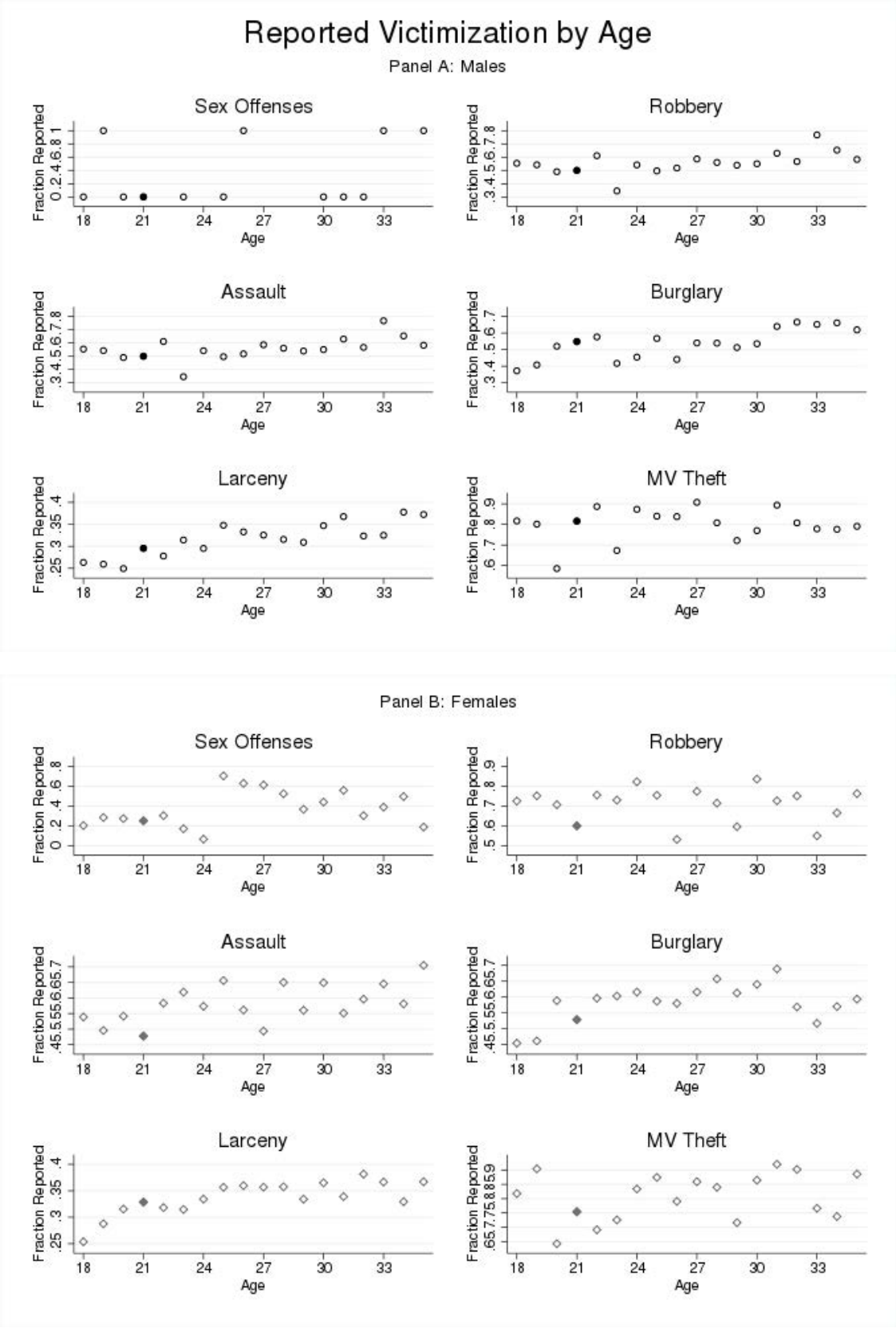

Figure 3: This figure contains average rates of reporting various victimizations to the police for respondents to the 2006-2016 waves of the NCVS.

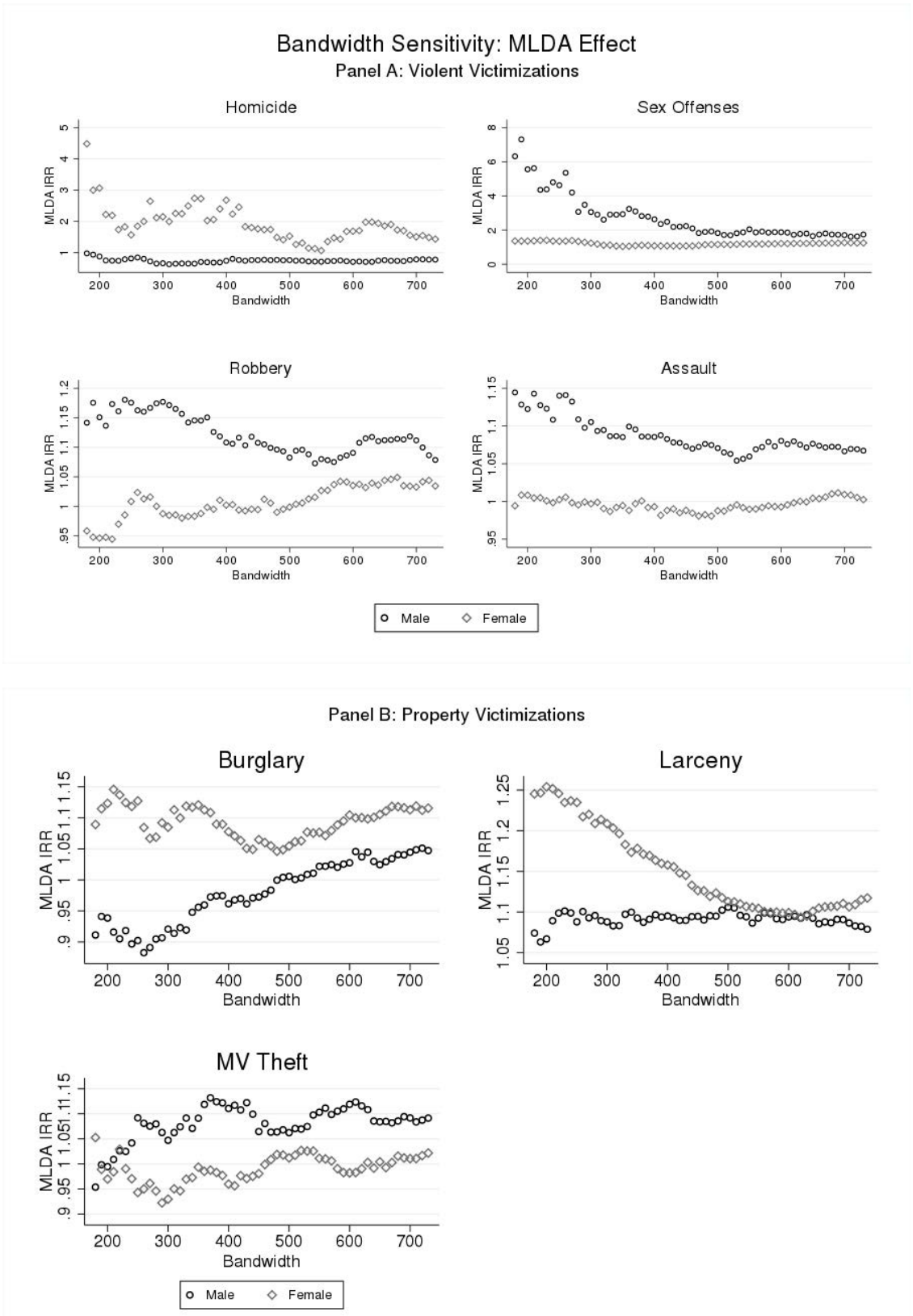

Figure 4: This figure contains IRR estimates of the RD effect of the MLDA on victimization. Regressions include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

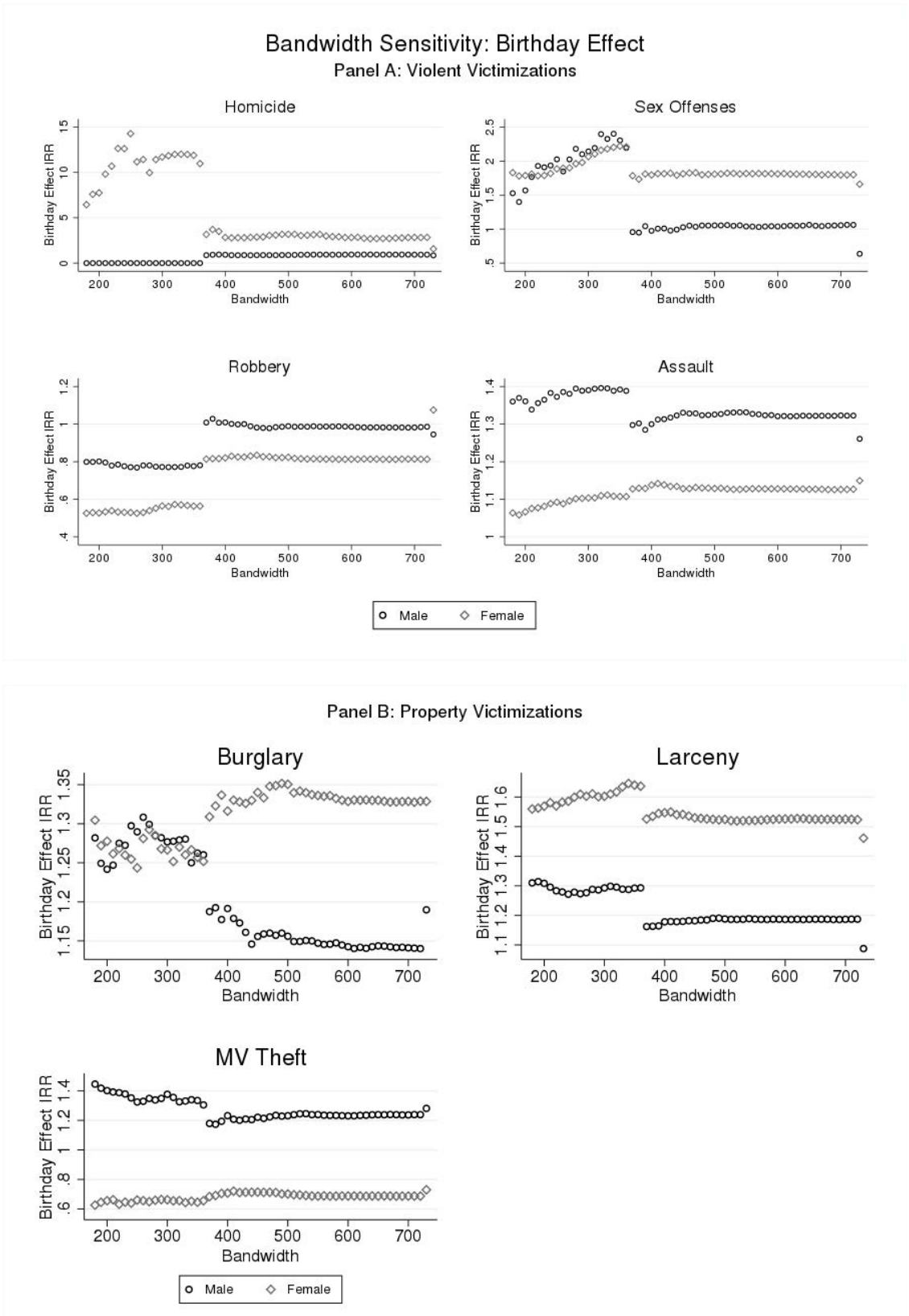

Figure 5: This figure contains IRR estimates of the birthday effect of the MLDA on victimization. Regressions include indicators for exact birthdays and a second order polynomial in age fully interacted with an indicator for age over 21.

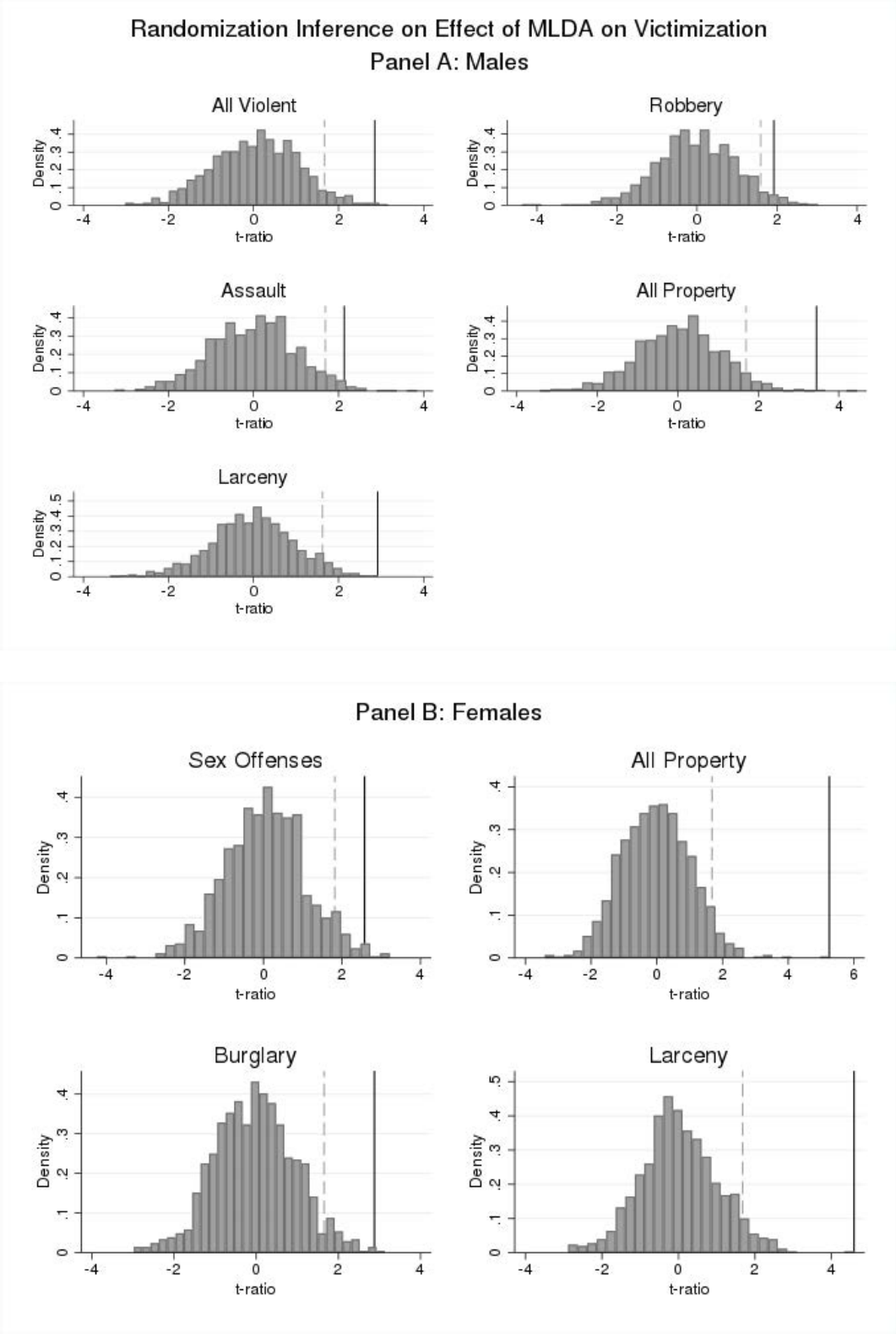

Figure 6: This histogram contains t-ratios from 1000 randomization inference replications of the effect of the MLDA on victimization. The 95th percentile of the distribution is indicated with a dashed gray line and the t-ratio from the true model is indicated with a black line.

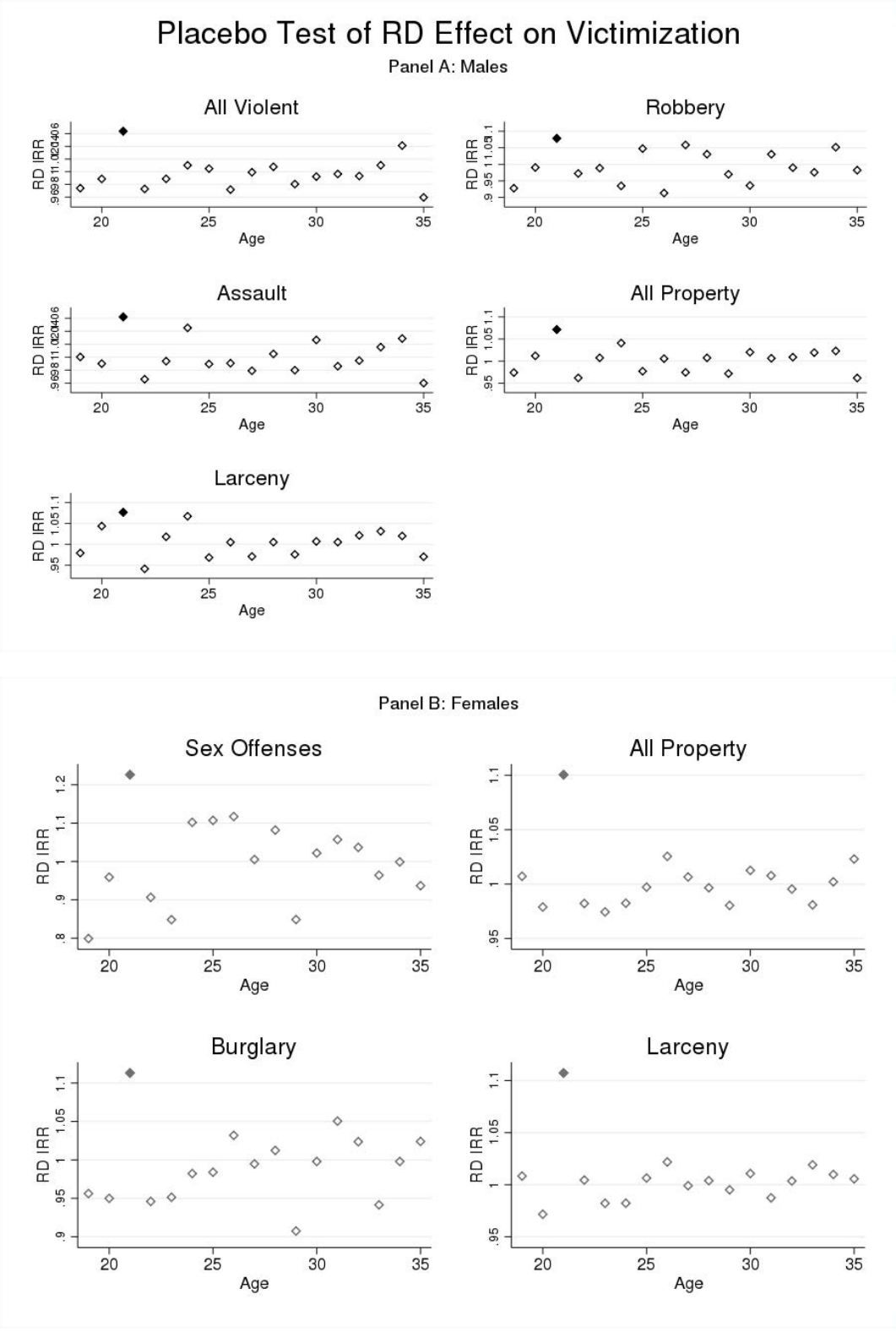

Figure 7: This figure contains IRR estimates for the RD effect of various ages on victimization. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

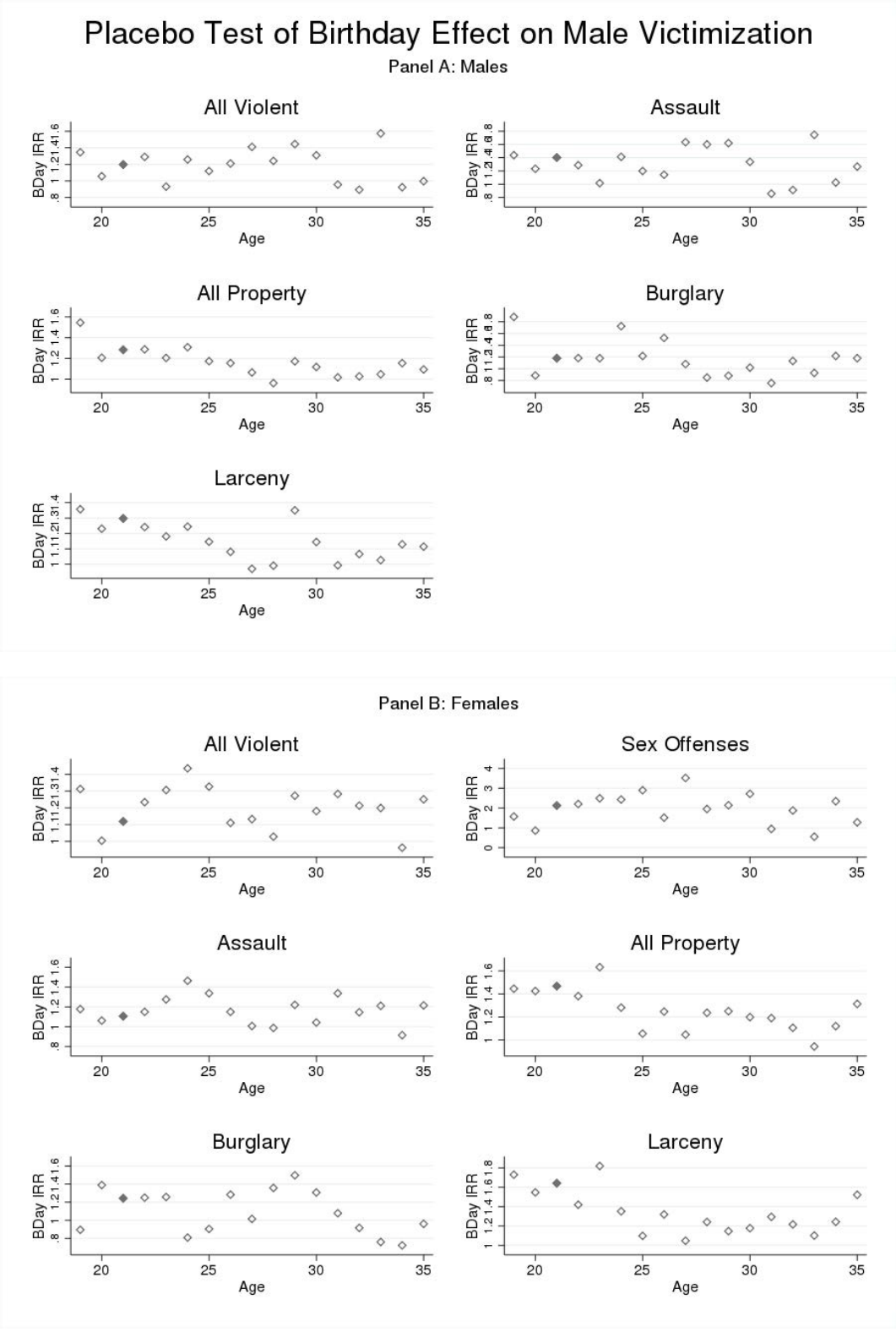

Figure 8: This figure contains IRR estimates for the birthday effect of various ages on victimization. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

Appendix A: Supplementary Tables

Table A1: Log-Linear Male RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	0.0535***	0.0699***	0.0897***	0.0681***	0.0657***	0.0691***
	(0.0158)	(0.0235)	(0.0310)	(0.0234)	(0.0229)	(0.0232)
Homicide	-0.00613	-0.0886	-0.125	-0.0880	-0.0939	-0.0896
	(0.0428)	(0.0647)	(0.0863)	(0.0648)	(0.0646)	(0.0646)
Sex Offenses	0.0154	0.0796*	0.105*	0.0803*	0.0808*	0.0795*
	(0.0298)	(0.0445)	(0.0602)	(0.0446)	(0.0447)	(0.0445)
Robbery	0.0479*	0.0764*	0.139**	0.0771*	0.0788*	0.0771*
	(0.0282)	(0.0416)	(0.0545)	(0.0416)	(0.0418)	(0.0417)
Assault	0.0619***	0.0735**	0.0789*	0.0705**	0.0664**	0.0721**
	(0.0203)	(0.0302)	(0.0407)	(0.0300)	(0.0294)	(0.0298)
Property						
All	0.0180	0.0699***	0.0776***	0.0682***	0.0663***	0.0689***
	(0.0142)	(0.0209)	(0.0283)	(0.0207)	(0.0207)	(0.0204)
Burglary	-0.0104	0.0451	-0.0200	0.0424	0.0454	0.0450
	(0.0330)	(0.0502)	(0.0676)	(0.0501)	(0.0504)	(0.0503)
Larceny	0.0263	0.0701***	0.103***	0.0687**	0.0660**	0.0690***
	(0.0183)	(0.0269)	(0.0358)	(0.0268)	(0.0266)	(0.0265)
Motor Vehicle Theft	-0.00326	0.0974	0.112	0.0931	0.0900	0.0949
	(0.0448)	(0.0708)	(0.0974)	(0.0707)	(0.0706)	(0.0698)

This table contains estimates for the RD effect of the minimum legal drinking age on male victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the natural log of the total number of victims in each age (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Log-Linear Female RD Effects

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	0.00520	0.0165	0.00191	0.0145	0.0133	0.0159
	(0.0123)	(0.0187)	(0.0252)	(0.0187)	(0.0185)	(0.0186)
Homicide	0.0132	0.0186	0.0340	0.0181	0.0176	0.0183
	(0.0175)	(0.0254)	(0.0348)	(0.0252)	(0.0251)	(0.0252)
Sex Offenses	0.174***	0.241***	0.156	0.234***	0.230**	0.239***
	(0.0589)	(0.0909)	(0.125)	(0.0905)	(0.0900)	(0.0906)
Robbery	0.00111	0.0414	0.000315	0.0408	0.0417	0.0410
	(0.0387)	(0.0591)	(0.0813)	(0.0593)	(0.0597)	(0.0592)
Assault	-0.00382	0.00276	-0.00649	0.000896	-0.000319	0.00223
	(0.0131)	(0.0198)	(0.0262)	(0.0197)	(0.0196)	(0.0197)
Property						
All	0.0326**	0.106***	0.0821***	0.102***	0.0988***	0.104***
	(0.0132)	(0.0195)	(0.0262)	(0.0189)	(0.0185)	(0.0186)
Burglary	-0.00183	0.103**	0.0567	0.0990**	0.0969**	0.101**
	(0.0278)	(0.0404)	(0.0528)	(0.0404)	(0.0405)	(0.0403)
Larceny	0.0466***	0.114***	0.104***	0.110***	0.106***	0.113***
	(0.0160)	(0.0239)	(0.0326)	(0.0232)	(0.0227)	(0.0229)
Motor Vehicle Theft	-0.00464	0.0464	-0.0222	0.0498	0.0451	0.0463
	(0.0484)	(0.0734)	(0.0974)	(0.0733)	(0.0734)	(0.0734)

This table contains estimates for the RD effect of the minimum legal drinking age on female victimization rates for each crime type. The regressions in Columns (1) to (3) include first through third order polynomials in age fully interacted with an indicator for age over 21. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21 and birthday effects 1-3, respectively. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the natural log of the total number of victims in each age (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Log-Linear Birthday Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	0.146***	0.116***	0.0671**	0.164***	0.0891***	0.0532**
	(0.0372)	(0.0349)	(0.0315)	(0.0506)	(0.0313)	(0.0232)
Homicide	-0.0465	0.147	0.0880	0.0361	0.0258	0.0263
	(0.184)	(0.123)	(0.0850)	(0.0906)	(0.0469)	(0.0436)
Sex Offenses	-0.0544	-0.0313	0.00877	0.560***	0.300**	0.147
	(0.0825)	(0.0518)	(0.0550)	(0.173)	(0.129)	(0.104)
Robbery	-0.0618	-0.0669	-0.0635	0.0459	-0.0105	0.0298
	(0.125)	(0.0704)	(0.0580)	(0.224)	(0.115)	(0.0855)
Assault	0.248***	0.196***	0.122***	0.152***	0.0844***	0.0446*
	(0.0396)	(0.0383)	(0.0363)	(0.0413)	(0.0325)	(0.0260)
Property						
All	0.147***	0.0993**	0.0868***	0.299***	0.188***	0.125***
	(0.0515)	(0.0443)	(0.0307)	(0.0492)	(0.0343)	(0.0274)
Burglary	0.221***	-0.00693	0.00644	0.297***	0.156**	0.140***
	(0.0303)	(0.0983)	(0.0702)	(0.0766)	(0.0669)	(0.0445)
Larceny	0.113	0.111***	0.0923***	0.368***	0.221***	0.138***
	(0.0790)	(0.0395)	(0.0277)	(0.0519)	(0.0434)	(0.0347)
Motor Vehicle	0.349***	0.202	0.206**	-0.283***	0.0357	0.00772
Theft	(0.0611)	(0.131)	(0.0818)	(0.0648)	(0.0867)	(0.0790)

This table contains estimates for the birthday effect on male and female victimization rates for each crime type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Birthday 1 includes indicator variables for exact birthdays. Birthday 2 includes indicator variables for exact birthdays and the following three days. Birthday 3 includes indicators for the week around each birthday. Each observation is the natural log of the total number of victims in each age (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Poisson Birthday Effects – Residential vs. Non-Residential

	(1)	(2)	(3)	(4)
	Male		**Female**	
	Residential	Non-Residential	Residential	Non-Residential
Violent				
All	1.248**	1.090	1.058	1.141*
	(0.114)	(0.0844)	(0.0547)	(0.0889)
Homicide	1.278	0.941	–	–
	(1.125)	(0.833)		
Sex Offenses	–	–	1.691**	1.377
			(0.353)	(0.336)
Robbery	1.526	0.846	0.786	1.231
	(0.508)	(0.156)	(0.456)	(0.190)
Assault	1.170	1.255***	1.044	1.108
	(0.150)	(0.0983)	(0.0570)	(0.0954)
Property				
All	1.031	1.142*	1.343***	1.316***
	(0.0772)	(0.0826)	(0.0861)	(0.0567)
Burglary	1.191**	0.751	1.272***	1.243
	(0.0947)	(0.292)	(0.0709)	(0.462)
Larceny	0.880	1.120	1.476***	1.471***
	(0.138)	(0.149)	(0.185)	(0.0710)
Motor Vehicle Theft	0.852	1.281***	1.162	0.831***
	(0.182)	(0.0619)	(0.190)	(0.0557)

This table contains IRR estimates for the birthday effect on male and female victimization rates for each crime and location type. All regressions include second order polynomials in age fully interacted with an indicator for age over 21 and indicator variables for exact birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Missing estimates are due to sparse outcome data. Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Poisson RD Effects – Weekend vs. Weekday

	(1)	(2)	(3)	(4)
	No Birthday Control		Birthday Effect Control	
	Weekday	Weekend	Weekday	Weekend
		Panel A: Males		
Violent				
All	1.088	1.162***	1.091	1.163***
	(0.0582)	(0.0600)	(0.0582)	(0.0601)
Homicide	0.598	1.153	0.597	1.167
	(0.258)	(0.444)	(0.258)	(0.450)
Sex Offenses	2.004	1.217	1.968	1.184
	(1.037)	(0.703)	(1.020)	(0.686)
Robbery	1.109	1.118	1.116	1.122
	(0.111)	(0.120)	(0.111)	(0.120)
Assault	1.086	1.179***	1.087	1.179***
	(0.0681)	(0.0699)	(0.0682)	(0.0700)
Property				
All	1.105**	1.087	1.106**	1.088
	(0.0463)	(0.0602)	(0.0464)	(0.0604)
Burglary	1.049	0.942	1.049	0.946
	(0.0954)	(0.123)	(0.0955)	(0.124)
Larceny	1.125**	1.138*	1.127**	1.138*
	(0.0653)	(0.0820)	(0.0655)	(0.0823)
Motor Vehicle Theft	1.117	1.099	1.116	1.101
	(0.0927)	(0.115)	(0.0928)	(0.115)
		Panel B: Females		
Violent				
All	1.029	1.051	1.026	1.052
	(0.0383)	(0.0449)	(0.0382)	(0.0450)
Homicide	2.163	2.966	2.185	3.023
	(2.069)	(2.992)	(2.092)	(3.057)
Sex Offenses	1.075	1.299*	1.072	1.286
	(0.171)	(0.205)	(0.171)	(0.204)
Robbery	0.996	1.260*	0.997	1.264*
	(0.133)	(0.172)	(0.133)	(0.172)
Assault	1.028	1.006	1.026	1.008
	(0.0405)	(0.0470)	(0.0403)	(0.0471)
Property				
All	1.126***	1.151***	1.127***	1.152***
	(0.0438)	(0.0543)	(0.0439)	(0.0544)
Burglary	1.086	1.109	1.084	1.105
	(0.0897)	(0.119)	(0.0897)	(0.119)
Larceny	1.203***	1.258***	1.203***	1.259***
	(0.0644)	(0.0791)	(0.0645)	(0.0792)
Motor Vehicle Theft	1.003	0.951	1.008	0.956
	(0.0812)	(0.0957)	(0.0815)	(0.0963)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime type by weekend versus weekday. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Birthday 1 includes indicator variables for exact birthdays. Weekend victimizations occur from Friday at 8 PM through Monday at 6 AM. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B: Sex Offenses, by Police Department

Charlotte-Mecklenburg: forcible rape, forcible fondling, forcible sodomy, sexual assault with object

Dallas: rape, sex offenses and indecent conduct

Denver: harassment - sexual in nature, sex aslt - fondle adult victim, sex aslt - fondle child, sex aslt - fondle-child by pot, sex aslt - non-rape, sex aslt - non-rape pot, sex aslt rape, sex aslt - rape pot, sex aslt w/ object, sex off incest, sexual exploitation of child

Houston: other sex, rape, sex offenses

Kansas city, mo: forcible fondling, forcible rape, forcible sodomy, sexual assault with an object

Milwaukee: ejaculation, forcible fondling, forcible rape, forcible sodomy, sexual assault with object

San Diego: act in concert to commit rape w/foreign object, aggravated sexual assault of a minor with a foreign object, aggravated sexual assault:minor under 14 and 10+ yrs younger, assault w/intent to commit rape/other sex acts, assault with intent to rape, assault with intent to rape in commission of 459, attempted rape, burglary/unspecified, continuous sexual abuse of child, crime against nature/sodomy not specified, oral cop:victim unconscious or asleep, oral copulation, oral copulation / victim unconscious of the nature of the act, oral copulation by force or fear, oral copulation in concert: victim incapable of giving consent, oral copulation w/person under 16, oral copulation w/person under 18 years, oral copulation: victim intoxicated/etc, oral copulation:minor under 14 10+ years younger, oral copulation:victim unaware act occurred, oral copulation:victim under 10 years of age, rape, rape by fear or force, rape by threat of retaliation, rape by threats to use authority of public official, rape of drugged victim, rape of spouse by force/fear/threat, rape of spouse unable to resist: under controlled sub/etc, rape of spouse under controlled sub/etc, unable to resist, rape of spouse unable to resist: under controlled sub/etc, rape spouse by force/fear/etc, rape where victim is incapable of giving consent, rape/etc in concert with, orce/violence, rape/etc in concert with force/violence:minor 14 yrs or older, rape: force/fear/etc., rape: spouse unconscious of nature of act, rape: victim believed person is spouse, rape: victim believes person is spouse, rape: victim drugged, rape: victim incapable of consent, rape: victim unconscious of nature of act, rape:victim unconscious of the nature of the act, sex penetration:foreign obj/etc victim unaware:nature of, sex penetration:foreign obj/etc:victim unconscious/asleep, sex penetration:victim unaware act occurred, sexual battery, sexual battery as defined in this section, sexual battery involving restrained/institutionalized person, sexual battery of restrained or incapacitated person (f), sexual battery of restrained or incapacitated person (m), sexual battery on institutionalized person, sexual penetration by threat of retaliation victim/etc, sexual penetration w/ foreign object w/ force, sexual penetration w/force/etc 14 years or older, sexual penetration w/force/etc under 14 years old, sexual penetration w/foreign object w/victim under 18 yrs, sexual penetration w/foreign ob-

ject w/intoxicated victim, sexual penetration w/foreign object w/victim under 16 yrs, sexual penetration w/foreign object w/victim under 18 yrs, sexual penetration w/foreign object: vic believes is spouse, sexual penetration w/foreign object:threat by auth to arrest, sexual penetration w/foreign object; victim incapable confined, sexual penetration w/foreign object; victim incapable of consent, sodomy by force or fear, sodomy by force/violence/fear, sodomy by force/violent/fear victim 14 yrs of age or older (f), sodomy w/person under 18 yrs, sodomy/concert/force, sodomy/victim unconscious of the nature of act, sodomy:minor under 14 10+ years younger, sodomy:victim under 10 years of age, sodomy:victim under influence anesthetic/etc/any control s, sodomyw/o consent: drugged victim defendant in mental fa, touch person intimately against will for sexual arousal/e, unlawful sexual intercorse w/minor: 3 yrs old or younger, unlawful sexual intercourse / victim under 18, unlawful sexual intercourse w / minor 18, unlawful sexual intercourse w/minor: more than 3 years old, unlawful sexual intercourse w/minor: perp 21+ victim -16

St. Louis: forcible fondling, forcible rape, forcible sodomy, human trafficking - commercial sex acts, human trafficking, commercial sex acts, sex offenses - forcible fondling, sex offenses - forcible sodomy, sex offenses incest, sex offenses - statutory rape

Appendix C: Sample Period, by Police Department

We obtained data from the following municipal law enforcement agencies for each of the following time periods:

- Charlotte-Mecklenburg, NC: 1/1/2008 - 12/31/2017
- Dallas, TX: 1/1/2007-12/31/2017
- Denver, CO: 1/1/2008-12/31/2017
- Houston, TX: 1/1/2007 - 12/31/2015
- Kansas City, MO: 1/1/2007-4/26/2018
- Milwaukee, WI: 1/1/2007-12/31/2017
- San Diego, CA: 1/1/2008-12/31/2017
- St. Louis, MO: 1/1/2007 - 12/31/2017